The History of Science in a World of Readers

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The History of Science in a World of Readers

Dagmar Schäfer and Angela N. H. Creager (eds.)

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History of Science in a World of Readers: Frames of References for Global Exchange

Dagmar Schäfer and Angela N. H. Creager

The idea for this collection was born on a foggy day in March 2015 during a scholarly retreat on the outskirts of Berlin. A photo of a reader sitting in a crowded bookshop in Shanghai, China, holding the 1990 Chinese translation of Joseph Needham's introductory volume, *Science and Civilisation*,[1] elicited a lively debate about the purpose, themes, and reception of translations in the globalizing discipline of the history of science, technology and medicine.[2] What role should historians of science have in communicating their own body of literature—its methods and concerns—across linguistic boundaries?

This anthology, published in both English and Chinese, is an initial response to that debate, reflecting a wish to counteract and complement both market-driven and individual efforts with a collective reflection on some of the influential literature in this field published in English since 1990. The Max Planck Institute for the History of Science (MPIWG), Berlin, and the History of Science Society (HSS) organized a selection committee including representatives from six other societies—the American Association for the History of Medicine (AAHM), the British Society for the History of Science (BSHS), the Division of the History of Science and Technology of the International Union of History and Philosophy of Science and Technology (DHST/IUHPST), the European Society for the History of Science (ESHS), the Society for the History of Technology (SHOT), and the Society for the Social History of Medicine (SSHM). Twelve chapters were selected for translation into Chinese based on an online poll that ran between October and December 2015. This version features seven chapters in their original form, as the other five were unavailable for publication due to copyright restrictions.[3]

The selection committee reviewed, evaluated, and ranked an initial shortlist of 78 articles before deciding on the final list. Given how geographically dispersed English-language historians of science are, this arrangement enabled the committee to use current global technologies to crowd-source candidate publications while ensuring an appropriate balance of articles on science, technology, and medicine over a range of time periods and regions, as well as a diverse group of authors.

We believe that translations—in the linguistic sense of the word—serve an important function as a scholarly practice in internationally competitive research, which is characterized by an increasing number of diverse, multilingual actors on the one hand, and an increasing tendency towards a hegemonic linguistic approach on the other.[4] Literary translations, in contrast to collaborative monolingual publications (often produced by authors from varied

[1] Needham ([1954] 1990).
[2] If not otherwise indicated, history of science is hereinafter used to include history of technology and history of medicine.
[3] Rosenberg (1992); Kohler (1999); Blair (2003); Galison (2003); Green (2008).
[4] Gordin (2015, 219). See also the statistics of the European Union on translation activities (https://www.ceatl.eu/current-situation/translation-statistics).

language backgrounds), make explicit their negotiations over content, format, and meaning. Agreements, compromises, and misunderstandings are laid bare, as decisions have to be made about which word best represents another, and which socio-cultural associations and historical or political contexts need to be taken into account to make the meaning clear.

Historians have assigned translations an important, though not always unambiguous, role in the historical dynamics of scientific development—especially in discussions of exchanges between China and "the West" (Europe and North America). The research focus has shifted from the expansion of "Western science" to one of multilateral effects. For instance, the story of the Chinese Euclid, once solely emphasizing how the Jesuit Matteo Ricci (1552–1610) taught Xu Guangqi (1562–1633) Western science, has turned into the study of a reciprocal process that affected various European groups as much as it affected the Chinese and other East Asian actors. While early studies once concentrated on the transmitted contents of books and their reception, Harold Cook and Sven Dupré have recently emphasized the hugely important historical role of translators and their roles as "brokers" and "go-betweens." Bettina Dietz has shown how translators' one-off situational choices turned into terms that others legitimized by usage: in this way, Linnaean botanic terms have developed into a global standard since the twentieth century.[5] Thus, the translator's expertise matters.

Such insights also resonated with the selection committee while producing this volume. Special attention was given to the historical dynamics that shaped word choices and conceptual frameworks. By offering an expansive index, this volume also acknowledges the historical insight that the

> scientific term is not formed at once, every word or better say every new thing is constructed gradually using works of predecessors. … [T]here are some terms which are invented in special fields and it [the scientific understanding and meaning that this term now carries] will take time to become popular and common among all experts.[6]

Each article translated from English into Chinese underwent several reviews by experts with multiple language skills from the relevant fields. In its production, the volume illustrates a point which the growing discipline of translation studies has repeatedly emphasized—that translation is far more than a simple act of transmission or transfer: its interpretative character and effects cannot be ignored.[7] Over the last two years, conversation among the various participants has generated new ideas and forged new scholarly connections. Moreover, the selection exercise stimulated individual and joint reflections about how the history of science, technology, and medicine has evolved and where it is heading. We hope that these productive debates will continue and the publication will help advance methodological developments in the globalizing field of the history of science.

This introduction which was compiled in English first, reviews historical research on translations in the fields of science, technology, and medicine, and inquires into its role in the history of Chinese-English exchanges to illustrate some of the challenges and opportunities impacting international historical research. Then the selected works are contextualized within the broader landscape of Anglophone research in the history of science, technology, and medicine.

[5] Yu-lan Fung 馮友蘭 (1983); Engelfried (1998); Blue, Engelfried, and Jami (2001); Brockey (2007); Cook and Dupré (2012); Dietz (2016).

[6] Tabrizi and Pezeshki (2015, 1173–1174).

[7] Holmes (1988).

Translations, the History of Science, and East-West Exchange

Ever since the late 1980s, scholars and politicians in the People's Republic of China have been advocating the need to improve the general public and academics' understanding of both historical dynamics (*lishi gan*歷史感) and scientific change. This agenda is also part of the general education of scientists, a contemporary initiative which no longer has an equivalent in the US or most European universities but a generation ago was also ubiquitous there.[8]Historians themselves are increasingly branching out to provide an inclusive global view in their research, both regionally and with regard to time frames. As curricula are rethought, scholars are also beginning to analyze the historical dynamics that shape their methodological apparatus.

The beginnings of historical research into the sciences in China were characterized, as in most twentieth century nation-states, by a focus on China itself. From Ding Wenjiang 丁文江 (1837–1936) and Zhang Zehong 张资珙(1904–1968) to Joseph Needham (1900–97), twentieth century scientists were also historians who transferred scientific content not by itself, but supplemented with science histories (or stories) in order to illustrate how Western scientific knowledge was created. Often they exemplified ideals and modes of "modernization" in their prefaces and accompanying reports.[9] By the 1920s some English histories of science had already been translated by Chinese students studying abroad who translated their teachers' work into Chinese. One example of this is the contribution of Chu Cho-ching (竺可楨), founder of the Annual of the History of Science.[10]

Over the following decades, a confluence of individual interests and arbitrary encounters generated a rather motley selection of translated historical studies by Western writers, such as Florian Cajori, *A History of Physics* (1899), Edwin A. Boring, *A History of Experimental Psychology* (1929), J.R. Partington, *A Short History of Chemistry* (1937), W.C. Dampier, *A History of Science and its Relation to Philosophy and Religion* (1948), J.D. Bernal, *Science in History* (1954), and Morris Kline's compendium of *Mathematical Thought from Ancient to Modern Times* (1972).[11]

Most of these works remained in academic hands and brought heroic stories of Western scientists and sciences to China, thus transmitting viewpoints that the Anglophone community itself had begun to question and effectively deconstruct at the end of the Cold War era, a time when the frequency of international exchange increased.[12] Importantly, as many

[8]Shen Xianjia 申先甲 (1991, 306–291). See also Jiang Qian 蒋茜，Li Xinxin 李欣欣，Shen Xianjia 申先甲 (2013). Liu Bing 刘兵 (2011). For instance, he asserted that scientific education would lack stimuli if it did not impart students with a knowledge of history. Letter from D. K. Djang [?Chang Tzu-kung/Zhang Zigong] (1945) at Christ's College, Cambridge about his work on translations for a "Collection of Essays on History of Chinese Astronomy and Calendar Making" and other matters and related correspondence, SCC2/3/9.

[9]Li Yuese 李约瑟 (1975, 23).

[10]For an overview of science translations, see Li Nanqiu 黎难秋 (2000). Translators or authors often contextualized the works historically in their prefaces, while historical arguments were often part of scientific tracts at that time.

[11]For reasons of space, it is not possible to provide a comprehensive list here. Translations often targeted physics, chemistry, mathematics, and astronomy, such as Zhang Zigong 张资珙 (1952).

[12]Most of these works have been republished many times. For example, Dai Nianzu's 戴念祖 translation of Florian Cajori's *A History of Physics* has been published by at least three publishers: (1981, 2002, 2010); Gao Juefu's 高觉敷 translation of E. G. Boring's *A History of Experimental Psychology* was published four times: (1935, 1981, 2009, 2011); Li Heng's 李珩 translation of William Cecil Dampier's *A History of Science, and its Relations with Philosophy and Religion* was published twice: (1989, 2009); the translation of John Desmond Bernal's *Science in History* from Wu Kuangfu 伍况甫 et al. was also published twice: (1959, 2015); the translation of Morris Kline's three-volume *Mathematical Thought from Ancient to Modern Times* was published three times: (1979, 2002 , 2013).

scholars have discussed, the "scientific revolutionist," historian, and philosopher of science Thomas S. Kuhn (1922–1996) gained renown in the Asian academic community, first in Taiwan, Japan, and Korea, and then among Chinese scholars.[13] Shigeru Nakayama coined the term "scientific community determinism" for the perspective emerging in Asia during the post-Kuhnian era.[14] Marta Hanson explains how historians of China in both the East and the West considered Kuhn to be offering a "non-Eurocentric approach to science, the concept of normal science." This approach worked for scientific practice in the West as well as in East Asia, and Kuhn's concept of the paradigm opened up research on the mutually-constitutive basis of scientific communities and scientific knowledge.

Since the 1990s, we can see that historical research in China and the West has paid increasing attention to the role of linguistic translation in scientific exchange. The discipline of translation studies has evolved across the globe simultaneously, creating a sustained academic interest in Asia, Europe, and the Americas and thereby reviving the reputation of an almost lost scholarly practice.[15] For instance, by the turn of the millennium, several groundbreaking historical studies on translation and scientific exchange in China and across East Asia were published in English. In an early discussion of scientific translation in Meiji-Japan, Scott Montgomery observed that "most people do not appreciate" the complexity of translation and its role in scientific exchange. Later in the same year, David Wright foregrounded translation's profound impact on the written language, generating or resuscitating scores of "new" characters in nineteenth-century Chinese chemistry.[16]

Around the same time, historians, philologists, and linguists in China and worldwide have expanded their views on the act of translation and its impact on scientific research itself across the globe.[17] Lydia Liu, for instance, emphasized its importance as "a reciprocal process of negotiating 'meaning-value' " and its role as "a primary agent of token making in its capacity to enable exchange, producing and circulating meaning as value among languages and markets."[18]

Although we find similar actors and stakeholders in these studies, the nature of their engagement shifted in the 1970s and 1980s, once the academic community in China more obviously began to promote the circulation of high-impact (Western science) writing, as demonstrated by the story of the early translation of Thomas Kuhn's writings. Since the opening of its markets and borders in the late 1990s, research into non-Chinese sciences, medicine, and technology has gathered pace in Chinese research, while at the same time the number of Chinese translations on such historical research published outside China has also increased. More recent examples of this trend are Steven Shapin's *The Social History of Truth* and Peter Burke's *Social History of Knowledge*.[19]

Also, early- and mid-twentieth century interventions and state institutional support—both in China and the West—often targeted monumental compilations such as Joseph Needham's edited series, *Science and Civilisation in China*, or Morris Kline's compendium of

[13]Wu Guosheng 吴国盛 (2012, 2005a, 2005b).
[14]Hanson (2012, 561–505).
[15]Cook (2010).
[16]Wright (2000, 246).
[17]Chinese academia is spearheading the field of translation studies.
[18]Liu (2000).
[19]Shapin (2002); Burke (2016). The translation of John Pickstone's *Ways of Knowing: A New History of Science, Technology, and Medicine* illustrates that readers' interests were considered important, see Pickstone (2008). The authors themselves also initiated translations of their works.

Mathematical Thought compiled by the Beijing University Faculty of Mathematics, Department of History of Mathematics, Translation Group. In the current decade, such efforts have also been made by individuals such as Zhang Butian 张卜天. Currently associate professor at the Chinese Academy of Sciences, Zhang was educated in thermal engineering and majored in physics before deciding to follow a doctorate program in the philosophy of science and then become a specialist in the academic translation debate. To date, he has produced more than thirty books in his *Translation Series on the Origins and Development of Science*, published by Hunan kexue jishu chubanshe, among others.[20] In 2016 he initiated a new series offering translations of classic Western scholarship on Chinese sciences: *The Grand Titration, Science and Society in East and West*, originally published by Joseph Needham in 1969.[21] Although his work includes Ancient Greece and the Middle Ages, it focuses on the sixteenth and seventeenth centuries, the period at the heart of debates about a "scientific revolution," concentrating on the continents of Europe, Africa, and Asia. It is Zhang's aim to introduce the history of science into China from the viewpoint of contemporary sciences, using historical experience as a means to critically analyze Western modernity as it appears in China.

Hence, while Western studies on China's sciences as well as some studies and overviews of science history outside China have been translated into Chinese, and while the landscape of history of science research studies translated from Western European languages (English, Latin, French, German, etc.) is rich, it remains the product of a somewhat arbitrary course of transmission. The choice of subject matter is defined by the serendipitous conjunction of academic collaborations, individuals' interests, and market forces, while, as of yet, there is a dearth of collective reflection on the field, the discipline's current position, its methodological choices, and the major and minor research questions and significant topics that could be interesting for curricula building and for furthering international scholarly exchange.

A Western View of the Development of Science, Technology, and Medicine

This anthology aims to offer Chinese readers a taste of intellectual influences in the history of science from the viewpoint of literature published in English. To that end, it is worth noting the major trends among English-speaking historians of science, technology, and medicine pre-1990. Thomas Kuhn's *The Structure of Scientific Revolutions* is usually acknowledged as the single most influential work on almost all subsequent histories of science.[22] Kuhn argued that the development of science was not cumulative and progressive, but rather discontinuous and reliant on the social organization of scientists into communities with shared beliefs. From this perspective, scientific knowledge was organized into "paradigms," which depended on key examples to illustrate how theories explained natural phenomena. According to this theory, anomalous findings that cannot be explained by these paradigms create a crisis for the community of practitioners, some of whom may try to resolve the problem by proposing a new paradigm. For example, the shape of the spectrum of black-body radiation was an anomaly for classical physics—at the highest frequencies, radiant energy is low, not infinite. Max Planck proposed the solution that electromagnetic radiation was emitted in

[20] For example, Grant (2010); Burtt (2012).
[21] Needham ([1969] 2016).
[22] Kuhn (1962).

quanta, and there could be no radiation below one quantum of energy. In this way, Planck contributed a key conceptual element to the new paradigm of quantum mechanics. The acceptance of any new paradigm entails a rejection of the old, even if some features of the earlier mode of explanation can be translated into the terms of the new paradigm. In this sense, knowledge is lost as well as gained when the paradigm shifts.

Kuhn's book received fierce criticism from philosophers and scientists, who felt that it presented scientific change as "irrational" (Kuhn referred to the shift from one paradigm to another as a matter of "conversion"), and milder criticism from historians who thought that the kind of dramatic discontinuities his theory posited—revolutions—could not account for the numerous incremental changes in scientific knowledge.[23] Kuhn himself moved towards a more evolutionary understanding of science later on in his career. Today, few (if any) scholars would still try to apply Kuhn's *Structure of Scientific Revolutions* to their historical case studies, but his underlying depiction of science as resting on communities of like-minded practitioners engaged in puzzle-solving and transmitting their approaches and methods through textbooks and teaching permeates the literature from the 1970s. It still remains a theoretical touchstone, but it also served to catalyze other influential schools of thought.

In the late 1970s and 1980s, the sociology of scientific knowledge, especially as articulated by British scholars in Edinburgh and Bath (such as Barry Barnes, David Bloor, and Harry Collins), extended Kuhn's sense of scientific communities, arguing that scientific knowledge reflected the broader social and economic interests of its creators.[24] Classic examples of this scholarship are the depictions of phrenology and eugenics as representations of certain social and class interests in Victorian and Edwardian England.[25] In the history of medicine, this kind of reference to social history would mean taking patients' views into account, as well as the sociology of the professionalization of physicians and other healthcare workers.[26] In the history of technology, this exploded the traditional view, which had focused on the world of design and production (usually featuring male engineers and entrepreneurs), to include the activities of users as well—including workers, consumers, and, consequently, women.[27] Ruth Schwartz Cowan argued that users shape how technologies develop by adopting or rejecting them at the "consumption junction."[28]

This new attention to the "social" in science, technology, and medicine became ever more localized in the 1980s and 1990s, focusing on particular spaces and institutions. A forerunner of this trend was Bruno Latour and Steve Woolgar's *Laboratory Life* (1979), which articulated a new focus on the actual sites of production of scientific knowledge.[29] Along similar lines, Karin Knorr-Cetina emphasized the artificiality of laboratory conditions as well as the wide differences in experimental practices among the various branches of science (physics and molecular biology, for example).[30] Steven Shapin and Simon Schaffer's *Leviathan and the Air-pump* (1986) offered a view of the scientific revolution that centered on a specific apparatus—the early vacuum pump—to argue that both live demonstrations of

[23] Gutting (1980); Daston and Richards (2016).
[24] Barnes (1974); Bloor (1991); Collins (1985).
[25] MacKenzie (1976); Shapin (1979).
[26] Porter (1985); Numbers (1988).
[27] Bijker, Hughes, and Pinch (1987); Oldenziel (1999).
[28] Schwartz Cowan (1987).
[29] Latour and Woolgar (1979).
[30] Knorr Cetina (1983, 1999).

experimental findings and their documentation through written publications served as key ways to establish credible knowledge. Scientific disputes, notably those among members of the Royal Society of London, could be settled using these methods, in part due to their shared gentlemanly culture.[31] In presenting this picture, Shapin and Schaffer argued that the methods developed for resolving scientific disputes and authenticating reliable knowledge resulted in a lasting separation of (English) civil society into politics on the one hand (represented by Thomas Hobbes) and learning on the other (represented by Robert Boyle).

Presenting an instrument as the focus of *Leviathan and the Air-pump* inspired other scholars to think about the importance of experimentation and its key technologies. After this, it was no longer sufficient to situate scientific knowledge in terms of broad class interests or social beliefs; scholars reconsidered the significance of materiality in order to understand how knowledge was created and extended. There remained—and remains—a lively debate about how best to account for the relationship between material agency and social realities in the worlds of science, technology, and medicine. The essays in this volume reflect that ongoing conversation. They are also indicative of the increasingly close integration, both methodological and institutional, of the history of science with general history. With their detailed attention to contexts beyond the narrowly scientific, almost all of the articles chosen for the reader strongly reflect this trend.

This Collection

The articles in this collection were all published between 1990 and 2015, and are presented according to date of publication rather than grouped by subfield or theme. Each responds to the earlier literature outlined above and the key problems it identified. For example, since *Leviathan and the Air-pump* was published in 1985, scholars' widespread focus on knowledge as *local* rather than universal has raised the question of how scientific theories and findings travel. This provides the central problem in James Secord's essay, "Knowledge in Transit." Secord suggests that scientific communication, including translation, has a fundamental impact on how knowledge is generalized. Other authors in this anthology also focus on the transmission of scientific knowledge, especially—since antiquity—through written texts. Reviel Netz's "Deuteronomic Texts" and Ann Blair's "Reading Strategies for Coping with Information Overload" show how learned readers in the early modern West canonized certain texts through updated editions, commentaries, note-taking, and excerpts. These methods of book history enrich the traditional tools of intellectual history by enabling scholars to trace the reception and transmission of knowledge at the level of individual texts and readers (often through written marginalia). Looking at different spheres of learning (mathematics and humanism), Netz and Blair show how scholars wrestled with the issue of establishing the reliability of knowledge given that an over-abundance of texts and theories had accumulated by the early modern period. Pamela Long and Peter Galison also look at the transmission of scientific knowledge through texts, but focus on authors, openness, and credit. Pamela Long's "The Openness of Knowledge" examines the emergence of printed technical manuals about mining and metallurgy in sixteenth-century Germany and Italy, which codified and transmitted craft knowledge. This information was not only useful for miners but also for investors. Even more significantly, these manuals' authors began

[31] Shapin and Shaffer (1985).

asserting the importance of *open* knowledge, which became an enduring scientific value. Galison's "The Collective Author" examines publications from a very different time and place—particle physics in the late twentieth century. Experiments in this field involved literally hundreds of scientists and engineers, so assigning credit through authorship was complicated. When a finding resulted from the collective work of 500 researchers, who should be named the discoverer? Was there any single knowledgeable creator? Galison suggests that both the conventional understanding of scientific epistemology and the responsibility of authorship have been fundamentally reshaped by this kind of collective scholarly activity.

Another way to move beyond the local nature of knowledge is to examine specific moments when new knowledge and its applications cross borders, to ask *why* certain technologies are adopted or exported. Two articles look at the national and geopolitical purposes behind the development and transfer of new technologies. Gabrielle Hecht's "Political Designs" examines how the development of French nuclear reactors—even down to the level of specific design choices—was shaped by the politics of postwar (and Cold War) reconstruction. Her nuclear case offers a convincingly positive riposte to Langdon Winner's classic question, "Do Artifacts have Politics?"[32] In "Peasant Friendly Plant Breeding," Jonathan Harwood considers the development of the Green Revolution through the US Rockefeller Foundation's programs for promoting science and technology in under-developed countries. The end result of the Green Revolution was the preference for large-scale industrialized agriculture in Mexico (and elsewhere in the developing world), at the expense of the needs and viability of small-scale farmers. Harwood shows that the Rockefeller Foundation officers on that project understood that subsistence farmers had distinct needs and constraints, but ultimately prioritized farmers who possessed the capital to adopt high-tech agricultural methods, to purchase agrochemicals and high-yield seeds. Hence, Harwood's analysis unearths continuities with the earlier period of colonial expansion, when European science and medicine was exported to colonies for their modernization and development, often at the expense of indigenous systems of knowledge and production.

Warwick Anderson's "The Possession of Kuru" also examines a colonial situation in Papua New Guinea, Australia's territory. Two teams of biomedical researchers worked there to identify the cause of a previously-unknown neurological disease, kuru, which was a common cause of death among the Fore people (including youths)—and thus of concern to the colonial administration. Anderson places special emphasis on the role of specific materials—such as human brain specimens—as particularly valuable objects in the encounter between scientists and the native peoples. He draws on notions of "gift exchange" from economic anthropologists (such as Marcel Mauss) to account for the social negotiations that enabled these materials to change hands. This explanation is particularly significant because the people of Papua New Guinea provided key ethnographic evidence that anthropologists used in positing this pre-capitalist means of exchange. In Anderson's hands, "gift exchange" provides another way to account for the way that science and its materials circulate. A closely related idea in the recent history of science literature is that of "moral economies" of science, which refer to the practices, norms, and expectations that characterize a discrete community or collaborative network of scientists. A prominent example of the approach in this anthology is Robert Kohler's article, which looks at how fly geneticists in the US developed habits of sharing strains and scientific credit during the early decades of the twentieth century. Kohler

[32] Winner (1980).

focuses on the renowned early-twentieth century biologist Thomas H. Morgan, whose laboratory contributed to the birth of genetic studies, particularly through its techniques for mapping genes to chromosomes through breeding experiments with visible mutations (such as white eyes). Kohler shows that Morgan's small group produced many more mutants than they could map themselves, leading to a culture of sharing strains with outside scientists and teachers in the expectation of reciprocal sharing of information and scientific credit. For Kohler, this way of doing science was not simply a product of American culture or genetic work habits, but derived simply from the fecundity of the fruit fly, a "breeder reactor" in the production of new mutants. His emphasis on the fruit fly's biological features—which, through genetic inbreeding and standardization, became a laboratory technology—reflects a broader interest in the role of the *agency* of experimental materials in science. Kohler's work on the fruit fly laid the groundwork for an entire genre of further historical studies of "model organisms" in biomedical research.

Charles Rosenberg's essay, "Framing Disease," is also a response to the new materialism of the early 1990s and the associated move away from the social constructivism of the 1980s. For Rosenberg, the notion of "framing" brings together several facets of how medicine operates: the initial biological experience, the encounter with a physician who identifies and treats a disease, and the collective making of the meaning around that disease. Social negotiation is part of each step, so that, for example, diagnosis is as much about the narrative of illness and expectations of a patient as about the scientific tests and expert judgment involved. Yet framing disease also enables historians to take into account the bodily, experiential nature of sickness, the particularity of disease entities, and the ways in which biomedical knowledge is employed by physicians.

The other two articles in the volume address longstanding assumptions about modern science, technology, and medicine. Monica Green's essay, "Gendering the History of Women's Healthcare," challenges the belief that knowledge about female reproduction was in the women's domain in the West before the Scientific Revolution. This idea was popularized during the 1970s, as feminists challenged male medical control of female bodies (through their professional dominance of obstetrics and gynecology). The notion that a previous "golden age" existed when female health was controlled and managed by and for women led many writers to overlook the more complex historical realities. Green argues against the assumption that only women possessed knowledge about female reproduction, using her extensive knowledge of gynecological treatises from the medieval and early modern periods to show that both men and women participated in this world of practical learning. Her contribution exemplifies how historians have drawn on feminist and gender theory to produce enhanced accounts of scientific and medical knowledge. For her part, Francesca Bray challenges the idea that technology should refer to the machines and products of industrial capitalism. In her elegant essay, "Technics and Civilization in Late Imperial China," she argues that the domestic shrine in medieval Chinese homes functioned as a key social technology. In this, she not only expands the realm of technology to include domestic objects, but also demonstrates how methods used in the history of technology can be effectively applied to pre-modern history, as well as to non-Western societies. She also shows how materiality imposes and reasserts values of knowledge categories along gender lines, binding the everyday to the female and profane and the universal to the scholarly patriarch. To fully understand the development of technology and medicine, Green and Bray remind us, one must pay attention to the users of knowledge as well as to learned texts. They also illustrate the attention to objects and materials that characterizes much of the best recent scholarship in the history of science, technology, and medicine—a theme highlighted above. These two essays, as well as Secord's "Knowledge in Transit" and Rosenberg's "Framing Disease," offer in-depth analyses of the major scholarly debates over the past few decades in the specific subfields of the history of medicine, history of technology, and history of science. Readers who are particularly interested in using these overviews to orientate themselves to the English literature that this anthology represents would do well to begin with these chapters.

By bringing together this collection of influential articles, we do not mean to suggest that Western science serves as a unitary sphere of knowledge. Indeed, the premise behind much of the scholarship presented in this volume is that natural knowledge is locally produced, and thus it is important to explain (rather than presume) the apparent universality of science and its applications. These articles represent very different fields, times, and places, and those particularities are critical for understanding each historical episode. That this contextualization has become axiomatic in the history of science is indicative of its integration into general history.

Nonetheless, there is an overall intellectual coherence to the anthology, diverse as its topics are. The studies in this volume demonstrate that knowledge (of sciences, technologies, and medicine) and its transmission are an integral part of social and cultural ideas and of the sociological imagination. The remarkable development of scientific results and technical achievements is not just the product of social-cultural dynamics, but also a contribution to the socio-technical development of diverse human societies. As the West and the East become ever more closely related through travel, trade, and—not least—the globalization of knowledge, we hope that these essays will stimulate new engagements between English and Chinese readers about the centrality of science, technology, and medicine to our histories and future.

Conclusion

Since the 1950s, the history of science, technology, and medicine has undergone several phases, internationally as well as in the Sinophone world. In a recent review of historical research on Post-Republican mathematics in China, Liu Qiuhua 刘秋华 identified the year 1976 as a watershed. In this interpretation the period from 1947 to 1976 then focused on Chinese classical mathematics, distinct from the period afterwards, when the new open-door policy enabled researchers to discover the modern era and began to publish in foreign languages. He himself supported an interpretation of modernity as beginning in 1904. Liu situates a substantial shift again in the 1990s, when foreign exchange increased and Chinese scholars increasingly published in European languages. Looking to the future, he identifies an urgent need to collate historical materials from the modern period (i.e., post 1900) and to internationalize this work.[33] This periodization also signifies changing attitudes towards the translated historical research on scientific change in China and elsewhere, approaching such works less as a tool of transmission than as one that facilitates a globally diverse research culture with new comparative methods.

In 2013 Lynn K. Nyhart described a fictitious meeting set in 2038, predicting a future where "the increasing outward looking of China, where the history and philosophy of

[33] Liu Qiuhua 刘秋华 (2011, 92).

Western science have long formed a compulsory part of the science and technology curriculum, has engendered closer interactions with technology-oriented institutions in North America."[34]

The collective endeavor of assembling this volume reveals that such conversations, multiplying in an increasingly internationalized community of scholars, have long surpassed Nyhart's dichotomous view of Chinese Western relations. Translations help both sides to develop new methods and train new researchers in innovative fashions. Like all collections, our selection of the chapters includes some biases. The decision to concentrate on English as the source language and Chinese speakers as the targeted community was a pragmatic one, which we hope will lead to a further series of translations providing up-to-date information on methodological and topical developments in the history of science, technology, and medicine across language boundaries. We are already preparing a corresponding volume of Chinese publications on the history of science translated into English.

Selection Process

A general invitation to nominate possible articles was disseminated over academic email-lists in France, Germany, the US, and the UK in October 2015. Over the following two months, members of the seven participating societies (in fact, anyone who wished) could nominate one article for each of the three sections: history of science, history of medicine, and history of technology. Each person could participate only once. The poll suggested a list of journals as a starting point but any other journal or edited volume could be added. The original article had to be published in English. In the first half year of 2016, the committee met in online conferences to discuss the selection of the 12 articles from 219 nominations and divided the process into five steps: 1) each committee member selected the 10 "most influential" articles. 2) Each committee member could select three additional articles to be included in the selection; they were then assigned to three more committee members for reading; an article would qualify for round 3 if at least one person voted for it. 3) Published works of committee members were excluded from the list for reasons of fairness. 4) The committee members graded all articles. 5) The committee discussed topic balance in the final score and selected the finalists.

Committee Members

- David Beck (University of Warwick)
- Angela N. H. Creager (HSS)
- Christopher Cullen (DHST/IUHPST)
- Lorraine Daston (MPIWG)
- Yao Dazhi (SHOT)
- Olga Elina (Institute for the History of Science and Technology, Russian Academy of Sciences)
- Florence Hsia (University of Wisconsin)
- David S. Jones (Harvard University)
- Jürgen Renn (MPIWG)

[34] Nyhart (2013).

- Dagmar Schäfer (MPIWG)
- Carsten Timmermann (SSHM)
- Hans-Jakob Ziemer (MPIWG)

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Chapter 1
The Openness of Knowledge: An Ideal and Its Context in 16th-Century Writings on Mining and Metallurgy

Pamela O. Long

The scientific societies of the 17th century emphasized the importance of openness for scientific methodology. A significant goal was to facilitate communication among appropriate persons interested in the new experimental philosophy.[1] My underlying presupposition in this article is that the explicit endorsement of openness in the natural sciences and its association with empiricism were significant "events" in intellectual history and in the development of scientific methodology. Openness was by no means universally accepted as an approach to empirical knowledge in the early modern period. Practitioners within the highly respected discipline of alchemy, for instance, usually endorsed esoteric transmission to a small group of initiates. How then did the opposite value of openness become so central to the stated methodology of experimental philosophy? Herein I suggest that a particular group of 16th-century authors on mining and metallurgy made an important contribution to such a viewpoint. I further argue that the views expressed in their writings emerged from a social and economic context that shaped authorship in very specific ways.

This article constitutes a study of the practice of authorship, not the practice of science or technology per se. Openness as a stated value and openness in practice can be two very different things. A clearly written treatise is open only to those who are literate and can read the particular language in which it is written. A craft procedure can be described in writing, but often it is truly accessible only to those who have practiced the technique with their own hands. Steven Shapin's recent essay on experiment within the Royal Society underscores the point that openness can be a highly complex matter, that it can depend on differences between private and public space, on degrees of access, and on the social status of participants.[2] Alice Stroup's work shows that the ideal of openness sometimes conflicted with secrecy and exclusionary practices in the Parisian Royal Academy of Sciences.[3] Recent investigations

[1] See Middleton (1972, 91) where the academy's statement of purpose includes the hope that others would be encouraged to repeat experiments "with the greatest rigor"; that they wished "for nothing else but a free communication from the various Societies"; that, when members repeated the experiments of others, they "always cited the authors"; and, finally, that, from the first days of the society, they had always shared the experiments with anyone passing through who wanted some account. For the Royal Society, Henry Oldenburg (1665, 1–2) noted that there was "nothing more necessary for promoting the improvement of Philosophical Matters, than the communicating to such, as apply their Studies and Endeavours that way, such things as are discovered or put in practise by others… To the end, that such Productions being clearly and truly communicated, desires after solid and usefull knowledge may be further entertained, ingenious Endeavours and Undertakings cherished, and those, addicted to and conversant in such matters, may be invited and encouraged to search, try, and find out new things, impart their knowledge to one another, and contribute what they can do to the Grand design of improving Natural Knowledge, and perfecting all *Philosophical Arts*, and *Sciences*."

[2] Shapin (1988).

[3] Stroup (1990, 199–217).

and controversies illustrate the point that the ideals of both openness and accurate credit of authorship within science are sometimes very far indeed from the realities of scientific practice.[4]

Nevertheless, the development of the explicit ideal of openness within the empirical sciences is worthy of study in its own right. The belief that knowledge should be openly transmitted in writing belongs to a complex tradition that originates in antiquity.[5] I treat here only one part of that larger history. Implicit in my discussion are two claims. The first is that writings on the practical and mechanical arts are important sources not only for the history of techniques, but for intellectual history as well. The second constitutes a revision of the traditional view that associates science with openness and technology with secrecy in the premodern era.[6]

1.1 Mining and Authorship in the 16th Century

The 16th century was the great age of mine and metallurgical literature both in terms of quantity and originality.[7] I have found it useful to divide these writings into three separate categories—recipe books, alchemical writings, and exoteric mining and metallurgical treatises. In making these divisions I impose what I consider to be a useful typology. However, it is important to emphasize that the categories are overlapping to some extent, and that there is significant diversity within each. It is the third group, exoteric treatises, with which this article is primarily concerned.

Recipe books, the first category, are often referred to as books of "secrets," or Kunstbüchlein. They contain recipes for assaying and separating metals, as well as for other procedures such as dyeing and mixing medicinal remedies. Recipe writings belong to an ancient tradition that continued to flourish in the 16th century and beyond. The printing press gave particular impetus to their production. Although some medieval examples of this genre, such as the *Mappae Clavicula,* contain limited evidence for craft secrecy, it should not be assumed that books of "secrets" necessarily contained secrets. Rather, they outlined well-known techniques and recipes usually as mnemonic aids to practitioners.[8]

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[4]See especially Hull (1988); Nelkin (1984), and *Science, Technology, & Human Values* (La Follette 1985), an issue devoted to openness and secrecy in science and technology.

[5]A useful discussion is Eamon (1985a). However, I differ with Eamon both in his placement of the origins of the concept of scientific openness in the early modern period and in his overly comprehensive identification of the medieval period with secrecy. Key early texts elaborating the ideal of openness are the Roman architect Vitruvius's *De architectura* (n.d., 3.preface 1–3, and 7. preface 1–18); and the 12th-century monk Theophilus's treatise (1961, 1–4).

[6]See Solla Price (1975, 117–35); and McMullin (1985), both of whom associate science with openness and technology with secrecy.

[7]Useful discussions of this literature and its context include Koch (1963, 19–59) for the 16th century; Baumgartel (1965) and Wilsdorf (1954). For the technology background, see Bromehead (1956) and Forbes (1956); Smith and Forbes (1957); Tylecote (1987). Molloy (1986) is particularly useful for some of the older, relatively inaccessible works on the history of German mining.

[8]The pioneering bibliographical work on the pamphlet tradition was done by John Ferguson (1882, 1883, 1886a, 1886b, 1888, 1890, 1894, 1909, 1911, 1912). (These articles are collected in a reprint edition; see Ferguson (1959).) His work was furthered by Ernst Darmstaedter (1926). For English translations of three of the booklets with useful notes, see Grünhaldt Sisco and Smith (1949) and "On Steel and Iron: The Anonymous Booklet, 'Von Stahel und Eysen…' (Nuremberg, 1532)" (1968). See also Eamon (1977; 1985b; 1979). Also see Paisey (1980). For references to secrecy in the Mappae Clavicula, see Smith and Hawthorne (1974, 28, 31, 32, 35). The genre is

The second group, alchemical writings, formed a tradition that had originated in late antiquity. Alchemy overlapped with craft traditions, particularly those of the goldsmith trade, and it developed its own laboratory techniques for processing metals and other substances. It also was imbued with a complex group of religious and philosophical ideas from the ancient Near East. In the 15th century, influenced by Ficino's Neoplatonism, it enjoyed a surge of popularity and would remain a respected art until the 18th century. Here it is sufficient to emphasize alchemy's view of transmission as an esoteric process, in which an authority transmitted alchemical knowledge to a few initiates usually within an apprenticeship relationship. The cryptic writing of the alchemists is well-known as a method whereby alchemical knowledge was hidden from the uninitiated. Alchemical authorship could be hidden as well. The real author of all alchemical writings was considered to be the ancient Egyptian god Toth. The attribution of alchemical books to the highest authority was a customary practice.[9]

A third type of mining and metallurgical book, the more formal exoteric treatises and pamphlets, appeared for the first time in the 16th century. Although these books were indebted to both recipe writings and the techniques of alchemy, they were distinct from both traditions. They include pamphlets such as the *Bergbüchlein* on ores and the *Probierbüchlein* on assaying; elaborate treatises such as the *Pirotechnia* by the Sienese Vannoccio Biringuccio, the famous *De re metallica* by the humanist Georgius Agricola, and the *Treatise on Ores and Assaying* by Lazarus Ercker; and less well-known works such as Ercker's pamphlets on assaying and on minting, the books on assaying by Ciriacus Schreittmann, Modestin Fachs, and Samuel Zimmermann, and, finally, the *Schwazer Bergbuch*.[10]

The authors of these books came from varied backgrounds. Some, like Biringuccio and Schreittmann, were practitioners. Others, like Calbus of Freiberg (author of the *Bergbüchlein*) and Georgius Agricola, were university-educated physicians and humanists. Moreover, the books themselves are diverse in physical form. Some were printed. Others, such as Lazarus Ercker's early pamphlets and the *Schwazer Bergbuch*, were hand copied. Yet, as I shall elaborate, authors from different backgrounds expressed similar views concerning authorship and openness. They also shared a common context that included the early modern capitalist expansion of mining.

Exoteric mine and metallurgical writings represent a flowering of technical authorship that demands inquiry beyond the "explanation" that they were written for artisans or the suggestion, partially true but insufficient, that they were a by-product of the printing press.[11] Further questions need to be addressed. Who were the authors? What backgrounds did they come from? What motivated them to undertake technical authorship? And, finally,

represented today by such household manuals as "Hints from Heloise," and, now as then, the "secret" to, say, removing a particular kind of stain refers to the details of a technique more than to hidden knowledge as such.

[9] On the relationship between alchemists and assayers, see Halleux (1986). For an introduction to alchemy and the large bibliography on the subject, see especially Halleux (1979) and also Eliade (1978, 142–68) on initiation and secrecy; Holmyard (1957, 153–64) on signs, symbols, and secret terms); and Multhauf (1966).

[10] The three major treatises and their respective English translations are as follows: Biringuccio (1977; 1959); Agricola ([1912] 1950; 1556); Ercker (1960; 1951). The other treatises mentioned and further bibliography are specified in the footnotes below.

[11] Elizabeth Eisenstein (1979, esp. vol. 2, 520–635) has elaborated in detail the importance of the printing press for technical and scientific literature. Although the press is obviously of paramount importance for any printed work, it constitutes only a partial explanation for the dissemination of technical literature. As will be elaborated, mining and metallurgical literature was published only in certain geographic areas. Moreover, there were a significant number of manuscript treatises written (and sometimes copied) but never published in the 16th century.

who constituted their patrons and prospective audience? What has emerged is that these 16th-century mining and metallurgical books seem to have been among the products of a European mining boom. In terms of authorship, patronage, prospective audience, and explicit attitudes, they can best be understood in the context of particular developments in late medieval mining.

Well before 1350 European mining had reached a peak of productivity. Thereafter, metal production began a decline that was to last for more than a hundred years. The catastrophic plague that swept through Europe between 1348 and 1350 decimated the population by one-third to one-half and left many mines abandoned. A rapid recovery was inhibited because efficient exploitation of existing mines required greater depth. But deeper mines presented engineering difficulties involving water and ore removal, difficulties not solved in the early 15th century. Then the devastation of the Hussite Wars (1419–34) between the Holy Roman Emperor Sigismund and the followers of John Huss in Bohemia brought to stagnation the most productive mines of Europe —those of Bohemia and Saxony.[12]

Gradually, as the population achieved some recovery in the first half of the 15th century, demand for metals needed for both specie and guns exceeded the supply. The shortage made mining and metallurgy a profitable business, providing the motivation to solve technical and organizational problems that had stultified late medieval mining. The result was a central European mining boom. Between 1460 and 1530 the production of silver, copper, and other metals in central Europe and elsewhere increased several times over, sometimes fivefold. Expanded production brought with it rapid changes in technology and organization. Deeper mines, more costly to construct and operate, necessitated greater outlays of capital. As John U. Nef described it, these developments caused a striking cleavage between capital and labor. Small cooperative groups of miners were replaced by wage earners paid increasingly by the absentee shareholders who provided needed capital and also reaped profits. Sharing the wealth were princes and others who held regalian rights over the land. Miners lost most of the special privileges that had been granted by princes and overlords in the 12th and 13th centuries.[13]

On the other hand, wealthy investors with little specific knowledge of mining and metallurgy and holders of regalian rights both became ready patrons and consumers of mining literature. Authors of mining and metallurgical books wrote for these rulers and other wealthy investors who wanted to maximize the productivity of their mines, as well as for the expanded number of new practitioners whose skill in prospecting, mining, and processing metals provided the key to the profits of their employers. Local craft knowledge transmitted orally no longer sufficed for a far-flung group of literate but inexperienced investors. Princes and others seeking wealth from mining bestowed their patronage on individuals who were able and willing to explain mining and metallurgical practices in writing. Technical authors often obtained rich rewards from these patrons. The belief in the openness of knowledge and its written transmission, in fact and as an ideal, was an important by-product of the commonality of interest among wealthy investors and the authors of mining and metallurgical books.

[12]See Miskimin ([1969] 1975, 112–15).

[13]Two good general accounts are Kellenbenz (1976, 79–88 and 106–18) and Nef (1987). The fundamental study of capital investments in German mining is Dietrich (1958; 1959; 1961). For Goslar, see also Schmidt (1970). The best single introduction to more recent research on German mining is Kroker and Westermann (1984). Articles in this collection particularly relevant to the general developments discussed herein include Gleitsmann (1984); von Stromer (1984); Ludwig (1984). For technical innovation in mining and metallurgy during this period, see Braunstein (1983); Molenda (1988); Suhling (1980; 1984; 1978).

Early modern mining and metallurgical literature was thus fueled by the rapid expansion of mine investments—an essential aspect of the growth of industrial capitalism in the first half of the 16th century. Mining of precious metals such as silver and gold, of other metals such as copper, tin, lead, and iron, and of substances such as saltpeter, an essential ingredient of gunpowder, and alum, needed to stabilize dyes used in the textile industries, had long existed in many parts of Europe, including France, Italy, Sweden, Poland, and England.[14] Yet working mines by no means led invariably to writings on mining and metallurgy. Indeed, the great majority of 16th-century mining books were written by Germans in the regions of the empire where the capitalist transformations of mining were most pronounced —the Harz Mountains near Goslar, the Erzgebirge Mountains in Saxony and Bohemia, and the Tyrolian Alps to the south.[15]

The most important exception to the rule of the German provenance of 16th-century mining literature, the Italian treatise by Biringuccio, was written by one who had visited mines in the empire and was exhorting his compatriots to expand their investments in Italian mining in imitation of the Germans.[16] Historians of mining and metallurgy agree that the great age of mining literature was over by the end of the 16th century.[17]

1.2 Authorship and Audience in Two Early Pamphlets

The author of the first printed book on mining, the anonymously published *Bergbüchlein*, is accepted on 16th-century evidence as Ulrich Rülein von Calw, known as Calbus of Freiberg (d. 1523). Calbus studied medicine at the University of Leipzig and then became the town physician of Freiberg, a great mining boomtown in Saxony. He was a mathematical practitioner who assisted with the site planning and measurement of two new mining towns, Saint Annaberg and Marienberg. He was active in the city government of Freiberg and as *Bürg-*

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[14]For France, see Benoit and Braunstein (1983); Braunstein (1987; 1984); Gille (1947); Hesse (1986); Laube (1964). For Italy, see especially Braunstein (1965; 1977). See also Menant (1987) and Tucci (1977). For Sweden, see Svanidze (1981). For Poland, see Molenda (1984; 1985). For England, see Donald (1955); Gough (1967); Hamilton (1967); Hatcher (1973); Lewis ([1908] 1965); Penhallurick (1986). See also Kellenbenz (1977); Multhauf (1978); Richards (1983); Delumeau (1962); Jenkins([1936] 1971); Singer (1948); Sprandel (1968); Westermann (1971a).

[15]The large specialized bibliography on mining in the empire includes the following: Wilsdorf and Quellmalz (1971), suppl. 1 of Prescher (1955–1974), in which the work by Wilsdorf and Quellmalz is an encyclopedia of German mining organized by region and place-name; Wächtler and Engewald (1980); Kellenbenz (1981); Westermann (1986). For the Harz, see Bornhardt (1927; 1931); Boyce (1920); Brüning (1926); Henschke (1974); Kraschewski (1984); Rosenhainer (1968); Westermann (1971b). For the Erzgebirge region and Bohemia, see Wagenbreth and Wächtler (1986); Laube (1974); Sieber (1954, 1–75). For the Tyrol, see Egg (1958); Palme (1984); Reichsritter von Wolfstrigl-Wolfskron (1903); Worms (1904).

[16]For some of Biringuccio's German travels and observations, see Biringuccio (1959, 20, 48, 93–94, 110, 144, 166, 431). Also of Italian provenance was the late 16th-century Latin treatise on mineralogy by Mercati (1717). Mercati was a physician whose papal service included supervision of the Vatican botanical garden. He arranged his treatise as if in the same order as the specimen cases of the Vatican collection of minerals and fossils. The work was not published until the 18th century. See especially Accordi (1980) and Premuda (n.d.).

[17]See Sisco and Smith (1951, xiv–xv); and Koch (1963, 60). Much of the 17th-century literature is derivative, with the important exception of the Spanish treatise on metals by Alvaro Alonso Barba, published in 1640. Barba wrote his book in the context of the mine boom enjoyed by the Spanish crown in the new world. See Barba (1923); Smith (n.d.); and Barnadas (1986), which supersedes all previous accounts and adds significantly to our knowledge of Barba's life. For a discussion of the larger context, see Cross (1983).

ermeister he helped to establish a humanist Latin school. He was also a miner (i.e., a mine investor) at various locations and enjoyed considerable prosperity as a result.[18]

The dialogue form of the *Bergbüchlein* provides clues about Calbus's prospective audience. Daniel, "the mining expert" (Saint Daniel was a patron saint of miners), elaborates his knowledge of ores to Knappius, a young miner. Writing to comply with Knappius's "frequently expressed wish and. . .persistent request," Daniel bases his information on "the books of ancient philosophers and the experience of practicing miners." The illustrated pamphlet treats the birth and growth of ores such as silver, gold, tin, copper, iron, and lead through the influences of the heavenly bodies and the ways that those ores could be discovered, including the possible directions of their veins and stringers. Knappius is described as an investor in mining, for whom the book will provide useful information on how to locate potentially productive veins and on the characteristics of metals. Thus, as Knappius himself notes, "I shall be given a reasonable understanding which mines can be worked gainfully so that my investment will not be wasted but will show a profit."[19]

The interlocutor, Daniel, insists on the close relationship of knowledge and practice and emphasizes that his general principles must be applied with great skill to particular cases. Such advice was highly appropriate for a potential investor in need of practical knowledge. Knappius agrees that to become an expert he would need practice. The young miner's lack of knowledge is evident from his question concerning the divisions of a mine, which he assumes to be determined by location rather than by percentages of the mine's yield. Daniel, having enlightened his student on this score, admonishes him not to mind if the book "uses simple words and unpolished phrases." They convey something useful, which should be valued more than the "smoothness of words."[20] Clearly, Knappius the miner is not an artisan but an uninformed potential investor who stands in great need of useful, practical mining knowledge in order better to realize profits.

A second metallurgical pamphlet, the *Probierbüchlein*, is an anonymously authored work on assaying. It consists of a group of recipes for testing metals that suggest a preexisting collection. Perhaps recipes used by a practicing assayer were later organized (whether by a printer or assayer is unclear) for publication.[21] Evidence from various editions suggests an audience of both practitioners, including those concerned with minting and coinage, and individuals with an interest in mine operations. The title asserts that the work was "compiled with great care for the benefit of all mintmasters, assay masters, goldsmiths, miners [meaning, we can assume, mine investors like Knappius], and dealers in metals."[22]

Some editions of the *Probierbüchlein* contain an anonymous dedication to one Hans Knoblach, an administrator of the Harz Mountain mining operations of Elizabeth, duchess

[18]For a facsimile and transcript of the first edition of the *Bergbüchlein*, as well as a detailed discussion and documentation of the life of Calbus, see Pieper (1955). See also Darmstaedter (1926, 13–24); and, for an English translation and further discussion, Sisco and Smith (1949, 17–56); and Mendels (1953).

[19]Daniel replies that knowledge of the generation of metals was most important, but "as a mere side issue" profits should not be spurned. Yet, if "his aim is solely and predominantly profit and gain" rather than knowledge about minerals, it would "cheapen and condemn this little book and the art." If one really values profits more than art, one will have to do without both (Sisco and Smith 1949, 17–19). This prohibition against unadulterated avarice was a nicety that would be dropped in subsequent mining literature wherein large profits are repeatedly invoked as the chief incentive for investment in mining.

[20]Sisco and Smith (1949, 19).

[21]See Darmstaedter (1926, 25–36); and Sisco and Smith, (1949, 157–78 for the editions and 179–90 for technical content).

[22]Sisco and Smith (1949, 70).

of Braunschweig (Brunswick) and Lüneburg. Duchess Elizabeth (1435–1520?), the widow of Duke William the Younger of Wolfenbüttel, was a key figure in the renewal of iron mining and the introduction of steelmaking in the upper Harz. Her efforts, which brought economic prosperity to the entire region, led to her being eulogized as, among other things, *inventrix metallorum*. The dedication to the booklet on assaying informs us that Elizabeth's mine administrator Knoblach had encouraged the unknown author to publish his collection of information on the assaying of ore, which he had gathered "from writings and from his own experiments."[23] Clearly the active promotion of mining and metallurgy and the encouragement of technical authorship went hand in hand.

1.3 Biringuccio: Advocate of Openness and Investment in Mining

Far more ambitious than these German pamphlets was the Italian treatise *Pirotechnia* by the Sienese Vannoccio Biringuccio (1480–ca. 1538), which was published in 1540, after the author's death. Biringuccio wrote with remarkable freshness and self-confidence, largely from his own practical experience.[24] His expertise is evident in the technical descriptions and explanations of a treatise that contains a wealth of information on ores, assaying and smelting, the separation of gold and silver, alloys, bronze casting, metal melting, guns, furnaces, fireworks for warfare and festivals, and numerous related topics.

Biringuccio's expertise in mining, metallurgy, and gun founding led to his varied and successful career supported by the patronage of the nobility. One of his earliest patrons, Pandolfo Petrucci (d. 1512), aggressively exploited mining wealth by constructing many iron plants in the Boccheggiano Valley near Siena.[25] Biringuccio himself, during his lifetime, traveled widely in the German states and in Italy, gaining firsthand knowledge of mining and metalworking operations. His positions at various times included overseer of a silver mine in Carnia in northern Italy, supervisor of the iron mines in the Boccheggiano Valley, head of the Sienese armory and of the Sienese mint, director and architect of the *Opera del Duomo* in Siena (following Baldassare Peruzzi), and head of the papal foundry and munitions in Rome, where he died about 1538. He also worked for Italian princes, such as the Farnese of Parma and Ercole d'Este, and for the Florentine and Venetian republics. At one point he was given a monopoly for saltpeter in the territory of Siena.[26]

Biringuccio's audience included his noble patrons and wellborn potential investors. Notwithstanding Friedrich Klemm's statement that Biringuccio wrote for technical workers, evidence from the text points to a readership that included the unpracticed wellborn. The Sienese author noted that he had written extensively and in detail "because I have thought

[23] *Probir buch/leyn tzu Gotes lob/unnd der werlth nutz geordent* (*Probir buch/leyn tzu Gotes lob/unnd der werlth nutz geordent* 1524, dedication): "auss erfarnheit der schrifft und selbst versuchung." See also Sisco and Smith (1949, 159–60). For Duchess Elizabeth's mining activities, see Boyce (1920, 20–22).
[24] Both the Italian edition and the English translation contain useful introductions and notes. See also Brunella (1985). For references to Biringuccio's own experience, see Biringuccio (1959, 20, 48, 63, 70, 72, 75,93, 110, 131, 144, 166, 168, 215, 233, 251–52, 272, 275, 289, 291, 306, 308, 317, 444).
[25] Biringuccio (1959, 63).
[26] For biographical information with further references, see Tucci (1968) and Biringuccio (1977, xxxv–lix).

that you [the identity of the person addressed is unknown] had not hitherto had the slightest shadow of knowledge of what I have described in this treatise of mine."[27]

An early and eloquent partisan of industrial capitalism, Biringuccio advocated the aggressive exploitation of mineral resources and harshly condemned as a poor substitute the commercial capitalism of merchants. He noted that every knowledgeable investigator agreed that Italy was rich in copper, but little was mined there "perhaps because of a cowardly Italian avarice" that had "the power to make us lazy and indolent in carrying out those lofty and fine designs which should reasonably make us proceed swiftly." Biringuccio elaborated the various (and to him unacceptable) reasons that Italians hesitated to invest in mining. He particularly deplored "princes and all rich and powerful persons" who refrained from "the profitable and laudable affair" of mining ores. If they hesitated solely because of "cowardice" or because they listened to "the bayings of ignorant hounds" or "if because of their own willfulness" they wished "to remain prisoners of a detestable and ugly avarice," then that was their own loss.[28]

Although Biringuccio condemned usury (a decidedly old-fashioned view by the 16th century), he was unmoved by ancient prohibitions against mining. Men could mine copper, for instance, "without any danger or trouble to themselves, but only to their hirelings," whereby they could gain wealth "in greater abundance than from shameful usury, dangerous navigation or any of the other unreasonable or pernicious occupations." He considered minerals and metals to be "copious blessings conceded by heaven" and believed that men "wrong themselves, their fatherland, and the province where they were born" in failing to mine them. They "also wrong Nature" for they regard what she has produced as nothing or "something only useless and vile," and, finally, "they wrong all living beings both present and future, since they do not avail themselves of the universal creation as we are bound to do."[29]

On the positive side, Biringuccio praised the courage and persistence needed for successful mining operations. Turning to the empire for exemplars, he described a copper, lead, and silver mine in Austria where the owners persisted despite a layer of very hard limestone. He was amazed by their habit of "working in both night and day shifts," a thing that "surely seemed … great and marvelous." If these owners "had begrudged the expense, or the long road, or if through fear of not finding they had despaired of it and cowardly abandoned the undertaking or had stopped before penetrating that hard rock, they would have thrown away in vain all their money and all their efforts both physical and mental, and they would not have become very rich… ."[30] Moreover, they would not have profited their superiors, relatives, native country, or poor and rich neighbors. But they did profit them "through their strength and goodness of soul and through their hope and tenacity." Theirs was an example to follow if one wanted to become rich and to have "honor, authority and every other benefit."[31]

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[27] Klemm (1964, 135). For the citation, see Biringuccio (1959, 329). Further evidence for a well-born prospective audience is the chapter on precious stones (pp. 119–25), included because "it is a fine accomplishment for a gentleman to have some knowledge concerning gems" (119).
[28] Biringuccio (1959, 49).
[29] Biringuccio (1959, 49–52), for a remarkable denunciation of commercial and seafaring capitalism and usury, and an equally passionate apology for mining. For ancient prohibitions against mining and the rites surrounding it, see Eliade (1978, 53–64, 71–78) and Merchant (1980, 29–41).
[30] (1959, 20–21).
[31] Biringuccio (1959, 21). See also pp. 33–34 for another example in which Biringuccio cites courage and persistence in excavation after the discovery of gold by a washerwoman in Hungary.

A practitioner and overseer writing for potential patrons and investors, Biringuccio insisted that technical knowledge should be open. Discussing gold ore, he elaborated his reasons for writing: "I have done this [writing] willingly in order that you may acquire more learning and because I am certain that new information always gives birth in men's mind to new discoveries and so to further information. Indeed, I am certain that it is the key that arouses intelligent men and makes them, if they wish, arrive at certain conclusions that they could not have reached without such a foundation, or even nearly approached."[32] His own openly written account, Biringuccio here suggests, would lead to an increase in knowledge.

Biringuccio also condemned secrecy. He particularly disliked the secret operations of alchemy. He had derided the alchemists, he explained, so that the inexperienced might be prevented from throwing away their talents by following the same path, and so that alchemists themselves might be encouraged to share their knowledge openly: "I am also content because, in order to show my ignorance to the world, the desire may come to some worthy philosopher and alchemist to bring to light at least the open arguments for their art, if not the completed work." If this were done, Biringuccio jested, great utility would result because the art would be made clear and "all good men of ability" would begin to make gold in great quantities and thus "make men rich, secure, and happy."[33]

The accurate crediting of authorship, in Biringuccio's view, was an important aspect of openness. He expressed incredulity at the alchemical custom of disguising the true authorship of a work with a fictitious (usually more authoritative) author. The hopes of the alchemist's "fantastic writings are but masked shadows," and "in order to lend authority to their recipe books they head them with the name of an author who not only did not write them but perhaps never even thought about the subject."[34]

Biringuccio's scorn extended to those who protected craft secrets. Having noted the differences of opinion on how to make the chamber of a gun, he suggested that secrecy was used fraudulently to suggest expertise and special technique that did not exist: "Under this veil [of differing opinions about how to make gun chambers] these men pretend to have a great secret and puff up their reputations by telling lies which deer could not leap over, promising that from their guns not only balls but lightning flashes will issue."[35] In the end, Biringuccio concluded, they make only what others have made, and, when asked what theory is behind their work, they give "only a surly answer."[36]

Indeed, Biringuccio happily revealed craft secrets, seemingly at every opportunity. Referring to metal melting, he promised to tell "some methods that are held as secret by the masters."[37] Concerning techniques of the goldsmith, he did not wish "to fail to tell you of some things concerning their operations which they withhold from most people almost like secrets, so that you may know these as well."[38] In a section on ironwork, he listed what he called "secrets," which the editors suggest may be from editions of the *Kunstbüchlein* that might have been known to him.[39] Finally, tarsia work was "a very great secret and one still

[32] Biringuccio (1959, 28).
[33] Biringuccio (1959, 35–43 and passim; citation on p. 43). For a discussion of Biringuccio's antialchemical stance, see Rossi (1970, 43–46).
[34] Biringuccio (1959, 41).
[35] Biringuccio (1959, 241).
[36] Biringuccio (1959, 241).
[37] Biringuccio (1959, 323).
[38] Biringuccio (1959, 364).
[39] Biringuccio (1959, 371).

not known to me although I have practiced it diligently in order to learn it."[40] Biringuccio even described how he paid to learn the secret of using mercury to extract gold and silver from sweepings, in what is the earliest clear discussion of an amalgamation process: "Wishing to know this secret, I gave to the one who taught it to me a ring with a diamond worth twenty-five ducats, and I also pledged myself to give him the eighth part of whatever profit I should gain from this operation." In turn, Biringuccio wanted to reveal the secret to the reader "not in order that you would repay me for teaching it to you, but in order that you should esteem and value it so much more."[41]

1.4 Georgius Agricola and Humanist Mining Authorship

Whereas Biringuccio was a practitioner who had unusual access to the rich and powerful, his younger contemporary Georgius Agricola (1494–1555) was a learned humanist who benefited from the generous patronage of the Saxon electors Maurice (1521–1553) and Augustus (1526–1586) but who also had lifelong connections to practitioners. Agricola was born in Glauchau, Saxony, at the time when the region was experiencing a tremendous expansion of metal mining (particularly silver) to the great enrichment of the Saxon princes and many other residents. He came from an artisanal family but was himself (along with two brothers) university trained. His family gave him a close and lifelong association with artisans, a social circumstance undoubtedly central to his appreciation for empirical knowledge and practical techniques. His father (probably Gregor Bauer) was a dyer and woolen draper, a profession also followed by his younger brother Christoph. Two of his sisters were married to dyers. His first wife, Anna (née Arnold), was the widow of Thomas Meiner, director of the Schneeberg mining district. His second wife, Anna Schütz, was the daughter of a guild master and smelter owner, Ulrich Schütz.[42]

Agricola's matriculation at Leipzig University at the age of twenty was uncommonly late for the time, but consonant with his social background and upwardly mobile status. He received a bachelor's degree in 1515, remaining to lecture on elementary Greek. His first work was a booklet on grammar. He later traveled to Italy, stopping at Basel to visit Erasmus. He studied medicine in Bologna, Padua, and possibly Ferrara and remained three years in Bologna and Venice to help edit the Aldine editions of Galen and Hippocrates. Thus steeped in humanist culture and editorial practice, he returned to the empire. He first went to Saint Joachimsthal (now Jáchymov, Czechoslovakia), a mining town on the eastern slope of the Erzgebirge in Bohemia close to the Saxon border, one of the most productive mining areas of central Europe. As town physician and apothecary, Agricola tended the sick but also visited mines and smelters day and night, learning as much about mining and metallurgy as about the diseases of miners. In 1533 he moved to the quieter town of Chemnitz in Saxony to become town physician. While continuing his medical work and his scientific and technical writing, Agricola also invested in mining. His knowledge allowed him to profit —by 1542

[40] Biringuccio (1959, 373).

[41] Biringuccio (1959, 384–85).

[42] The influence of Agricola's family connections on his writing is pointed to by Eberhard Stimmel (1966, 377), who suggests that Agricola probably described his father-in-law's copper smelter in detail in *De re metallica* (1556, book 11). For Agricola's life, see Wilsdorf (1971, , esp. pp. 82–98) for his family background. For brief summaries, see Wilsdorf (n.d.); Agricola ([1912] 1950, vi–xii). For bibliography before 1963, refer to Michaelis and Prescher (1971). A particularly insightful discussion is Suhling (1983).

he was one of the twelve richest inhabitants of Chemnitz. Given a house and plot by the Saxon prince Maurice in 1543, he was made *Bürgermeister* in 1546 by command of the prince. At this time he was also appointed a councillor in the court of Saxony and sent on various diplomatic missions on behalf of Charles V.[43]

Agricola wrote his first metallurgical book while working as a physician in Joachimsthal. The *Bermannus sive de re metallica*, published initially in 1530, is a little dialogue among the physicians Johannes Naevius and Nicolaus Ancon and a mine overseer, Bermannus, as they stroll through the mountainous region near the town. The book mainly consists of a discussion of regional ores and those mentioned in ancient writings. The introductory letter by Erasmus was obtained by Petrus Plateanus, a distinguished teacher who at that time was rector of the local Latin school. In addition to his Latin-German glossary, Plateanus contributed his own letter of introduction dedicated to Heinrich von Könneritz, the region's mine superintendent.[44]

The introductory letters of the *Bermannus* emphasize the ideal that knowledge should be open. Erasmus praised the work for its vivid descriptions of "those valleys and hills and mines and machines" almost as if one had seen rather than read about them.[45] Plateanus further explicates the ideal of openness through writing. None are more deserving "than those who transmit to posterity through writings the secrets either of the arts or of nature invested by oneself or by others." Although men are endowed with powers of reason, understanding, and knowledge, making them superior to the mute beasts; although they are capable of virtue and of various skills and disciplines; and although they are even able to be inventors and therefore can "penetrate into every very concealed thing of nature," nevertheless knowledge would be completely narrow if it were limited to one person's experience. Plateanus pointed to the very learned men of former ages who had made discoveries after much work and had committed them to writing. In turn he condemned those predecessors who had lost these writings or allowed them to be destroyed. He admonished that we should take care that the same fate does not overcome our writings or those of successors. There are many ingenious and learned men in our own age, but they are often reluctant to publish the "records of their genius" either because of modesty or because of fear of criticism. We should give our caring to any of the noble men who suffer this shame or fear so that their work, which aids public studies, is not cut off.[46]

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[43] Wilsdorf (1971, 99–275); Agricola ([1912] 1950, vi–xii); Suhling (1983, 157–60). For a list of Agricola's writings, their subsequent editions, and translations with bibliography, see Horst (1971). A cogent discussion of one aspect of Agricola's work that provides useful context is Ruffner (1985).

[44] The edition I have used is Agricola (1541). I have also consulted Agricola (1955a). The "condensed" English translation made from the 1955 German translation (Paul 1970, 252–311) contains many omissions and should not be relied on. For a history of the text itself, see Horst (1971). For discussion and documentation of Plateanus's role in securing the support of Erasmus, see Wilsdorf (1971, 184–88). Heinrich von Könneritz and Plateanus's careers and relationships to Agricola are summarized in Agricola (1955a, 295–312). For Plateanus see also Kaemmel (1888, 241–43). Agricola named at least two of the interlocutors after friends. Lorenz Bermann, about whom little else is known, translated Agricola's *De bello adversus Turcam suscipiendo*, an oration against the Turks, into German for its first publication in 1531. Johannes Naevius was a physician who, like Agricola, attended the University of Leipzig and spent some time in Italy. Nicolaus Ancon is unknown—the editors suggest it may be a pseudonym for a student friend in Italy. For the three interlocutors, see Agricola (1955a, 271, 306–8, 268, respectively).

[45] Erasmus (1541, 3): "valles illas & colles, & fodinas & machinas." Since the work contains no description of a machine, one wonders how carefully Erasmus read it.

[46] Plateanus (1541, 5–6): "quam illi, qui vel arteis vel naturae arcana, per se aliosque inventa, literis ad poteriate[m] transmittunt"; "ad abstrusissima quaeque natura[e] penetrare"; "ingenij sui monumenta."

Agricola's own view of openness in this relatively early work was profoundly influenced by his experience of editing humanist texts. He placed openness in opposition not to craft secrecy or to the failure to write or preserve past writings, but to the corruption of terminology that rendered obscure what once had been clear. He lamented the damage not only to natural and man-made things, but also to names. Either they had been changed ineptly or barbarisms had been substituted in their place. Because of this obscurity of language, darkness had been drawn over good studies and excellent art, forgetfulness had crept in, much destruction had followed. Metallurgical studies had made progress only because divine providence had intervened to excite the industry of every favorable person. These people had taken pains with high and extraordinary efforts "to lead back into light, those things snatched away from darkness, to call back into memory, those things freed from oblivion, to liberate into freedom, those things preserved from extreme devastation… ."[47]

Agricola's aim was to integrate ancient knowledge with contemporary information, in part by developing a uniform technical vocabulary. He suggested that eloquence and purity (as opposed to precise terminology) were flourishing in both the Latin and Greek languages but that knowledge of things had been neglected for the most part until the present. He particularly condemned the physicians who used the names of metals so often and the apothecaries who dispensed them, both without knowledge of substances.[48]

Elaborating his own reasons for writing, Agricola stressed the value of openness. He had written the *Bermannus* to give the studious a taste of a work to come. He also wished to motivate his contemporaries to more diligent investigations. Finally, he wanted to bring to light useful things to be found in German mines that had been unknown to antiquity.[49] As for the ancients, they provided a model not only by their learning, but particularly also because they had transmitted their own knowledge and that of others to their successors in writing: "For if the Greeks, the most learned people of all, have transmitted not only their own written accounts [*memoriae*] but even those of foreigners, it is shameful for us that our things through our own negligence and idleness indeed now are almost concealed by darkness and lack their own light."[50]

In his own time, Agricola portrays the interlocutor Bermannus as a model of one who combines direct observation and experience with knowledge of ancient texts. Only near the end of the dialogue do we learn that he is the overseer of a particular mine. And while he leaves his new friends briefly to talk to the mine captain, the other two praise him for, among other things, his openness in sharing his knowledge: "that which he discovers with great labor, he explains very easily and very diligently to others, and by no means is one who, with a certain envy, conceals, as in mystery and arcana, a very bad habit of not a few."[51] Openness was a central value, a necessary condition for Agricola's study of ancient invention and authorship and contemporary data.

[47] Agricola (1541, 10): "eas e tenebris ereptas, in lucem reducere: Ab oblivione vindicatas, in memoriam revocare: ab extrema clade servatas, in libertatem asserere…"

[48] Agricola (1541, 11–12). See also Halleux (1983). I have not yet been able to see the recent *Belles Lettres* edition with French translation and commentary: Agricola (1990).

[49] Agricola (1541, 13–14).

[50] Agricola (1541, 14): "Si enim Graeci gens omnium doctissima, non sua solum, sed etiam externa, memoriae tradideru[n]t, turpe nobis sit res nostras per socordiam & ignaviam nostram etiam nunc tenebris quasi obrutas esse & sua luce carere."

[51] Agricola (1541, 100): "ea quae magno labore invenit aliis facillime & dilligentissime explanat, ac minime qui non paucis mos est pessimus, invidentia quadam ta[n]quam mysteria & arcana celat."

In the *Bermannus*, the encouragement of capital investment in mining is explicit. When Ancon, the physician trained by scholastic methods, suggests that miners lose money, Bermannus scoffs and points to miners in the area who had begun to excavate with little means and had become wealthy as a result.[52] Ancon later elaborates that he would not pay money just for hope —that mining involves great expense for such hopes, and he would not spend "what was certain for uncertain things" and rashly give up his fortune. In a rejoinder worthy of any 20th-century stockbroker, Bermannus insists that Ancon is too cautious and that such extreme caution would always be in his way. Ancon's attitude reveals a good Aristotelian, but he would never be a good miner or a rich man. If a farmer had such a view, fearing catastrophe, he could never sow; if a merchant had it, fearing a shipwreck, he could never trade; nor could anyone go to war because of the uncertainty of the outcome. On the other hand, "all hope for good and often it turns out well. No one truly with an abject and timid soul ever did anything or indeed ever will do anything."[53]

Agricola's attitude toward wealth represents the endpoint of a continuum that begins with the medieval ideal of Franciscan poverty and develops into the more positive evaluation of the Italian humanists whose dialogues presented the positive as well as the negative effects of riches.[54] For Agricola, wealth was unambiguously good. He elaborated in his *De veteribus et novis metallis, Lib. II*, a small treatise on ancient and contemporary metals published with a group of other writings in 1546. Defending authorship on metallurgical subjects, he also encouraged the activity of mining itself. Quite simply, mining will make one rich. Or so might be the conclusion drawn from Agricola's list of those who had acquired wealth thereby. His examples ranged from the highest princes to ordinary people, including one Conradus, "*cognomento pauper*" whose economic status had been radically transformed by the discovery of some silver in the Jura Mountains.[55]

Steeped as he had been in the values and practices of humanism during his years in Italy, Agricola's views concerning openness and authorship were influenced by the Romans. Vitruvius, Pliny, and Columella emphasized respect for past authorship, advocated open written transmission, and condemned plagiarism. Although these Roman values developed in very different contexts, they were apposite to Agricola's own environment. Without following him slavishly, the 16th-century author adopted Pliny's lead in appending the names of relevant past authors to some of his treatises. As he noted in his treatise on mineralogy, "Pliny gives credit openly and frankly to those whose writings he uses and likewise I shall give credit by name to those whom I quote."[56]

In his masterpiece, *De re metallica*, published posthumously in 1556, Agricola's acknowledged model text was the *De re rustica* of the Roman agricultural author Columella. Columella's unusual skill in balancing respect for past authorship with the ability to maintain a critical stance is everywhere apparent in Agricola's own writings. Columella was an

[52] Agricola (1541, 16–17).
[53] Agricola (1541, 27–28): "quae certa erant, incerta"; "bene sperant omnes & foeliciter saepius p[rae]cedit, nemo vero animo qui abiecto & timido fuit, unqua[m] re[m] fecit, aut etia[m] faciet."
[54] See, e.g., Bracciolini (1978). For views toward wealth in this period, see especially Baron (1938); and, for the medieval background, see Little (1978).
[55] Agricola (1546, 384–85), for a defense of writing on metals, and pp. 394–95, for the list of individuals enriched by mining. For a discussion of Agricola's defense of mining, see Vogel (1955).
[56] Agricola (1955b, 1–2), where Agricola summarizes his attitude toward Pliny and other past authors. For the views of Roman technical authors toward openness and authorship, see especially Vitruvius (n.d., 7. preface 1–10); Columella (n.d., 1.1.1–20) (where he discusses past authors); and Pliny (1938, 14–15).

important influence not only for the structure of the *De re metallica*, but also for many of its most significant values. The 16th-century treatise, which was dedicated to the Saxon princes Augustus and Maurice, included an eloquent defense of mining modeled on Columella's defense of agriculture. Following Vitruvius's similar requirements for the architect, Agricola listed the disciplines necessary to the miner —philosophy, medicine, astronomy, surveying, arithmetic, architecture, drawing, and law. He defended mining against every critic. He dismissed those who emphasized the dangers and unhealthiness of mining. A physician of miners who was in a position to know better, Agricola suggested that accidents were rare and caused by the carelessness of workmen. He believed that mining was profitable to the competent and useful to the rest of mankind, and he emphasized the uses of wealth against those who pointed to its evils. The dignity of mining and of investment in mining was greater than that of commerce and equal to —although more profitable than —that of agriculture.[57]

In addition to his defense of mining, Agricola advocated openness and credit to authorship. Past writers should be properly credited: "No one should escape just condemnation who fails to award due recognition to persons whose writings he uses, even very slightly." As before, the value of openness was centered on the clarity of technical language. Alchemists were to be condemned particularly because all of their writings are "difficult to follow, because the writers upon these things use strange names, which do not properly belong to metals, and because some of them employ now one name and now another, invented by themselves, though the thing itself changes not." Beyond its obscurity, Agricola complained about the lack of efficacy of alchemy (which consistently failed to produce riches) and about alchemical frauds. Finally, he condemned the alchemical practice of assigning false authorship.[58]

1.5 Assaying and Authorship in the Postboom Decades

By the mid-1550s the mining boom of the German states had spent itself. As the rich veins became less productive, efficient methods of assaying and of extracting and refining metals became increasingly crucial to overall productivity. Not only was the removal of ores from poorer veins more costly, but also the growing influx of precious metals from the New World tended to lower the value of the gold and silver that were extracted. The oversupply of precious metals both from the German states and from the New World contributed to the inflationary trend known as the price revolution. Exacerbating the problems of the declining value of money was the chaos of specie that had long been the rule in the German states and had encouraged widespread fraud in minting. The mint became a particular focus of attention. In addition to attempts to reform the coinage, accurate assaying in the mint became a priority. Although the mining boom was over, the clock could not be turned back. Capitalist mining and metal production continued, while the literature that it produced focused increasingly on efficient methods of assaying and metal processing, on the effective organization of labor, and on the minting of specie.[59]

[57] Agricola ([1912] 1950, 1–24). For Vitruvius's list of disciplines necessary to the architect, see Vitruvius (n.d., 1.1 3–10). The publication history of *De re metallica* is outlined in Horst (1971, 741–831).

[58] Agricola ([1912] 1950, xxvi–xxix).

[59] For a general discussion of the decline of mining and the confusion and widespread fraud in the mints, see Janssen (1910, 70–106). Harry A. Miskimin (1977, 35–43), discusses the decline of mining in relationship to the

The *Probierbüchlein* by Ciriacus Schreittmann is an intriguing example of technical writing in the 1550s. All we know about the author is that he was an assayer in the service of Johann Abel von Weissenburg from Weissenburg-am-Rhein in Bavaria. Johann's son, Valentin Abel, undertook publication of the booklet in 1578 more than twenty years after it was written and after the death of both his father and Schreittmann.[60] Above all, Schreittmann was concerned with accurate weighing and measurement. The first part of the pamphlet concerns the construction, testing, and accurate use of an assay balance. As Cyril Stanley Smith has shown, the second part contains a striking innovation in its elaboration of a decimal system of weights for assayers.[61] Not until the third and last section does Schreittmann get around to discussing the assay of metal in both specie and ores, the construction of assay ovens, and the like.

In his dedication to Georg Friedrich of Brandenburg (1539–1603), Valentin Abel elaborated the theme of openness. He praised the ancients for their foresight in having bequeathed and communicated their many useful discoveries to successors. He noted the utilitarian value of the arts, including the mechanical arts, and insisted on their ongoing progress and contribution to civil society. He questioned whether we should "hide in ourselves and bury in the darkness of ignorance" the art of assaying. Unfortunately, assaying had been wholly abandoned for a time by Germans, who considered it useless speculation. The particular reason for this was that very few had sufficient understanding to write down the results of what had been learned. An exception was Ciriacus Schreittmann. Yet his beloved father Johann Abel had kept this author's book in secret for more than twenty years. He, Valentin, was now publishing it because it had such great uses for every lover of skill. Many rulers and nobles were now decreeing that all the arts be written about. Nothing on earth was begun without favor from God and the protection of the princes and nobles. On the other hand, he did not doubt that some jealous artisans would be "very grieved against this instruction on assaying without favor from God and the protection of the princes and nobles. On the other hand, he did not doubt that some jealous artisans would be "very grieved against this instruction on assaying, as if on account of it some damage to their livelihood would follow," and would believe that the subject should not be made public.[62]

Schreittmann himself wrote a preface to the reader, an indication that he had intended to disseminate the work beyond his employer. He noted the many books already published on his subject that he honored greatly. Nevertheless, it took trouble, work, and time to read them because they were "written so obscurely and in a scattered way." The person who improved a known subject was more to be praised than the first who discovered it. Further,

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price inflation. Production statistics documenting the decline are occasionally available—see Westermann (1971a, 313–15).

[60] I have used the 1580 edition—Schreittmann (1580). See also Darmstaedter (1926, 189).

[61] Smith (1955). As Smith noted, Schreittman's system provided an elegant and simple substitution for the complicated legal weight systems in which 16th-century assayers worked. It also furnished a method whereby assayers could convert from one system to another. Schreittmann, who clearly used the new system in his own work, proposed it years before Simon Stevin's much better known elaboration of a decimal system of weights and measures, Stevin (1585). The failure of Schreittmann's system to be taken up by other assayers presents an important case for the study of innovation vs. tradition in the history of technology.

[62] Valentin Abel, "Den Hochwirdigsten/Durchleuchtigsten/…," in Schreittmann (1580, n.p.): "in unns verborgen/ und in finsternuss der unwissenheit begraben"; "wider diese anleytung dess probirens sehr bekämmern/als ob ihnen der halben etwas abbruchs ihrer Nahrung darauss folgen wirdt." The ruler to whom the work is dedicated is not mentioned initially. However, at the end of the preface the date is given as 39 years after the birth of Georg Friedrich, margrave of Brandenburg.

bad things had sometimes been written in old books and learned carefully to the detriment of the art. Through his book, Schreittmann believed that he could cut many costs and avoid trouble and fruitless work. He had written it for the inexperienced who could learn assaying from it, for the more skilled for their greater understanding, and "for those seeking with subtle understanding" (i.e., the learned). Finally, he encouraged his readers not "to gnaw at my writings with envious teeth," but to use them and "to correct and make [them] better."[63]

Modestin Fachs, a master of the mint at Leipzig, wrote an assay book in the 1560s in which he praises metals as gifts of God that have many human uses. Fachs suggests that God has openly disclosed what is necessary for the preparation of gold, silver, and other metals, and he insists that handwork is essential for learning proper assaying. He points out that he has shown no one "obscure" alchemical ways that are "deceptive and untrue." In his first chapter Fachs gives detailed instructions for constructing an assay oven. He continues with specifications for handling various types of ores and metals and includes sections on weights. His interests are technical, but also historical. He concludes with chapters on assaying and coinage from biblical times to the present (1569). His book was published posthumously in 1595 at the behest of his son, Ludwig Fachs, who dedicated the work to Mathias Geyerbost, duke of Anhalt, and noted that his father had served the dukes of Anhalt for many years.[64]

Samuel Zimmermann published his book on assaying in Augsburg in 1573 and included as part of his introduction a poem on the five senses.[65] Zimmermann's emphasis on the senses was consistent with his belief in clear, open, and visible assaying, and his opposition to the obscure, fraudulent, and often false operations of the alchemists. Initially, he had been undecided about writing his book because of the "heaped up meanderings" in many alchemy books. He had questioned the value of publishing or even of further reading. Little truth had been discovered by "present-day supposed philosophers and alchemists," and many had died before even one had become rich. He himself had transformed copper, lead, and tin to make them look like gold, but such change was only a vision, a counterfeit, or a shadow, as if a reflection in a mirror or water. Just as one is duped by such reflections, so the alchemical art is illusory. Even where transmutations are possible (Zimmermann gives examples of changing iron and lead to copper, copper into brass or lead, iron and steel into lead), the cost of the attempt is greater than the value of the resulting metal. Although alchemy is the source of many mechanical and medical skills, supposed alchemists are, nevertheless, often the source of deceptions concerning metal, minting, and previous stones.[66]

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[63]Schreittmann (1580, "Vorrede zu dem Leser," n.p.): "so dünckel und weitläufftig beschrieben sind/"; "die begerenden/mit spitzfünderigem verstandt."; "mein ausschreiben/mit neidigen Zänen zernagen"; "corrigiren unnd bessern."

[64]Fachs (1595, "Vorrede des Authoris/ an den kunstliebenden Leser"): "ungewisse," "betriegliche unnd unwarhafftige Wege."

[65]Zimmermann (1573, n.p.). The poem, "Beschreybung der füinff Synnen/ darinn der gantz Inhalt dises probier Büchs/ auffs kürtzest begriffen/ und in Reymen weiss gestalt," appears after the letter to the reader. The treatise is described by Darmstaedter (1926, 89–90). I have not been able to discover any biographical information on Zimmermann beyond his own statement (p. 88) that he also wrote a book on gun projectiles (Büchsen geschoss) and the fact that (as Samuel Architectus) he wrote a dedication letter for Paracelsus's treatise on the diseases of miners, *Von der Bergsucht oder Bergkranckheiten drey Bücher*...(1567). For the latter, see Sudhoff ([1894] 1958, 138–40).

[66]Zimmermann (1573, "Dem kunstliebhabenden Leser…," n.p.): "haussen umbfahren"; "jetzigen vermainten Philosophen und Alchimisten." Elsewhere (pp. 99–102), Zimmermann elaborated that he believed that the transmutation of metals occurred but through the grace of God, not the skill or knowledge of men. Later (pp. 128–31) he decried the lying and shouting over the philosopher's stone and suggested that the obscurity of alchemical writings

Zimmermann decided to publish his own book so that "both the correct and the false, the good and the bad become recognized." He believed that he would encounter two kinds of hostility. The first would be from "untrue artisans who help themselves and are needy of these things, and do not wish that such things become public." The second would be from "very false people and swindlers who beget namely a particular secret hatred and hurl envy on me, meanwhile I discover their false intelligence and their fraud sufficiently." Despite this hostility, the author assured his readers that he would explain things clearly, but he also reminded them that, as in any handwork, practical experience was necessary for true understanding.[67]

True to his promise, Zimmermann attempted to expose fraudulent metallurgical practices. He described how some made assay needles of brass, copper, and lead to look like gold, and of copper to look like silver, thereby deceiving "pickers and farmers."[68] He pointed to fraudulent alchemists who, with a "transmuting powder," convinced people that they could transform silver, copper, tin, and lead into gold, when in fact there was already gold in the powder. In order not to be deceived "by such so-called alchemists … with their false assays and powders" he advised that everyone do their own assay and that assayers make their own powders rather than use those given by others, by which "many princes and honorable people" are deceived. He has revealed the methods of these deceivers, so that you "know how to injure, to ward off and thereto also to warn other people before them."[69] Finally, he treated precious stones, through which "so many splendid aristocratic people" had been deceived, as their descendants still were being deceived, so that some had fallen from great wealth into total ruin. Zimmerman's purpose was to see that "the true cheats and deceivers with their false truths" were truly recognized and laid open so that they themselves are roused to desist.[70]

1.6 The Role of Authorship in the Career of Lazarus Ercker

The rewards of technical authorship in the mid 16th-century empire are particularly evident in the career of Lazarus Ercker (ca. 1530–1594), a skilled practitioner and overseer of mining and mint operations. Ercker was born in Saint Annaberg, Saxony, the boomtown that one of his predecessors, Calbus of Freiberg, had helped to lay out. He attended the University of Wittenberg in 1547–1548. His marriage in 1554 to Anna Canitz led to his appointment in 1555 as assayer at Dresden. He was chosen by Elector Augustus through the intervention of his wife's relative Johann Neef, whom we can recognize as the interlocutor Naevius in Agricola's *Bermannus*. Neef had been the town physician of Annaberg since 1527 and

was a result of the fact that alchemy comes from God, not man, thus making it impossible for man to "describe [it] clearly, transparently and perfectly" (klar/hell/und volkommenlich beschreiben (p. 130)).

[67] Zimmermann (1573, "Dem kunstliebhabenden Leser…," n.p.): "beyde das gerecht/ und falsch/ güts und böses erkendt wurde"; "den untrewen Künstlern/die sich diser dingen behelffen und nören müsen/und nicht wöllen/das soliche ding gemain werden"; "felscher und betrüger seind/ die werden fürnemlich ein sondern haimlichen hass un[d] neid auff mich werffen/dieweil ich iren falsch anzeig/un[d] iren betrug genugsam entdeck." See also p. 36 for a further discussion on the importance of actual handwork.

[68] Zimmermann (1573, 4–5): "die Lazen und Bauren."

[69] Zimmermann (1573, 111–15): "ein Transmutier pulfer"; "von sollichen vermainten Alchimisten… /mit sollichen irhren falschen proben/und pulvern"; "vil Herzen und Redlicher Leüt"; "schaden/wissest zuverhütten/unnd darzu auch andere Leüt vor inen gewarnen."

[70] Zimmermann (1573, 115–16): "sovil Statlicher/ fürnem[m]er Leüt"; "die wahr felscher und Betrieger/ mit irer falschen wahr."

physician to the electors Maurice and Augustus since 1544. The Saxon princes had already bestowed many favors on Georgius Agricola. Augustus in particular was an enthusiast of mine, metallurgical, and alchemical operations; his resident castle at Dresden contained a well-equipped smelting and assaying room that was the site of numerous metallurgical and alchemical experiments.[71]

Less than a year after his appointment, Ercker completed his first technical book, *Das kleine Probierbuch*. Hand copied by a scribe and dedicated to Augustus, it is a practical handbook that includes instructions for the construction of an assay oven, directions for assaying, and discussions of weights and measures, of cementation, and of the assaying of coins and other aspects of minting. It also provides assorted metallurgical recipes. Although the manuscript remained unpublished, it soon had the desired effect. Shortly after Ercker presented it to the elector, he was appointed general assay master for all matters relating to the mineral arts and minting for Freiberg, Annaberg, and Schneeberg.[72]

Although he was demoted (for unknown reasons) to warden of the Annaberg mint, Ercker found a new patron in Prince Henry of Braunschweig who appointed him assay warden at the mint at Goslar in the Harz mountains. Prince Henry (1489–1568), the grandson of Duchess Elizabeth, had continued his grandmother's work of expanding the productivity of the region's mines. Much of the reign of this Catholic prince was spent in armed conflict in an effort to gain or regain and consolidate territory under his own power. Although the conflicts in which he was involved were episodes of the struggles brought about by the Protestant Reformation, his own religious affiliation seems to have been motivated by the desire for the political support of the emperor. For him the consolidation of political and territorial power and the development of his most important economic base—mining—were prime motivations and went hand in hand. Encouraged by his friend Duke George of Saxony (1471–1539), the father of Augustus and Maurice, he had revived the ancient silver mines of the upper Harz, investing his own income and encouraging other investors. In 1552, after years of struggle, he conquered the imperial (but Protestant) city of Goslar and from that time on controlled the mines in the Rammelsberg, a mountain to the south.[73]

Ercker found himself, therefore, in a familiar environment—working in a mint, the appointee of a prince deeply interested in and dependent on the productive exploitation of mining. Once again he turned to technical authorship as a way of achieving advancement. He wrote the *Münzbuch*, a treatise on minting, which he presented in 1563 to Henry's son Julius, duke of Braunschweig-Wolfenbüttel (1528-89). By 1563 the enmity between Henry and his son Julius (brought about in part by Julius's conversion to Protestantism) had ameliorated. At his succession in 1568 Julius, by right of the religious peace of Augsburg, introduced Lutheranism into his duchy.[74]

Less dramatic but just as important was the continuity represented by Julius's intense interest in the aggressive exploitation of mining in his territories. Most significant economically by this time were the iron mines and the accompanying manufacturing industries, particularly of artillery, to which Julius contributed numerous inventions and experiments.

[71] Beierlein (1955, 12–18); Hubicki (n.d.); Ercker (1968, 9–11).

[72] See Ercker (1968, 5–144), for a transcription of *Das kleine Probierbuch*, and pp. 145–214 for a facsimile of the manuscript. See also Beierlein (1955, 14–16, and 56–68).

[73] Bornhardt (1931, 147–54); Boyce (1920, 23–65); Henschke (1974, 24–26, and passim (see "Personenregister," s.v. "Heinrich der Jüngere, Herzog von Braunschweig-Wolfenbüttel")); Schmidt (1969).

[74] Beierlein (1955, 19–68); see Ercker (1968, 267–326), for an introduction to the *Münzbuch* and a transcription of the text. For Julius, see Kraschewski (1978).

The duke, who was intensely interested in metallurgy and alchemy, opened many new mines, expanded old ones, and made administrative reforms to prevent corruption. Julius also had a hand in technical authorship. The finely illustrated *Instrumentenbuch*, which, according to the subtitle, was "in part conceived by Julius and drawn and painted by his own hand," exists in a single manuscript copy. It concerns machines for removing ores from mines and transporting them. A second section that includes material on ships has recently been reported. Ercker apparently understood Julius's interests well when he dedicated his *Münzbuch* to him in 1563. Shortly thereafter he was promoted to master of the Goslar mint.[75]

In the *Münzbuch* Ercker elaborated why he was presenting a practitioner's knowledge of minting to a ruler. If nobles and potentates who control mines and mints are not well-informed of such practical operations, they will be taken advantage of by unfaithful servants and indeed will be unable to distinguish between true and untrue employees. Conversely, if they understand metallurgical practice, they can cast off false subordinates, appreciate true service, and not be subject to overreaching from unfounded hope. Ercker insisted that his information, based on the efficacy of experience, would be many times used and useful to nobles and dukes in relation to new mines.[76]

In writing down craft knowledge for a ruler, Ercker was acting on the side of openness. Yet his criticism of alchemy, which becomes explicit in the *Münzbuch*, is based not on its secrecy but on its lack of practical results. Ercker admitted that many of the practices of assaying, silver and gold refining, and similar arts had their origins in alchemy. Yet few alchemists of his own time had kept assaying a useful art by practicing it correctly and becoming experienced in it. Concerning the mint, Ercker supported its traditional secrecy. He cautioned Prince Julius "not to let this my work come before everyone so that it remains a beautiful art as up to now it has been."[77] Secrets of craftsmen and secrets of state were very different matters.

Ercker was once again seeking employment in the mid-1560s. After the death of his first wife, he married Susanne, daughter of a Dresden official. His new brother-in-law Caspar Richter was a minter in Prague. Through him Ercker was appointed control assayer (*Gegenprobierer*) in Kutná Hora (Kuttenberg), Bohemia. Susanne herself also served for many years as the manager of the mint in the same place with the title "manager-mistress." They had two sons, Joachim and Hans, both of whom became assayers.[78]

Ercker remained in Bohemia for the rest of his life and continued to advance himself by means of technical writing. He wrote a little book on testing ores, *Zkoušeni rud*, in 1569.[79] His masterpiece, *Beschreibung der allervornehmsten mineralischen Erze und Bergwerksarten*, first published in 1574, was dedicated to the Emperor Maximilian II (1564–1576).

[75]For Julius's mining activities, see Kraschewski (1978, 151–65). Julius's *Instrumentenbuch* (1575) is in the Niedersächsisches Staatsarchiv, Wolfenbüttel, 2 Alt 5228. See Spies (1978); Moran (1981, 261–62). The second part of the *Instrumentenbuch* is in the Staatsarchiv Magdeburg. I rely here solely on newspaper reports of a lecture in Wolfenbüttel by Gerd Spies for notice concerning this second part, "Vortrag über Technik der Renaissance: Vom Harz zur Nordsee" (8 August 1989) and "400 Todestag von Herzog Julius" (1989).

[76]Ercker (1968, 284). Indeed, Ercker's book on minting is organized very much from a ruler's point of view in that Ercker begins by describing the various offices of mining and mint operations and their respective duties (pp. 285–96) before discussing practical aspects of assaying and minting.

[77]Ercker (1968, 284–269): "diese meine arbeit nicht vor Jeden komen lassen, uff das es eine schöne Kunst, wie bieshero bleibe."

[78]Beierlein (1955, 24–34); Hubicki (n.d.).

[79]See Hubicki (n.d.). I have not seen this booklet, which apparently remains unpublished and exists in manuscript form in the National Archives, Prague, MS 3053.

Ercker elaborated that he wrote for the benefit of the emperor's vast mineral resources and of those who made their living from them, in the hope that these resources would be further developed and long maintained "through serious effort stimulated by complete information." The information he provided concerned the ores and assaying of silver, gold, copper, lead, tin, and saltpeter. Ercker's masterpiece undoubtedly was inspired by Agricola's *De re metallica*. Unlike his previous works, most of which remained in manuscript, this was a comprehensive, illustrated treatise clearly intended for publication. At the outset, Ercker boasted that his experience was greater than that of his predecessors (the allusion to Agricola is unmistakable). Soon after its publication, the emperor named Ercker courier for mining affairs and a clerk in the supreme office of the Bohemian crown. Maximilian's successor, Rudolf II (1576–1612), appointed him chief inspector of mines. He was knighted in 1586.[80]

1.7 The *Schwazer Bergbuch*: Emblem of Noble and Capitalist Mining Interests

The form of the beautifully hand-copied and illustrated *Schwazer Bergbuch* is very different from printed mining and metallurgy books. The treatise consists of an extensive compilation of mining law, customs, and regulations, and also contains more than a hundred hand-painted miniature illustrations, probably by Jörg Kolber. Unpublished until the 20th century, the work exists in at least seven manuscript copies. It is the most important 16th-century source for Tyrolian mining law and custom, mine technology, and the conditions and responsibilities of mine officials and workers. The author was almost certainly Ludwig Lässl (d. 1561), an official in a mine court in Schwaz in the Tyrol between 1543 and 1555.[81]

Erich Egg has reconstructed some aspects of the life of Ludwig Lässl. Born into a peasant family, Lässl's career exemplifies the upward mobility that the 16th-century mining industry could sometimes provide. Lässl obtained his post as clerk of the mining court through his father-in-law, Hans Möltl, who occupied the position before him. His appointment as mine clerk and his later retirement (with pension) because of ill health are recorded in the papers of the archduke Ferdinand (1503–1564), ruler of Austria and one of Lässl's patrons. Lässl is also known as the founder of the first paper mill in the Tyrol.[82]

Egg has suggested that the *Schwazer Bergbuch*'s emphasis on the localities of particular mines (which is irrelevant to mining law) strengthens the presumption that the work was not written primarily for mine workers. He has proposed that the prospective audience was much farther afield and was conceived in the context of a financial crisis in the early 1550s. Capital investments for Tyrolian mining came primarily from commercial firms in Augsburg, most importantly the Fuggers, but also many others. In 1552 two Tyrolian mining firms, plagued by the overextension of credit and the high costs of deeper mines, went bankrupt. Creditors from Augsburg were pulling back. In 1553 the Augsburg firm of Baumgartner, the most important investor next to the Fuggers, gave up its Schwaz mining interests. Egg suggests that the *Bergbuch* was intended to rouse both Augsburg investors and rulers to provide financial help in the form of mining investments.[83]

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[80] Ercker (1960); Ercker (1951, citation on pp. 3–4). See also Armstrong and Lukens (1939) for the great influence of Ercker's treatise; and Beierlein (1955, 32–55) for Ercker's career in Bohemia, and pp. 68–97 for subsequent editions and translations of the work.

[81] See Winkelmann (1956, v–viii) for a useful introduction. See also *Der Anschnitt* 9 (1957), an issue largely devoted to the *Schwazer Bergbuch*; Berniger (1980); Kirnbauer (1956).

[82] Egg (1957).

[83] Egg (1957, 18).

Lässl's text supports such a view. He argued that the wealth produced by mines was a gift of God and pointed to the great riches and improvements brought about by mining. Many dukes and others had risked great sums and goods to build more extensive mines. Not only had workers and miners received benefits, but so also had all other persons of high and low station, as well as towns and businesses. Many had gathered in lightly populated areas, property values had increased fivefold, land had been developed, what once had been worth little or nothing was bought and sold for much money. All this showed that mining was a divine gift, created for the sustenance and benefit of man. Because of its great benefits, Lässl insisted that the welfare and rights of mine workers should always be considered. He was writing because over the years mine laws and decisions had become confused. Often two or more regulations referred to the same topic. He was correctly laying out the old regulations in new form.[84] As Lässl brought order to mine regulations, he also created an emblem for the riches that mining might bring in a book beautifully copied and illustrated by hand, a book fit for the libraries of wealthy burghers and kings. The shrewd intelligence evident in his text can be seen elsewhere as well, for in this postboom decade of the 1550s, Lässl put his own money not into mining but into paper manufacturing.

1.8 Conclusion

The exoteric tradition of mine and metallurgical writings encompassed great diversity in the books themselves and their authors. It included printed books and hand-copied manuscripts. Authors included practitioners from artisanal backgrounds and university-educated humanists. Such diversity suggests that there were also some differences in the aims of authorship and in intended audience, differences sometimes apparent within the corpus of a single author's writings. Lazarus Ercker wrote his early works for specific patrons with promotion undoubtedly in mind. In his masterpiece, on the other hand, he recognized from Agricola's example that he could achieve even more, namely fame, from a printed and illustrated treatise disseminated to a larger audience. Agricola himself wrote primarily for the world of humanist learning and aimed to legitimize mining and metallurgy as one of the learned disciplines.[85] Other authors such as Biringuccio and Ludwig Lässl were writing as much for wealthy potential investors as for noble patrons.

Yet, as soon as a practitioner took pen in hand to elaborate his technical skill in writing, he undertook also a new craft, one traditionally associated with more "learned" subjects. On the other side, the learned humanists Calbus of Freiberg and Georgius Agricola sustained a lifelong interest in the details of practice. Sixteenthcentury mine and metallurgical authors occupied a border area between learned, elite, and craft cultures. To a greater or lesser degree they were familiars of both worlds. Those with artisanal backgrounds were not only literate, but engaged in literary practice as well. Those who were university trained had acquired extensive knowledge of mining and metallurgical technology. This study confirms that the gap between the scholar and the craftsman was not as great in the early modern period as has sometimes been suggested.[86]

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[84] Winkelmann (1956, 10–12).

[85] A point stressed by Owen Hannaway in the seminar "Technologia," Folger Institute, spring 1989. In this regard, see also the perceptive essay by Suhling (1977) and also Roger (1979).

[86] Recent scholarship that has emphasized the early modern interaction between scholar and craftsman includes the following: Bennett (1986); Eisenstein (1979, esp. vol. 2, pp. 520–635); Keller (1985); Rossi (1970, 1–62); Vasoli

Despite their diversity, these authors shared the context provided by the capitalist expansion of mining. As a result, they elaborated a group of seemingly unrelated attitudes from a remarkably consistent point of view. Their affirmation that knowledge should be transmitted openly was closely associated with beliefs related to early modern mine and metallurgical capitalism: wealth is a positive good; investment in mining should be encouraged and would pay off in riches; clear technical language and understandable discussions of technical processes, careful measurement, honest and precise assaying, and practical skill all are necessary to high productivity. They criticized alchemy not on the basis of whether transmutation occurred, but in terms of the criteria of clarity, openness, honesty, and productivity. They also condemned craft secrecy.

All of these authors except one (Julius of Braunschweig) were from artisanal or middle-class backgrounds. All for whom we have biographical information were upwardly mobile. Many found patrons in those rulers whom Bruce Moran in his ground-breaking studies has called "prince-practitioners."[87] The prince-practitioners (Julius is a prime example) supported exoteric mine and metallurgical authorship, but often they patronized the esoteric discipline of alchemy as well.

For "openness" did not necessarily refer to wide public dissemination of knowledge. Rather it could signify the act of writing down orally disseminated craft knowledge, making it accessible to an unskilled learned and noble audience. It could mean (as it did for Agricola) the development of a clear technical vocabulary. It meant for most of the authors a clear explanation of metallurgical techniques (in opposition to alchemy) as a way of increasing the productivity and efficiency of metallurgical operations. In a context of great social and economic fluidity, the idea of "openness" entailed the elevation of certain practical arts through authorship. Thus they were more accessible to a reading (as opposed to a skilled) audience, including the prince-practitioners. The princes of course did not need to choose between the exoteric and the esoteric since they themselves now had access to both.

Exoteric mining and metallurgical authors elaborated notions that had an important influence on 17th-century science. Particularly significant in this regard was the ideal that knowledge should be transmitted openly in writing and the association of that ideal with empirical practices. The influence of these 16th-century writers on 17th-century experimental philosophy has been obscured, I believe, by some of Francis Bacon's influential views. The close relationship of Bacon's "great instauration" to prior writings on the practical arts is suggested by his project of the histories of the trades. These histories were to be complete written accounts of the products and operations of the mechanical arts. Scholars would compile them by seeking out and thoroughly inspecting all of the crafts. They would be "unincumbered with literature and book learning" because the sciences transmitted through books were stagnant. On the other hand, the mechanical arts, which Bacon described as outside the written tradition, had "some breath of life" and were "continually growing and becoming more perfect."[88] Bacon also admonished, "Never cite an author except in a matter of doubtful credit."[89] In his rejection of book learning and in his view of the mechanical arts as the product of an oral tradition of nonliterate practitioners, Bacon disregarded the

(1974). Pamela O. Long (1985) discusses the ideal of the unity of theory and practice in architectural writings. For some of the older discussions, see Zilsel (1942); Hall (1959); Houghton, Jr. (1957).

87 Moran (1981), and, for a discussion of one particular court, Moran (1985).

88 Bacon (1960, 6–8). Bacon was particularly interested in mining and metallurgy and wrote inquiries on these subjects. See Webster (1975, 346).

89 Bacon (1960, 274).

extensive prior tradition of writings on the practical arts. He failed to acknowledge that for many of the trades, including mining and metallurgy, extensive histories had already been written.

In the 1660s, inspired by Baconian ideas, the Royal Society of London set out to write histories of all the trades. Robert Boyle initiated the history of mining and metallurgy by positing an elaborate series of more than a thousand questions, which were published in the *Philosophical Transactions* of the Royal Society.[90] With the help of the questions, scholars or philosophers were to go out and interview illiterate craftsmen. Both would thereby benefit. The craftsmen could contribute a wealth of particulars otherwise inaccessible to the scholar, and the scholar with a broader overview could suggest improvements to the trades.[91]

When Boyle initiated this Baconian project, he also followed Bacon's admonition concerning credit to authorship. For he failed to mention that his elaborate series of questions on mining were derived, not from interviewing craftsmen, but from one of the most comprehensive histories of a trade ever written, the *De re metallica* of Georgius Agricola.[92] Subsequently, members of the Royal Society at times attempted to make prior writings such as Ercker's *Treatise on Ores* available in translation. However, they also frequently failed to cite their 16th-century written sources when they used them.[93]

Yet those sources had an important influence. Mining and metallurgical writers consistently urged that knowledge be open. They parlayed craft knowledge into openly written form and condemned the obscurantism of alchemy. The economic and social context in which they wrote encouraged them to oppose both artisanal and alchemical secrecy. Although credit for their authorship was rapidly obscured by some of the myths of 17th-century science, the actual presence of their influence is evident in the connections made then and now between openness, empiricism, and the progress of the sciences.

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[90] Boyle (1966a). For a systematic study of the histories of the trades see Ochs (1981; 1985).

[91] For his ideas about the histories of the trades, see Boyle ([1772] 1966b).

[92] Robert Boyle's groups of questions often reveal line-by-line correlations with passages of *De re metallica,* Agricola's statements having been changed into interrogatory form. Such comparison leaves no doubt that Agricola's masterpiece was one of Boyle's important sources.

[93] See Armstrong and Lukens (1939). An example of indebtedness of a mining industry to a 16th-century source is Samuel Colepresse's description of the Devon and Cornish tin mines (1671). The introduction to this history (which discusses the flood) is too close to the 16th-century antiquarian Richard Carew's introduction to the same subject to be accidental. Compare Carew ([1602] 1969, 7). See Ochs (1985, 137) for a discussion of Colepresse's account.

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Chapter 2
Political Designs: Nuclear Reactors and National Policy in Postwar France

Gabriele Hecht

The image of technology marching forward to the beat of its own drum, sending repercussions throughout society, has been repeatedly and successfully challenged in the past decade. We have yet to persuade wider audiences, but at least in the general field of science and technology studies, technological determinism is dead.

The demise of technological determinism means that studies of technological design and development must now do more than just show how political, social, economic, and cultural considerations shape and become part of technology. We are presently poised to use technological artifacts as lenses through which to view broader historical questions, to understand how the process of shaping technology can also be the process of shaping politics, society, and culture. The time is ripe for the history of technological design and development to become a more integral part of mainstream history.[1]

This article delineates one way in which a close examination of technological design can contribute to broader historical questions. By placing the design of two nuclear reactors in the political framework of postwar France, I show that these reactors were more than technological artifacts. Engineers and managers inscribed their political agendas into the design of their reactors, and for a variety of reasons, including the instability of government leadership in the 1950s, these designs in turn became part of French political discourse. Each reactor came to embody a particular vision of the French state, and each became a powerful tool in shaping both nuclear and industrial policy in postwar France.[2]

The article begins with a quick sketch of postwar France and an outline of the creation of the two institutions that made up the core of the French nuclear program: the Commissariat à l'Énergie Atomique (CEA)—the atomic energy commission—and Électricité de France (EDF)—the nationalized electric utility company. Through the early 1950s, the nuclear program rested solely in the hands of CEA leaders, so I next discuss how these men picked the gas-graphite reactor design over other choices. Finally, the bulk of this article compares the designs of two early gas-graphite reactors, one financed by the CEA, the other by EDF. The two institutions collaborated on the design and construction of both reactors: France did not have the financial, human, or political resources to sustain separate programs. But, even

[1] Works that have implicitly or explicitly made this point recently include (but are by no means limited to) Hughes (1989); McGaw (1987). I do not wish to imply that mainstream historians have ignored works in the history of technology or that these works have not contributed to other areas of historical scholarship—they have done so most fruitfully. But, studies of technological design have tended to concentrate on deconstructing the processes of invention, development, and diffusion, thereby speaking primarily to audiences already interested in technology. Such analyses have many virtues, but contributing to broader historical debates is not among them.

[2] Other studies that point to ways in which technological designs became political tools include Pfaffenberger (1990); MacKenzie (1990).

though each institution needed the other in order to build a nuclear program, they had different, sometimes conflicting, political, industrial, and technological agendas. Each reactor thus became a distinct political, industrial, and technological statement.

2.1 France after the Second World War

The Second World War left France both economically and psychologically devastated. The German occupants had rendered more than half of the railroad network unusable, they had requisitioned most of the machine tools under twenty-five years of age, they had exported 15 percent of all agriculture products—and the list goes on. Almost as damaging, the French had suffered the third defeat at the hands of the Germans in seventy years—and this was by far the most mortifying, as it subjected them to four years of foreign occupation and the opprobrious spectacle of their fellow citizens collaborating in their humiliation.[3]

No surprise, then, that after the Liberation left-wing politicians and followers of Charles de Gaulle alike shared the pressing priority of reconstructing their nation's economy and morale. Further, they shared the belief that France's defeat resulted largely from the "economic feudalism" and "Malthusianism" practiced by private industry and politicians before the war. The state, they concluded, should provide the impulse for, as well as direct, the reconstruction. By engaging in and promoting investments aimed at modernizing and expanding French industry, the state would accomplish the dual aim of resuscitating the economy and restoring the country to its rightful place in the ranks of great nations.[4]

This reconstruction effort advanced on many fronts. A new government institution known as the Commissariat Général au Plan (the Planning Commission) set nationwide production goals and coordinated economic development in various private and public industrial sectors.[5] The electricity, coal, and gas industries were nationalized, making the state the only shareholder in these companies. Thus, the new economic structure would, in principle at least, serve the French people rather than private interests.[6]

In order to ensure the success of this drive for industrial strength, politicians drew on a resource that France had had for almost two hundred years, the *grands corps de l'état*, and in particular on the two engineering corps, the Corps des Mines and the Corps des Ponts et Chaussées. These corps, made up almost exclusively of graduates of the École Polytechnique, had long espoused an ideology of engineering in the public service. Their power within the state administration, however, had waxed and waned over the years. In the I9th century, their engineers had built French railroads and exploited French mines.[7] But their efforts to "rationalize" the state economy through planning during the Depression had failed because of their insistence on remaining above the "irrational" processes of politics.[8] Postwar France saw a massive resurgence of grands corps engineers in positions of power in the ministries, on the Planning Commission, and at the head of the nationalized industries.

[3] See, e.g., Rioux (1980); Larkin (1988).
[4] Asselain (1984, 109). See also Rioux (1980).
[5] The first head of this Commissariat Général au Plan was Jean Monnet, and the first five-year plan became known as the Monnet Plan. For more on this, see Rioux (1980); Asselain (1984); Bonin (1987); Rousso (1986); Massé (1965).
[6] Kuisel (1973).
[7] For more on these engineering corps, see Smith (1990); Thoenig (1973); Thépot (1985); Suleiman (1978).
[8] Kuisel (1973). See also Brun (1985).

One such establishment was Électricité de France. Before the war, a multitude of private companies had supplied the nation's electricity, using different networks that ran on different frequencies and voltages. To many members of the postwar provisional government, these companies represented the epitome of capitalist evil, privileging short-term profit making over providing coherent public service. Further, the heterogeneity of the distribution and transmission network undermined the reliability of the electricity supply. Engineers, labor unions, and politicians agreed that the new France should be on a single, standardized electrical network run by a single, nationalized institution. After heated debates on the precise ways in which such an institution should be structured, a nationalization law finally passed in April 1946. It regrouped the private companies into a single electric utility, EDF, which was accountable for its expenditures to the Ministry of Finance and for its development program to the Ministry of Industry. The new utility had a mission to provide France with a reliable, cheap, and abundant supply of electricity, and it immediately embarked on a massive hydroelectric program.[9]

Whereas EDF was the subject of much public discussion, the atomic energy commission grew out of backstage negotiations. After Hiroshima and Nagasaki, politician Raoul Dautry and Communist physicist Frédéric Joliot had no trouble convincing de Gaulle, the head of the postwar provisional government from 1945 to 1946, that a nuclear program would both elevate France's stature in international politics and accelerate its industrial and economic recovery. Following de Gaulle's recommendation, the National Assembly approved the creation of the *Commissariat à l'Énergie Atomique* in October 1945. The agency's stated mission was to pursue "scientific and technical research with a view to the utilization of atomic energy in the several areas of science, industry, and national defense."[10] In order to fulfill this mission, the creation ordinance continued, "[The CEA has to be] very close to the Government and, so to speak, mingled with it, and nevertheless vested with great freedom of action. … It must be very close to the Government because the fate of the nation can be affected by developments in this branch of science, and it is therefore indispensable that it be under the authority of the Government. It must, on the other hand, be vested with great freedom of action because this is the *sine qua non* of its efficacy."[11]

The CEA was the only public institution with such a high degree of autonomy, the only one not accountable to a specific ministry, and the only one not subject to the same financial controls as other state enterprises. Its internal organization reflected an ambiguous marriage of science and politics: it was a dyarchy headed by High Commissioner Joliot and Administrator General Dautry.

Both EDF and the CEA, then, grew out of the postwar vision of the kind of relationship between industrial development and the state that would ensure the reconstruction of the nation. Most high-level officials in each institution belonged to one of the elite engineering corps and had therefore been inducted into the great French tradition of engineering in the public service.[12] In accordance with this tradition, these officials saw themselves as guardians of the public interest in matters technological. But EDF, as a nationalized com-

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[9]Picard, Beltran, and Bungener (1985); Frost (1991).

[10]Scheinman (1965, 8).

[11]Scheinman (1965, 12).

[12]CEA engineers tended to belong to the Corps des Mines, and EDF engineers tended to belong to the Corps des Ponts et Chausees. This difference exacerbated the tensions between members of these institutions. See Picard, Beltran, and Bungener (1985) and Simonnot (1978).

pany, reflected the left-wing side of the postwar coalition government, while the CEA, especially after Joliot's departure in 1950, represented the Gaullist side.[13] Officials in each institution, therefore, tended to have different definitions of the public interest. As we shall see, these definitions, more than any formal government decision, would determine the course of the nuclear program.

2.2 Negotiating a Design

In the late 1940s and early 1950s, atomic development rested solely with the CEA. Its scientists and engineers, therefore, set the initial parameters of the nuclear program. Initially, that program concentrated on locating uranium mines and building research reactors. Although "national defense" had been mentioned in the original ordinance, building an atomic bomb seemed out of the question, both technologically and politically. In its twelve years, 1946–58, the Fourth Republic saw over twenty different heads of state; for eleven out of those twelve years, the French representative to the United Nations asserted that France would never build a bomb. But, these statements did not suffice to soothe the United States in an atmosphere of intensifying Cold War. When the CEA's high commissioner, Joliot, publicly declared in 1950 that he would not build a bomb because such a weapon could only be destined for the Soviet Union, American leaders protested the presence of a Communist at the head of such a strategically sensitive institution. In April 1950 Joliot was dismissed and replaced a year later by another eminent (but less vocal) scientist, Francis Perrin.[14] In August 1951, the CEA's administrator general Raoul Dautry died.

These developments left the CEA without apparent direction. It had found an important political ally, however, in parliamentary deputy Félix Gaillard.[15] In 1951, Gaillard became a state secretary, in which capacity he served as the government's official representative to the CEA's steering committee.[16] Convinced that France's future lay in the strength of its nuclear program, he urged the committee to draft an ambitious five-year plan for the development of atomic energy—one that would seduce Parliament by promising material benefits not in fifty years, but in the near future. It would be easier, Gaillard said, to justify a 20-billion-franc plan that included developing atomic energy on an industrial scale than a 3-billion-franc plan devoted only to basic research.[17] Perrin and other scientists expressed doubts about whether the CEA could carry out such an extensive program. But, technocrats such as François de Rose of the Ministry of Foreign Affairs supported Gaillard. The CEA should aim high, he

[13]The minister of industry who pushed through the nationalization of EDF, Marcel Paul, had been appointed by de Gaulle; however, he was also an active member of the Communist labor union, the Confédération Général du Travail, and de Gaulle had appointed him in pan for this reason. Fourth Republic governments were generally coalitions of those parties best represented in the National Assembly, and in 1944–46 the Communists made up the single largest faction in the assembly. De Gaulle thus had to have Communist ministers in his cabinet, something to which he was not averse in this period, as the Communists had been among the most active members of the Resistance.

[14]For more on this incident, see Weart (1979).

[15]Gaillard was the Secretaire d'État à la Présidence du Conseil under René Pleven (1951), Edgar Faure (1952), Antoine Pinay (1952), and René Mayer (1953). He had authority over the CEA thanks to the following decrees: August 14, 1951; January 23, 1952 (52-105); March 22, 1952 (52-328); January 10, 1953 (53-10). See Lamiral (1988).

[16]This committee was made up of ten members, *haut fonctionnaires* scientific or industrial leaders, and presided over by either the prime minister or his representative.

[17]These are "*anciens francs*."

said. True, France led the second-tier nuclear nations, but, he argued, this might change if Germany decided to start a large-scale nuclear program. In such an event, France's future leaders would thank the CEA for having the foresight to plan extensive nuclear development.

Persuaded by these arguments, steering committee members agreed that the next step was to build a full-scale reactor. But what sort of reactor? Primary reactors, such as those built by the British, ran on natural uranium, of which France had plenty. So-called secondary reactors, developed in the United States, ran on enriched uranium. To run secondary reactors the CEA needed enriched uranium, which was not for sale anywhere. It would therefore have to build a uranium enrichment plant, which would take years. Primary reactors could produce weapons-grade plutonium, in addition to electricity. Without further ado, the committee settled on primary reactors.

No, the French government had not decided to build an atom bomb. At the time of these meetings, in 1951, no head of government had seriously considered this possibility.[18] But the CEA steering committee members, as the guardians of French nuclear interests, felt that stockpiling plutonium certainly could not hurt. Ostensibly, this plutonium was des tined for breeder reactors in the distant future, but almost everyone on the committee had its military potential in mind. Without specifying the end use, they set a production goal of 15 kilos of plutonium within five years.[19]

Having made this decision, the committee next had to pick a moderator for its reactor. The choice was between graphite and heavy water, and before continuing, I must digress briefly in order to explain some basics of a fission reactor.

Natural uranium, which France had, contains two isotopes of uranium: U_{238}, and U_{235}. Fission occurs when a U_{235} atom absorbs a neutron, causing the lighter uranium atom to split and liberating a great deal of energy as well as some more neutrons. Some of these neutrons will be absorbed by more U_{235} atoms, causing more fission; scientists and engineers at the time understood that with enough uranium piled up—what is known as critical mass—this fission reaction will be self-sustaining. Other neutrons, absorbed by U_{238} atoms, will not cause fission. Rather, on absorbing a neutron, a U_{238} atom becomes U_{239}—which eventually changes into Pu_{239}, or weapons-grade plutonium.

The committee members knew that in order for a U_{235} atom to absorb a neutron, that neutron must be traveling at a speed lower than that at which it is released. Therefore, a moderator was required to slow down the neutrons; further, the ideal moderator would not absorb any neutrons. Finally, in order to extract the heat from the reactor core, a coolant was needed.

By the time the steering committee met in September 1951, the CEA had already built heavy-water experimental reactors.[20]Physicists preferred heavy water as a moderator because it absorbed fewer neutrons. But heavy water could only be manufactured by electrolysis, which itself required electricity.[21] Further, argued the engineers on the committee,

[18]See Scheinman (1965, 8).

[19]In his various books, Bertrand Goldschmidt also asserts that the military goal of plutonium production was at least tacitly understood by everyone. See, e.g., *Les pionniers de l'atome* (1987) and *Le complexe atomique* (1980). This assertion was widely confirmed by the many CEA engineers whom I interviewed in 1989–90.

[20]For more on the CEA's experimental reactor program, see Weart (1979) and Goldschmidt (1980, 1987).

[21]During the course of this research, I interviewed about seventy engineers who worked either for the CEA or EDF in the 1950s and 1960s. For legal and procedural reasons, I cannot provide individual names for specific citations. I can, however, list the names of interviewees used as sources for this article (interview dates are in parentheses): Pierre Bacher (May 11, 1990), Claude Bienvenu (October 27, 1989), Rémy Carle (February 27, 1990), André

a heavy-water manufacturing plant seemed complicated and expensive to build, whereas France already manufactured graphite. Such were the official reasons given for choosing graphite over heavy water as a moderator.

An additional reason, however, was suggested by an engineer who worked on the early gas-graphite designs. Many of those who had worked with the heavy-water research reactors were Communists. Some but not all of these people had been dismissed along with Joliot in 1950. Since the plutonium produced by the first industrial-scale reactors might go into a future French bomb, certain committee members wanted an easy way of excluding Communist scientists and technicians from the new projects. Not picking the technology in which they had experience greatly facilitated this task.[22]

The CEA steering committee thus settled on a five-year plan for 1952–57 that committed the CEA to building two reactors, powered by natural uranium and moderated by graphite. The plan also included a factory to extract plutonium from the spent uranium fuel which would emerge from the reactors. Pleased with these goals, Gaillard easily convinced his fellow parliamentary deputies to approve the plan. Because it conferred prestige and glory, he argued, France needed nuclear energy. He emphasized the nation's weakness in energy re sources and noted that expanding the nuclear program meant developing France's industrial base and ensuring its future energy supply. Without specifying that the reactors could produce weapons-grade plutonium, he opined that France should not publicly renounce the right to build a bomb when countries on both sides of the Iron Curtain had such weapons programs. Presumably because they trusted the experts who had conceived the plan, the other deputies did not question Gaillard on any of the details. With remarkably little debate, they voted a budget of 37.7 million francs to the CEA in July 1952.[23] Having funded the plan, Parliament left its implementation to the scientists and grands corps engineers who headed the CEA: after all, most of them had been explicitly trained to serve the state and should be trusted to do so. Parliament had other, more pressing political problems to resolve.

The CEA committee members had chosen the gas-graphite design knowing that it *could* yield weapons-grade plutonium. But, it was the agency's new administrator general, Pierre Guillaumat, appointed by the committee in November 1951, who ensured that the reactors *did* produce this plutonium. Guillaumat had graduated from the Ecole Polytechnique and belonged to the Corps des Mines; he was also a longtime friend and ally of de Gaulle. He aggressively pushed the plutonium production program, while successive heads of government continued to proclaim France's sole interest in the peaceful atom, at a time when debates within ministerial circles over this position had barely begun.

Guillaumat created the Direction Industrielle to coordinate the construction projects, placing Pierre Taranger, another Polytechnique graduate, at its head. Guillaumat and Taranger made it clear to their top engineers that they had to build a plutonium-production

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Grégut (June 18 and 20, 1990), Adrien Mergui (December 18, 1989), Jean-Pierre Roux (December 20, 1989), Boris Saïtcevsky (February 27, 1990), André Teste du Bailler (November 28, 1989), and Pierre Zaleski (December 22, 1989).

[22]Interviews (fn. 21).

[23]The Communist deputies, whose party had recently focused on obtaining signatures for the Stockholm Appeal, a worldwide petition calling for a ban on nuclear weapons, tried to introduce a clause into the plan that would formalize France's commitment to the peaceful atom. The rest of the assembly interpreted this effort as a piece of Communist propaganda, and the clause was shelved. There was thus no parliamentary discussion of the military implications of the plan. For more on parliamentary action—or inaction—on this issue, see Scheinman (1965, 8).

facility as quickly as possible. In less than five years, both the first reactor, G1, and the plutonium extraction factory were operating at Marcoule. Studies for a second, larger reactor—G2—were under way, and Guillaumat had even negotiated an agreement with the Ministry of Defense to build a third plutonium-producing reactor, thereby more than doubling the CEA's funding.[24]

Parliament had approved the Gaillard plan in part because it supposedly represented the first step toward a more extensive nuclear *energy* program. Yet no one even mentioned extracting electricity from G1 until its design was almost finalized. Then Pierre Ailleret, the head of EDF's research division and a CEA committee member since 1950, suggested appending a 5-megawatt plant to G1.[25] Small frictions had already arisen over which institution would provide France with nuclear energy: EDF, the country's official electricity supplier, or the CEA, the official guardian of all things nuclear. For Ailleret, Gl provided the perfect opportunity to involve an EDF team in the nuclear adventure. The CEA agreed, stressing however that the project should not interfere with plutonium production. With this same caveat, the CEA also agreed to let EDF build a 25-megawatt plant for G2.[26]

The unstable political climate of the early to mid-1950s, plus government indecision about nuclear policy, thus gave CEA leaders a free hand in shaping the French program.[27] By deciding what kind of reactor to build, they, not the government, made French atomic policy. They could not go so far as to openly pursue the military nuclear option—nor did all of them want to. But they could take steps in that direction.

That is but the beginning of the story. Given the inherent ambiguity of this design, we must now seek to understand how engineers and managers in the CEA and in EDF played on that ambiguity in order to further their political and industrial goals. Comparing the contracting process, the design, and the rhetoric surrounding two early gas-graphite reactors—G2 and EDF1—reveals how the reactors became the policy-making tools of engineers.

2.3 Putting Together a Reactor Project, CEA-Style

The method by which Taranger and Guillaumat chose the companies that would design individual components for G2 marked the beginning, in France, of a new kind of industrial policy: the "policy of champions."[28] Both men felt that the best way to convince private industry to participate in a venture that would not yield immediate profits was to handpick the most technologically "advanced" company to design each component, regardless of cost. Such a policy, they argued, meant that French companies would not waste time or resources competing against each other. Further, it would provide a nurturing environment in which industry could develop new technologies. French industry would acquire valuable

[24] Vallet (1986, 50).

[25] Ailleret was in fact EDF's first representative at the CEA. In principle, other members of EDF who should also have been involved in CEA committees were the président du Conseil d'Administration (decree of January 3, 1951); the directeur général or one of his adjuncts (April 19, 1951); two directeurs généraux adjoints (December 12, 1952, and November 18, 1952). See Lamiral (1988).

[26] Picard, Beltran, and Bungener (1985, 187).

[27] This is Scheinman's thesis (1965, 8), but he reaches this conclusion in a political study without examining the meetings in which the gas-graphite decision took place, much less the reactor design itself. He is therefore unable to say much about the tools and tactics used by engineers and managers to shape the nuclear program, or about the implications of their having had such powers.

[28] Interviews (fn. 21); Lamiral (1988).

know-how, which it could use in the future to export technology. Guillaumat and Taranger maintained that this policy would thus enhance France's industrial base in the short term and its economy in the long term.

Under Taranger's guidance, the CEA's Direction Industrielle grouped the chosen companies into a consortium and placed the Société Alsacienne des Constructions Mécaniques (SACM), itself a conglomerate of electrical and mechanical engineering companies, at the head. The CEA signed a contract with the SACM, which subcontracted to the other companies and coordinated the overall design and construction process of G2. The design process was a cooperative effort: after CEA engineers defined the function of a reactor component, industrialists would propose an initial design, which would then be discussed in a series of meetings. Two sorts of meetings occurred: meetings between a CEA team and a single company to talk about a specific component, and large monthly meetings grouping together representatives of all the CEA teams, all the companies, and EDF when relevant.[29] The reactor had to be built quickly,[30] and this decision-making process had the short-term advantage of producing solutions that industry was capable of building. Left to their own devices, said an EDF engineer present at these meetings, CEA teams would have envisioned complex solutions beyond the means of French industry, and the deadline would never have been met Even so, he added, the solutions chosen were often costly and cumbersome.[31]

Cost was not a consideration for the CEA, however, and for G2, EDF had little to say in the matter. From the beginning, CEA engineers made it clear that EDF had to play a subordinate role. It had also signed the main contract with the SACM, but the "energy recuperation installation," as EDF's part was called, was considered an auxiliary device to the reactor. So while the EDF engineers sat in on the monthly meetings with industry, they were not expected to voice their concerns over the design of their installation. Industry had to give the CEA contracts priority over those signed with EDF.[32] Furthermore, EDF engineers did not always know about design changes that had a direct impact on their work.[33] As we shall see, this meant that no part of the reactor—as opposed to the generating unit—was designed to optimize electricity production.

The "nuclear" part of the reactor, then, took priority over the "classical" part.[34] The "best" companies were chosen to build the trickiest, "most nuclear" parts, with no regard for cost. Contracts signed for such parts were contracts of principle, in which the company agreed to build a device that would perform certain functions, but the specifications were not fixed ahead of time. In contrast, the "less nuclear" parts of the reactor, such as the energy recuperation installation or the prestressed concrete vessel, were covered by contracts detailing both specifications and cost.[35]

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[29]Lamiral (1988).

[30]Virtually all of the CEA personnel I interviewed stressed that solutions had to be chosen and implemented quickly because they were in a hurry to get sufficient quantities of weapons-grade plutonium to make one or more bombs.

[31]Lamiral (1988, 26).

[32]Interviews (21); Lamiral (1988, 27).

[33]For example, in a letter to the CEA's Direction Industrielle dated March 4, 1958, accompanying a report on the "Installations de récupération d'energie G2-G3" (personal papers of Claude Bienvenu, interviewee), Georges Larniral wrote, "We attract your attention to the following fact: as a result of the latest modifications effected on the fuel load, the Société Rateau put together a study in order to determine the new characteristics of the CO_2 in the [energy] recuperators. The results of this study have not been communicated to us" (my translation).

[34]The terms "nuclear" and "classical" were used by CEA and EDF engineers alike throughout the 1950s and 1960s.

[35]Interviews (fn. 21).

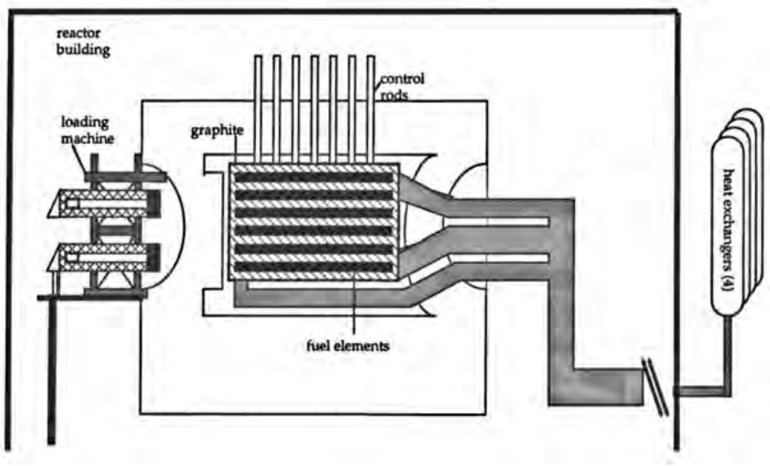

Fig. 1: The CEA's G2 reactor at Marcoule. Schematic diagram adapted from *Bulletin d'informations scientifiques et techniques du CEA* (1958). Drawing not to scale.

So, what sort of reactor emerged from all this? Figure 1 depicts G2. Most of the reactor was housed in a large building designed to protect its contents from the vagaries of the weather (like many of their Soviet counterparts, the Marcoule reactors did not have containment buildings). The core, contained in the large cylinder, was made up of a stack of graphite bars piled in horizontal layers. Distributed through this pile were 1,200 channels, into which the uranium fuel was loaded. The uranium came in small cylindrical rods hermetically encased in aluminum cans. Each channel could contain up to twenty-eight of these uranium rods, or fuel slugs. When enough slugs were loaded into the reactor, it "went critical," setting off a self-sustaining fission reaction. The fission taking place inside the slugs liberated a great deal of heat, which the cans absorbed. Carbon dioxide gas, entering the channels through openings on the back face, flowed around the slugs and cooled the reactor by absorbing this heat. On leaving the core, the coolant traveled to the "energy recuperation installation" where the heat was converted into electricity.

Even this quick overview of G2 reveals how the electricity-generation function of the reactor was relegated to secondary status. The energy recuperation installation stood outside the building that housed the reactor, both physically and symbolically removed from the fission reaction. In order to show in greater detail how the political agenda of plutonium production took precedence over the political and industrial agenda of electricity production, I will focus on two aspects of G2's design: the loading and unloading of fuel in the reactor, and the energy recuperation installation itself.

For the CEA, the key point in making weapons-grade plutonium was to obtain as much Pu_{239} as possible with as few "poisonous" isotopes of plutonium as possible. The Pu_{239} produced when a U_{238} atom absorbed a neutron and decayed was not a stable isotope: with time, it changed into Pu_{240} and Pu_{241}. Among other reasons, these were called "poisonous" because, when present in sufficiently concentrated quantities, they could spontaneously fission.[36] A bomb containing too large a proportion of these isotopes might, therefore, explode unpredictably. The CEA team working on Marcoule's plutonium extraction factory had already settled on a chemically based process to separate the plutonium from the spent uranium fuel, a process which did not distinguish between different isotopes of plutonium. The G2 teams had little knowledge and a severe time constraint to work with; under these conditions, the only solution they could devise that would minimize the poison involved removing the fuel slugs before too much Pu_{240} or Pu_{241} appeared. The shorter the time that each uranium slug was irradiated—that is, allowed to undergo fission—the less poison was produced. CEA engineers calculated that at the optimal irradiation for producing the right balance of isotopes, any given fuel slug should not stay in the reactor longer than 250 days.[37] In terms of electricity production, this short irradiation period represented extremely inefficient use of fuel: ideally, the slugs should remain in the reactor until the uranium was all burned up for maximum heat yield.

These considerations led CEA engineers to impose a technopolitical constraint on the SACM, the company in charge of designing and building this system. With twenty-eight fuel slugs in each of 1,200 channels, stopping the reactor, then unloading and reloading the core channel-by-channel and restarting the reactor every 250 days would have wasted far too much time.[38] And saving time was crucial to the CEA, both technologically, to avoid getting poisonous isotopes of plutonium, and politically, since they wanted the maximum amount of weapons-grade plutonium as quickly as possible. CEA engineers hence asked for a loading system that could function while the reactor was operating.[39]

SACM engineers chose a costly and cumbersome solution, but it fulfilled the CEA requirements perfectly. A cement block containing tubular holes was built flush against the northern face of the cylindrical vessel. This block contained one tube for each channel. On the far left-hand side the tube connected with the loading device, placed on a traveling crane built on a platform adjacent to this block. The device itself consisted of two lock chambers side by side. By maneuvering the crane up and down and from side to side along the cement block, an operator sitting on top of the crane could couple these lock chambers with any of the channels of the core. Because the operation took place while the reactor was on line, these chambers were constantly exposed to radioactivity. They were therefore encased in 56 tons of metal and concrete.[40]

The lock chamber linked up to a storage chamber containing the new fuel slugs. New fuel slugs would be loaded onto an elevator and brought up to the storage chamber. The lock chamber, moving back and forth on a track, would pick up the slugs, bring them over

[36]More specifically, even-numbered isotopes of uranium and plutonium are not fissionable, and for this reason Pu240 was a "poison." Plutonium 241 was spontaneously fissionable.

[37]Power is a measure of energy per unit time; in the case of a nuclear reactor, it measures how many decays occur in a given time period. G2 operated at a power of around 200 thermal megawatts (MW) and contained 100 tons of uranium. The optimal irradiation (measured in power multiplied by the number of days in the reactor, divided by the tonnage of uranium) was set at 500 MW days/t. To find the number of days, engineers made the following calculation: 500 MW days/t = [(200 MW)/(100 t)] x N days. So N days = [500 MW days/t] x [100 t/200 MW] = 250 days, the amount of time a slug should remain in the reactor (data obtained from interview).

[38]From the standpoint of today's reactor technology, one might imagine that EDF would also be interested in loading the core while the reactor was on line in order to avoid losing money in plant downtime. As we shall see in the section on EDF's reactor, however, the technical and economic considerations that EDF engineers privileged in the mid- to late 1950s led them to prefer loading reactors that were stopped rather than those that were operating.

[39]Interviews (21); Ertaud and Derome (1958, 69–88).

[40]Ertaud and Derome (1958, 69–88).

to the channel, and a mechanical arm would reach into the tube and undo the plug. The new slugs would be loaded into the channel, pushing the irradiated slugs out. Because this entire procedure took place while the reactor was under pressure, a complex system of locks and sensors had to guarantee a perfectly hermetic seal every time the device coupled with the storage chamber or a channel.[41]

Fuel loading was by no means the only aspect of G2's design shaped by its plutonium-production goal. Another example can be found in the CO_2 cooling circuit and the energy recuperation installation itself. In essence, this installation contained four heat exchangers, one turbogenerator, and auxiliary equipment. The hot CO_2 gas exited the reactor core into the heat exchangers, where it cooled by transferring its heat to water. In the process, the water turned into steam and, after passing through a series of pressure stages, the hot steam would arrive in the turbogenerator, where its heat would be converted into electricity.

Had the main purpose of the reactor been electricity production, EDF engineers would have calculated the pressure, temperature, and flow of C02 that would have yielded the most efficient energy retrieval. G2's plutonium priority, however, imposed severe constraints on this energy recuperation cycle. First, the reactor had to operate continuously to avoid thermal shock to the fuel slugs. Second, because they had little interest in energy efficiency, CEA engineers had not designed the aluminum cans surrounding the fuel slugs to withstand high temperatures. These two constraints led the CEA to determine specific values for the pressure and temperature of the CO_2—values which did not correspond to those for optimal electricity production.[42] A third, more significant constraint was that the CEA wanted to operate the reactor at maximum power all the time. Maximum power meant more decays per second of U_{238} into Pu_{239}. Combined with the rapid unloading of the fuel slugs, this in turn meant that a maximum quantity of Pu_{239} could appear and be removed before too much of it decayed into poisonous isotopes. Running the reactor continuously at maximum power, however, could not be handled by the electrical network to which the heat generator was hooked up; because of variations in energy consumption, the network could not always absorb all that energy. So all these constraints forced EDF engineers to add a "desuperheater" to the circuit, placed just before the steam generator, to absorb excess heat. Furthermore, the fourth heat exchanger existed only as a safeguard in case of breakdown; in fact, three exchangers would have sufficed to run the reactor and the plant. Finally, in order to run the reactor at maximum power and low temperature, the CO_2 had to be blown through the core at a very high rate. This did not favor energy efficiency, first because the high rate required more electricity to power the blower, and second because the exiting CO_2 was at a lower temperature, and therefore contained less energy.[43]

CEA engineers thus translated Guillaumat's enthusiasm for a French atomic bomb into a reactor design whose ideal function was producing weapons-grade plutonium. The story just told about G2 could easily be that of the other two Marcoule reactors, G1 and G3. While the French government waffled over whether or not to build a bomb, CEA engineers took the crucial first step toward that bomb. They had almost finished building G2 in April 1958, when Felix Gaillard, by that time prime minister, signed the order to have a bomb ready in early 1960. Without these Marcoule reactors, France never could have exploded its first bomb on schedule.

[41] Ertaud andDerome (1958, 69–88).
[42] Passérieux and Scalliet (1958); Kieffer (1963).
[43] Passérieux and Scalliet (1958).

To the engineers and technicians at Marcoule, the military aspect of their work was no secret. A sense of excitement and urgency pervaded the offices in which engineers struggled over design problems and the construction sites where the huge reactors took shape. They were creating a brand new technology, of singular importance to their nation. Although they had indirect knowledge of some American, British, and Canadian nuclear work, they apparently did not have access to many of the technical solutions worked out by Anglo-Saxon researchers.[44] They thus relied on their limited experience with experimental reactors and their own ingenuity. Sometimes they favored solutions because they had heard that the British were working on something similar. More often, as explained above, they favored ideas that appeared to provide the quickest, if not always the most elegant, route to completion—and frequently they had no idea whether or not a device would work until it had been built and attached to the reactor. The uncertainty that thus dominated their work created what many later referred to as a "pioneering atmosphere" on the job, and it was this atmosphere and its accompanying excitement that saw them through the sixty- or seventy-hour workweeks that prevailed throughout the project. When the first French atomic bomb, loaded with plutonium produced at Marcoule, exploded on February 13, 1960, one engineer said that he and his colleagues were so proud of their country and the part that they had played in this achievement that they "shed tears of joy."[45]

The characterization of Marcoule as a French achievement, piloted by the CEA and implemented by French industry, pervaded the science and engineering press.[46] Some descriptions of Marcoule even referred to great French engineering achievements of the past. For example, in the introduction to a special journal edition devoted exclusively to G2 and G3, the CEA's high commissioner Francis Perrin wrote: "[The CEA] deemed it necessary to associate French industry with these great achievements which prepare the way for the development of the industrial use of nuclear energy. French industry answered this appeal, despite deadline and supply constraints that were often severe. … Above all, the result of this collaboration is apparent through the two massive buildings that dominate, by their 50 meters, the banks of the Rhône. [They are] a modern replica of the ancient wall of Orange that faces them."[47] And in an article for a mining engineering journal, the director of the Marcoule site gave his readers an idea of the scale of the reactors: "The Arc de Triomphe of the Étoile would easily fit in the vast metallic structure that shelters … G2."[48]

Before Gaillard signed the bomb order in April 1958, however, the fact that the plutonium that would be produced at Marcoule was destined for use in a bomb remained secret from those not intimately involved with the nuclear program. Thus, G2 both constituted French nuclear military policy and represented that policy's ambiguity. Ostensibly, the plutonium produced therein was destined for future experiments and reactors. G2 itself could in all legitimacy be presented as a prototype for a power-producing reactor—what other reason,

[44]According to the CEA personnel interviewed, they did not gain access to the knowledge that the British had accumulated while building gas-graphite reactors until well into the Marcoule projects. Rumors exist that some scientific and technical knowledge was secretly imparted to the French by the British, but even if these are true the amount of knowledge transferred in the mid-1950s is still likely to have been rather small.

[45]Interview.

[46]What is distinctive is the frequency with which articles specified that the reactors, or the site, were French. See Papault (1957); de Rouville (1958); Nous avons visité pour vous … le centre français de production de plutonium à Marcoule (1957), and many others.

[47]Perrin (1958) (my translation).

[48]de Rouville (1958, 486) (my translation).

after all, would EDF have for being involved with the project? That G2 did in fact produce a modicum of electricity (25 megawatts on a good day) enabled engineers and managers to speak of G2 as a successful prototype worthy of the tremendous investments made in the nuclear program. A 1957 article in a French civil engineering journal said:

> The first stage of the plan called for the construction of two nuclear reactors (Gland G2) that were supposed only to produce plutonium destined to fuel the secondary reactors of the future. … But during the study, we were led to envisage using the heat released by these reactors to produce electric energy.
>
> Currently, the predicted total investment amounts to 60 billion francs. This financial effort is justified by the necessity to develop, on the industrial scale, the production of electrical energy of nuclear origin, due to the insufficiency of European resources in fossil fuel. The role of Marcoule is essentially to allow this development, to train teams of operators at different levels, and to promote technical and industrial progress in this field.
>
> Investments should thus not be measured against the power of the installations, but against the development potential that they bring. In fact, the [amount of these investments] is quite in proportion to the increase in our energy needs.[49]

This was by no means the only such article written in the French engineering press. Seemingly, CEA personnel felt a need to justify their privileged financial status to their fellow engineers. In this way, the Marcoule reactors were the perfect instrument of French nuclear policy; they had primed the path to the French bomb, but until this path became public, they could easily be justified in terms of electricity generation.

We can see a striking example of the role played by the ambiguity of the Marcoule designs in French nuclear policy by briefly examining the first ministerial level discussions on the French bomb. These occurred in late 1954 in a series of meetings presided over by Pierre Mendès-France, then head of government. Those present included Guillaumat and Perrin of the CEA, as well as the minister of finance, the minister of national defense, the secretary of state for research, and various ministerial cabinet members. Guillaumat, the national defense minister, and others in favor of building a French bomb tried to push Mendès-France to make an official decision to that effect, arguing among other things that a bomb effort would have advantageous fallout for the civilian sector.[50] In the words of Mendès-France:

> I remember asking which part of the research under way was of economic interest, and which was only of military interest. They retired to a comer of my office to discuss matters in a low voice, and several moments later, they came back and told me, "for another three years, we won't be able to distinguish the military from the civilian; only after three years will we reach a branching point when we can say: this is purely military, and that holds a purely economic interest." Under those conditions, I said, there's no problem: we must continue

[49] Papault (1957, 389–398) (my translation). Measuring the worth of a large technological program in terms of its general value to the nation rather than in terms of direct economic returns on investment was an argument with a long tradition among French public engineers. See, e.g., Smith (1990, 683).

[50] Coutrot (1985). See also Simonnot (1978); Scheinman (1965, 8).

> to do research. … There was no question of amputating the positive aspects of such research work from the French economy.[51]

Thus Mendès-France, who until that moment had been one of the most decisive and forceful leaders of the Fourth Republic,[52] chose not to decide. His government only lasted two more months, so he never reached that fateful “branching point.”

Successive governments also flirted with making a firm decision. Until the Gaillard government, though, they all shied away from this task.[53] Thus, the ambiguity of the Marcoule design stood Guillaumat and others in favor of the French bomb in good stead. Depending on the political climate, they could bill the Marcoule reactors, which were the centerpiece of the CEA program, as purely civilian, purely military, or somewhere in between. Meanwhile, Guillaumat’s engineers had inscribed a military agenda into the reactors, enabling France to stroll discreetly toward the bomb.

2.4 Building a Reactor, EDF-Style

In early 1955, shortly after the start of the G2 project, top engineers in EDF and the CEA drafted a long-term gas-graphite reactor program. EDF1, a 60-megawatt electricity-generating reactor, was the first step in this plan. A succession of increasingly powerful reactors would follow, which by 1965 were to produce a total of 800 electric megawatts. The engineers submitted their plan to the PEON commission,[54] recently formed as a government advisory council on matters of nuclear energy development. Members of PEON included high-level engineers and managers from both institutions; not surprisingly, the commission approved the plan.[55] In July 1955, engineers began designing EDF1.

Despite the fact that the Marcoule reactors did not produce energy in the way that EDF wanted them to, the CEA’s efforts there did benefit the utility both technologically and politically. It had, after all, been able to experiment a little with nuclear energy production. Furthermore, it had neither the time, money, or expertise to launch an independent nuclear program. Politically, EDF’s participation in the Marcoule projects had buttressed the CEA’s claims that the Marcoule reactors were prototypes for power reactors. Conversely, Marcoule’s success strengthened EDF’s case for funding its own power reactors. Clearly, the two agencies collaborated in large measure because they needed each other.

In the course of working out the parameters of this collaboration, however, each agency was also eager to establish its role in defining the future not only of the nuclear program but also of French industrial development. In theory, the terms of cooperation were clear. CEA teams would design the “nuclear” parts of the reactor, and EDF teams would design the “classical” parts, but ultimately EDF headed the project and would thus make most of the final decisions. In practice, though, the EDF1 project was fraught with tension between engineers in the two institutions. This tension centered around two issues: the role of private

[51] Quoted in Simonnot (1978, 228–29) (my translation).
[52] See Larkin (1988); Rioux (1983); also Bédarida and Rioux (1985).
[53] In particular, the governments of Edgar Faure and Guy Mollet. See Simonnot (1978); Scheinman (1965, 8); Buffotot (1987).
[54] Its full name was the Commission Consultative pour la Production d’Électricité d’Origine Nucléaire; it was created in April 1955.
[55] The first members included Guillaumat, Perrin, and Taranger from the CEA, and R. Gaspard, Ailleret, and R. Guiguet from EDF.

industry in the project, and the actual design of the reactor. Conflicts did not emerge because the CEA did not want to build an electricity-producing reactor. Part of the CEA's mission was to develop nuclear technology in any form. But CEA engineers wanted to build these reactors their way. Further, they wanted to preserve the dual nature of the gas-graphite design, thinking that they might eventually get some plutonium out of EDF reactors, just as EDF had gotten some electricity out of Marcoule. Engineers in each institution considered their particular expertise as the key to designing EDF1: CEA engineers held that their intimacy with nuclear knowledge gave them the edge, while EDF engineers maintained that their experience with conventional power plants gave them the upper hand.

Finally, more was at stake than the EDF1 project alone. It was still far from clear that the nuclear program would receive long-term support. Precisely what would receive support (a military program, a civilian program, or both)—and to what extent—was also uncertain, particularly before 1958. Furthermore, project participants expected that the set of working methods and expertise that prevailed in the EDF1 endeavor would surely dominate, or at least influence, future reactor projects; they therefore felt that they were conceiving not just one but a whole series of French nuclear reactors.[56] In the 1960s, once the nuclear bomb program stood on firm ground and EDF had clearly established its authority over nuclear energy production, tensions between the two agencies lessened considerably.[57] But in the mid- to late 1950s, each institution was fighting for its future and the future of France.

The point of examining the EDF1 project, however, is not to find out who won this series of conflicts: almost invariably EDF did, because the protocols governing the collaboration did give EDF the final power of decision over its own reactors. Rather, the point is to understand how EDF engineers inscribed their political, economic, and technological agendas into the project and made EDF1 into their own policy-making tool. The conflicts, then, serve as additional proof that each institution really was doing more than merely designing a reactor: each was trying to impose a way of creating technology and a route for French nuclear development.

Tensions between the two institutions first became manifest in the organization of the project. As in the case of G2, Guillaumat and Taranger wanted private industry to coordinate the design and construction of EDF1. The nuclear team at EDF, however, believed that it should have the role of project coordinator[58]—the function that the SACM had fulfilled for G2. Team members espoused the anticapitalist sentiment that had originally led to the nationalization of the electric utility. By building EDF1, they were providing a public service. The best way to do this was to optimize the cost and efficiency of the reactor.[59] Ailleret, who by then had risen to the post of managing director of the utility, argued that EDF, not private industry, should conduct the optimization studies "in order to be sure that we are not influenced by the industrialist's tendency to develop certain types of materials rather than

[56]Tension was such that Ailleret insisted on naming the reactor EDFl rather than G4—or, as it was later known (once EDF had firmly established its place in the nuclear program), Chinon Al (information based on interview).
[57]Frost (1991) shows that in the 1960s tensions arose less between the two institutions than within EDF, between engineers involved in the technical design of power plants and financial experts concerned with medium-and long-term economic planning.
[58]This role was officially termed "industrial architect." The industrial architect coordinated the overall project, fitting the various components of the reactor into a whole and managing the contracts passed with individual companies.
[59]"Étude préliminaire d'une installation de récupération sur EDF1" (1956).

others."[60] Another team leader commented disparagingly, "Guillaumat and Taranger [were] oil men, who dreamed only of private industry."[61] Furthermore, it seemed appropriate that EDF should coordinate the overall design and building as well. The best way to keep costs down, EDF engineers argued, was to divide the reactor into parts and launch requests for bids for each part. EDF would thus have greater control over both the knowledge needed to build the reactor and the cost of the project. To top it all, this method of working was "politically correct": in the ironic words of a high-level EDF manager, "The pure and white EDF, a nationalized company, would acquire the know-how while leaving the builders, the capitalist companies, with the banal task of supplier."[62]

Overriding the objections of Guillaumat and Taranger, the EDF team decided to proceed in this manner.[63] The first step now was to draft a preliminary design project. Jean-Pierre Roux, the head of EDF's design team, had asked the CEA to do so in July 1955. EDF engineers found this proposal, heavily based on G2's design, unacceptable: they intended to generate electricity "optimally," something G2 did not do.[64]

The EDF team sought to change practically everything in the CEA's preliminary proposal.[65] In order to optimize the reactor for electricity generation, they wanted to control the definition of almost all the components and parameters, including components such as the uranium-graphite pile and the devices for loading and unloading the fuel, as well as parameters such as the pressure of the CO_2 cooling gas and the operating power of the reactor.[66]

The finished design of EDF1, shown schematically in Figure 2, looked quite different from that of G2. The most noticeable and perhaps the most symbolic change was that the heat exchangers were right next to the pressure vessel that contained the core, rather than many meters of energy-losing pipes away, and inside the reactor building (which was a spherical containment structure, unlike G2's building) rather than outside. The reactor still ran on natural uranium, encased in fuel slugs similar to those of G2; it was still moderated by graphite and cooled by CO_2. Just about everything else, however, had been modified.

The EDF team insisted on changing the operating pressure of the reactor and the pressure vessel containing the core. The CEA team had suggested a prestressed concrete vessel like the one at Marcoule. It argued that, in addition to being a tested technique, prestressed concrete was a domain in which France had outdistanced other nations. CEA officials felt

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[60] Ailleret's declaration to the Conseil Économique et Social (1963). Ailleret's post was called directeur général adjoint.

[61] Claude Bienvenu, quoted in Picard, Beltran, and Bungener (1985, 191).

[62] Jean Cabanius, quoted in Picard, Beltran, and Bungener (1985, 191). EDF's Direction de l'Équipement, the division in charge of building the reactors, had already espoused this working method while developing the hydroelectric sites.

[63] Interviews (21). Taranger had in fact pushed very hard for private industry to take the role of industrial architect and was furious when the EDF engineers rejected this course of action. The animosity left over from this meeting did much to increase the tension between the two institutions.

[64] Lamiral (1988, 280); "Memo RETN 1" (1957b).

[65] In order to expedite the development of EDF1's design, Ailleret formed a Comité Nucléaire within EDF. This committee met about once a month to discuss technological problems associated with reactor design as well as EDF's overall nuclear policy. It was in the course of these meetings that EDF engineers agreed on the basic characteristics for EDF1.

[66] "Étude des réacteurs énergétiques EDF, projet d'organisation dans le cas d'un réacteur du type uranium naturel-graphite-CO2" (1957a).

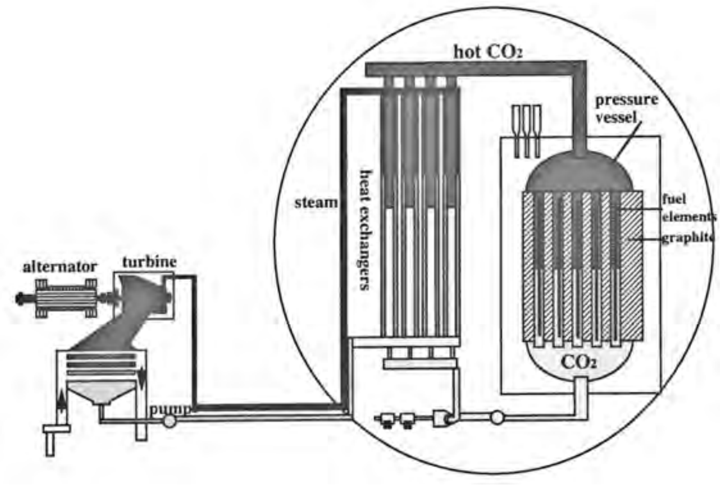

Fig. 2: The EDF1 reactor at Chinon. Schematic diagram based on EDF's *Rapport de sûreté Chinon A1* (1980). Drawing not to scale.

a responsibility to encourage French industry to reinforce its areas of excellence.[67] But EDF found this vessel too expensive. The prestressed concrete could not withstand the temperatures at which EDF planned to operate the reactor and would require a special cooling circuit. This would increase overall operating costs and lower the efficiency of the reactor: the blowers needed to pump the CO_2 through the special circuit would use up 10 percent of the electricity generated by the reactor.[68] Instead, EDF engineers chose an all-steel vessel, cylindrical in shape, capped by a steel hemisphere on either end.[69] A steel vessel could withstand higher temperatures and pressures. In addition, the fact that the United States and Great Britain had built steel vessels for their reactors gave EDF project engineers confidence that they too could build a working steel vessel for their reactor. They cared less about promoting technology uniquely or distinctly French than about using the cheapest, most reliable techniques.[70]

Early on, EDF engineers decided that EDF1 should function at a higher pressure than G2, with twenty-five bars instead of fifteen. Lower pressure reactors were easier and faster to build, and speed had been of prime political importance to CEA engineers in the G2 project. But a lower operating pressure meant that a higher flow of CO_2 was needed to extract the heat, which required more powerful blowers, thereby lowering the efficiency of the reactor.[71] EDF engineers had also decided, from the beginning of the project, that the

[67]The minutes of the EDF-CEA meeting of March 16, 1955, read, "Mr. Guillaumat feels that it is essential that an original French technology exist. But Mr. Gaspard [president of EDF] does not want to play the role of patron that much." Quoted in Lamiral (1988, 29).
[68]Interviews; Lamiral (1988, 280).
[69]They had considered various other possibilities before hitting on this one, including one which combined steel and prestressed concrete. Interviews (fn. 21).
[70]Interviews (21); Roux (1957).
[71]"Étude préliminaire d'une installation de récupération sur EDF1" (1956) and Ailleret's declaration to the Conseil Économique et Social (1963).

loading and unloading of the fuel would take place while the reactor was stopped. Unlike the CEA, EDF wanted to bum up the fuel slugs as much as possible in order to extract the maximum amount of heat. It had no use for a device that could move slugs in and out of the reactor at a high pace. Building a loading device that could work only with a stopped reactor limited the amount of weapons-grade plutonium that the CEA could demand from EDF1.[72]

Having decided on this loading principle, the EDF team then decided to orient the channels containing the slugs vertically, rather than horizontally as in G2. In a vertical configuration, the CO_2 could be pumped in at the bottom. It would thus follow the natural convection of heat, reaching the hotter parts of the reactor as it rose. This meant that less pumping power was required for the CO_2 and made the overall design safer in case of blower failure. A vertical pile also led to fewer openings in the pressure vessel, thereby making it easier to ensure that the core was hermetically sealed. It also meant that the reactor could be loaded and unloaded from the bottom. Bottom loading involved using "a single loading arm capable of reaching all the channels and requiring only one opening in the shell, although clearly a large one."[73] EDF engineers found this system much simpler and cheaper than the G2 design,[74] which had, as we saw, separate openings in the vessel for each channel and a huge, heavy machine designed to have access to every channel.

EDF engineers thus advocated a design they felt would make most efficient use of both fuel slugs and investments and that would be as simple as possible so as to provide a good basis for future reactors.[75] Both through the design itself and through the industrial contracting process, EDF engineers sought to redefine what a reactor was, how it should be built, and what it should be used for. By modifying pressure or temperature, EDF engineers had changed the performance and the capabilities of the reactor. They wanted to ensure that in future collaborative efforts, the CEA would have to work with such new parameters.

EDF engineers found that the process of designing a power reactor involved a great deal of guesswork and intuition. In the mid-1950s, they had no more access to foreign technology than did their CEA colleagues; at that time, furthermore, no nation had a functioning power reactor. They thus chose technical solutions that they thought would further their political, economic, or industrial goals. Often, they assumed solutions had to be fundamentally different from those advocated by the CEA. By the time they began designing EDF4 in the mid-1960s, they had revised several such solutions, finding ways, for example, to make prestressed concrete vessels and continuous fuel loading suit their purposes. What mattered in the mid-1950s, though, was that those within the utility and in the relevant ministries believed that EDF1 engineers had designed the project that best fulfilled the utility's goals.

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[72] Starting up any gas-graphite reactor for the first time inevitably requires that some slugs be removed before they are fully irradiated, thereby generating what was known as "fatal plutonium." Thus, the CEA did recover some plutonium from spent EDF1 slugs, but only a minimal amount. The point remains that EDF1 was not designed to produce plutonium. Later EDF reactors, however, did have loading devices that functioned while the reactor was on line. This was not solely a matter of the CEA imposing its will; EDF engineers gradually became convinced that such a system also benefited the economics of running a nuclear power plant. That, however, is a separate story which I treat elsewhere.

[73] Leo, Kaplan, and Segard (1958).

[74] Interviews (fn. 21).

[75] Interviews made it quite clear that simplicity of design was a major goal for EDF engineers. It should be noted here that while EDF engineers strove for low cost in designing EDFl, they did not attain that goal, in part because of an unforeseen event: the spherical steel containment vessel cracked, and repairing the crack added tremendous costs and delays to the project. But EDFl represented a first step toward optimizing reactor costs, and the general goal of minimizing costs held throughout EDF's reactor program.

EDF engineers' view of their work shows in the way they promoted their achievements to other French engineers—inside as well as outside their institution, for not everyone at EDF believed that nuclear energy could ever compete with conventional power plants.[76] Some of their prose paralleled that of CEA engineers, explaining that "anguishing" shortages in energy resources justified the huge "financial sacrifices" made for the nuclear program, sacrifices which, in any event, would soon pay off, since nuclear energy increasingly seemed like a "providential solution."[77] At the same time, though, EDF engineers tended to discuss ways to improve reactor technology more readily and more specifically. In particular, some looked to the day when they would no longer have to use natural uranium, a choice in which they had played no part: "The inferiority of natural uranium piles is less economic than it is energetic. Later, when we move to another kind of reactor that allows us to use enriched fuel, it will be less to lower the cost of the kWh than to reduce the specific consumption of fuel and increase, in considerable proportions, the amount of energy that can be drawn from natural reserves."[78]

Indeed, EDF engineers had reason to be preoccupied with the overall "efficiency"—both energetic and economic—of their electricity-generating technologies. In order to get France's energy sector back on its feet, EDF had focused, after the war, on building as many conventional power plants as possible, as quickly as possible. The resulting hydroelectric program had therefore paid less attention to cost than it had to speed and reliability. In the face of sharp criticism in the early to mid-1950s, EDF had adopted an institution-wide policy of *rentabilité*, best translated here as "economic viability," which coincided with the priorities of the Second Plan elaborated by the nationwide Planning Commission.[79] Engineers hence had to show that their designs would not lose money and would make efficient use of fuel. Already, the engineers who had built the hydroelectric plants were fighting with those in charge of thermal plants over whose work best fulfilled these requirements.[80] EDF's nuclear team therefore aimed its arguments about the benefits of nuclear energy as much at other, nonnuclear EDF engineers as at the world outside EDF.

It is also significant that EDF engineers compared their achievements with those of other nations, especially England. Jean-Pierre Roux compared EDFl with the Calder Hall reactor and concluded, "After this rapid overview of the principal characteristics, it appears that this French project holds up under comparison with the English projects."[81] This became even more true, he said, when one considered that the British took five to six years between reactors, whereas the French were only taking two.

Finally, EDF engineers waxed eloquent on the benefits of nuclear energy to emphasize that by building nuclear reactors, they were fulfilling their mission of public service to the French state and the French people:

> The path taken in giant steps during the past few years in the four large atomic countries, and especially in France, allows the highest hopes.
>
> It is not chimerical to think that the moment of massive realizations approaches rapidly.

[76]See Picard, Beltran, and Bungener (1985).
[77]Teste (1957, 73).
[78]Teste (1957, 73) (my translation).
[79]Bungener (1986).
[80]Picard, Beltran, and Bungener (1985).
[81]Roux (1957, 309) (my translation).

> Placed at the disposal of all, in the workshop and in the home, nuclear energy will allow economic and social progress to continue everywhere in the world, and in the European community in particular.
>
> France must reap the moral and material benefits that she has the right to expect from a technology so often fecundated by her scientists and already so widely developed by her engineers.[82]

2.5 Conclusion

We have seen here two gas-graphite reactors, two ways of organizing technological work, and two ways of promoting that work. There existed no single best way to build these reactors; they were not the inevitable results of some progressive logic inherent in the technology itself. Rather, each embodied a different set of political, economic, industrial, and technological agendas. The reactors were thus as much political artifacts as they were technological artifacts.

The story further reveals the importance of examining technology when discussing national policy-making. French military nuclear policy in the 1950s was not made by government officials carefully analyzing their nation's place in the postwar world and firmly deciding to build a bomb. In the political chaos of the Fourth Republic, officials worried primarily about their own political survival; in the 1950s, such concerns precluded any deep consideration of nuclear policy. Heads of state, ministers, and elected officials were more than happy to let engineers and managers in state-owned agencies do most of the work toward formulating a nuclear policy for France. And so, in the absence of a firm decision about a French bomb, the Marcoule reactors *were* France's nuclear military policy, containing both the ambiguities and ambivalences of Fourth Republic governments and the agendas of CEA technocrats. *Grands corps* engineers such as Guillaumat and Taranger insisted that their personnel inscribe in the Marcoule reactors their conviction that France should build an atomic bomb: thus, technological work became political work.

The Marcoule reactors did not make a French bomb inevitable, but they did, by virtue of the tremendous technological, financial, intellectual, and organizational resources poured into them, constitute one of the most powerful arguments in favor of that bomb. Historians, politicians, and others have debated ad nauseum the question of who "decided" to build the French bomb and when that "decision" took place.[83] By delving into reactor technology and examining the interaction of technology and politics, we can see that framing the question in that way makes no sense. In the tumultuous political climate of Fourth Republic France, developing an atomic bomb was a process, not a decision, and it was a process in which technocrats, engineers, and the technology that they produced played a far greater role than did politicians.

CEA technology also shaped France's nuclear energy policy. The Marcoule projects proved important to EDFl project engineers both technologically and politically. Lack of expertise, time, and money prevented EDF engineers from designing a different kind of reactor. But they were able to transform the gas-graphite design, imbuing it with their own

[82] Teste (1957, 75) (my translation).
[83] See, among many others, Coutrot (1985); Simonnot (1978); Peyrefitte (1976); Goldschmidt 1980, 1987); Bonin (1987); Scheinman (1965, 8).

imperatives of electricity production, efficiency, and simplicity. Thus, they too made policy through their technological work. They played on the inherent ambiguity of the gas-graphite design to direct French nuclear policy more firmly toward energy production. They used their technological choices in the EDFl project—and the fact that those choices differed from the CEA's—to convince other engineers in their institution, as well as politicians who could fund their program, that nuclear energy could present a viable economic alternative to conventional power sources.

In the early to mid-1950s, the two agencies had obtained funding for their programs by making vague allusions to issues of international prestige and long-term industrial and economic development. By the late 1950s, the technological and political work that the CEA and EDF had accomplished together had buttressed these earlier arguments and firmly established the nuclear program as an arena in which issues of great significance to the French nation would be played out. But although they had a common interest in promoting a nuclear program and a shared heritage of public service, engineers and managers in the two institutions had different conceptions of the public interest and specific visions of their role in the future of their nation. So when CEA and EDF engineers sought to define the relationship between private industry and state enterprises that would best promote the nation's industrial and economic development, they engendered two different definitions. The CEA's "policy of champions" reflected the Gaullist tendencies of that institution. EDF's policy of picking the lowest bidder and controlling the overall design stemmed, perhaps ironically, from its left-wing desire to keep economic control of energy production firmly in the hands of a state agency. Each institution's policy, and resulting technology, embodied a pole of Fourth Republic politics. The nuclear program thus became a testing ground for broader issues in France's economic and industrial policy.

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Chapter 3
Technics and Civilization in Late Imperial China: An Essay in the Cultural History of Technology

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In about 1169 the philosopher Zhu Xi set down a series of recommendations on the construction of an ancestral shrine in what was to become his best-known work, *Family Rituals*. The first chapter began as follows: "When a man of virtue builds a house his first task is always to set up an offering hall [ancestral shrine] to the east of the main room of his house."[1] This passage has not usually been thought of as a key document for the history of Chinese technology. I should like to propose it as such, arguing that in the context of Chinese society of the late imperial period (between about A.D. 1000 and 1800) domestic architecture was a technology of significance comparable to that of machine tool design in the nineteenth-century United States: the shrine was a core element in the formation of a pervasive, flexible, and enduring sociotechnical system, a material artifact around which crystallized a characteristic ideology and social order.[2] By taking the construction of ancestral shrines rather than the production of steel as my subject, I hope to suggest how history of technology can be enriched and connected to other fields of history from which it is at present all too frequently estranged.

3.1 Opening up the History of Technology

In his critique of the disjuncture between technology studies and anthropology, Bryan Pfaffenberger uses the phrase "Standard View of technology" to describe a widely accepted overview of what technology is, the work it performs, and how it evolves—an account that, as he says, is routinely purveyed to the public, for example, in high school and undergraduate courses. It goes roughly as follows. The essence of technology is that it extends our physical capacities to alter the natural world: a stone ax allows us to grub the soil deeper than we can manage with our bare hands; the ox plow and then the tractor plow are still more efficient grubbers of soil, substituting alternative sources of energy for human labor. Technological knowledge is cumulative, based on an increasingly accurate understanding of the processes of the natural world that permits the development of increasingly effective solutions to our needs: as human societies advance they invent more complex and more efficient tools for controlling the environment. If a given society does not follow this path

[1] For quotations from the *Zhuzi jiali* I have followed Hsi (1991); this quotation is from p. 5. In fact, this sentence was itself quoted from a handbook on etiquette published a century earlier by the statesman Sima Guang. Zhu was most famous for his numerous scholarly works on moral philosophy and cosmology, but none reached as wide an audience as the *Family Rituals*.

[2] Bryan Pfaffenberger defines "sociotechnical system" as "the distinctive technological activity that stems from the linkage of techniques and material culture to the social coordination of labor": Pfaffenbeger (1992, 497).

of historical progress or fails to adopt more advanced technology when it is made available, this is probably due to indigenous cultural factors that hamper development – thus "culture" is set up as the antithesis of the material rationality that technology embodies.[3]

The common understanding is that technology (like science) is "culture free," yet for decades critics have protested that this view naturalizes and justifies the culture of capitalism. Among others, Jacques Ellul stressed the moral risk of evaluating technical artifacts purely according to their functional efficiency as part of a system of material production, thus treating them as independent of the human relations surrounding their use. Lewis Mumford complained that the "tendency to identify tools and machines with technology" was "merely to substitute a part for the whole," going so far as to claim that the material effects of technology were merely secondary: man engages in technical activities "less for the purpose of increasing food supply or controlling nature than for utilizing his own immense organic resources to fulfil more adequately his superorganic demands and aspirations."[4]

In 1959, in naming their journal *Technology and Culture*, the founding members of the Society for the History of Technology seemed to be taking up Ellul's and Mumford's challenge of rethinking what technology is and does. But in fact their goal was more limited: namely, to foster a new contextual approach in the discipline based on the premise that "technical designs cannot be meaningfully interpreted in abstraction from their human context." The industrial world continues to provide the definitions of what constitutes a technical design; nor are the boundaries between the technical and the "human context" really in question. A survey of the contents of the journal shows how little interest there has been in nonindustrial societies or technologies. Four decades after *Technology and Culture* was launched, its current editor, John M. Staudenmaier, was obliged to conclude that mainstream history of technology had still failed to come to satisfactory grips with the concept of "culture" in the sense of technology *as* culture, rather than the "standard view" of technology as noncultural in itself but affected by the affordances of culture.[5]

As Pfaffenberger says, the standard view is "a commonsense view of technology … that accords perfectly with our everyday understanding." It slips easily into a theory of need-driven technological evolution that continues to direct most archaeology, remains an influential paradigm in the comparative history of technology, and underpins worldwide public understanding of technology and its historical role. Staudenmaier notes that the teleology of this model makes it essentially ahistorical and, therefore, of little interest to other historians—except as a historical phenomenon in itself. The metanarrative is an impediment to a genuine engagement of history of technology with the broader field of history; furthermore, it is unlikely to be productive as applied to non-Western societies, since it is "based on the forcible exclusion of others' stories."[6]

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[3]Pfaffenberger (1992, 493–495). John S. Staudenmaier (1985) calls this the "myth of progress" or "Whig view" and analyzes its implications for the history of technology; see pp. 134–148.

[4]Ellul (1962); Mumford (1966, 306). Mumford had already explored this theme in his periodized study of technology in human history, Mumford (1934).

[5]Staudenmaier (1985, 165and 1990). Staudenmaier distinguishes nine key subject areas in *Technology and Culture*. Four "have dominated the field for many years: technological creativity; the science-technology relationship; the American system of manufacturing; and electricity. Two are attracting renewed interest: the military history of technology and technology from a capitalist perspective" (p. 717). He also notes a recent interest in issues of work and gender, and even of the symbolic construction of technology.

[6]Pfaffenberger (1992, 495); Staudenmaier (1985, 164–174). See also Staudenmaier (1990, 725). The standard view also, of course, determines development policies around the world. See Basalla (1988).

Among the "questions seldom asked" in *Technology and Culture*—questions that he believes would help banish tendencies toward whiggishness and develop a richer understanding of technology *as* culture—Staudenmaier includes analyses of technologies that did not catch on, analyses of technology from the worker perspective, critiques of capitalism—and studies of non-Western technologies. We might indeed imagine that since non-Western societies notably failed to generate either the technology or the value systems[7] that generated Western industrial capitalism, they would provide excellent opportunities to question accepted models of historical change,[8] to explore alternative ideologies of materialism, and to elaborate our understanding of the kinds of work technology performs.

An added incentive is that, at least as applied to non-Western societies such as China, the standard view is approaching its useful limits. For two hundred years Western nations have used technological difference to determine human hierarchies, over time and across space.[9] The mechanical-economic reductionism of the standard view presumes that technologies perform the same work, more or less efficiently, in every society; broadly speaking, the same technologies are *significant* in every society; and continuity is to be explained as *stasis* rather than as *stability*. Transformation is normal: unhampered, technology progresses constantly, generating changes that eventually disrupt modes of production and propel societies into new eras. Most studies of premodern technologies, whether in East or West, trace the lineages of the modern world; they focus on engineering, timekeeping, the conversion of energy, and the production of commodities like metal, food, and textiles—in other words, on the domains that from our perspective seem most significant because they came to shape the world of industrial capitalism.

Though some historians have recently preferred to view Europe's experience in the light not of inevitability but of a miracle, the path taken by the West still emerges as the best because the most "natural" one, permitting the most rational and efficient use of resources. By contrast, in all non-Western societies, however technically accomplished (the medieval Islamic world, the Inka empire, or imperial China up till about A.D. 1400), the natural energies of technological progress were somehow prevented from taking this natural course: the metaphors used are those of blockages, brakes, or traps.[10] The non-Western experience is then presented as a failure to build on achievements, and it is this failure that requires explanation; usually culture (in the form of epistemologies or institutions) is to blame.[11]

Joseph Needham's lyrical accounts of Chinese achievements in science and technology transformed the public image of China and its place in history around the world. Needham criticized using science to bolster Western supremacism, but like the other scientists of his generation he fully shared the teleology of the "whig position." However, by demonstrating

[7]Note, however, current arguments that Confucian values are responsible for the Asian Tiger phenomenon. This directly contradicts the long tradition of historical interpretation that saw Confucian values as antithetical to the development of capitalism. See Brook (1995).

[8]For instance, recent scholarship suggests that, contrary to conventional wisdom, in the early Chinese steel industry large-scale production preceded small-scale operations. See Wagner (1993, 1997).

[9]Adas (1989).

[10]Jones (1981). Bertrand Gille lists China, the Muslim world, and the pre-Columbian empires as *blocked systems*: Gille (1978). Fernand Braudel uses the metaphor of *brakes*: Braudel (1992, 430–435). Mark Elvin uses the metaphor of a *trap*, i.e., the "high-level equilibrium trap": Elvin (1973).

[11]Joseph Needham was inclined to blame this perceived failure on external social and intellectual factors like the "bureaucratic feudalism" of the Confucian state; see Needham et al. (1954–). Elvin's terms "high-level equilibrium trap" and "involution" sought explanation in terms of what one might call the dynamics of the Chinese sociotechnical system.

that China had contributed to the world many important elements of modem science and technology—including Francis Bacon's famous modem trio of printing, gunpowder, and the compass—Needham contrived to tum the Western triumphalism of the standard view into a liberatory platform for other traditions. He proposed the concept of "ecumenical" (universal modem) science and technology, to which various local traditions (not just those of Europe) had contributed, just as rivers flow into the sea.[12] The path of technological progress is still plotted according to the criteria of the standard view, however, and so inevitably we end up having to account for failure to progress—or, rather, failure to become like the West—a problem that continues to enthuse comparative historians of science and technology and to preoccupy economic historians of China, whose discipline leads them to treat technology essentially as a factor of production.[13]

James Clifford has noted how ethnographic museums put together exhibits by selecting artifacts according to categories that fulfill Western expectations of a "primitive" or "traditional" society, thus creating what he calls the illusion of adequate representation.[14] In history of technology, the standard view imposes the categories of industrial capitalism on non-Western societies; it then appears to have represented the m adequately by identifying the cause s of their failure to follow the Western path. Once that has been accounted for, what more is there to be said about native technologies?

Yet precisely what is most interesting about non-Western societies is that the material worlds they produced did *not* embody the same values as our own. "We must remind ourselves time and time again that the European experience since the Middle Ages in technology, in the economy, and in the value system s that accompanied them, was unique in human history until the recent export trend began. Technical progress, economic growth, productivity, even efficiency have not been significant goals since the beginning of time … other values held the stage." As Mumford argued with regard to the superorganic functions of technology, the work performed by a sociotechnical system may have little or nothing to do with the energy-efficient solving of material problems or the profitable production of commodities. How did other societies see their world and the human place in it, what were their needs and desires, and how did the technologies they developed help fulfill those needs and desires?[15] Such anthropological (or cultural) questions dispel the illusion of adequate representation and provide a creative framework for exploring the complex roles of technology in non-Western or nonindustrial societies and for integrating technology into broader historical studies.

This approach requires a new materialism that takes into account social and symbolic as well as—or, where appropriate, instead of—economic and mechanical efficiency. Technolo-

[12]Greatly influenced by Needham, Arnold Pacey suggests an ecumenical model in which different civilizations continually succeed each other as world leaders; see Pacey (1990). The structure of *Science and Civilisation* classified technologies as applied sciences (my own contribution to the series was Vol. 6, Pt. 2, *Agriculture* [1984], a field of expertise that Needham slotted into his framework as "applied botany"). Lynn White, Jr. took Needham to task for presuming too readily that technical skills represented scientific understanding; see White (1984).

[13]"No category of scholars has given more attention to the history of technology than the economic historian. Technology (or technological knowledge) is clearly a factor of production, and can no more be ignored in a comprehensive study than can capital, labor, or raw materials": Pursell (1984, 71). On the economic history of China and the metaphors of failure that guide it see Wong (2002). For comparative work see, e.g., Huff (1993); Mokyr (1990).

[14]Clifford (1988, 220).

[15]Finley (1985, 147). According to Basalla, a human technology is a "material manifestation of the various ways men and women throughout time have chosen to define and pursue existence": Basalla (1988, 14).

gies in this definition are specific to a society, embodiments of its visions of the world and of its struggles over social order; they produce ideas about being human and about relations between people.[16] The world of food, shelter, clothing, and other goods that each society constructs for itself is a domain of material experience that shapes and transmits ideological traditions in unique fashion. Technical activities and artifacts can be understood as a form of communication or as symbols; and technologies that seem to us of minor importance may hold great symbolic significance, as in the case of butter making and fire lighting in India or the construction of the Chinese ancestral shrine that I will discuss here.[17] Like all symbols, technologies are polysemic: their meaning depends on how they are read in relation to other elements in the symbolic system; they mean different things to different people; and sometimes the ambiguities they embody serve to defuse conflict, while at other times they provoke it.[18]

If technology performs symbolic and ideological work, we need to examine how it might construct and stabilize as well as undermine or transform a social order. A successful technology is not necessarily one that destroys the society that produced it. I was offered an interesting variant on the standard view by an environmental scientist who characterized technology not as a more powerful extension of the human arm but as a "damper" or shock absorber, a means of blunting the extreme effects of our environment. The examples he chose as illustrations were agriculture and food storage, which smooth over surplus and dearth; and the house, which stabilizes temperatures. Extending the metaphor from the physical to the social, sociotechnical systems have the capacity to absorb as well as to generate disruptive social energy. Like ecologists, we should look closely at the processes by which a sociotechnical system forms, consolidates, and resists dissociation.[19] The utility of this approach for studying modern technology has been demonstrated;[20] it clearly has great potential applied to a case like late imperial China.

Insofar as political, social, cultural, and intellectual historians of China are engaged with *longue durée*, they are fascinated not by the absence of change but by the astonishing resilience of a political and social system in constant evolution, repeatedly subjected to dramatic shocks and pressures. Between 1000 and 1800 China was three times conquered by foreign invaders and experienced numerous internal rebellions and civil wars. The geopolitical center of China shifted from the northern plains to the cities of the south. Population fluctuated but in the long term grew from roughly 100 million to 400 million. The economy became increasingly commercialized, cities expanded, and more and more of rural China was tied into a web of inter-regional commerce that replaced subsistence farming with local specialization and household commodity production. Printing and publishing industries

[16]See, e.g., my own study of how a set of three different material technologies contributed to the historical structuring of gender identities in China: Bray (1997).

[17]Technological activities and artifacts are treated in this way in the French tradition; see e.g., the journal *Techniques et Culture* or the interdisciplinary collection of papers in Lemonnier (1993). For an example see Mahias (1989). My own work has been strongly influenced by my connections with the French group.

[18]C.A. Bayly gives a marvelous illustration of such ambiguities in his discussion of the conflicting symbolisms, liberatory and oppressive, of cloth production in preindependence India; see Bayly (1986).

[19]"A successful sociotechnical system achieves a stable integration of social and nonsocial actors, but it is no static thing": Pfaffenberger (1992, 502). The variant on the standard view was offered by Ramon Guardans in a personal communication, 1990.

[20]Following Thomas P. Hughes's foundational work on electrification, Staudenmaier discusses the potential of the related concepts of "momentum" and "inertia" for exploring the enduring nature of sociotechnical systems; see Staudenmaier (1985, 148–161).

emerged, expanded, and flourished; ever-greater numbers of candidates attempted the imperial examinations that offered access to the governing elite; social boundaries between the old categories of scholar and merchant, peasant and craftsman, became increasingly blurred and permeable. For most historians of late imperial China, the puzzle is what held China together over this period.

Currently the history of technology is isolated from the broader field of Chinese history, despite a growing interest among social, cultural, and intellectual historians in themes that would benefit from a linking of the cultural and the material, such as the body or consumption.[21] Man y historians of late imperial China associate the enterprise of history of technology with an inherent Western supremacism that they find distasteful, and the questions it asks appear largely irrelevant or alien to their own concerns.[22] While the "blocked system" model focuses on the failure to transform, the majority of historians of late imperial China face the challenge of accounting for continuity. The history of Chinese technology might engage very fruitfully with the broader field if it turned its primary question on its head to look at continuities not as stasis or as absence of change but, rather, as systemic stability or resistance to dissociation. Among the factors that contributed to the consolidation and reproduction of the sociopolitical order and to the diffusion of orthodoxy in late imperial China, scholars have pointed to elite control over the written word, to death rituals (ceremonies of paramount importance in Chinese society, since they converted dead relative s into ancestors), marriage practices, and family rules.[23] I propose to add technology to this list, and here I shall discus s the development of one key feature of the domestic dwelling that came to tie families of all classes into history and the broader polity: the domestic shrine.

3.2 Machines for Living

The American system of manufacture shaped the modern world. It had its roots in "armory practice," a system of standardization of machine tools specially designed to produce large numbers of interchangeable parts that started in the U.S. small-arms industry in the early nineteenth century and spread rapidly into other areas of manufacture. In Europe skilled craftsmen adjusted their machines to produce components of the required design and dimensions; to profit from an influx of unskilled immigrant labor, U.S. manufacturers designed machines where the craftsman's skills and tolerances were inbuilt. These machines propelled the development of a new manufacturing system that engendered the labor relations, consumption patterns, and vision of technology typical of Fordist industrial capitalism. The normalization of industry crystallized around "the U.S. Ordnance Department's 1816 commitment to the philosophical ideal of standardization and interchangeability" and gradually

[21] For cultural historians who have difficulties coming to grips with the material, du Gay, Hall, Janes, et al. (1997) offers a superb model for linking production, consumption, regulation, representation, and identity around a material object and its design.

[22] It is to be hoped that cultural historians of China, who characteristically pay very little attention to the material conditions of life, will soon become aware of the relevance of work by scholars like Dieter Kuhn (1987), who considers Song material culture as an expression of political relations and social identities; Klaas Ruitenbeek (1993) whom I will discuss below; Françoise Sabban (1994) who—à la Sidney Mintz—incorporates tastes and cuisine into her analysis of the Chinese sugar industry; and Lothar von Falkenhausen (1994) who discusses music and pitch as forms of political expression and control.

[23] See, e.g., Johnson, Nathan, and Rawski (1994); Woodside and Elman (1994); Watson and Rawski (1988); Watson and Ebrey (1991).

came to encompass not only the structure of manufacture but also “the growth of standardized and centrally controlled rail systems, the centralization and standardization of corporate research and development, the use of consumer advertising to program individual buying habits, [and] the increasing centralization and complexity of electricity and communication networks.”[24] In other words, the machine tool came to underpin a whole system of goals and values; it was not just a machine for producing, but a machine for living.

When identifying significant technologies, those that have contributed most to shaping the nature of society, historians of Chinese technology have usually followed the example of Western historians and focused on technologies that produced the key commodities of the industrial world—metallurgy, agriculture, and the textile industry. However, late imperial Chinese society was not capitalist, and its characteristic social order was organized around other than modernist goals and values.[25] The institution that most fundamentally shaped late imperial society and culture was the patriarchal lineage, constructed around an architectural feature: the domestic altar. In China, domestic architecture was a significant technology that normalized late imperial society just as the machine tool shaped and cemented the values of the Fordist world.

In speaking of his architectural designs as “machines for living,” Le Corbusier expressed the modernist ideal that domestic space should be designed to meet scientifically defined needs as efficiently as possible: a modern dwelling should be hygienic, thermally stable, efficient in its use of energy, and ergonomically designed. The phrase also expressed the converse modernist principle, that living spaces produce lifestyles. Bauhaus workers’ flats reduced drudgery and increased comfort—and they were also designed as environments that would help the occupants live as responsible modern worker-citizens. In contemporary Western society new housing is professionally designed, incorporating the skills of architects and engineers; the building materials are industrially produced; the house itself and its standard equipment tie the occupants into a complex technological network of utilities suppliers, consumer durables industries, roads and automobile manufacturers, supermarkets, TV cable companies, and so on. The house is a machine for living the particular lifestyle of late capitalism, with its characteristic systems of consumerism, property rights, privacy, and gendered or generational identities. Excellent studies by Dolores Hayden and Mike Davis illuminate the moral, cultural, and political messages embodied in such dwelling forms as the “executive housing” now spreading from the United States to middle classes around the world and invite us to ponder the deeper meanings of gated estates, house frontages that give pride of place to two-or even three-car garages, the absence of shared facilities with neighbors, a separate bedroom, however tiny, for each child, and, of course, multiple bathrooms.[26]

We have naturalized the moral and cultural messages of this architecture. We think of a separate bedroom for each child and a closed-off space for private, solitary defecation and ablutions as needs all societies would meet if they had the means—the standard view looms again. However, as anthropologists and cultural critics have shown, architecture is not neu-

[24]Staudenmaier (1985, 200). On the growth of the American system of manufacture see, e.g., Pursell (1994).

[25]This is not to say that seeking for profit was unknown—far from it—but it was a “muted” discourse in late imperial society. See Brook (1995, 84–90); Bray (1997, Ch–6).

[26]Dolores Hayden discusses the ideology of American middle-class housing design from a feminist perspective in *Redesigning the American Dream: The Future of Housing, Work, and Family Life* (1986). Mike Davis focuses on how contemporary architectural design and its emphasis on security reinforce class differences in *City of Quartz* (1992).

tral. A house is a cultural template; living in it inculcates fundamental knowledge, skills, and values specific to that society. It is a learning device, a mechanism that converts ritual, political, and cosmological relationships into daily experiences of space.[27] The encoded messages of the house teach some lessons that are the same for all and some that are different. As a child grows up and learns the practices of living in a house she learns her proper place within society; she internalizes the hierarchies of gender, generation, and rank that are marked by walls and stairs and practiced in the rules and etiquette of receiving guests, performing rites of passage, and going about daily tasks. She learns about respectability and how the differences between high and low, rich and poor, are concretely expressed.

Much of this cultural expertise forms around material objects, skills, and habits. Within the framework of our own cultural expectations, the technical hardware of modern domestic architecture "dedicates" certain spaces, restricting their use. When we move into a new house we do not contemplate setting up a dining table in the bathroom, nor would we consider relieving ourselves in the bedroom now that the flush toilet has replaced the chamber pot in our lives. But these uses of space and of equipment are neither self-evident nor "natural"—they have to be learned. It is in the nature of capitalist society that new features (the fitted kitchen, the "study") are constantly being developed and presented to consumers as needs; in such cases we find ourselves instructed and enticed by a range of users' guides that may include floor plans in the realtor's window, magazines on furnishing and design, and TV advertisements. Occupants draw upon these as well as their own experience and expectations in order to turn four walls into a home.

3.3 Engineering a New Social Order in Late Imperial China

In what follows I describe the interplay between *hardware* (that is to say, the characteristic architectural features that framed spatial practices) and *users' guides* (texts and shared practices that taught or reminded people what the spaces they occupied signified) in the construction and diffusion of a "standard" Chinese house. The normalization of domestic space, I argue, was a key element in the standardization of social practices and values.

In his superb study of the Chinese carpenter and his technical skills, Klaas Ruitenbeek makes an arresting observation: "One gets the impression that in China *an imaginary architecture existed which was superimposed on ordinary architecture, and which was the primary concern of the owner, geomancer and carpenter alike*." Ruitenbeek is referring to the superimposition on the physical building of an energetic (geomantic) structure. In my own study of domestic architecture and the construction of gender I eventually came to see three "imaginary architectures" superimposed upon the material shell of the house, each conveying a different set of messages about the relations between its inhabitants, the cosmos, and society at large. First, the Chinese house was a space of decorum, an embodiment of orthodox neo-Confucian values; second, as Ruitenbeek and many anthropologists

[27] In *Mechanization Takes Command: A Contribution to Anonymous History* (1948), Siegfried Giedion provides wonderful insights into the machine-age ideology inherent in such apparently culture-free concepts as "hygiene" and "comfort." The classic text on house and habitus is Bourdieu (1973). Comparing sedentary and hunter-gatherer cultures, Peter J. Wilson argues that the material experiences of inhabiting dwellings have profoundly reshaped our cognitive patterns and social values, indeed that we owe such institutions as "the family" to the spatial experiences of the house; see Wilson (1988). For an exploration of the connections between house and family structure see Carsten and Hugh-Jones (1995).

have demonstrated, the house was a cosmic, energetic space; and third, it was a space of culture, representing a Chinese view of what home and shelter should be.[28] In the following analysis of the diffusion and reception of the shrine, I concentrate on the interplay between the material structures of the house and the two "imaginary architectures" that produced the space of decorum and the energetic space.

While details of the structure and use of domestic dwellings in China obviously varied according to period, region, class, wealth, and taste, what I call the "standard" house, a concrete material embodiment of neo-Confucian social values, was first defined in texts written in the early Song (from about A.D. 1100). At that time only a few gentry families were claiming for themselves the aristocratic privilege of an ancestral shrine in their homes, but in order to consolidate their own status as social leaders the neo-Confucian elite encouraged ever-larger segments of society to adopt the same configurations of spatial layout and practice in their own homes, along with the social ties and values that they symbolized.

Until about A.D. 1000 the ruling elite of China had consisted principally of aristocratic families who preserved their own culture and privileges by excluding other social groups; in particular, they reserved for themselves the right to domestic rituals of ancestral worship. By A.D. 1000 a new elite, formed of public servants qualified by education rather than by blood, was predominant. Rather than drawing impermeable boundaries around themselves, this new elite saw themselves as members of a meritocracy open to talent. They developed and propagated a social philosophy and accompanying social practices usually referred to in English as neo-Confucianism. The roots of neo-Confucianism go back to the late Tang dynasty, to the eighth and ninth centuries, when the formalization of an examination system for the civil service allowed non-noble scholar-bureaucrats to undermine the political and cultural supremacy of the aristocratic elite. Classic Confucian philosophy had emphasized the organic nature of the state, the continuities between the regulation of the family and that of the polity, and the key importance of ritual in ordering society. Debates on these themes resurfaced in the Tang, sharpened by the threat to orthodoxy represented at that time by the "alien" doctrines and practices of Buddhism. By the early Song commoner scholar-bureaucrats had definitively displaced the aristocracy in government, and the new forms of Confucian orthodoxy they developed justified and extended their own claims to social and cultural leadership, placing great stress on ritual.

The neo-Confucian social order rested on principles of ranking and complementarity. A subject owed respect and obedience to his ruler, who should respond with consideration and compassion. Within the patriline the lineage head occupied an analogous position with respect to junior lineage members, and the relations between father and sons, husband and wife, were similarly conceived as complementary but not equal. The performance of joint rituals served to unite people of different status in a common goal or set of beliefs while reaffirming the proper social hierarchies.

It has frequently been observed that the late imperial ruling class relied on instruction rather than force as a method of social control. Chinese thought emphasized the connection between body and morality; the physical performance of daily rituals inculcated the proper

[28] Ruitenbeek (1993, 62) (emphasis added). Again, while not wishing to essentialize "Chineseness," and despite the range of variation across China, we can legitimately point to obvious shared aesthetics, preferences for particular construction materials, the absence of open hearths, the understanding that the house should be constructed and decorated in such a way as to bring good fortune to the family. See, e.g., Lu Yuanting (1991–1996); Knapp (1998); Bray (1997, Chs–3).

feelings and appreciation of relationships that the rituals celebrated. Instructing the common people in ritual, wrote the statesman Ouyang Xiu (1001–1072), "not only would prevent disorder but also would teach them to distinguish superior and inferior, old and young, and the ethics of social relations." Note that in this formulation ritual has an active rather than simply a symbolic role: it secures political order by reinforcing social relations. It has been said that Chinese authorities were more concerned with orthopraxy (correct practice) than with orthodoxy (correct belief), since where practice led, belief must follow.[29] Hence the moral significance of bodily practices, including work, and of material artifacts: everyone knew, for example, that the philosopher Mencius (fourth century B.C.) had announced his brilliant moral career as a mere infant by playing with sacrificial vessels long before he could lisp their names.

As Michel Foucault has reminded us, everyday artifacts and practices are the more powerful as disciplinary devices because their messages are silent. Domestic architecture was a particularly powerful instrument of moral inculcation in China because of the integration of private and public ethics characteristic of late imperial society. In the West we have experienced a series of separations between spheres of activity that has progressively reduced the dominance of the house in shaping people's lives.

The Greek and Roman worlds already marked a clear separation between the domestic and the public, political domain; in Christian Europe most important acts of worship and rites of passage came to be performed in the church; and with industrialization came a separation of workplace from dwelling for large numbers of the population. The house became a private domain, a refuge from the pressures of work and of political or religious orthodoxy. In China a contrary historical process occurred, whereby an increasing proportion of the population came to be tied into a polity that did not recognize our distinctions between public and private: rituals of birth, maturation, marriage, and death were performed in the house; the liturgical rituals of the domestic ancestral cult paralleled the ceremonies of state orthodoxy; and the family was the ethical and behavioral training ground for political life.[30]

Nineteenth- and twentieth-century Western observers remarked that every Chinese house, whether of peasant or gentleman, was first and foremost an ancestral temple: the entire structure was centered on the shrine, and when a household divided each brother set up an altar of his own in his new dwelling.[31] Though it seemed that such customs were immemorial, in fact peasants—and even scholars—had not always been entitled to their own ancestral shrines.

Before the Song, the domestic altar with its ancestral tablets was an exclusive emblem of aristocratic status. Commoners were not allowed to worship their ancestors in their own homes. However, in the early Song dynasty neo-Confucian gentry families began constructing patrilineal descent groups and setting up shrines for themselves as marks of their elite status.[32] Domestic shrines (the architectural hardware) and the practices associated with them (described in various users' guides) served this new elite as an effective tool in their construction of a stable and integrated social order with themselves at the top.

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[29]de Bary (1960, 443) (quotation); Watson (1991).

[30]The continuity was reflected in architectural design: in late imperial China ancestral temples, magistrate's courts, and imperial palaces all shared the basic layout of the farmhouse, if on a larger scale. See Boyd (1962, 48); see also the papers in the colloquium on "*Kongjian, jiating yu shehui*" (Space, house, and society) (1994).

[31]"See, e.g., Myron Cohen's classic, *House United, House Divided: The Chinese Family in Taiwan* (1976) and Clément, Clément, and Shin Yong-hak (1987).

[32]Ebrey (1991).

In 1169 Zhu Xi, one of the most eminent and influential founders of neo Confucian thought, published a compendium of domestic rituals for use among gentry families, the *Family Rituals*, which—as I have already noted—incorporated the *Miscellaneous Etiquette for Family Life* composed a century earlier by Sima Guang (1019–1086). The combined text constitutes the foundational formulation of the "standard" spatial configuration of domestic space and practices.[33]

Zhu Xi represented the house as a ritual space with the ancestral shrine at its heart: "When a man of virtue builds a house his first task is always to set up an offering hall to the east of the main room of his house." *Zhu* stated that this section on setting up the shrine came first in his book because it was fundamental to all that followed, not only in moral and metaphysical terms but also for the inculcation of proper deportment: the first chapter, in his words, "provides the basis for understanding the fine points in the later chapters concerning movements and postures, for walking here and there, getting up and down, going in and out, and facing various directions."[34]

Zhu specified the general principles of construction of the shrine but made allowances for disparities of wealth. Ideally it should be three bays wide, with chests to hold family genealogies and ritual utensils, but poor families could make a shrine a single bay wide or even just use the east end of the main building. Zhu also made allowances for other material inadequacies, such as the lack of a south-facing main building: "Here and throughout this book, in organizing the room, *no matter which direction it actually faces*, treat the front as south, the rear as north, the left as east, and the right as west." The absolute requirements of orientation were thus reformulated as a set of transformations such that anyone could conform to them. The center of the room was the altar table and the niche containing the ancestral tablets. Again, considerable variation was possible in the number of tablets and the order in which they were arranged; the main thing was that there should be an order.[35]

The daily offerings made to the ancestral tablets and the choreography of the liturgy—"walking here and there, getting up and down"—embodied the principles of filiality, ranking, and harmonious and fecund complementarity that underpinned the neo-Confucian patrilineal and patriarchal order: the respect and obedience due from descendants to ancestors was the pattern of filiality, *xiao*, and was thus the model for relations between children and parents, wife and husband, subject and ruler. One very important feature of shrine construction was the east and west steps of the room, illustrated clearly in Figure 1. Family members ascended and descended the steps in ranked order, according to generation, birth order within the generation, and sex, passing from the daily world up to the holy level of the ancestors and back; men and women were distinguished by using the eastern and the western steps. Here let us note that the basic unit in the liturgy (essentially a reproductive activity) was

[33]"One fundamental element of Chinese domestic spatial norms was the segregation of the sexes. Sima Guang's *Miscellaneous Etiquette for Family Life* laid down very clear rules for this, and his were the words quoted ever after on the subject. The famous passage begins: "In housing there should be a strict demarcation between the inner and outer parts, with a door separating them. The two parts should share neither a well, a wash room, nor a privy. The men are in charge of all affairs on the outside, the women manage the inside affairs": *Hsi* (1991, 29). The two domains were thus presented as complementary. The complementarity of male and female participants in domestic ritual is described below.

[34]Hsi (1991, 8).

[35]Hsi (1991, 8) (emphasis added). A bay is the space between two pillars supporting a roof beam, about ten or twelve feet across. On the preeminence of order over particulars see Watson and Rawski (1988). The illustration from *Shinzoku kibun* (Records of Qing customs) shown in Figure 2, for instance, depicts a merchant family shrine containing five, not four, sets of tablets.

the married couple: every act a man performed (such as offering wine) must be matched by a complementary act by his wife (offering tea). Family ceremonies reinforced patrilineal descent principles both by their choreography and by their inclusions and exclusions (see Figure 2): married sons (who lived in their parents' house) participated with their wives and children; married daughters (who joined their husband's lineage on marriage) were absent; servants were not involved, nor were concubines—who, unlike legal wives, were not presented to the ancestors when they come into the family.[36]

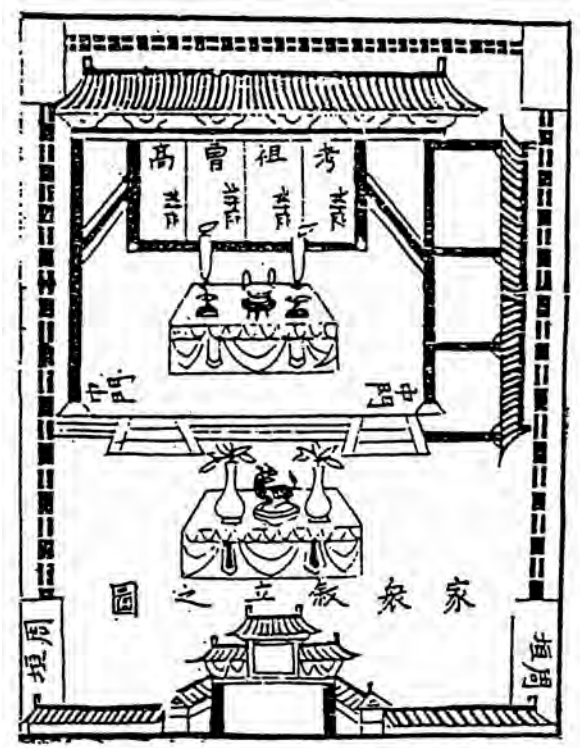

Fig. 1: Family offering hall, from the 1602 edition of Zhu Xi's manual on ritual; see Hsi (1991, 7). The characters on the screen at the back of the chamber indicate the genealogical order in which the tablets should be arranged. Note the two sets of three steps leading up to the chamber from the courtyard.

According to neo-Confucian precepts, all family life and events should be organized around the altar: the ancestors must be informed of comings and goings and of success and failure; new brides were presented to them; dying family members were set down beside the shrine to breathe their last. The senior couple in the family occupied the room closest to the shrine,

[36]On gender complementarity, patriarchal control, and the different meanings that seclusion of women could assume once productive work like weaving or reproductive work like the raising of children was taken into account see Bray (1997, esp. ch. 2 and 3). Concubines never became part of their husband's lineage, since the children to whom they gave birth were considered the offspring of the legal wife, and they were never allowed to conduct ancestral rites even if the wife died; in that case, the husband's duty was to remarry (Bray 1997 ch. 8 and 9).

as befitted both their rank within the family and their closeness to death and ancestral status. If robbers broke in or there was a fire, the first things you should save, said Zhu, were the ancestral tablets and family documents, leaving jewels and money till later (one suspects that this injunction was often ignored in real life).[37]

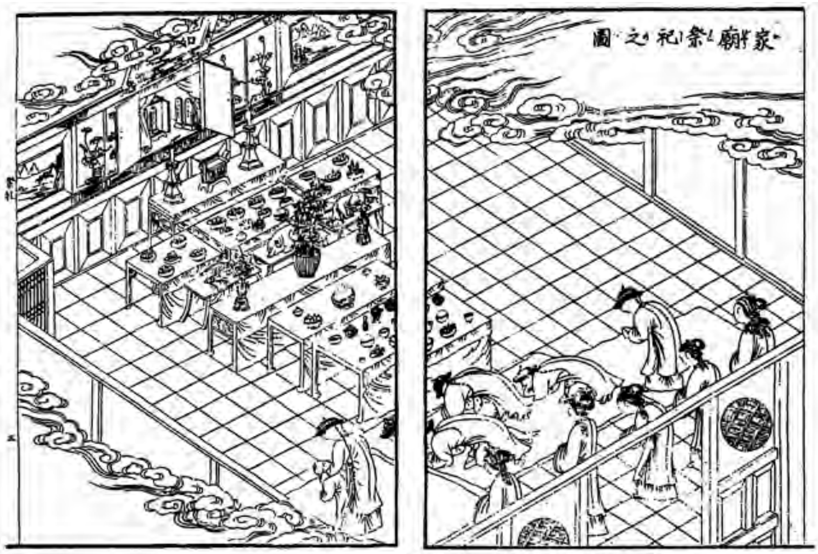

Fig. 2: Celebration of an ancestral sacrifice, from an illustrated work based on the accounts of Chinese merchants and published in Nagasaki, Japan, in 1800; see Nakagawa (1983, 496–7). The sacrifice is performed by all the couples in the family, each wife standing behind her husband, and also by the boys.

Sima Guang, writing in the eleventh century, appears to have been the first person to suggest that the practice of constructing one's house around the family shrine should become general among the literati elite. A century later Zhu Xi took a much more radical step: his goal was to extend the privileges of lineage rituals to whole agnatic groups. He wrote his *Family Rituals*, intended as a manual to help local magistrates educate the people in their charge, in a conscious effort to popularize, to provide a clear and practicable set of liturgical practices for general use—though it is probable that his goal was to educate families of standing rather than the masses.

In placing such emphasis on the shrine, Zhu Xi was not innovating (after all, he begins by quoting from a text a century old) but trying to systematize a practice that had been spreading among the nonaristocratic gentry in the eleventh and twelfth centuries. The established elite disseminated the shrine and its associated neo-Confucian practices as they extended their control over local society, tying their poorer kin into the formal institutions of patrilineal descent groups; meanwhile, other families eager for elite status laid claim to

[37]Hsi (1991, 5).

lineages and kinship networks of their own.[38] Joint ceremonies honoring the founding ancestor took place at special lineage temples that were shrines writ large; they confirmed the landowning and educated elite as lineage heads and as ritual experts and advisors. Though ranked, the system was inclusive: if rich and poor were to be tied together in loyalty as descendants of a common ancestor, then poor families had to have genealogies (and ancestors, and daily rituals at family shrines) too.

Since Zhu's liturgy allowed ceremonies to be carried out with propriety with the most basic of material hardware, poor families could achieve this significant form of respectability at low cost. Thanks to the transformations suggested by Zhu, a simple niche in the wall sufficed as the organizing device for the liturgy.

The influence of Zhu's formulation of the "standard house" and its spatial practices and meanings spread like ripples in a pool. One source of ritual and spatial instruction for ordinary families was the joint lineage ceremonies: for important domestic rituals like weddings, junior families would often invite a related gentry member to participate as sponsor. Another was the actual construction of houses: the carpenters who were principally responsible for building learned from the *Carpenter's Canon* the orientation, proportions, and dimensions of the hall or room housing the shrine and of the shrine itself. In the process of building a new house or adding to an old one, a family went through a period of conscious concern with such issues.[39]

Zhu's book itself became very popular and was produced in various editions. The first illustrated versions appeared at the end of the Song (Figure 1 is from a Ming edition). The original text was popularized and excerpted in household encyclopedias—for example, in the *Householder's Vademecum*, first published in 1301 and widely circulated in the revised Ming edition of 1560. Although only well-to-do families could afford printed books until about 1500, thereafter the reading public grew rapidly, especially in the cities. The middle class, which developed between 1500 and 1800, liked to buy books, both to learn from them and as a sign of status, and books on ritual were very popular.[40]

As concern with proper domestic ritual spread both socially and geographically, along with the spread of lineages, neo-Confucian thinkers became increasingly preoccupied with the problems posed by "vulgar practices" and "local customs"; they wrote commentaries on Zhu Xi's work, or general reconsiderations of ritual, that tried to negotiate these problems so as to include even greater numbers within the circle of orthodoxy. State techniques for extending orthodoxy frequently overlapped or coincided with gentry initiatives. They included legislation on ritual conformity (for instance, the Yuan dynasty [1279–1368] code stipulated that only marriages conducted according to the *Family Rituals* would be considered legal), the setting up of schools, the organization of lectures (especially popular in the early Qing dynasty, during the eighteenth century), and the granting of honors to subjects of outstanding virtue and merit.[41]

It would be wrong to construe this relationship between state or elite and ordinary people as straightforward domination. Ritual and spatial conformity constituted a desirable and

[38] Lineage building was common right through the late imperial period: upwardly mobile local families would identify or invent a famous ancestor and compose advantageous genealogies for themselves. See the classic study by Rubie S. Watson (1985).

[39] Ruitenbeek (1993); Bray (1997, 159–166).

[40] The 1560 (Ming) edition of the *Jujia biyong shilei quanji* was edited by Tian Rucheng. See, e.g., Ko (1994, 34–7) on the "publishing revolution" in the reign of Jiajing (1522–1566).

[41] On the Yuan code pertaining to marriage see Ebrey (1991, 151). On lectures in the Qing, see Mair (1994).

attainable sign of respectability. Norbert Elias decoded the spatial structures of the Parisian *hôtel* as a carefully designed stage on which the French aristocracy under Louis XIV gave uninhibited performances of what they saw as the role of their class; its finely tuned aesthetics, relationships, and forms of rationality were thrown into relief by contrast with the emergent bourgeoisie. Only fellow aristocrats could judge or join in these performances; others were either too vulgar (the bourgeois, confined to the role of envious onlookers) or not really human (the servants—thus a French noblewoman could undress without embarrassment in the presence of a male servant).[42] The lineage temple was likewise a stage designed by the neo-Confucian elite to display their own moral values and social preeminence. But this particular form of social preeminence required the participation of lower-ranked groups who shared and enacted the same values. The neo-Confucian elite invited their social inferiors not only to join them as actors on the stage of the temple but to reproduce the performance in their own homes.

The Chinese elite saw their task as drawing the lower orders firmly into the domain of civility while maintaining the distinctions of rank and learning that justified their own authority. In setting up their own domestic shrines, ordinary families recognized this social order but stood to gain respectability, ancestors, membership in the lineage, control over women (an attraction for men) and over juniors (also an attraction for senior women), and a sense of political incorporation by participating in it. In neo-Confucian doctrine the house was a microcosm of the state, and relations between husband and wife paralleled those between emperor and minister. As the spatial norms of the house diffused through Chinese society in the course of the late imperial period, the daily rituals around the altar came to tie increasing numbers of ordinary and even illiterate families into an ordered social space stretching geographically to the bounds of the Chinese polity and historically to the great scholar and moralist Zhu Xi, and so back to Confucius himself.

All I have said so far suggests that the family altar was instrumental in propagating hegemonic, orthodox values. There is another side to the coin, however. One important factor in the popularization of the altar was a typically Chinese process of accommodation between "orthodoxy" and "popular custom"—the geomancy of the house, which modulated the decorous symbolism of neo-Confucian liturgy, superimposing its own rather different morality upon the family shrine.

Geomancy was applied cosmology; its skills consisted of reading a local land scape in terms of flows of cosmic energy *(qi)* through time and space and then manipulating the landscape so as to direct energy in the desired direction. It was an ancient science that entered a new phase of elaboration with the surge of cosmological studies in the Song dynasty; only scholars and specialists had expert knowledge of the cosmological principles involved, but almost everyone had a smattering of geomantic lore and was familiar at some level with the general precepts concerning the siting of graves and the layout of dwellings. Carpenters, who were the craftsmen responsible for building the wooden houses typical of China, were practically speaking geomantic experts: the carpenter's rule was marked with lucky and unlucky inches, and when he calculated the width of a lintel or the length of a beam, he was carrying out a series of cosmological computations. Anyone building or altering a house,

[42] Elias (1985).

however poor, first consulted a geomancer—and if the family couldn't afford a specialist, the carpenter who did the building work was sufficiently expert with a compass to stand in.[43]

As in neo-Confucian liturgy, the altar was the heart of the geomantic house.[44] After the most auspicious site for the altar had been located, the rest of house was designed around it, in a configuration intended to channel *qi* into the main hall where the altar stood (see Figure 3). In the siting and design of a house, geomancy worked to direct *qi* to the advantage of its occupants. A well-sited and well-laid-out house brought health, wealth, happiness, and numerous male heirs; a badly sited dwelling brought strife between father and son, shameless daughters, loss of wealth, and illness (see Figure 4).

Fig. 3: The An Tai Lin residence in eastern Taipei; see Li (1980). The geomantic and social status of the buildings and the rooms within them is differentiated by the height and style of the roof; the apex of the building is the roof of the main hall (at the back of the central courtyard) that houses the ancestral shrine.

Neo-Confucian philosophy said that virtue brings happiness and order brings success; everyday rituals around the shrine were a training in morality and an affirmation of social structure. Differences in roof height, for instance, reflected difference in rank between occupants but did not produce it. Geomancy, however, said that the manipulation of *qi* through architecture *produced* virtue and proper relationships. "A family temple is not like an ordinary house: whether or not sons or brothers will attain wisdom depends wholly on this place. Moreover the rear hall, main hall, corridors and triple gate may increase only gradually in height, since only then do sons and grandsons know their rank; and does not the younger aspire to the older's place. The builder must take careful notice of this."[45]

Geomancy was not necessarily incompatible with Confucian values, yet, as Stephan Feuchtwang emphasizes, its underlying principles were asocial or even amoral. Geomantic

[43]Bennett (1978). There is a huge Chinese-language literature on housing geomancy; Knapp (1998) cites the most important works in the bibliography. On carpenters as geomantic stand-ins, see Ruitenbeek (1993, 6).
[44]The siting of the kitchen stove was also extremely important, and there are good grounds for thinking of the Chinese house as a bipolar construction in which the stove was as important as the altar in constituting group identity and reproduction. See Bray (1997, 106–14).
[45]A Yuan (1279–1368) passage from the *Carpenter's Canon* (the guild handbook known by heart to every carpenter); translation in Ruitenbeek (1993, 197).

techniques were competitive: they did not increase the cosmic energy within a local landscape; they simply channeled it in new directions. Siting a house or a grave, building a wall, or even planting a tree disrupted existing flows of *qi*; such acts were regarded by the community as a means of attracting fortune for one's own house at the expense of others. "Let one man in a village build a fraction too high; let him build a window or a door which can be interpreted as a threat; and he has a struggle on his hands."[46]

In the cultivated neo-Confucian reading, the ancestral cult was the paramount symbol and instrument of social harmony and political order; the shrine was an object of respect, the focal point around which the performance of family virtues was organized; the ancestral tablets were the symbols of patrilineal descent. However, most people considered that their ancestors' spirits actually inhabited the tablets and intervened actively in their lives. The ancestors' power to help their descendants was affected by the supply of *qi* channeled onto the altar by geomantic means—the shrine was in that sense a machine for converting cosmic energy into human benefits. As with the knowledge of Zhu Xi's formulation of "standard" domestic spatial practice, popular understanding of how the efficiency of this machine could be enhanced was disseminated in the late imperial period by consultations with technical experts (geomancers and carpenters) and by access to users' guides. These included illustrated excerpts from the *Carpenter's Canon* included in the same household encyclopedias and almanacs in which excerpts of Zhu's work were prominently displayed. As print culture evolved, the market grew for specialist monographs on geomancy and for the services of geomantic experts.[47]

The material artifact of the altar thus incorporated two ideologies simultaneously. In the invisible architecture of decorum it embodied the hegemonic neo-Confucian ideal of social harmony and stability founded on filiality, virtue, and collaboration. The altar was also the focal point of an energetic architecture of the house that tied families not into the ordered and socially harmonious space of the polity, but into an anarchic local landscape of competition between households; here it embodied an ideology of uncertainty and competition in which each family must ruthlessly manipulate the local environment to its own advantage if it was to survive.

The shrine, then, was a key device in the normalization of the late imperial world order. The sociotechnical system of neo-Confucian values underpinned by domestic architecture was not static: it gradually expanded from a small new elite to incorporate almost the entire population of China, and it resisted dissociation for centuries despite the considerable social tensions generated by population growth, urbanization, commercialization, and other factors enumerated earlier. The altar celebrated the respectability of families who knew their place in society and strove for higher blessings than material success. At the same time, people did want to succeed and opportunities were limited; they feared failure, and competition and uncertainties grew. The energetic functions of the domestic altar allowed families to take their fate into their own hands—and the blame was then theirs if things went wrong, for their technical skills had been insufficient. So the altar also acted as a safety valve, channeling potentially disruptive social energy in safe directions.

[46] Feuchtwang (1974); Freedman (1979, 330).
[47] Feuchtwang (1974); Smith and Kwok (1993); He Xiaoxin (1990).

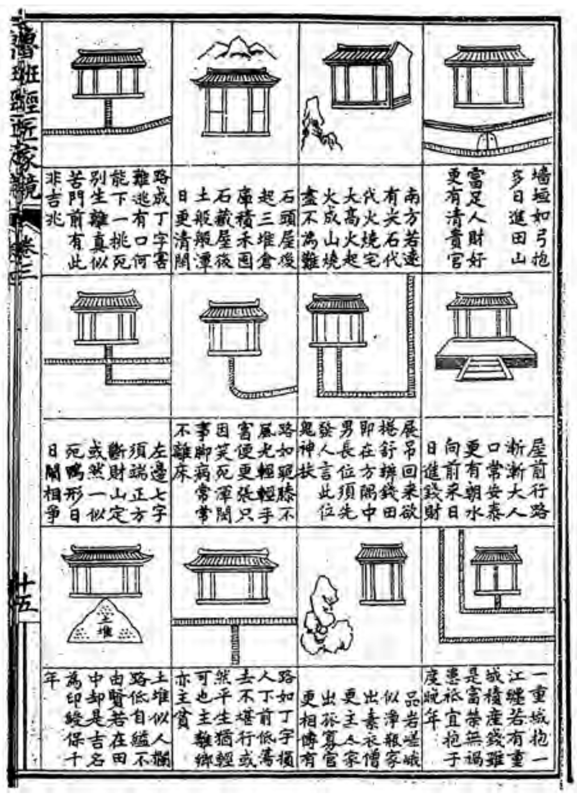

Fig. 4: Page of diagrams from an 1808 edition of the *Lu ban jing* (Carpenter's canon) (1808) depicting lucky and unlucky building forms.

3.4 Concluding Remarks

I have focused in this essay on the transformative effects of a technology that is conventionally considered "nonproductive" in order to suggest the benefits of a more organic, anthropological approach to technologies and the work they perform. For the history of technology in non-Western societies this revisionism has clear attractions: it extracts the society in question from a system of negative comparisons with Europe to concentrate instead on material domains perceived as significant within that society. Moreover, by treating technologies *as* culture it constitutes an intellectual basis for the integration of history of technology into the broader historical discipline. The meritocratic elite of the Song justified their status through the social philosophy of neo-Confucianism and extended their influence and control through the elaboration of extended patrilineal descent groups. The domestic shrine was a material symbol of lineage ties and values. I have analyzed here how, starting in the Song dynasty, the Chinese intellectual and political elites used the rituals and etiquette centered on the shrine to incorporate ever-widening circles of the population into the orthodox fold. I have also suggested that as a material artifact the shrine embodied ambiguities of meaning, and a corresponding moral flexibility, that aided its successful dissemination and made it a powerful instrument for the reproduction of the social order in the face of potentially disruptive forces.

"Technology" is a modern term for which there is no equivalent in most premodern languages. Certainly there is none in premodern Chinese. Yet I do not think we should therefore reject the uses of technology as a heuristic category. In my own search for what I have labeled "significant technologies" in late imperial China I have looked for categories of material production and practice that were prominent in the debates of the time. (This of course biases me toward the concerns of the literate male elite—but it gives me somewhere to start.) By this reckoning several of the technical domains that we now consider fundamental (like metallurgy and engineering) were at best marginal. Others, like agriculture and textile production, were central concerns, yet their local meaning and value cannot be adequately understood in the terms of modern engineering and economics. Agriculture, the "fundamental occupation," of course produced food commodities and could be organized more or less productively in economic and environmental terms; but at a higher level the proper organization of farming was viewed as the basis of the ideal relationship between ruler and subject, the root of the state's strength, and the confirmation of a moral order in which selfish striving after profit was rejected in favor of modest and stable prosperity. Some late imperial authors of treatises on farming wrote in their capacity as landowners intent on running a viable and, if possible, profitable enterprise; others wrote as servants of the state whose goal was the maintenance or restoration of proper relations between the emperor and the male members of the "common people," who canonically worked as peasant farmers (while their wives and daughters worked as weavers). The two genres commonly quoted the same sources and discussed the same problems, but with different goals. Agriculture was a highly significant technology in late imperial China, but it is not possible to make sense of the distribution, dissemination, and evolution of farming skills and knowledge or their representation in our historical sources unless we take into account the meanings attributed to the occupation of farming and to its political role by different members of late imperial society in a period marked by increasing commercialization and occupational diversification—and by intermittent crises that threatened the collapse of the state.[48]

So some technologies we think of as significant today may be absent from the discourse of other societies; others may figure prominently, but with important differences in their social and material signification from those we now attribute to them. Some that we may not now consider significant (or even technological) may have been central in other societies' calculations and strategies for producing desired material worlds—as I have argued here in the case of the domestic shrine in late imperial China. Or again, we might look for significant technical systems that linked what to us seem unlikely combinations of technical domains. For instance—again in the case of late imperial China—I have argued that one approach to understanding how gender difference was construed over time, and its place in the evolving social order, is to look at gender relations and other social systems of inequality and power through the lens of what I call a "gynotechnics;' a set of technologies that was fundamental in construing female identities within the prevailing social order and in constructing the corresponding material worlds.[49] For this I brought together three technical domains that our

[48]Will (1994); Bray, "Who Was the Author of the Nongzheng quanshu?" a paper presented at the conference on "Xu Guangqi, Seventeenth-Century Scholar, Statesman. and Scientist" (1995).
[49]Bray (1997).

modem experience does not necessarily see as connected: the production of differentiated domestic space; the making of textiles (denominated "woman's work" even at a time when men were rapidly taking over from women at the loom); and "reproductive technologies;' that is, the complex of social and physiological techniques used to attain desirable families. Much conventional history of technology has treated women as marginal players, at best consumers rather than creators both of material worlds and of ideology. In my study of Chinese gynotechnics I rooted social ideas in material experience and connected the production of culture to the production of objects to demonstrate that the female dimensions of material existence were neither marginal nor passive, but fundamental in creating what Norbert Elias would have called the Chinese "civilizing process." Since history of gender and women's history are currently among the liveliest and most productive arenas in Chinese history, my hope is that the approach embodied in my study of technology and gender will contribute to the reintegration of history of technology into the broader cultural history of China, from which it is currently estranged.

It is easy to see what the history of technology in non-Western societies might have to gain from rethinking its objects and methods, and I think the attractions for historians of Western technology should be no less evident. While clearly the discipline of history of technology will continue to draw strength and analytical power from its associations with economics, engineering, and science, if we allow ourselves also to think more imaginatively about what a technology might be and the social work it performs, if we can conceive of technologies as forms of cultural expression and, thus, key instruments in the creation and transmission of ideology, we open up a whole range of new possibilities for understanding the past, as well as new possibilities of dialogue with other branches of history and cultural studies. This essay, then, is a plea for a new and more imaginative materialism.

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Chapter 4
Deuteronomic Texts: Late Antiquity and the History of Mathematics
Reviel Netz

4.1 Introduction

In this article I offer a reassessment of late antiquity and the Middle Ages in the history of mathematics. For this purpose, I develop a more general notion, of "deuteronomic" texts, i.e. texts depending fundamentally on earlier texts. I describe in detail some of the features typical to this period in the history of Western mathematics—late antiquity and the Middle Ages—where deuteronomic texts were crucial. I then argue briefly that those features had significant consequences in a changing practice, and image, of mathematics, and also that those features derive directly from the role of deuteronomic texts. Thus the argument is that ate antiquity and the Middle Ages had a real historical contribution to make; and that this derived from the basic nature of texts produced in this period.

Now late antiquity—and, largely speaking, the Middle Ages—did not fare well with the historians of mathematics. Pappus[1]—to take the most favorable case—is often considered the most competent mathematician in late antiquity, and Jones is his most careful contemporary reader. It is thus worth noting how Jones introduces his subject:

> In the later Hellenistic period, after several hundred years of progress, the main stream of Greek mathematics, synthetic geometry, experienced a deep and permanent decline. The subject did not stop being studied and taught, but original discoveries became less and less frequent and important …
>
> Pappus of Alexandria is the first author in this degenerate tradition of whom we have substantial writings on higher geometry [Jones (1986, 1)].

Wary of teleological readings of the past, many historians would probably react instinctively against such terms as "decline" or "degeneration." Yet Jones' judgment is inescapable. Something did happen at the end of the Hellenistic period, and "decline" is the term which comes to mind. My purpose in this article, therefore, is not to try to show how original late antiquity was—for it was not. It was deeply conservative. Yet, I shall argue, it still had a real contribution to make, if inadvertently: it developed a new project which differed qualitatively from that of early mathematics and which shaped the future history of mathematics. The paradox is that such a change came about without any intention, on the part of Late Ancient mathematicians, to change their mathematics, and my argument is that in certain circumstances, and especially inside mathematics, conservatism can act as a force

[1] Active in 4th century AD Alexandria, his biography is practically unknown. Dealing with a wide range of topics from arithmetic to mechanics, his most significant work is *The Collection*, a sort of mathematical encyclopaedia in eight books, nearly seven of which are extant. See Jones (1986); Cuomo (2000).

for change (we shall need, however, to specify precisely the intended sense of this "conservatism" later on in the article).

So, to start, one should notice that Late Ancient texts often take the form of commentaries, and even when they are not commentaries they often are what I call "deuteronomic texts." Late antiquity is the age of new editions, epitomes, and encyclopedic collections; later, during the Middle Ages—which, in this respect essentially pursue trends already discernible in late antiquity—another kind of deuteronomic text was added, namely translations—into Syriac, Arabic and Latin.[2] The entire period from late antiquity to the Middle Ages is the age of scholia and marginalia. All such texts are "deuteronomic": they explicitly start from an established text (or texts, in the plural), and aim at producing a new text, which reenacts the earlier text (as in a translation or a new edition), or uses it in more radical ways (an epitome or an encyclopedia). At least from the modern point of view, commentary is the most important kind of deuteronomic text, because it is also the most ambitious: it is the deuteronomic text standing on its own, apart from the original text; but in this article I shall stress commonalties among different kinds of deuteronomic texts rather than distinctions. In general, I shall see the commentary form as key to the understanding of deuteronomic texts.

Generally speaking, commentators are not highly regarded by modern authors, and they are often referred to by such pejorative terms as "pedantic" or "scholastic."[3] And once again, I do not wish to contest this characterization: my purpose is to identify in detail what makes an author appear "scholastic." I shall then argue that such "scholasticism" may have real historical and philosophical significance and is, in fact, the vehicle through which conservatism can act as a force for change.

4.2 What is "Scholasticism"?

What do we mean by "scholastic" or "pedantic"? I shall now try to unpack such concepts with the aid of examples. I shall argue that there are several things we may mean by such terms, all closely linked. After we have seen some of these possible meanings, we shall try to investigate the possible link: what exactly do commentators tend to do, which earns them their pejorative epithets?

4.2.1 Scholia and "Vertical Pedantry"

First, one thing commentators do is *explain the obvious*. This is vertical pedantry: they dig too deep. Of course, the "obvious" is difficult to define, and it is clear that shaped by its distinct mathematical education each mathematical culture will consider different things as "obvious,"[4] but it is necessary to stop somewhere in a proof, otherwise Carroll's well-known paradox ensues [Carroll (1985)]: in this paradox, to prove that Q derives from P you must prove that P yields Q, and then you need to prove that from P, and P yields Q, Q derives, and so on *ad infinitum*. This is not an empty philosophical worry: this type of regress may be called "the scholiasts' regress," and it is well-attested historically. Take, for instance,

[2] One can add at least one case of translation inside Greek culture itself, namely the translation of some works by Archimedes (*Sphere and Cylinder, Method*), from the original Doric dialect, into the dominant *koine* dialect.

[3] See e.g. Knorr (1989a, 238–239, 812–816).

[4] See especially Goldstein (1995) on the historical variability of such seemingly neutral concepts as "the obvious."

the final proposition of the first book of Archimedes' *Sphere and Cylinder*. The following quotation has been taken out of the text itself (i.e. not from a separate commentary), but it is clear that Archimedes is not the sole responsible for this text. Scholia have accumulated and entered the Archimedean text, so that the deuteronomic text directly manipulates the original—a crucial point to which we shall have to return. This is how it works explicitly (Figure 1).[5]

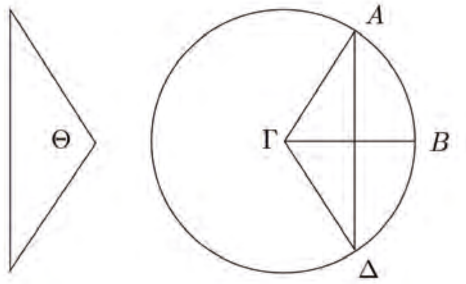

Fig. 1

[A conclusion of Archimedes' line of reasoning:]

> Therefore the figure inscribed in the sector, too, is greater than the cone Θ; which is impossible.

From the mathematical context, impossibility can be seen directly: in fact, it derives immediately from the proposition just preceding this one. To note this fact, some scholiast added the following comment:

> for it has been proved in the [proposition] above that it is smaller than a cone of this kind.

This is the first pedantic note, the first explanation of the obvious. Now the same scholiast, a minute later, or another one, a century later, hastens to add:

> that is [a cone] having [as] a base a circle whose radius is equal to the line drawn from the vertex of the segment to the circumference of the circle (which is [the] base of the segment), and [as] a height the radius of the sphere.

This is of course the standard description—scholiasts care about standard descriptions, a fact to which we shall return. But wait – is this really the cone we have here? Yes, assures the scholiast:

> and this is the said cone Θ.

Or is it? Wonders the same scholiast, or yet another one, yet another century later: We must say why!

[5] Heiberg (1910, 162.25–164.11). It should be clear that much modern editorial work – and much subjective judgement – is implicit in any reference to "Archimedes" or to "the scholiast." Still, this is a case where the two terms seem warranted, on linguistic and other grounds.

for it has both: [as] a base, a circle equal to the surface of the segment, that is [equal] to the said circle, and a height equal to the radius of the sphere.

This is, then, what one calls "pedantic": explaining explicitly what should be understood implicitly—a process to which there is in principle no end and which therefore can make the pedant look not only dim- witted, but also absurd. At any rate, this is clearly one type of pedantry, vertical pedantry: digging too deep. Apparent absurdities of the type quoted above are relatively infrequent (though more can be easily added: e.g., in the same book, Proposition 13, [Heiberg (1910, 56–24)], because scholia become recursive only when an original scholion becomes part of the transmitted text—a common but far from universal phenomenon; while marginalia to marginalia are less common. However, this is almost the most common type of scholion we find in mathematical works: a brief, essentially trivial, mathematical explication, showing why a derivation works—a question which in principle could always be raised and therefore was less frequently raised by the original Greek mathematicians.[6]

4.2.2 "Horizontal Pedantry"

So far I have described what I call "vertical pedantry," where you dig too deep. Another, related type of pedantry is "horizontal pedantry": digging too wide. Just as one can go on proving obvious things, anterior to the proof, so one can go on proving implied things, posterior to the proof. From the point of view of both classical Greek mathematics and of modern mathematics, such proof of implied results may seem redundant. A proof, certainly a classical Greek proof, typically deals with one single case, and leaves several other ones merely implied. Commentators, then, often go on to prove these implied cases. Take Figure 2 as an example: this is a lemma proved (probably) by Archimedes and quoted by Eutocius[7] in his commentary to the second book of the *Sphere and Cylinder*. The lemma shows that on line AB, a certain maximal area is obtained at point E. This is shown by taking another arbitrary point Σ, and showing that the area obtained by it is smaller than the one obtained by E. This is because a certain hyperbola is always, in the area above the line AB, "contained" by a certain parabola, and in such a way that these conic sections are tangent at point K, just "above" E. Thus the result is implicitly seen to be general: since the point of the tangency is unique, the area obtained at E must be maximal. Such implicit generality is typical of classical Greek mathematics, indeed of mathematics in general: you take an arbitrary case and show a certain result, and since the grounds for the result are seen to be general it becomes immediately clear that the same result is obtained in general, and not only for the arbitrary case taken for the sake of the proof. So this is a general proof, taking Σ as an example, where nothing in the proof relies upon taking Σ on this side of K and not the other.

The above, however, only is one part of the proof as it reached us. As it stands in the available text, following this first part, the proof goes on to address the special case of another arbitrary point, ς, on the other side of E. In Netz (1999a), I have argued that this second part is an addition due to Eutocius. So, Archimedes produced a general proof that only considered

[6] See Knorr (1996, 222–242), for a full discussion of this phenomenon of such, usually very brief, explications to arguments (e.g. in the form of cross-reference–on which more below).

[7] Eutocius is the only commentator on Archimedes extant from antiquity. He was active in the sixth century AD. The diagram is taken from Netz (1999a, 19), where I discuss in detail the historical background to this text [Heiberg (1913, 140–146)], as well as develop the argument for showing the presence of "horizontal pedantry" there.

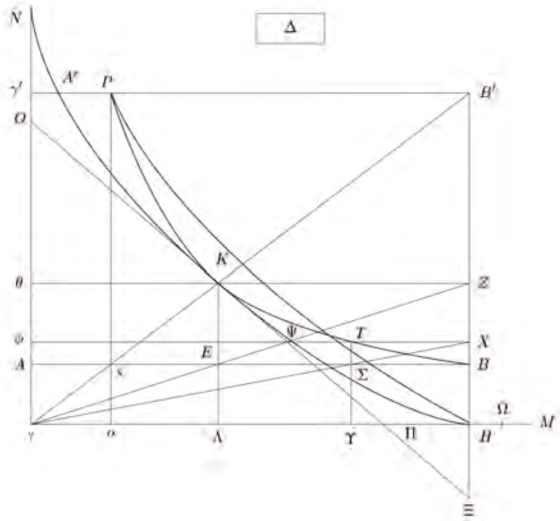

Fig. 2: Reproduced with kind permission of Springer-Verlag.

the case right of point *E*, and the commentator Eutocius inserted a completely redundant proof for the left side of *E*. This is horizontal pedantry, literally horizontal in this case. You dig *too wide*. You go on proving what has already been implicitly proved, by adding cases that actually are no more than trivial extensions of cases already considered by the original. Notice that, if my interpretation is correct, the commentator has once again manipulated, and intervened on, the text—just as we saw with the accretion of scholia in Archimedes' *Sphere and Cylinder*. This, of course, was not done in the intent of misleading—I believe Eutocius expected his readers to recognize the point where Archimedes' proof ended and his own appendix began. But he in essence produced a sequence of texts, partly authored by Archimedes, and partly his own, and later readers were led to take Eutocius' special case as a part of Archimedes' own proof. As an historical figure, Archimedes was therefore partly constructed by Eutocius.

While the example mentioned here is special due to its great complexity, it nonetheless is very typical on other counts (which obviously is a necessary part of the argument developed in Netz (1999a)). Commentators and other deuteronomic authors do often pile up cases. For example: Brentjes (1998) has analyzed the main additions to Euclid's *Elements* I in Arabic translations/editions. Of the eighteen clusters of additions dis- cussed by Brentjes (we use the term "clusters," for those additions often differ substantially from one line of tradition to another), 14 involve such case-additions, in which, Euclid having proved one case out of several possible ones, the Arabic and Latin authors (or their Arabic, or Greek sources), added further cases either in a commentary, or inside the translated, viz. edited, text itself. To pile up more examples always repeating the same pattern would be unnecessary. In every example as in the above, the main form of addition to a given text—the main form of deuteronomy—was the addition of cases.[8]

[8]While case-analysis is by far the most common form of horizontal pedantry, other forms exist, e.g. explicitly proving converses left out by the original text (as Proclus does in his comment on Euclid's *Elements I.5*), or that

4.2.3 Standardization

Besides adding trivial proofs, there are other ways by which commentators revealed their "scholastic" nature, such as drawing attention to allegedly trivial aspects of the text, such as form *versus* content. Of course, the word "trivial" may be misleading, since it will be obvious that form is not unimportant in a subject-matter like mathematics. Still, we are here facing the same principle: a deuteronomic author pays special attention to what may have been of lesser importance for the original author. While the latter cares mostly for mathematical facts and proofs, the former delves on the form in which such facts and proofs are presented. This has at least two levels.

First there is the level of the local form, that is the form of the individual proposition. Commentators devote time to describe this specific form, to refer to it in various ways. It must be understood that the very vocabulary we have for this is an invention by commentators. The Greek terms for the parts of the proposition are familiar: *protasis*, *ekthesis*, *diorismos*, *kataskeue*, *apodeixis*, *sumperasma.*[9] Here is how Proclus describes this,[10] "*every problem and every theorem that is furnished with all its parts should contain the following elements: an enunciation, a setting-out, a definition of goal, a construction, a proof, and a conclusion*." This is one of the most well-known and influential passages in Proclus. What must be stressed is that Proclus himself most probably invented these terms and this analysis. Not a series of terms used by the classical Greek mathematicians, these are rather specific to commentators' ways of speaking; they betray an interest in the description of form, which is typical to commentators.[11] It is also very typical that, starting from a practice to a great extent already present in Euclid's own text, Proclus' description goes beyond the actual systematizing present in it: this is a typical process of canon-formation, where the granting of canonic status goes hand in hand with the transformation of the original text. Even more pedantic is Proclus' actual claim that these forms should be present: he wants to standardize the form of mathematical texts. Now, at this point, Proclus' commentary is part of a commentary on the first proposition of the *Elements*. In this connection, it is therefore interesting to consider the text of this proposition as printed by Heiberg, in his standard critical edition. The end of the first proposition includes a stretch of text in square brackets, i.e. belonging to a textual tradition that is rejected by Heiberg. The text runs as such:

> Therefore an equilateral triangle has been set up on a given limited straight line [Heiberg (1883, 12–17)].

Heiberg's critical comment reads: "*omitted by all the manuscripts; from Proclus only; accepted by August; hardly could be genuine*." ("From Proclus" refers to the fact that Proclus,

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of providing a synthesis automatically recoverable from the original analysis (as Eutocius does with his alternative proof for Archimedes' *Sphere and Cylinder* II.8 [Heiberg (1913, 206–12)]), or, finally, offering a complete analysis and synthesis pair when the original follows a direct synthetic approach (this is the case of the alternative proofs of Euclid's *Elements* XIII.1–5, extant in some traditions).

[9] I translate them by "enunciation," "setting-out," "definition of goal," "construction," "proof," "conclusion," but the reader should note there are other traditional translations for these terms. For a discussion, see Mueller (1981, 11–14).

[10] Proclus was a 5th-century AD philosopher, among whose many extant works is a commentary on the first book of Euclid's *Elements*: more philosophical than mathematical in its interests. The quotation is taken from that commentary [Friedlein (1873, 203–5)]. I use Morrow's (1992) translation, substituting my terms for the parts (p. 159).

[11] A detailed philological argument in favor of the later invention of this system by commentators is given in Netz (1999b).

who quotes extensively from the text, quotes this sentence, too, as if it were in his text of Euclid.) While Heiberg rejected this sentence, other editors have however accepted it, and followed Proclus instead of the manuscripts. But what *is* the text? It is the *conclusion*—the final part of the proposition which according to Proclus *should appear in every proposition*. It is almost certain that, while commenting on the first proposition and in this context introducing his general analysis of the desirable form of propositions, Proclus changed the text, so that it fits his own schematic system.[12] He added a part not present in Euclid's original text, simply so as to make Euclid comply with his formal stipulations—and this addition has later become part, not mainstream admittedly, but still part, of the Euclidean tradition.

This case hardly was isolated. According to Proclus, the enunciation should for instance be repeated *verbatim* in the conclusion, and this is generally the case. But there is evidence that, when faced with conclusions deviating from enunciations, later scribes sometimes modified them. Indeed, in some but not all manuscripts, disagreements are to be found between enunciation and conclusion. Whenever such disagreements are not obvious scribal mistakes, a *lectio-difficilior* type of argument makes probable the hypothesis that they were already present in the archetype, and that in some manuscripts either the enunciation or, more probably, the conclusion were corrected so as to agree with the other.

As one gets further away from original texts, such formal awareness of proposition structures becomes increasingly striking. In many translations of Greek mathematics, there is a tendency to signal the various parts of the proposition in standard ways, to introduce the proof, e.g., with the words "proof of that,"[13] or to label the various parts of the proposition (or at least some of them) by explicit titles such as "demonstratio," "conclusio," etc.[14] As a result of an analysis devoted to the form of individual propositions, a general standardization process was therefore undertaken by late commentators.

4.2.4 Classification

Beyond standardization, this attention paid by deuteronomic authors to the various parts of propositions manifested another of their tendencies, namely to offer metamathematical terminology and classification. Other forms of this tendency had a more direct mathematical significance. In this context, probably, was for instance developed a classification of geometrical solutions in antiquity: "planar" (i.e. requiring only elementary Euclidean methods), "solid" (requiring conic sections), and "linear" (requiring some specially generated lines).[15] While the accumulation of mathematical solutions from antiquity suggests that authors were happy to push ahead with whatever means they had at their disposal, later authors show a more restrictive approach. Pappus for example criticized a suggested solution for *failing to belong to the correct class*. This demand of Pappus—that solutions should belong to certain correct classes—is analogous to the demand of Proclus mentioned above about the arrangement of propositions in certain, correct parts. While the classification of kinds of solutions

[12]The deep reason why such a gap between analysis and text occurred at all is that Proclus' analysis is geared towards theorems (the more common type of proposition in Greek mathematics), while this first proposition happens to be a problem. Problems, in Euclid and elsewhere, tend not to have conclusions.

[13]This is very common in Arabic translations: see e.g. Toomer (1990, passim).

[14]This happens in some Latin traditions: see e.g. Knorr (1989a, 681–63).

[15]The *locus classicus* for this is Pappus, Book III; see the discussions in Knorr ([1986] 1989b, 341–ff), as well as Cuomo (2000, Chap–4)–which is much more sensitive to the historical setting from which such schemes derive (in particular, Cuomo discusses in detail Pappus' criticism of the purported solution I mention).

has, in principle, a sound mathematical basis, this mathematical basis could not be proved by ancient means. The Greeks simply did not know the mathematical relations between the various types of solutions: all they had, as a basis, was the corpus of accepted solutions. The classification of solutions relied, therefore, on the authority of a mathematical canon: a typical "scholastic" use of tradition.

4.2.5 Systematization

We have examined above one level of attention paid to formal structure, namely that of the form of individual propositions. At a more global level, there also is a similar "formal" attention. Here we definitely begin to approach some very deep transformations effected by commentators. One especially significant example will be introduced here.

Book II of the *Elements* was at the heart of the controversy surrounding the so-called geometrical algebra.[16] The book proves geometrical relations which may be considered as giving solutions to basic algebraic equations. Unguru and others have argued that this is not algebra, for two main reasons. The first point is this: there is no attempt to derive one equation from another, to show the interdependence of the equations—which would have been very natural if you were to approach them *as equations*. Each proposition is proved separately and there is no tight deductive structure—in fact, this is one of the least deductively organized books of the *Elements*.[17] So what are the proofs based upon? Here comes the second point: each of these propositions is proved, separately, through geometrical relations holding only in the specific configurations of a given proposition; everything is done geometrically, through the diagram. This is how Euclid's *Elements*, Book II looks like, and therefore it is geometry and not geometrical algebra.

But then, it was transformed by a later tradition, and I do not refer just to modern historians of mathematics. An Arabic commentator to Euclid who died c. 922, Al-Narizi reports a comment of Hero's in which alternative proofs are offered to most propositions.[18] Since seen from our distant perspective they seem to belong to the same project, we need not go into the question of distinguishing what in Al-Narizi's text is due to Hero or to Al-Narizi. But what exactly is this pro ject and what are the alternative proofs reported in Al-Narizi's text? These proofs have two main features: (1) since a single chain of deduction unifies the book, each proof refers back to earlier ones; and (2) as a result, proofs are less dependent on diagrams. Hero and/or Al-Narizi have taken a series of self- sufficient proofs and turned them into a single unity. This is an example of global formal systematization, and here we see very clearly that the entire nature of the work has been changed. It is still premature to call this algebra, since the intention is still spatial; yet something deep has been changed. This text, one must emphasize, bears no trace whatsoever of any philosophical consideration underlying the transformation. Al- Narizi/Hero did not set out to be as deductively unified as possible (not a feature of Hero's own work). Moreover, in other parts of the commentary on Euclid, Al-Narizi/Hero still heavily relied on diagrams, just as Hero did in his personal work. Simply, Al-Narizi/Hero tried to introduce a global organization into a work originally very discrete in structure. The goal is to add cohesion and systematization of which the text seems to be lacking. Such types of unification therefore are on a par with the transformation

[16]See Unguru (1975, 1979) (and references therein), as well as Høyrup (1990).
[17]On the structure of this work by Euclid, see Mueller (1981, 41–52).
[18]See Heiberg and Besthorn (1900).

introduced by the scribes who have changed conclusions to fit enunciations. It is on this level of "scholastic," formal detail that Hero and Al-Narizi work. Yet, we begin to discern the possible significance of these transformations. Not surprisingly, to add cohesion and systematization clearly is a significant transformation.

There are many other ways in which deuteronomic texts are more systematic than the originals from which they start. One typical process occurred with the text of Ptolemy's *Almagest*. Our text can be shown to derive from a Late Ancient recension, one feature of which is the systematic insertion in the text of titles at the head of each chapter, and summarized at the beginning of each book [Toomer (1984, 4–5)]. It should be realized that in all probability Ptolemy's text totally lacked chapter divisions (not an Ancient practice), let alone chapter headings. Whereas the original Ptolemy was a discursive text that proceeded smoothly and without breaks through the entire astronomical corpus, the Late Ancient tradition created a Ptolemy progressing from one discrete, well-signaled point of argument to the next. Once again, this process is typical of a type of canon formation which transforms the original. Briefly, the Late Ancient tradition created a more explicitly structured Ptolemy, not by changing the nature of his *astronomy*, but merely by adding to his *presentation* of this astronomy.

4.2.6 The Phenomenon of Epitome

Yet another kind of global systematization is the epitome, one of the main forms of deuteronomic texts (or of canon formation): a new edition whose main feature is to abridge the original. This gives rise to a certain paradox, as noted recently by Vitrac[19]: while late editions often add to the text (this is the gradual accretion of scholia and marginalia, noted above), they may also subtract from the text. This important phenomenon is worth a detour.

Many Arabic and Latin manuscripts of Greek mathematics take the form of abridgments, a fact which is not often noted, since those abridgments are not used for the printed editions (which rely, instead, on fuller versions). But the printed editions give in this respect a misleading sense of the nature of the world of manuscripts: this often consists not of complete versions, but of abridgments. Sometimes the abridgments form distinctive editions, which may even be the only extant versions: for instance, Archimedes' *Measurement of the Circle* is extant, *already in the Greek*, only in such an abridged form.[20] Most important, it is known from Proclus' commentary to Euclid that a certain Aigeias (?) of Hierapolis has prepared, in ntiquity, an epitome of the *Elements*.[21] We are told that Aigeias combined together two different propositions on parallels, that are Propositions 27 and 28 of book I. In principle, such a procedure could result in a monster-proposition having a double proof, dealing separately with each of the conditions. Another possibility, perhaps more natural in an epitome, is that Aigeias did not offer any proofs at all, but provided his readers, instead, with an abridged version of the *Elements* consisting of the enunciations alone: at the level of the enunciation, such a combination of Propositions 27 and 28 is indeed natural.[22] It is of

[19]Talk given at the Fourth International Conference on Greek Mathematics, Les Treilles, July 1998.
[20]See Knorr (1989a, part–III) for the argument that the extant text is an abridgement–and for an ambitious attempt at following the historical process of this abridgement.
[21]Friedlein (1873, 361–22). We know nothing about the date of Aigeias.
[22]The enunciations are (following Heath's (1956) translation): (27) "If a straight line falling on two straight lines make the alternate angles equal to one another, the straight lines will be parallel to one another," (28) "If a straight line falling on two straight lines make the exterior angle equal to the interior and opposite angle on the same side,

course impossible to say whether this is the case, but it is clear that such epitomes (which are common in our manuscript libraries) circulated in antiquity. There are four known papyri surviving from antiquity which can be considered to derive from a "text" of Euclid, in some sense.[23] Of these, P. Mich. iii.143 only contains definitions, and therefore could belong to a "complete" or "abridged" text—if to a text at all (and not just to a private memorandum).[24] P. Fay. 9 most probably belongs to a complete text of Euclid (though different from our established text).[25] Dating from the second century AD, P. Oxy. i.29, already is different: it contains no letters inside the diagram (which the text of the proof would have required) and we happen to have only the enunciation of Proposition II.5 (immediately following the conclusion—or the enunciation?—of Proposition II.4). It was therefore conjectured by Fowler that this is not a "complete" text of the *Elements*, but some abridgment containing only the enunciations; this conjecture is now corroborated by a recently published papyrus, P. Berol 17469 [Brashear (1994)]. Also dating from the second century AD, this papyrus contains the (unlabeled) diagram and enunciation of *Elements* I.9, surrounded by tiny remnants of the same for *Elements* I.8, 10. Thus it must come from a similar abridgment which gave only enunciations and unlabeled diagrams.[26] Thus, while the archeological evidence is very slim indeed, it is also, after all, the most direct evidence we have, and it shows that, at least by the second century AD, the *Elements* circulated not only in the "full" Euclidean form, but also as an (Aigeian?) epitome. And this has an immediate significance, for it is clear that such a text of the *Elements* has a meaning radically different from that of the complete text: that is, it is no longer a set of mathematical explorations, efforts to show the truth of mathematical claims; it becomes a static repository of mathematical results, whose truth is taken for granted just on the basis of trust in an author. The interest now is no longer in getting to know *why* (i.e., getting to know why such and such mathematical claims are true), but in getting to know *what* (i.e., *what Euclid has asserted*—which is also taken to represent *what the only mathematical truth is*). Once again, this is a "scholastic" transformation: from critical exploration, we move, perhaps, to a more passive absorption of established results. So while epitomes are the opposite of scholia, in that epitomes subtract where scholia add, epitomes are also akin to scholia in nature: both, I suggest, result from an attempt to get to a text which will stand as a perfect repository of the truth on a given subject, and both represent what may be considered a more "scholastic" cast of mind.

4.2.7 Correctness

There are further senses of "scholastic" or "pedantic," which we need to unpack. For instance, I have mentioned above one way in which commentators care for allegedly trivial things; namely that they care for form rather than for content. Moreover, they may be said to care for "correctness" rather than "intelligence": that is, an adherence to values which are

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or the interior angles on the same side equal to two right angles, the straight lines will be parallel to one another." A possible Aigeias version: "If a straight line falling on two straight lines make the alternate angles equal to one another, or it make the exterior angle equal to the interior and opposite angle on the same side, or it make the interior angles on the same side equal to two right angles, the straight lines will be parallel to one another."

[23] I ignore a few papyri which reveal an acquaintance with the contents of Euclid, without being "texts of Euclid," such as the important series of ostraka from the third century BC (discussed in Mau and Mueller (1962)).

[24] Turner, Fowler, Koenen, et al. (1985).

[25] For this and for the next papyrus, see Fowler (1987, 209–ff).

[26] Notice that an important advantage of such an epitome is that it could obviously "squeeze" all of Euclid inside a single papyrus roll.

projected into the canon. This is related to the effort to standardize things, to the interest in form. Proclus is almost like a schoolmaster, who praises his pupils especially for their neat handwriting. He often gives marks to Euclid, almost always for logical correctness, never for the brilliance of ideas behind the proofs. I have found four places where Proclus was moved to express real emotion concerning details of mathematical practice, saying that he "likes" this or is "amazed" by that,[27] and they are all about precision—e.g. that Euclid is making all the necessary qualifications, or is having the proof fit for all cases (again the interest in cases!). It is interesting that this is what Proclus gets *excited* about.

4.2.8 Consistency

I wish to concentrate on a very special kind of "correctness," namely, consistency. Deuteronomic authors often introduce consistency—e.g., as we have seen already, they may "correct" the text where it diverges from patterns such as the Proclean parts of propositions. A special example of such consistency may be seen with Euclid's *Optics*. While the philological problems involved in this case go beyond the scope of this article (and are by no means completely solved), the picture which seems to emerge from recent studies is as follows:

1. We have two versions for the so-called "Euclid's *Optics,*" which may be called version A and version B.
2. Heiberg thought that version B derived from version A [Heiberg (1895, xxx–ff)].
3. Jones (1994) and, following him, Knorr (1994), now argue the opposite, i.e. that B antedates A.
4. This is a typical situation in text transmission: albeit closely linked, no version can be shown to have been derived from the other. So, they most probably represent two separate transformations of a lost archetype.[28]

Assuming therefore that version B is a post-classical transformation of a classical original, one may note with profit that it exhibits a very interesting kind of consistency. It is the only Greek mathematical work in which a letter is very consistently attached to an object: the letter K to the center of the circle[29]: of the 24 centers of circles, 12 are denoted by K, while the remaining 12 can be seen to be part of diagrams closely related to those in version A (where, for comparison, there are 22 centers, out of which only one is denoted by the letter K). So version B introduced a new kind of consistency, not known before in Greek mathematics: the letter K, by being consistently used, acquired a meaning as a *symbol* (and not merely an index referring to a point in the diagram): regardless of the diagram, K "meant" a center.[30] I believe that version B's author wanted to produce a more correct text by adopting such usage. He made sure that the same object was consistently referred to in

[27] Friedlein (1873, 232.11–12, 251.2, 260.10, 426.11).

[28] Jones' position is that clearly both versions are not Euclid's "original text," although he suspects version B is closer in its mathematical content to the original (personal communication). Interestingly, Jones (1994) mentions the more standard lettering of version A as an argument in favor of its being, in my own terminology, "deuteronomic." In his view, an original deviant system of lettering has indeed been 'corrected' in some later edition. This is possible, too: an example of the fact that the dynamics of deuteronomic texts can sometimes lead in opposite directions (*cf.* comments above on "epitome"). For my immediate purpose, however, it is sufficient to take note of Jones' agreement to the view according to which version B may indeed be a deuteronomic text.

[29] This is of course an acronym: K for *kentron*, "center."

[30] I adapt, of course, Peirce's notion of "symbol," as a sign signifying by convention: see the discussion of the application of this terminology to Greek mathematics, in Netz (1999c, 47).

the same way. This is of course speculation piled upon speculation: it is impossible to prove that the K -convention is deuteronomic, and it is impossible to be certain about its origin. But while speculative, this is also the simplest explanation: a deuteronomic author, caring for consistency and "correctness," obtaining, inadvertently, some far-reaching results.

4.2.9 Erudition

Finally, yet another thing which commentators care for is erudition.

Fig. 3

Instead of understanding itself—just reading something and understanding it—they display their erudition, that is, their understanding of how the text may be situated in a wider context of other texts. There is an obvious way in which this can be done in mathematics, and in fact here is one of the great innovations of commentators. They add references to previous propositions, which supply the grounds for the mathematical claim. I attach a figure showing a typical page from a Byzantine classical mathematical text (Figure 3).[31] In this example, while the main body of text reproduces the classical text, which of course has often been transformed in the various ways I have described above, margins contain scholia introduced by Late Ancient commentators. On the right-hand side are more detailed mathematical scholia. On the left-hand side, there are two very brief marginalia which state

[31] This is fol. 268r from Euclid (0888).

respectively (typically, using a shorthand): "through a *reductio* argument" (another example, therefore, of scholiasts being interested in classifying their material according to some metamathematical scheme), and—what I wish to draw attention to right now—"through the 5th proposition of the 10th book."

There is nothing corresponding to this last statement in classical Greek mathematics, where you just understand, directly, the proof, based on your internalized toolbox of mathematical results.[32] You do not state explicitly which earlier book you may be referring to, and most probably you are unable to recall this—you will be *unable* to cite chapter and verse. You just know the fact, not the textual reference. This is *understanding*, which commentators replace by *erudition*: they supply chapter and verse reference and, for this purpose, they do a mighty thing: they *introduce chapters and verses*, the key to mathematical canon formation. Euclid may be an exception to this (again, based on the papyrological evidence) but other classical authors, such as Apollonius and Archimedes, probably did not give numbers to their propositions—which is shown by the fact that different manuscript traditions, as well as different commentators, adopt different numbering systems (often included in the margins of propositions, as any other marginal comment). This situation may be compared to that of chapter headings, such as those introduced in Ptolemy's text. Late Ancient readers introduced, moreover, references based on these numbers. As the numbered proposition is among the most prevalent features of the format of *contemporary* mathematics—without which we can hardly imagine a mathematical textbook!—it should be obvious that the kind of mathematics that is done with this practice is different, in a significant way, from the one done without it.

We have now compiled several significant transformations introduced by deuteronomic authors in classical texts; it is time we try to appreciate the overall significance of these transformations.

4.3 "Scholasticism" and Deuteronomic Texts: Their Historical Significance

We have reviewed above a number of interrelated phenomena. First, there is what I called "digging too deep" (a) and "digging too wide" (b), that is, respectively, the phenomenon of proving the obvious (adding further arguments in order to fill out the proof with details that were left out by the original mathematician, and therefore obtaining a complete chain of proof), and the phenomenon of proving the implied (adding in all cases, so that the proof actualizes all conceptual possibilities and covers them all). The result of both these processes is a text seen as aiming at becoming a one-to-one map of the conceptual world it captures.

Next, we have examined the introduction either of a brand of standardization that took the proposition as the textual unit (c), or else a similar kind of systematization of text structures at a larger scale (e). Many processes are related to such standardization: introduction of greater consistency, for instance in the use of symbols (h), or display of erudition, especially textual erudition (i). All of these processes have to do with a great attention paid to the text *as text*: attention is focused, not so much on the world captured by the text, as upon the text itself. The important thing is no longer simply to provide a *valid* proof, but also (and perhaps even more importantly) to display it in a correctly organized *text* of the proof. The

[32]For the phenomenon of the internalized toolbox, see Saito (1997).

basis for statements is no longer mathematical validity alone, but also a reference to some specified earlier units of text.

Related to the above is an interest for metamathematical features that may have seemed less important to original authors—but which are constantly referred to, as if they based on the accepted canon's authority. No one explicitly states that works by, say, Euclid, Apollonius, and Archimedes are to be considered "canonical," but the very fact that these, and no other, works are transmitted produces a new significance for them. Texts are important, not only for what they say, but also for the very fact that they are there—they come to represent mathematics as it was available to Late Ancient and Medieval cultures. This has several consequences. Most important, there is now a stress on more formal "correctness"—given by the canon as interpreted by, say, Proclus—instead of on mathematical originality as such (g); much freer in the original, the possible range of practices gets pigeon-holed and delimited by prescriptive rules, so that the mathematical enterprise becomes not so much the discovery of *new results*—hitherto unknown facts—but the presentation of known facts along prescribed rules (d); finally, epitomes create a new type of mathematics: not a set of discoveries which the reader is expected to react to and criticize, but a set of accepted results which are solely validated by their status of *canonical* texts.

To sum up, then, there are three main vectors involved in the "scholastic": the attempt to construct the text as a *one-to-one map* of the conceptual world it refers to; a focus on the properties of the text as text; and the introduction of a more limiting, prescriptive code of mathematical practice, invoking a *canon* (which is constructed and manipulated for this purpose), so that mathematics gets to be defined in part, I suggest, simply by its adherence to this canon.

What does all that mean in terms of the image of the mathematical practice?

First of all, it is important to stress that all this has to do with *mathematical* practice. While the authors I describe have a special fascination with texts, they are not philologists. They do not engage with mathematical texts, as a modern textual critic does, in order to say something about texts, hat is about the history of pieces of writing. While texts are the focus of attention, they are the focus of *mathematical* attention: an attention to mathematical texts is perceived as the way *to do mathematics*. It is only after textual criticism comes to be established as a separate activity, with its own goals and criteria – in the XVIIth or even XVIIIth centuries—that Western mathematics cuts its umbilical cord to the Greek canon; but up to this point, reference to the Greek canon simply was *mathematics*. This is not to say that this culture stressed philology at the expense of, say, mathematics; no, it rather is that mathematics itself saw its image shaped by references to a textual canon. The three vectors—the one-to- one map between text and conceptual world, the interest in texts as texts, and the role of the canon—all influence the way *mathematics* is done.

Furthermore, these three vectors influence mathematics in a clearly defined way. While these three vectors may point at somewhat different directions, so that, sometimes, "scholasticism" may lead to opposite results (as noted above, following Vitrac, for additions *and* subtractions to the texts), those three vectors, taken together, do imply a coherent image of mathematics. Let us try to put together this image in greater detail.

One central feature of this image is that it involves the idea of a *perfect text*. Of course commentators are well aware of the fact that the actual texts in front of them are not totally perfect: not all arguments are made explicit, not all cases are set out. But this very recognition of defectiveness implies the possibility of completeness. What Euclid elides, is something which in principle he could have added in, so that in principle—and as an aspiration—one can think of a perfect mathematical text.

Standardization implies, again, that there is a standard—that there is one unique preferred way of presenting mathematics. And while the perfect, unique text is not identical with the actual canonic texts, such as Euclid, the actual canon is often taken as a standard, and the very existence of a venerated canon further strengthens the image of mathematics as striving towards an ideal text.

This ideal text is also unified. It is of course all standardized, and completed in a logical sense; it is also tied together in a rich inter-textual web constituted by the internal references introduced in the canon. What original mathematicians had produced as isolated, *ad hoc* solutions to problems, becomes part of a single system of interdependent propositions, all embedded inside a single canon. Thus an image is projected, of a unique, ideal Mathematics—now with a capital 'M.'

Is this ideal Mathematics a conceptual, or textual, object? In a textual presentation aiming at an ideal one-to-one map between conceptual world and text, this question becomes blurred. While original texts are discursive, and present ways of thinking about a mathematical problem or situation (which are only referred to *through* the text), the new mathematical canon, at least ideally, may mirror everything inside the text itself. Thus the image may be defined as that of a *unique, perfect, text-like Mathematics*. This is the image of mathematics as constructed by "scholastic," deuteronomic authors. If we are able to unpack very easily the contents of such an image, this is largely because this image is still with us; but I claim that such an image did not exist in classical antiquity.[33]

In other words, "scholastic" authors approach mathematics as text, and construe it as text; and therefore they reinterpret the mathematical past as so many steps in the direction of constructing the perfect text. Proclus' account of the origins of the *Elements* is typical. In a very well- known passage on early Greek mathematics, Proclus describes a progress, where Euclid's *Elements* are gradually put together and re-edited by the entire sequence of mathematicians up to Euclid himself. Hippocrates of Chios, Leon and Theudius of Magnesia are explicitly said to have written earlier versions of the *Elements*; other authors are described as "adding results to the *Elements*" or "perfecting," "arranging better," previously known results, until finally:

> came Euclid, who brought together the Elements, systematizing many of the theorems of Eudoxus, perfecting many of those of Theaetetus, and putting in irrefutable demonstrable form propositions that have been rather loosely established by his predecessors [Friedlein (1873, 68–10)].

Yet clearly the line of historical development was in all probability radically different: people in the V$^{\text{th}}$ and IV$^{\text{th}}$ centuries BC were not preparing the grounds for Euclid, but were pursuing their own pro jects (which are only vaguely understood by us). It is far from clear that anyone prior to Euclid has tried to put together a book of *Elements*. Proclus has simply looked at

33 While such a discussion goes beyond the scope of this article, I will argue that ancient sources, such as Plato and Aristotle, think of mathematics predominantly in the context of individual acts of proof, not in the context of a single, idealised Mathematics. I hope to pursue this argument in a separate article. Note however that since this image of mathematics is so well-known, I hardly find it necessary to document in detail its presence in late antiquity and the Middle Ages: I refer in the text to two very typical examples, from Greek late antiquity and the Islamic world, respectively (besides, of course, referring to the totality of the practices documented in the first section of this article.)

the past through the spectacles of the mathematical practice he himself knew, so that he perceived mathematicians as aiming towards an ideal, unique text—the ultimate goal.

Five centuries later, Al-Nadim, the author of the *Fihrist*—an early biobibliographical study, i.e. a deeply deuteronomic text, most clearly expresses the same position[34]:

> Euclid, master of Geometry.
>
> He was Euclid, son of Naucrates son of Berenicus, and he was the discloser and proclaimer of geometry, preceding Archimedes and others.

Here finally Euclid—which, especially in the biobibliographic context of Al-Nadim, means the text of Euclid—becomes geometry personified. The religious overtones are perhaps not accidental, and it is immediately obvious that the same connection between deuteronomic texts, canon formation, and the ideal of a perfect text, can be found elsewhere in the cultures of late antiquity and the Middle Ages. But while such comparisons are meaningful, I will argue that there is a special story to be told specifically about mathematics. I therefore proceed to offer my suggested principle in the history of mathematics.

4.4 Conclusion: Deuteronomic Texts, a Suggested Principle in the History of Mathematics

My argument is embarrassingly simple. Because, in late antiquity onto the Middle Ages, deuteronomy became the main form of doing *mathematics* (and, as explained above, not just a way of working with mathematical *texts*), and simply for this reason, mathematical practices came to the fore which were marked by this second-order context of texts referring to texts. The center of activity became the correction, correlation, and manipulation of an established canon of texts. Commentators perfected proofs, added references, produced epitomes and so on—what else? They do not write directly about circles and triangles, what they write about is about writings about circles and triangles, and therefore it is a tautological result that what they write is a writing about writing.

What makes this story more interesting than just a tautological account of the way in which second-order texts are indeed texts about texts, is the fact that we can see here a causal mechanism, starting from more general cultural forces, and leading to a special result inside mathematics. I shall not attempt here an explanation of the forces which made late antiquity and the Middle Ages what they were, but (as already suggested briefly in the context of Al-Fihrist), these periods are characterized, in general, by the production of deuteronomic texts. Perhaps this has something to do with cultural forces such as the growing role of scriptural religion in these periods; perhaps this has to do simply with the inner logic of the proliferation of books leading on to canon formation and to the growing role of deuteronomic texts positioned relatively to such a canon. At any rate, deuteronomy could probably be found everywhere: in Jewish law as in Greek philosophy, in Arabic grammar as in church doctrine.

But while this process is universal—and thus may of course have universal consequences—it necessarily effects mathematics in a special way, because of its special conceptual nature. As has been pointed out by Corry (1989), mathematics (as well as, to a lesser

[34]Following Dodge's (1970, 634) translation.

extent, philosophy) is the one discipline where first and second-order discussions are part of the very same discipline. For instance, the question whether such and such proposition is provable on the basis of such or such tools is a *mathematical* question. Being essentially about arguments and reasons no less than about facts and objects, mathematics is extremely sensitive to shifts toward the second-order. The moment you begin asking questions *about* mathematics, you thereby begin asking new questions *inside* mathematics itself. Thus the mathematics where you clearly distinguish he parts of the argument (as Proclus insisted on), or in which you attempt to classify legitimate solutions to problems (as Pappus insisted on), is a different mathematics from the one in which you are simply giving arguments in order to try solving a specific problem in whichever way you can. In particular, once the image of mathematics becomes that of a perfect, unique, text-like object, this gives rise to new pro jects: for instance, attempts to remove blemishes from this ideal object (hence, for instance, the history of the parallel postulate[35]), fill gaps in the structure of the canon (so, for instance, Al-Haytham's completion of the *Conics*—which has so many early modern parallels[36]) or to produce total solutions, which will exhaustively cover the whole conceptual space (for instance, Al-Khayyam's algebra, as well as of course many strands of the scientific revolution). All of this, I argue, is a direct result of the intervening culture of deuteronomic texts, which changed the image of mathematics—how could it fail to do this, being essentially second-order?—and which therefore paved the way for modern mathematics.

Two *caveats*, finally, are necessary.

First, while I offer here a speculation on how certain changes emerged in the image of mathematics from antiquity to late antiquity and the Middle Ages, in this way "reducing," in a sense, images of mathematics to its textual practices, I am of course neutral as to the validity of such images of mathematics. To say that an image has arisen historically is not to say that it is either true or false. Perhaps is it more correct to see mathematics as an ideal, unique, text-like object; perhaps is it correct to see it as a set of more isolated, *ad-hoc* practices; perhaps, finally, is this entire question of "correctness" misguided here: I have no view to offer on this question.

Second, while I argue for a direct causal role for the very fact that those texts were deuteronomic, I avoid trying to delimit the possible ways in which the presence of deuteronomic texts may effect the practices involved in a given cultural domain. It is obvious that such effects are determined partly by the nature of the canon itself. The Greek mathematical canon was characterized by the presence of logical arguments, and it is therefore natural that deuteronomic texts based on this canon would focus on this aspect and transform its meaning. In other domains or cultures, the logical proof need not have been so dominant in the original canon, so that deuteronomic texts would have different effects: one may consider the case of Chinese mathematics (where commentaries are no less important as vehicles of mathematics), and it seems that, in this case, deuteronomic texts transformed mathematics in a different way.[37] From this point of view, my aim here just was to offer one possible line of development in what must be a richer typology, taking into account the nature of the canon as well as the specific characteristics of the deuteronomic texts.

[35] Beginning already in late antiquity: see Friedlein (1873, 191–195, 362–375).
[36] See Hogendijk (1985) for Al-Haytham's work.
[37] See, e.g., Chemla (1992).

To sum up, then, I have argued that:

- The main feature of Late Ancient and Medieval mathematical texts was their deuteronomic status.
- This deuteronomic status accounts for the new types of practices present in those texts (often pejoratively referred to as "scholastic" or "pedantic").
- Taken together, these practices project a new image of mathematics as an ideal, unique, text-like object.
- Ultimately, such images and practices change the nature of mathematics itself.
- Thus, deuteronomic texts change mathematics. The very attempt to preserve a canon and work within it, inadvertently produced a new mathematics: conservatism worked as an instrument for change.

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Chapter 5
The Possession of Kuru: Medical Science and Biocolonial Exchange

Warwick Anderson

"Naturally, everyone would like to get their hands on kuru brains," wrote D. Carleton Gajdusek in 1957.[1] A young medical scientist, Gajdusek was writing from his bush laboratory in the eastern highlands of New Guinea, and he had in mind the competition among pathologists in Melbourne, Australia, and Bethesda, Maryland, for the valuable specimens. But he may also have considered his own recent transactions with the Fore people, afflicted with what he thought was the disease of kuru, and on whose hospitality he was then relying. Blood and brains, the germinal objects of his field research, were richly entangled in local community relations and global scientific networks; they could convey one meaning to the Fore, another to Gajdusek, and yet another to laboratory workers in Australia and the United States. These objects could be exchanged as gifts or commodities in different circumstances, or on the same occasion the different parties might confuse gift exchange with commodity transaction. At times, the scientist would try to obtain goods through barter, or even to appropriate them; and, then again, he might find that what he wanted was out of circulation altogether. In the field, Gajdusek had become enmeshed in a complex and fragile web of relationships with the Fore in order to acquire specimens that, through further exchanges with senior colleagues, might yet make his scientific reputation.

In this essay I will examine a variety of transactions between the Fore and the anthropological and medical fieldworkers who first ventured into the highlands in the 1950s. My concern here is not with the "kuru story" itself, nor with an account of who got it right, for the rapid accumulation of kuru knowledge has already been well charted.[2] My question is not *what* did people learn about the Fore and kuru, but *how* did they learn it-and how, indeed, did they make such knowledge both valuable and identifiably their own. Accordingly, the true meaning of kuru-whether disease, sorcery, adjustment disorder, a slow virus, a people, or a territory-should ultimately remain as ambiguous or opaque to the readers of this essay as it was to everyone involved in kuru transactions. How does anyone make sense of a phenomenon as protean as kuru? How does one gain credit for knowing at the same time as one circulates that knowledge? How might Gajdusek, or anyone else, come to possess kuru?

I hope to make a place in this essay for exchanges between the history of science, economic anthropology, and post-colonial studies.[3] In studying the many exchange regimes that developed around kuru-the transactions between the Fore and other local groups, between

[1] D. Carleton Gajdusek to J. E. Smadel, 25 August 1957, in Farquhar and Gajdusek (1981, 121).

[2] For recent accounts of the investigations of kuru, see Nelson (1996); Rhodes (1997).

[3] This work is thus part of a more general effort to make connections between anthropology and science studies. Previously, this effort has been manifest in the introduction of ethnographic methods, as in the pioneering work by Bruno Latour and Steve Woolgar ([1979] 1986); or it has found expression in the increasing use of cultural analysis and a focus on identity formation. For recent surveys, see Hess and Layne (1992); Hess (1995); Franklin (1995); Layne (1998).

medical scientists and research subjects, between anthropologists and informants, between groups of scientists and anthropologists-it should be possible to provide an outline of the material culture of late colonial, postwar scientific exchange. I would like to take kuru brains, with related objects, and use them to think more generally about the creation of value and the circulation of goods in global science. The project is thus aligned with, and yet deviating from, recent work on the commodification of body parts and their insertion into a global medical market.[4] To mobilize kuru objects in a scientific exchange regime was not simply to commodify them. Instead, the material alienated from the Fore became, for a time, part of the inalienable wealth of Gajdusek in his dealings with scientific colleagues. As we shall see, the demands of scientific authorship in this case impeded a conventional process of commodification.[5]

The complicated misrecognition of exchange relations that occurs repeatedly in kuru research suggests that we should avoid a slavish adherence to transactional typologies. The general distinction between a gift economy and commodity economy can be heuristically useful, but such categories are not easily discerned in a cross-cultural setting, a situation where no one could agree on what was a gift and what was a commodity, what was available for barter or appropriation and what was out of circulation.[6] Typically, in the exchange of gifts, objects have a personal value; they are never completely alienated from those who made them or gave them. Gift exchange is intended to create a sense of social obligation, so it is important that the giver gives wisely and the recipient recognizes the character of the transaction. A gift is always to some degree attached to its maker or giver, and it carries with it a social debt, implying a relationship of reciprocity (even if an unbalanced one). But in more commodified transactions, whether local or global, the relationships between things and their transactors are more independent, incurring little or no social debt. In treating something as a commodity, the residual interests of other people can be denied, and the object appropriated.[7] It is tempting, then, to ask whether a "kuru brain" was a gift or a commodity in the exchange relations of late colonial science. If a gift, was the object inalienable from the Fore or from Gajdusek? If a commodity, what was its price? Such questions are tantalizing, but kuru exchanges were never so simple as to provide easy answers.

Since kuru research initially occurred within the disciplines of a colonial order, the exchange regime can appear speciously transparent and one-sided. When events take place in a region that most historians of science would regard as the colonial periphery, it may be that the inequalities and asymmetries of the transactional order, the differences in estimates of value, and the misunderstandings of intention are all fixed more easily in the mind, perhaps to the extent that we cannot readily identify any Fore involvement or agency in kuru

[4] See Radin (1996). For disputed and resisted commodification of body parts and fluids, see Titmuss (1970); Golden (1996).

[5] I outline the more recent trend to commodify science in the conclusion. See Nelkin (1984); Gold (1996) and the symposium on "Legal Disputes over Body Tissue," edited by Nelkin and Andrews (1999).

[6] The distinction is made most clearly in Gregory (1982) and Carrier (1995). I agree with Nicholas Thomas that the analytic distinction of gift and commodity is worth preserving, so long as this does not simply collapse into a distinction between indigenous and Western societies, and does not obscure "the uneven entanglement of local and global power relations on colonial peripheries, particularly as these have been manifested as capacities to define and appropriate the meanings of material things." See Thomas (1991, xi). On the cultural constitution of objects in general, see Appadurai (1986) and Parry and Bloch (1989).

[7] On gift exchange, see Mauss ([1925] 1970); Malinowski (1922); Sahlins (1987); Strathern (1988); Weiner (1985, 1992); Cheal (1988); Godelier (1999). Of course much of this work derives from studies of New Guinea societies, so it seems especially appropriate that it is reapplied to study scientific exchanges in New Guinea.

research. It may seem that their possessions were simply whisked away from them. And yet, as many historians of colonial science-informed by anthropological studies-have recently demonstrated, colonial order often disguised an unequal and disordered reciprocity.[8] In such out-of-the-way cases, our failure to recognize the local entanglements of scientific objects, and the features of reciprocity in their exchange, may simply derive from our convenient reliance on the estimates of value offered by scientists returning from a distant and mysterious field. Perhaps the most distinctively colonial feature of colonial science is that its history can *seem* purely a matter of extraction and appropriation, an insertion of previously valueless objects into a scientific exchange regime with the messy influences of local sociality and politics erased. But the complex transactions involved in kuru fieldwork, and in the later global circulation of scientific valuables, confirm that explanations framed in terms of dominance and subordination will often (but not always) misconstrue local meanings and global power relations.

Historians and sociologists of science have generally hesitated to draw on economic anthropology to explain modern scientific exchange in North America and Europe (or anywhere else for that matter). But in a pioneering analysis, Warren Hagstrom, a functionalist sociologist of science, observed that in return for the gift of research papers, the scientist receives recognition from the scientific community. This exchange seemed to him to create "particularistic obligations," to reduce the "rationality of economic action," and thus to ensure that the scientist conforms to normative behavior. In accordance with a functionalist tradition, Hagstrom therefore subordinated his description of a gift relationship between individual scientists and the scientific community to an explanation of the reproduction of scientific norms. But he wondered why "this frequently inefficient and irrational form of control" persisted in modern science. Why should gift giving be important in science "when it is essentially obsolete as a form of exchange in most other areas of modern life, especially the most distinctly 'civilized' areas?"[9] A decade or more later, Bruno Latour and Steve Woolgar echoed the same question. They protested against Hagstrom's recourse to "the archaic system of gift exchange" to explain exchange relations in science. In their study of the production of facts in a neuroendocrinology laboratory, Latour and Woolgar claimed instead that "the constant investment and transformation of credibility taking place in the laboratory mirrored economic operations typical of modem capitalism." But despite their attempt to commodify the relations they observed, Latour and Woolgar provided ample evidence for the inalienability of things in the laboratory, and the resilience of bonds between the value of objects and social status.[10] They were in fact describing a gift relationship, but one with elements of calculation and competition, similar to the strategic use of reciprocity that Pierre Bourdieu had identified among the Kabyle.[11] It is unfortunate that Hagstrom's linking of gift exchange to normative behavior impelled a generation of sociologists of science, most of them wary of functionalist pieties, to tum away from economic anthropology.

More recently, a few historians and sociologists of science have again come to use economic terms to explain local, and even global, research exchanges. In his innovative study of the work of the Morgan group on *Drosophila* genetics, Robert Kohler describes a "moral economy" of scientists, distributed within the laboratory-where credit and rewards for pro-

[8] See Arnold (1993) and Prakash (1999).
[9] Hagstrom ([1965] 1982, 28).
[10] Latour and Woolgar ([1979] 1986, 203–204).
[11] Bourdieu ([1972] 1997).

ductivity are distributed? and in the wider sphere of exchange between laboratories.[12] Mario Biagioli, informed by the work of Hagstrom and Bourdieu, describes gift exchange in the early modem Italian states as a "medium through which patronage relationships were articulated and maintained." A cycle of debt developed between client and patron, a reciprocal disequilibrium, which led Galileo industriously to try "to produce or to discover things that could be used as gifts for his patrons."[13] Although Biagioli restricts his account of early modern scientific transactions to the traffic between patron and client, and though on occasion he too seems to imply that this exchange regime is archaic, his economic approach might usefully be taken in the analysis of more modern scientific transactions.[14]

There are, of course, other ways to try to understand scientific exchange. In his celebrated study of the practices of experimentation, instrumentation, and theory in modern physics, Peter Galison recognizes the need for an analysis of transactions that occur in the "trading zone" between scientific "subcultures." However, his analytic framework is predominantly linguistic or discursive in style, an expansion "of the notion of language to include the disposition of laboratory objects." Galison thus sees the patterning of exchanges of material objects in ethnolinguistic terms, as the construing of "wordless pidgins" and "wordless creoles." In order to make an important epistemological argument Galison tries to "expand the notion of contact languages to include structural symbolic systems that would not normally be included within the domain of 'natural' language."[15] Still, something of the materiality of exchange, and its role in shaping the identity of transactors, seems to get lost when linguistic analysis substitutes for political economy. I hope that the study of kuru will confirm that one can explain a complex local and global patterning of modern exchange relations without resorting to such tempting linguistic models.

It is obvious that no essay can convey torment and suffering—not that of the Fore, not anyone's. Economic anthropology and the history of science certainly are not geared to such a task. But at least they might help us to understand how suffering was once—and perhaps still is—circulated as science.

5.1 Locating Kuru

The Fore people live in the eastern highlands of Papua New Guinea. During the 1950s there were more than ten thousand Fore living in stockaded villages, looking after their pigs and tending their gardens of sweet potato. The men kept to themselves in the men's house, while all the women and children occupied separate dwellings. The villages were controlled by "big men" and warfare was common: indeed, it was perhaps the major cause of death among males. Although a patrol post had been established at Okapa, for most of the 1950s large parts of the region were still not under government control. The Australian territories of Papua and New Guinea were administered from distant Port Moresby, and the authorities had difficulties enough just covering the controlled areas. The Department of Public Health, under Dr. John Gunther, had expanded enormously after the war in the Pacific, but in 1957 there were still no more than sixty-seven doctors, mostly European refugees, who were expected to prevent and treat the diseases of the whole population of the archipelago. Malaria,

[12] Kohler (1994). His use of the term "moral economy" derives from Thompson (1971).
[13] Biagioli (1993, 36–48). See also Findlen (1991).
[14] See also the important work of Oudshoom (1990); Clarke (1995); Lindee (1998).
[15] Galison (1997, 51–835).

tuberculosis, diarrheal diseases, pneumonia, and malnutrition were common; conditions that could have been prevented or treated still took the lives of thousands each year.[16]

In the 1930s, the Fore observed the first airplane flying overhead; during the war some Australians slipped out through Fore territory, and at least three combat planes crashed there; later a few hardy prospectors passed quickly over the land. Australian patrol officers began to make contact with the Fore in the late 1940s. The first administrative patrol was threatened with arrows at one point, but otherwise the local inhabitants greeted it warmly, if apprehensively. On this first occasion, and sometimes on later excursions, the patrol officers took the more daring of the Fore men back with them to learn some Tok Pisin and see what the government was doing; later, these officers might also appoint village officials and set up police posts; and always they told the people to build roads and stop fighting. The patrol officers found that many of the people would insist on them visiting their villages, where they were given great amounts of food and urged to stay. Gradually new crops were introduced to the region, and the Fore began eating potatoes and tomatoes; they also began to cultivate coffee; and many of them took to wearing laplaps.[17] The exchanges of food and other goods occurred with increasing frequency, and over the next twenty years few outsiders failed to comment on the region's profound social transformation and the remarkable adaptability of the Fore.

Most of the patrols included medical orderlies, whose reports noted widespread ill-health, usually the result of wounds or conditions such as yaws. Epidemics of measles, mumps, and whooping cough preceded contact with outsiders. In response to these (and other) afflictions, sorcery accusations abounded and officers frequently heard of sorcery deaths. Arthur Carey, patrolling the east Fore in 1950, saw some of the effects of this sorcery in the form of a few cases of intense shaking with no fever. The shaking or trembling was called *guria* or kuru, and Carey was told that those afflicted would die quickly.[18] In August 1953, patrol officer J. McArthur confirmed Carey's findings:

> Nearing one of the dwellings I observed a small girl sitting down beside a fire. She was shivering violently and her head was jerking spasmodically from side to side. I was told that she was a victim of sorcery and would continue thus, shivering and unable to eat, until death claimed her within a few weeks.[19]

For the government officers kuru was, as Hank Nelson puts it, "an impediment to orderly administration rather than a disease."[20] And they already had plenty of other impediments to administration, and plenty of diseases for that matter, with which to contend.

The first anthropologists visited the Fore between 1951 and 1953. Ronald and Catherine Berndt had trained in Sydney, and their interest in studying the destructive effects of violence drew them to the eastern highlands. They had wanted to restrict their fieldwork to Aboriginal Australia, but A. P. Elkin, the professor of anthropology at Sydney, advised

[16]Nelson (1996); Mathews (1971); Denoon (1989). More generally, see Bishop and Nelson (1973) and Nelson (1982). On the Fore, see Berndt (1962b) and Glasse and Lindenbaum (1971).
[17]Nelson (1996). In the early 1960s the Australian colonial authorities began actively to convert the local exchange regime to a cash economy.
[18]Cited in Nelson (1996, 188).
[19]J. McArthur, Okapa patrol report, quoted in Lindenbaum (1979, 9).
[20]Nelson (1996, 189).

them to undertake at least one period of fieldwork in a different culture.[21] Ronald Berndt discussed possible New Guinea field sites with E. W. P. Chinnery and K. E. "Mick" Read, and he read Leahy's *The Land that Time Forgot* and Hides' *Through Wildest Papua*, before deciding on the eastern highlands.[22] The Berndts flew into Lae, and then went on to Kainantu in late 1951. When they left Kainantu for their field site, Catherine Berndt, who had sprained her ankle, rode a horse ahead of a long line of porters: "An excited mob accompanied us and our carriers, grabbing at us and pulling us back, making sucking and hissing sounds, shouting and calling to us, greeting us with their welcoming words 'I eat you.' "[23] Still later, Ronald Berndt recalled that:

> Our progress consisted of scenes reminiscent in some ways from those of King Solomon's Mines. The terrain was rough and at times very steep; the track, often edged with jungle, was slippery and narrow. A long line of carriers (more than we wanted), with our boxes lashed to poles, stretched out further than I could see. At the head was Catherine, mounted on her horse and surrounded by plumed and decorated men with their bows and arrows, singing as they walked and danced along.[24]

The Berndts stopped at Maira, in Kogu, where a large house had been built for them, as though (so it seemed to them) they were the returning spirits of local ancestors. "Garden produce was heaped before us, pigs were killed, and dancing and singing went on until well after dark. … We were viewed as returning spirits of the dead who had forgotten the tongue of our fathers and wanted to relearn it"[25] The house was ready for the goods the Berndts were carrying.

During the first period of fieldwork, from November 1951 until May 1952, the Berndts focused on culture contact, violence, and issues of personal responsibility and social control. From their own observations and what the local inhabitants told them, the Berndts attempted to put together a coherent account of the local society, its kinship and language groupings, its feelings of insecurity, recourse to warfare, and patterns of commerce. Maira proved a difficult field site. "At times we did not like these people, but just as frequently we did; and this fluctuation is mirrored to some extent in their own response to life—aggressive and violent excitement contrasted with extreme and sometimes tearful sentimentality." The Berndts were unsure of their status and their role in the local exchange regime:

> We were spirits and aliens, on the one hand identified with themselves, on the other viewed as strangers there for their convenience. We were assumed to be capricious and undependable, possessed of "power" such as ghosts or malignant spirits have, to do them harm if we felt so disposed-beings who had to be propitiated by lengthy recordings and descriptions and explanations. This led to a certain strain in interpersonal relations and served as a basis for some misunderstandings.

[21] Interview with Catherine Berndt, August 1992; Berndt (1992) and Berndt (1992). Although the Berndts had been studying Aboriginal groups since the 1940s, they intended to use the New Guinea material for their PhD dissertations, supervised by Raymond Firth at the London School of Economics. But the Berndts were never fully satisfied with their fieldwork in New Guinea and later worked only in Australia.
[22] Leahy and Crain (1937); Hides (1935).
[23] Berndt (1962b, viii).
[24] Berndt (1992, 72).
[25] Berndt (1962b, viii–ix).

Initially, the Berndts tried to barter for information by handing out matches, shells, and salt. Later the intruders realized that they had become entangled in some sort of gift relationship-at the end of fieldwork, "gifts were distributed on the same basis"-but it seems that the Fore did not believe that their interlocutors gave well in this transaction. A story circulated among the Fore that the anthropologists were "going to round them up and take them to jail, first cutting off their hands and even their heads!"[26] Already, the Fore were on the alert for white headhunters.

The Berndts believed that the suspicion and social disruption that they observed were, in part, manifestations of the local effort to adjust socially and psychologically to European contact. The reactions of the Usurufa, Fore, and other local language groups appeared to follow the pattern set by other peoples in New Guinea.[27] According to the Berndts, the people of the area were convinced that the spirits of their ancestors had sent a cargo of European goods, but that Europeans either had misappropriated them or were unpredictable ancestors who refused to recognize their obligation to distribute these valuables. (The local attitudes toward Europeans seemed ambivalent.) In order to obtain their possessions the Fore and others engaged in magical performances related to spirit possession, hoping to cause planes to bring the materials directly to them. The Berndts thus associated the local behavior with a classic issue in functionalist anthropology, once known as the "Vailala madness" but more commonly described as a "cargo cult" or "adjustment movement."[28] In so doing, they emphasized the emotional insecurity of the Fore, their awe and fear of the coming of the white man, and their expectations of material benefit.

The cargo movement was fed by resentment and frustration. A "zona wind," or ghost wind, was thought to blow across the land, bringing with it the spirits of the dead. When the zona blew, many of the people became possessed by the spirit within it and began to tremble and shake. The Berndts reported that this shaking was called "guria," and was "similar to (but distinguished from) a shaking sickness caused through sorcery."[29] Collective paroxysms had also been reported in other adjustment movements, and the Berndts could cite a number of examples in the anthropological literature of apparent "contagions" of involuntary reeling, staggering, and violent shaking. Once possessed by the spirit of the zona, the people set to work to build a large house and fill it with stones, wood and leaves, objects which would magically be transformed into paper, rifles, and knives. After killing a pig, the Fore would anoint the objects and the house with blood and await the transformation of their holdings. These cargo movements were still springing up sporadically across the region when the Berndts were conducting their fieldwork, but the anthropologists had arrived at a time when government officials and missionaries were able to break up such movements as they arose. The Berndts reported that when two missionaries visited a nearby village, "the natives were told to bury all human bones and skulls, which had been placed in the village clearing as a result of the cold wind and shivering accompanying the initial manifestation of

[26] Berndt (1962b, vii, ix, xiii, ix).

[27] Their first field site was in Usurufa territory, but in their earlier publications the Berndts tended to generalize their findings to cover adjacent language groups, such as the Fore. (Although four language groups were defined, they expressed a "common culture with local variations," Berndt (1962b, 8).) Their later fieldwork was conducted among the Fore and it seemed to confirm their earlier generalizations from the Usurufa observations. The language groups do, however, become more clearly differentiated in later publications.

[28] Berndt (1952, 42–65, 137–58, 202–34). For a later treatment of the same topic, see Berndt (1962a).

[29] Berndt (1952, 57). On guria as, more generally, "a culturally-determined expression of a variety of excitatory themes including physical illness and interpersonal and ecological tensions," see Hoskin, Kiloh, and Cawte (1969).

the cargo movement."[30] But if this policy had suppressed the collective manifestations, it certainly did not eliminate all cases of guria.

In 1952 and 1953 the Berndts spent a further period in the field, this time further into Fore territory. They were now able to differentiate more clearly between collective and individualized spasmodic reactions, and they began to put more weight on alleged sorcery as a cause of some of the psychosomatic manifestations. Although possession by the spirits of the zona still seemed an important cause of guria, the Berndts reported another form of shaking which involved partial paralysis and lack of muscular control, and frequently led to death. According to Ronald Berndt, there were "involuntary twitchings, a feeling of abnormal coldness, dilation of the eyes which appeared to be glazed, and lack of control over the limbs."[31] The Fore attributed individual cases to "guzigli" sorcery, and the Berndts thought that these sorcery accusations were yet another manifestation of anxieties attendant on culture contact.[32] Sorcery might be used in an attempt to resolve increasing internal and external conflicts, yet at the same time it served to exacerbate these conflicts even further. On the whole, the Berndts were prepared to explain the strange behavior of many of the Fore as a form of hysteria, a psychosomatic response to recent stresses, but Catherine Berndt did recall trying, unsuccessfully, to get the medical authorities involved. She had spoken to Margaret Mead about kuru and Mead suggested that she should get the doctors in.[33] Even so, the Berndts would continue to urge later investigators not to rule out a psychosomatic cause. As late as 1959, Ronald Berndt was still trying to describe the widespread emotional insecurity in the Fore region, the sense of distrust and suspicion, the common allegations of sorcery, and the universal belief in sorcery poison. "Social or cultural events," he wrote, "may have far-reaching effects on the human organism itself, even to the extent of interfering so drastically with it that it ceases to function."[34]

Surprisingly, the Berndts rarely mentioned cannibalism in their early papers, but during the 1960s, when they were completing the publication of their fieldwork, they seemed, like so many other scholars of the time, to become fascinated by the subject. Patrol officers had occasionally reported stories of endocannibalism among the Fore, and the Berndts confirmed, almost in passing, that the Fore, especially the women, would engage in the ritual consumption of a loved one after death. Generally it was a relative who was "cooked and eaten almost immediately after death [although] a favored method was first to bury the corpse, and then to exhume it after a few days when the flesh was sufficiently decomposed to be tasty."[35] At first, cannibalism was little more than an interesting excursion from the Berndts' main themes. But by the time Ronald Berndt came to write *Excess and Restraint* he was prepared to expatiate on such unusual funerary practices. "Human flesh," he wrote, "is not eaten to absorb the 'power' or strength of the deceased, nor do men consider that female flesh will have a weakening effect on them." Rather, it was thought that the dead liked to be eaten, and that their wishes should be respected. Most Fore believed that the crops would

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[30] Berndt (1952, 65).
[31] Berndt (1954, 206). For another account of the reaction to contact, see Berndt (1953).
[32] Berndt (1962b, 218–9). Berndt noted that "the attacks are described as becoming more frequent and more intense, with death as an inevitable climax," p. 218.
[33] Interview with Catherine Berndt, August 1992.
[34] Berndt (1958, 25).
[35] Berndt (1952, 44). Catherine Berndt later claimed that Ronald had been offered some partly cooked flesh from a kuru victim, but was too squeamish to eat it.

increase with the eating of their loved ones.[36] Although Berndt, like most other analysts of cannibalism, never witnessed the feast, he accepted his informants' statements on the matter, even their more bizarre tales linking the consumption of the corpse with necrophilia. Walter Arens later condemned Berndt's "lengthy, titillating descriptions of often-combined cannibalistic and sexual acts," but he went too far when he suggested that *Excess and Restraint* was "aptly titled only in the sense that on intellectual grounds it displays too much of the former and too little of the latter."[37]

By 1957, kuru had been identified in a few government reports and anthropological treatises, but it remained a predominantly local phenomenon, entangled in Fore social life and mundane political arrangements. If the place where kuru occurred was known to the world at all, it was as the "Fore region." But before long it would be better known as the "kuru region." How did this change take place?

5.2 Mobilizing Kuru

The Fore believed that a sorcery poison caused kuru; the Berndts suggested that the stresses of culture contact might produce emotional insecurity and psychosomatic disorders, perhaps even something as lethal as kuru; but Dr. Vincent Zigas, the medical officer at Kainantu, suspected that the kuru which afflicted increasing numbers of the Fore was a manifestation of encephalitis, an inflammation of the brain. Initially, Zigas had endorsed the Berndts' idea that kuru was a hysterical reaction, but he changed his mind after spending twenty days with the Fore in 1956, and wrote to Gunther that year to request further medical investigation of the outbreak.[38] Gunther advised Zigas to cooperate with Dr. Gray Anderson from the Walter and Eliza Hall Institute in Melbourne on further studies of kuru. Anderson had been investigating other forms of encephalitis in New Guinea, and both Gunther and Sir Macfarlane Burnet, the director of the Hall Institute, were keen to work together to promote medical research on local problems. Kuru seemed to offer them a good opportunity to do so.[39]

But D. Carleton Gajdusek, an American working at the Hall Institute, heard about these negotiations just before he left to return to the United States. The investigation of kuru was just the sort of diversion he sought. Gajdusek had already decided to break his journey in New Guinea, where he was planning to continue his child growth and development studies, but now he hoped also to resume his field studies of infectious disease, turning his attention this time to kuru. Most of Gajdusek's associates regarded him as a scientific prodigy, if also an erratic and sometimes irritating colleague. After graduating in medicine from Harvard, Gajdusek had worked with Linus Pauling and Max Delbruck at Cal Tech, John Enders at Harvard, and Joseph Smadel at Walter Reed, before spending a year or so at Burnet's laboratory, where he had helped to develop an autoimmune complement fixation test. Throughout his career, Gajdusek would interrupt laboratory work to travel to remote regions, where he would conduct informal surveys of the local diseases and investigate the more unusual ones.

[36] Berndt (1962b, 271).
[37] Arens (1979, 99). Arens points out, rather sardonically, that "the list of New Guinea cannibals and the recorders of their unseen deed is almost endless," p. 98. To be fair to Ronald Berndt, his account of cannibalism takes up no more than twenty-one out of more than 420 pages of text. The Berndts went on to long and distinguished careers as anthropologists of Aboriginal Australia, based at the University of Western Australia.
[38] Nelson (1996, 189). See also Zigas (1990). Zigas had a reputation as a showman and someone likely to embroider a story. See Gajdusek, "Preface," in Zigas (1990) and Lindenbaum (1 July 1990).
[39] F. Macfarlane Burnet papers (1923–1980).

At Melbourne, Burnet had found Gajdusek's personality "quite extraordinary." Although he was obviously very bright, Burnet worried that "you never knew when he would leave off work for a week to study Hegel or a month to go off to work with the Hopi Indians." Gajdusek seemed "completely self-centered, thick-skinned and inconsiderate," but equally he would not let "danger, physical difficulty, or other people's feelings interfere in the least with what he [wanted] to do."[40] Smadel, at Bethesda, believed that Gajdusek was "one of the unique individuals in medicine who combines the intelligence of a near genius with the adventurous spirit of a privateer."[41]

Gajdusek met Zigas at Kainantu; the two of them talked for days, scarcely stopping, about kuru. In March 1957, Gajdusek and Zigas went south and based themselves at Okapa (which the Berndts had called Moke), where Gajdusek observed the condition in many of the locals:

> Classical advancing "Parkinsonism" involving every age, overwhelming in females although many boys and a few men have it, is a mighty strange syndrome. To see whole groups of healthy young adults dancing about, with athetoid tremors which look far more hysterical than organic, is a real sight. And to see them, however, regularly progress to neurological degeneration in three to six months … and to death is another matter and cannot be shrugged off.[42]

Although even Gajdusek thought that kuru in its early stages resembled a hysterical condition, he had few doubts that it would turn out to be a disease with a biological cause, either infectious, toxic, or genetic. From Okapa he and Zigas proceeded to map the distribution of kuru in the Fore region. First, though, Gajdusek had to define the disease entity, or the clinical syndrome, which meant he had to identify a typical history, set of clinical signs, and prognosis for kuru. Gajdusek quickly learnt the basics of the Fore language so that he might understand the symptoms of the illness and its usual course; he used his skills in neurological examination and the instruments, such as plessors, that he had brought with him in order to elicit its typical signs. Soon he was able to differentiate "real" kuru from "hysterical" kuru. Once he had discerned a distinctive clinical pattern, he was then able to track its prevalence across the region, so long as his physical endurance and his boots held out on his arduous "patrols." He was, in effect, compiling the first census of the region. Over the next ten months he found that kuru was far more common, especially among the south Fore, than any outsider had suspected. Gajdusek estimated that during the previous twelve months there had been at least one hundred deaths from kuru in a population of eight to ten thousand, and that one per cent or more of the Fore population had been dying each year from kuru for the past five or possibly ten years. In some hamlets up to ten per cent of the population was sick with rapidly progressive disease; and because women and children were

[40] F. Macfarlane Burnet to J. Gunther, April 1957, in Farquhar and Gajdusek (1981, 41). Burnet resented Gajdusek's intrusion into a territory he had reserved for Australian scientists. Burnet received a Nobel Prize for his work in immunology in 1960.
[41] Smadel, quoted in Rhodes (1997, 55). Joseph E. Smadel was associate director of the National Institutes of Health and Gajdusek's leading supporter. He later found Gajdusek a position at the NIH. (Gajdusek was thirty-four years old when he arrived in the Fore region.)
[42] Gajdusek to J. E. Smadel, 15 March 1957, in Gajdusek (1976, 50). Gajdusek later refers again to "a remarkable tremor that appears more hysterical than organic," in Gajdusek to Smadel, 3 April 1957, (1976, 65).

most susceptible, some areas had a great excess of men in the adult population. "Could any more astounding and remarkable picture be found anywhere?" asked Gajdusek.[43]

At the same time as he was defining the disease of kuru, and mapping its prevalence, Gajdusek was also seeking to identify its cause. Before he entered the Fore region, his earlier work on infectious disease had led him to suspect that some infectious agent was responsible. "We even delayed our departure," he wrote, "to obtain buffered glycerine in which to store autopsy tissues for virus studies," and "when we entered the kuru region, we brought with us equipment to do further autopsies and to collect further specimens for extensive microbiological studies, especially serological and virological."[44] Gajdusek entreated Gunther in Port Moresby, Burnet in Melbourne, and Smadel in Bethesda to supply him with more equipment and his living expenses; he urged the Fore to donate samples of their blood, their cerebro-spinal fluid, their urine, and the bodies of their loved ones, and when his importunity no longer worked, he began to demand their bodies for science. In order to investigate the genetics of kuru he assembled charts of their kinship relations. To rule out a toxic cause, Gajdusek and a visiting nutritionist took samples of the environment and the food of the Fore. By August 1957, Gajdusek had complete charts on more than 150 kuru sufferers—or "patients" as he began to call them—including their histories, circumstances, genealogies, signs of disease, and the results of blood tests and other investigations. From these documents, he wrote, "we can study all that has been done, all that our laboratory tests have shown, and make all the analyses we wish from kuru."[45] The bodies of the Fore, their social life and environment, might thus be reduced to a mobile archive of signs and numbers, available for analysis at Okapa, Melbourne, Bethesda, or anywhere else.

Many of the blood tests and all of the autopsies were completed on site. Gajdusek struggled to acquire the necessary equipment to analyze and preserve the specimens he had collected, to transform the field so it was as much like a laboratory as possible. Soon after he arrived in the area, he wrote that "our immediate need is a treatment hut," for "to study the disease in the home and the village is hopeless."[46] Within a month he had established his "mat-floor hospital … in which we have a microscope, hemocytometer, a host of reagents, and all the diagnosis instruments that such a 'bush' hospital would be expected to possess."[47] But while some information had to be elicited on the spot, many of the more important specimens, especially the autopsy tissues, could only be studied by pathologists and other experts in distant laboratories. Sometimes Gajdusek found it difficult to prepare the specimens as instructed, so that they would be readable elsewhere. "We have no appropriate cannulas," he warned on one occasion, "nor is the cold wind and rushed excitement of 'bush autopsies' conducive to careful and accurate perfusion."[48] But he managed to maintain a copious correspondence with his colleagues in the outside world and to create a valuable trade with them. "It is difficult writing and working here in bush isolation," he wrote, "and I sadly feel the lack of colleagues and critical discussion."[49] Yet hardly a day went by without him typing letters and clinical records, and preparing material for analysis. Specimens, pho-

[43] Gajdusek to Smadel, 28 May 1957, in Gajdusek (1976, 91).
[44] Gajdusek, "Introduction," in Farquhar and Gajdusek (1981, xxiii).
[45] Gajdusek to Smadel, 6 August 1957, in Gajdusek (1976, 172).
[46] Gajdusek to R. F. R. Scragg, director of public health, PNG, 20 March 1957, in Farquhar and Gajdusek (1981, 22). Here Gajdusek's attitude toward fieldwork clearly contrasts with that of the Berndts.
[47] Gajdusek to J. E. Smadel, 3 April 1957, in Farquhar and Gajdusek (1981, 29).
[48] Gajdusek to J. E. Smadel, 8 July 1957, in Farquhar and Gajdusek (1981, 87).
[49] Gajdusek to J. E. Smadel, 10 July 1957, in Farquhar and Gajdusek (1981, 91).

tographs, films, letters, and reports went out by road and on the small planes; and equipment, reagents, medications, and visiting experts came in. The brains and other tissues from the autopsies, along with containers of blood and urine, were airfreighted out to metropolitan laboratories, their destination dependent on Gajdusek's relations at the time with Burnet and Smadel. And if Gajdusek was unsure how to fix and prepare the specimens, instructions soon arrived from neuropathologists and toxicologists in Australia and the United States.

Pathologists at the National Institutes of Health at Bethesda soon reported that the brains of kuru sufferers showed degenerative changes, especially in the cerebellum,[50] and they pointed out that these lesions were similar to those found in Creutzfeld-Jacob disease, and not unlike those of Alzheimer's disease. But the cause of the neuropathology remained uncertain.[51] The absence of inflammation and the failure to grow any pathogenic organisms led Gajdusek, reluctantly, to rule out an infectious cause. No toxic elements had been identified, and the neuropathology was not, in any case, typical of a reaction to a toxin. And while a genetic explanation remained attractive, it was unlikely that a single gene so deleterious could have reached the frequencies necessary to explain the prevalence of kuru among the Fore.[52] But whatever the cause of the disease, Gajdusek had managed in less than a year to create objects of extraordinary medical value in the exchange of kuru material. "If we can't 'crack' kuru," he wrote, "with hundreds of cases available for full study during any 3–6 month period, I see little hope for Parkinsonism, Huntington's chorea, multiple sclerosis."[53] Kuru was not just an affliction of the Fore: Gajdusek had made it essential to the understanding of neurological disease, whether local or global.

In medical journals and the popular press, the Fore region had become the kuru region (or the region of "laughing death" as many newspapers called it). The bodies of the Fore and their social life were reframed in terms of kuru, the territory was being restructured along the lines of kuru, the census of the Fore was a kuru census, and the map of the Fore was a kuru map. As Shirley Lindenbaum has observed, in the investigation of kuru, "Western medicine and colonialism were brought to many in a single encounter."[54] A medical reterritorializing and colonization of this sort was more than just a textual or literary accomplishment. Some parts of the bodies of the Fore, and bits of their environment, had begun to circulate around the world; and, in exchange, bits of science and medicine circulated among the Fore. How were these transactions understood? How were they negotiated and contested?

5.3 Medical Cannibalism

Stories of Fore cannibalism fascinated Gajdusek. In his first letter to Smadel from the "kuru region," Gajdusek had boasted that he was "in one of the most remote, recently opened regions of New Guinea … in the center of tribal groups of cannibals, only contacted in the last ten years and controlled for five years—still spearing each other as of a few days ago, and cooking and feeding the children the body of a kuru case." But he was sure that

[50]Carl G. Baker to Gajdusek, 26 July 1957 in Gajdusek (1976, 164). The discovery of the new disease was announced in Gajdusek and Zigas (1957).
[51]Ronald Berndt maintained that the presence of neuropathology did not rule out a psychosomatic cause, but this argument carried little weight with the medical investigators.
[52]For many years Gajdusek believed that the most likely explanation was that the Fore were genetically predisposed to react to a peculiar toxin.
[53]Gajdusek to J. E. Smadel, 25 August 1957, in Farquhar and Gajdusek (1981, 121).
[54]Lindenbaum (1 July 1990).

"although the people are still current warriors and cannibals, they are well 'under control' and very cooperative."[55] A few months later, one of Gajdusek's Fore friends reported that his clansmen had eaten his grandfather "against his advice." "Such recent, nay current, episodes of cannibalism," Gajdusek wrote, "are not unusual here, but it is highly unlikely that all of our kuru patients have eaten human brain." All the same, it was an enticing thought. "It is so unique a concept, and such a romantic one, that I almost wish cannibalism was more prevalent than it is."[56]

During his fieldwork, Gajdusek would often try to titillate the readers of his letters with associations between cannibalism and his medical investigations, in particular the autopsies.[57] From the beginning he had tried to secure the brains and other viscera of those who died of kuru. "Autopsy material," he wrote to Smadel, "is most difficult to obtain and will require time and much persuasion, but we shall get it. We promised one brain to Melbourne, but if you can promise expert neuropathology, I shall get one off to you."[58] He had to dissect the bodies wherever he could and then perfuse and fix the tissues in his "bush hospital," on the same table where he wrote reports and ate his meals—his "autopsy-tea? lab-typewriting-bench-emergency surgery table that must be cleared for meals three times a day."[59] Once the tissues were ready he would send them away to Melbourne or Bethesda for further study. Usually, it was not easy to persuade the Fore to part with the body of their loved one. And yet, he was often successful. "I write at the moment to let you know that we have had a kuru death and a complete autopsy. I did it at 2 a.m., during a howling storm, in a native hut, by lantern light, and sectioned the brain without a brain knife."[60] But after another autopsy Gajdusek told Smadel that "they are precious specimens, and have cost us heavily in time and effort to obtain under these primitive conditions, where even the suspicion of sorcery worked on body parts or excreta is a great hindrance."[61] When sending some brains to Bethesda, Gajdusek warned that "we were lucky to get two and may get further ones, but our ex-cannibals (and not 'ex') do not like the idea of opening the head."[62] At the same time as Gajdusek was negotiating for the bodies of the kuru dead, then dissecting them on his dining table and ritually preparing them for scientific consumption, native cannibalism had been forbidden by the Australian authorities.

Gajdusek's facetious references to his medical cannibalism indicate some perplexity about the character of the transactions he was engaging in, and therefore some indecision

[55] Gajdusek to J. E. Smadel, 15 March 1957, in Gajdusek (1976, 50–51). Smadel was concerned that Gajdusek might get eaten: "What will happen to the records, the material, and the information that you carry in your head if the plane comes down in the jungle or if one of the indigenes decides to revert to cannibalism?" In Smadel to Gajdusek, 16 August 1957, in Gajdusek (1976, 177). On the continuing appeal of the cannibal metaphor in medicine and science, see Arens (1998).

[56] Gajdusek to J. E. Smadel, 27 September 1957, in Gajdusek (1976, 234). Gajdusek had discounted any connection of cannibalism and kuru as soon as he ruled out any infectious agent.

[57] Peter Galison observes that in physics "experimenters like to call their extractive moves 'cannibalizing' a device," in *Image and Logic* (1997, 54).

[58] Gajdusek to Smadel, 3 April 1957, in Gajdusek (1976, 67).

[59] Gajdusek to Smadel, n.d. (late May 1957?), in Gajdusek (1976, 95). This gives another meaning to Gajdusek's offhand remark that "I hope to begin digesting our data shortly," in Gajdusek to Roy Simmons, 30 June 1957, in Farquhar and Gajdusek (1981, 81).

[60] Gajdusek to Smadel, n.d. (May 1957?), in Gajdusek (1976, 88).

[61] Gajdusek to Smadel, 28 May 1957, in Gajdusek (1976, 90).

[62] Gajdusek to Smadel, n.d. (late May 1957?), in Gajdusek (1976, 94). For more on the difficulty of obtaining autopsies, see Gajdusek to Smadel, 29 June 1957, in Gajdusek (1976, 119). The comment on the distaste for opening the head is strange, given the later claims that kuru was transmitted through the eating of human brains.

about his own identity on the scientific frontier. What did it mean for a scientist to imagine himself as cannibal? For endocannibals, those like the Fore who may occasionally engage in the consumption of their relatives, the ritual permits, in Peggy Sanday's terms, the regeneration of "social forces that are believed to be physically constituted in bodily substances or bones at the same time that it binds the living to the dead in perpetuity."[63] Endocannibalism is generally a means of communicating social value from one generation to the next. But medical cannibals surely must be exocannibals, consuming the bodies not of loved ones but of outsiders. In casting himself as an exocannibal, was Gajdusek attempting to simplify and control apparent disorder, imagining a means of drawing on the resources of others without becoming other?[64] Was he, at the same time, indulging in an unsettling fantasy of consumption without reserve, a desire that implied its own impossibility? Medical exocannibalism could structure the work of colonial science in terms of absolute consumption, while acknowledging that the relations of dominance and submission that might permit such a feast were interdicted—thus "cannibal appetite is its own impossible desire."[65] Above all, the emergence of the metaphoric cannibal at this moment marks a crisis in Gajdusek's exchange relations with the Fore.

It may seem remarkable in these circumstances that Gajdusek resisted the trope of headhunting. Certainly, he believed that the Fore routinely resorted to headhunting; moreover, the Berndts had suggested that headhunting rumors circulated widely among the Fore; but Gajdusek, even in wildest flights of fantasy, neither represented himself as a headhunter, nor did he hear that he was so accused. And yet, is Gajdusek's reluctance to assume the role of scientific head? hunter really so surprising? According to Janet Hoskins, headhunting is "an organized, coherent form of violence in which the severed head is given a specific ritual meaning and the act of head-taking is consecrated and commemorated in some way." The severed head, a trophy of combat, embodies a form of vitality. In Melanesia, "headhunting" has been used "to speak metaphorically about other relationships, which might be characterized as ones of inequality, economic exploitation, and an unequal voice in political decision-making." Hoskins suggests that "heads are taken—in the imagination as in traditional practice-to seize an emblem of power, to terrify one's opponents, and to transfer life from one group to another."[66] Thus if cannibalism, even exocannibalism, implied an unresisted appropriation of the body of the other, an absolute corporeal consumption, headhunting called attention to its violent expropriation. Gajdusek was prepared, imaginatively, to simplify his exchange relations with the Fore, to joke about his medical cannibalism, but he was not ready to imagine any violence in his desire for an unreserved consumption. But even as he foreswore headhunting, the scientist must also have realized that he could never simply become cannibal.

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[63] Reeves Sanday (1986, 7). Marshall Sahlins has speculated on the role of ritual cannibalism in the origin of social order, in "Raw Women, Cooked Men, and Other 'Great Things' of the Fiji Islands," in Brown and Tuzin (1983).
[64] On the problem of social reproduction, see Weiner (1982). On similar means of renewing human energy, see Rosaldo (1977).
[65] Bartolovich, "Consumerism, or the Cultural logic of Late Cannibalism," in Barker (1998, 234).
[66] Hoskins (1996, 2–38). On exchange models for understanding the cultural logic of headhunting, see George (1991). For an attempt to use the trope of headhunting to explain the collecting practices of A.R. Wallace and H.O. Forbes, see Pannell (1992).

5.4 Kuru as Commodity, Kuru as Gift

The bodies that Gajdusek sought were entangled in a confusion of exchange relations and social obligations:

> It looks as though further autopsy materials may be unobtainable. Thus, the natives have given up our medicine … they know damn well they do not work … and I am fighting (verbal battles in Fore), bribing, cajoling, begging, pleading, and bargaining for every opportunity to see a patient, and strenuously working tongue muscles for hours for every further day we get a patient to stay in hospital, accept therapeutic trials, etc. etc. Vin is sick and tired of the "duress of personality" which is required to pressure every case into our care and I do not like the effort. It means, however, that unless we start curing cases quickly, we cannot expect any clinical material much less any autopsy specimens. I am willing to keep up the push using every ruse short of actual duress by force and authority … that we cannot contemplate.[67]

In making a gift of their blood, urine, cerebro-spinal fluid, and the bodies of their loved ones, the Fore had created a social obligation, a social debt that Gajdusek recognized and struggled to repay. (In such gift transactions, as Mauss pointed out, the "objects are never completely separated from the men who exchange them," suggesting that anxieties about the control of exchange are also concerns about the transformation of identities.[68]) In return, Gajdusek tended the wounds of the Fore and gave them antibiotics to treat mundane infections, and he plied the kuru sufferers with virtually every drug Western medicine had to offer. Gajdusek dispensed antihistamines, ACTH, sulfonamides, chloramphenicol, vitamins, iron, phenobarbital, artane, BAL, anticonvulsants, testosterone, and other medications to his kuru patients, all to no effect—or at least none that he could recognize.[69] Before long, it appeared to Gajdusek that many of the Fore were becoming indifferent to his gifts. Even so, he observed, with a degree of perplexity, that "to humor me and repay my many miles of mountain climbing to track them down, they haul the litters over miles of cliff-faced and precipitous jungle slopes to bring patients in for another shot at our therapeutic trials and experimental poking … I admire and respect them thoroughly."[70]

Kuru brains and the other local objects of interest to scientists could not simply be appropriated. Gajdusek, like the Berndts before him, was participating, perhaps unwillingly, in a complex and ambiguous moral economy. The Berndts had tried to resist it, for although they lived in a house that the local inhabitants built for them, they did not like the Usurufa

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[67] Gajdusek to Smadel, 24 November 1957, in Gajdusek (1976, 309–10). Earlier, Gajdusek had written to Gunther: "We have ticklish problems in trying to avoid any trace of coercion of the natives. We have gained their confidence around Moke." In Gajdusek to J. Gunther, 3 April 1957, in Farquhar and Gajdusek (1981, 28–9).

[68] Mauss ([1925] 1970, 31).

[69] See list in Gajdusek to Burnet, April 1957, in Gajdusek (1976, 72). Gajdusek later complained that "everyone wants *shoots* and pills and they want these in return for saying they are *sik*. To get a further history and symptomatology from them is a long and tedious task and to satisfy them they must have as many pills as the next man." In Gajdusek (1964, 98) [27 March 1960].

[70] Gajdusek to Smadel, late May 1957, in Gajdusek (1976, 92). On a later field trip, Gajdusek wrote of one of his Fore friends: "I admire him and am deeply grateful that my little attentions to him and his people have resulted in such an unusual show of allegiance and accord. I only hope I can justify it." In Gajdusek (1964, 59) [17 March 1960].

and the Fore peoples, and they refused to supply the valuables that were expected. When finally they left the house, complaining about the constant pilfering, their former hosts treated them with hostility.[71] But Gajdusek seems to have established more conventional exchange relations with the Fore, building an alternative men's house and a store house (or hospital), from which he dispensed medical goods. He engaged, too, in the local commodity transactions, bartering axes and other objects for pigs and vegetables, and carrying "trade items," such as salt, kina, beads, and tobacco, with him as he patrolled the region.[72] Gajdusek found the neighbouring Kukukuku (later known as Anga) to be especially keen traders, "reminiscent of Latin Americans. Rather than accept whatever we offer, they bargain and haggle. Furthermore, they know how to bargain shrewdly and set a price, to reject offers they deem unsuitable, to suggest better ones, and to insist upon prices we either cannot pay or have not the items to pay with."[73]

Some things had a price, but the ones that Gajdusek most wanted—blood, body fluids, corpses—either were out of circulation altogether or could only be given as gifts. On some occasions Gajdusek did try to commodify the exchanges, but with little success. "I did a complete autopsy in our treatment/laboratory hut by lantern light, and then at first cockcrow got the body borne homeward with the mourning mother well rewarded with axes and salt and laplap," but the mother paid little attention to these objects.[74] Although the Fore were not yet engaged in a monetary economy, Gajdusek could at least try to convert all his transactions into a form of barter. Generally, in barter transactions the relationships between the parties are discontinuous and unstable; an exchange ratio, or substitutability, is determined during the bargaining, and through this process, "barter exchange creates equality out of dissimilarity." Some trust between parties is still necessary, but the relationship formed tends more toward "reciprocal independence," in contrast to the "reciprocal dependence" of gift exchange.[75] However, as Nick Thomas points out, "what for one side is a gift relationship may be barter for the other."[76]

The advantages to Gajdusek of representing his exchanges with the Fore in terms of barter are evident. In receiving a gift from the Fore, Gajdusek knew he was incurring a debt he was unlikely to repay in a satisfactory manner. Moreover, the gifts he received would always be bound to their original owners, to some extent inalienable even if out of their control, to some degree still attached to the Fore even as they were received into other hands.[77] And yet, if Gajdusek was to take scientific credit for his work, he somehow needed to alienate these objects from the Fore, to treat them as commodities like any other, or to consume them, cannibalize them. He was attempting to mark out a boundary that separated the Fore from their goods, to put a line between local and global exchange regimes, and thus

[71] Berndt (1992). The Berndts certainly recognized that they were expected to present gifts to the Fore: see Berndt (1954, 271–2).

[72] Gajdusek to Yin Zigas and Jack Baker, 1 Sept 1957, in Gajdusek (1963, 40). Gajdusek often found it difficult to "retain equilibrium in the complex plurality of relationships which I have here in this region." Gajdusek (1964, 137) [16 April 1960].

[73] Journal, 5 October 1957, in Gajdusek (1963, 76).

[74] Gajdusek to Burnet and Anderson, 19 May 1957, in Farquhar and Gajdusek (1981, 57).

[75] Humphrey and Hugh-Jones (1992). See also Gregory (1982, esp–42).

[76] Thomas (1992, 38). Thomas also makes the point that "barter has always been associated with social margins," p. 21.

[77] On the notion of "keeping while giving," see Weiner (1985). In such cases, "the affective qualities constituting the giver's social and political identity remain embedded in the objects so that when given to others the objects create an emotional lien upon the receivers," p. 212.

produce a space in which he might assimilate or circulate scientific valuables. But much as Gajdusek may have wanted simply to "cannibalize" or consume the bodies of the kuru dead, he was never able to do so. Nor could he simply treat kuru brains as commodities, as objects of abstract or negotiable value alienated from their original owners and thus available for barter. "Kuru brains," Gajdusek wrote, "are not a commodity on the open market, nor will they ever be; we are lucky to get any."[78] Try as he might to possess or appropriate kuru brains, the exchange of gifts in medical research had bound him to the Fore, brought him into a relationship of mutual obligation and unbalanced reciprocity.

The character of exchange relations derives from prevailing cultural assumptions about the objects, the transactors, and the place in which their encounter occurs.[79] Depending on the social arena—whether, for example, it is a marketplace, a clinic, or a home—the object may move in and out of commodity or gift status. In cross-cultural encounters the possibilities for error, for misrecognition of transactions, are multiplied; and since gifts imply a social reciprocity, the "mistakes made in giving have consequences that commodity transactions almost never have."[80] When, for example, Gajdusek took blood from the Fore, he understood that it was given freely, that it had no price, and that it required something in return. But he seems to have no way to gauge the quality of the gift, its rank among the objects that the Fore might give to strangers. On one occasion, Gajdusek found that "no protest or difficulty bleeding anyone was encountered and the natives evidenced some disappointment when we ran out of bleeding containers."[81] But later, he conceded that "the fact is that I have done so much bleeding of primitive people that I am, in all possibility, a little over-confident."[82] His over-confidence in his judgement of value and decorum could lead to misunderstandings.

In accepting the gift of blood Gajdusek was becoming inextricably entangled in local ideas about wounds, menstruation, propitiation, and identity. In a society where female menstruation was regarded as demeaning and dirty, and imitative male bleeding was viewed as strengthening and purifying, the taking of blood must surely have had a different significance depending on the gender of the donor. It would seem likely that the Fore connected Gajdusek's efforts at bleeding the men with the bloodletting rituals that marked male initiation, but if so, Gajdusek was unaware of any definite association.[83] For Gajdusek, the meaning of blood was primarily medical. The Berndts had been more interested in the symbolic meanings of blood, but then, of course, they had never tried to acquire any of it. They noticed that blood was split and sprinkled, used for anointing participants in rituals, or sprayed on objects awaiting transformation in the store houses:

> Blood (whether of pigs or of men) is a "human" element, and is thus a desirable substance from the spirit's point of view. Blood is, in essence, "life," so that in presenting blood gifts to the spirit, the inference is that it will come into the human orbit. Moreover, blood being a symbol (more than that, a necessary

[78] Gajdusek to Smadel, n.d. (late May 1957?), in Gajdusek (1963, 93).
[79] Appadurai (1986). Thomas also points out that cross-cultural exchange "frequently entails differing assumptions or claims about whether a thing is a commodity or a gift, as well as divergent views of the commodity candidacy of things and the context of exchange itself" *Entangled Objects* (1991, 30).
[80] Thomas (1991, 15).
[81] Gajdusek to Yin Zigas and Jack Baker, 8 September 1957, in Gajdusek (1963, 48).
[82] Journal, 28 September 1957, in Gajdusek (1963, 57).
[83] On these rituals, see Berndt (1962b, 94–104) and Lindenbaum (1976, esp–57).

> component) of life or reality, the sprinkling of blood over leaves, sand and stones which are placed in the special house means that their reality is ensured: they are bound to tum into real objects.[84]

Gajdusek's specimens were gifts of a special order, but his apparent failure to recognize their status must have distorted or attenuated the bonds forged in the exchange.

5.5 The Brains Trust

Just as the Fore were seeking to bring Gajdusek into their orbit, Gajdusek was attempting to create social bonds with leading scientists in Melbourne and Bethesda. Above all, if Gajdusek was to receive adequate social credit in a scientific exchange regime, the recipients had to recognize the objects as priceless gifts, not commodities on an open market. (When Gajdusek wrote to Smadel pointing out that kuru brains were not commodities and that "we" were lucky to get one, he probably meant that it was Smadel who was lucky to get one.) As Marilyn Strathern observes, in gift exchange "people must compel others to enter into debt: an object in the regard of one actor must be made to become an object in the regard of another." In acquiring and making available the kuru brains, Gajdusek was attempting to anticipate the "extractive perspective" of his colleagues in Melbourne and Bethesda, to "objectify" his new assets.[85]

But even if the recipients recognized the objects as gifts, did they recognize the donor? As gifts, the objects would remain to some extent inalienable from their original owner, so it was necessary for Gajdusek in his relations with Burnet and Smadel to abstract the objects from any associations with the Fore, to recontextualize the brains as his possessions, not the Fore's. Thus, for Gajdusek to donate kuru material—for him (and not the Fore) to gain credit and visibility in the exchange—he would always need to construct a clear boundary between local and global exchange regimes. Fore bodies might be a local asset, but with clever "boundary-work" kuru brains would become part of Gajdusek's inalienable wealth in a series of scientific gift exchanges.[86] Burnet and Smadel thus accepted the gifts as "Gajdusek's kuru brains," not as generic and valueless Fore brains. The scientific exchange objects became part of Gajdusek's inalienable wealth, proof of his immortality, his power.[87]

But it is, of course, a little more complicated. For even when his colleagues came to regard them as "Gajdusek's kuru brains," these objects still retained some Fore aura. Indeed, an exotic association was part of their exhibition value. But the exchange value depended on inserting tissue fragments (Gajdusek's fragments) into a scientific network—indeed it required these reframed pieces to bring together such a network and thus to become meaningful and valuable. In making these brains—his brains—scientifically serviceable, Gajdusek was ensuring that the aura of the Fore shriveled: it was reduced, but did not disappear.[88] In a scientific exchange network, the attachment of these objects to the Fore would be no more, and no less, than a distant claim of provenance.

Gajdusek carefully allocated his gifts of blood, brains, and other tissues to competing scientific institutions so that leading metropolitan figures incurred increasing social debt to

[84] Berndt (1954, 226).
[85] Strathern (1992, 177–178).
[86] On boundary construction and maintenance in science, see Gieryn (1983). See also Gray (1984, 54–2).
[87] See Weiner (1985).
[88] Benjamin (1969).

him. As the objects were scarce and could be linked to important medical problems, great value was conferred on them in the exchange. In return, Gajdusek received recognition, an institutional affiliation, research support, and, eventually, a Nobel prize. Among the Fore, Gajdusek had observed big men manipulating competitive ceremonial exchange systems in order to enhance their social status. In science too, one could manage networks of exchange partnerships in a drive for credit. Whether among Melanesians or among scientists, "for big men it is important both to have large networks and to manage them well."[89]

But Gajdusek also found that not all objects of scientific interest could enter into circulation (and some of them, like kuru brains, might be withdrawn from circulation when exchange relations went awry). Early in his fieldwork, Gajdusek had promised Australian investigators a live kuru sufferer that they might study in their metropolitan clinics. He proposed

> sending an ideal case to Brisbane, Sydney or Melbourne for study in a unit such as Dr Wood's Clinical Research Unit. This would yield, in the long run, far more information and far more reliable results at a far smaller expense than all sorts of half-hearted efforts at getting experts and equipment into the highlands.... Now, I am not suggesting accepting a classical early case on the Clinical Unit ward for autopsy purposes, but rather for clinical study and evaluation.[90]

He thought at the time that shifting such a "case" out of the region would not bother the Fore and might make it easier to obtain the autopsy, but he soon found that the Fore were more likely to permit an autopsy than to allow someone to die away from their relatives and community. Some objects could not be abstracted from their local context, could not be mobilized and repackaged as gifts in a global scientific network. (And in any case, Gajdusek's own relations with the Melbourne researchers, the most likely recipients, had rapidly broken down.)

The exchange of materials and the reproduction of social relations in global science required constant work and unfailing tact.[91] Since gifts were exchanged within a common culture, mistakes were less likely than in the exchanges between Gajdusek and the Fore. But all the same, the transactions were complicated, requiring sensitive calculation and management. In regulating the brain wealth of Melbourne and Bethesda, Gajdusek was industrious, but not always discreet. His intrusion into Australian territory had already offended Burnet, and relations were not properly mended even after he gave the first kuru brain to the Hall Institute. Burnet soon realized that Gajdusek sent most of the other tissues from the first autopsy, and most of the brains from later autopsies, to Smadel at the NIH.[92] Dr. E. Graeme Robertson, the Melbourne pathologist who examined the first brain, thought that it was "reprehensible to send specimens to two places without informing the other. … I am baffled by it all, and obviously do not understand all the facets—therefore the less said the better. I have mentioned my reaction to Sir Macfarlane Burnet and he agrees about it."[93]

[89]Strathern (1971, 221). See also Sahlins (1963).

[90]Gajdusek to Burnet, 13 March 1957, in Farquhar and Gajdusek (1981, 6). Ian Wood was the director of the Clinical Research Unit, a division of the Hall Institute.

[91]I hope that I will later be able to link this analysis of the exchange regimes of kuru science to issues of trust and civility, as raised by Steven Shapin (1994).

[92]Gajdusek to Smadel, n.d. (May 1957?), in Gajdusek (1976, 88). He later wrote to Smadel (10 July 1957) that "we have two further brains on our hands already—one for you and one for Melbourne" in Gajdusek (1976, 145).

[93]Graeme Robertson to J. G. Greenfield, NIH, 31 October 1957, in Gajdusek (1976, 305).

Gajdusek defended himself to Smadel, his principal sponsor. "Although I have attempted to deal directly with all our collaborators," he wrote, "prestige and publicity considerations have brought in numerous 'intermediaries' at many stages. … Yes, Joe, Australian feelings have been hurt by not having everything on kuru studied in their hands."[94] But when Australians recognized his work and visited him, Gajdusek could be generous with his material. Toward the end of his first stay in the highlands, Gajdusek sent two brains off to Smadel but gave another to Sydney Sunderland, the dean of the medical school at Melbourne, who recently had visited him and praised his work.[95] At Bethesda, a gift of brains might increase Gajdusek's rank; at Melbourne it might repair relationships. In the gift economy of global science, mistakes in giving could be costly, but then, one could usually try again to give creditably.

5.6 The Fate of Kuru

In August 1957, while still in the Fore region, Gajdusek despaired of ever finding an adequate scientific explanation or treatment of kuru. "Sorcery," he admitted, "seems as good an explanation for it as any we can offer them."[96] After Gajdusek left New Guinea in November 1957, he continued to think about a possible cause of kuru, and for many years he favored the notion that the Fore were genetically predisposed to react in a peculiar and pathological way to some unidentified substance. Smadel found him a place at the NIH, where he spent the remainder of his career, but through the 1960s Gajdusek still managed to return frequently to New Guinea and visit his Fore friends.

Soon after he left the highlands in 1957, Gajdusek had assembled a travelling exhibition on kuru which displayed the most vivid pathological features of the condition. A veterinary specialist who viewed the exhibition in London noticed that many of the pathological findings in kuru resembled those seen in scrapie, a degenerative disease of sheep which was clearly infectious.[97] Initially Gajdusek was skeptical, but he arranged for Joe Gibbs to begin inoculation experiments in chimpanzees at the Patuxent Wildlife Center, using some fresh kuru brains sent over by Michael Alpers, an Australian who was studying kuru in New Guinea.[98] (The fact that Gajdusek and others could still identify that the inoculants had come from Fore patients named Kigea and Enage indicates the persistence of an aura of previous possession.) In 1965, a few years after their exposure, the chimps began to shake and lose their balance, and when the animals were "sacrificed," the autopsy on their brains found changes indistinguishable from those of kuru.[99] But if kuru was transmissible, what was the agent? How did it spread in natural conditions? And why did it take so long to become clinically obvious?

When Gajdusek returned to the Fore in 1961 he met Robert Glasse and Shirley Glasse (later Lindenbaum), anthropologists who had based themselves at Wanitabe to study kuru sorcery and the recent exacerbation of tensions between Fore men and women. Because

[94] Gajdusek to Smadel, 7 December 1957, in Gajdusek (1976, 336).
[95] Gajdusek to Smadel, 24 December 1957, in Gajdusek (1976, 342).
[96] Gajdusek to Smadel, 6 August 1957, in Gajdusek (1976, 173). Later, on 17 September 1957, Gajdusek wrote to Smadel, "THUS FAR WE CANNOT FIND A SINGLE CLUE," and "I can find no toehold from which to start infectious disease or toxicological study," in the Burnet papers (1957–1963).
[97] Hadlow (1959). See also Hadlow (1992).
[98] Alpers (1992).
[99] Gajdusek, Gibbs, and Alpers (1966). See also Rhodes (1997); Nelson (1996).

the aim of these two researchers was "to consider the effects of both the new sociopolitical order and epidemic disease on the Fore," their fieldwork relied in part on the new medical interpretation of events, making them, in contrast to the Berndts, participants in "a multidisciplinary project."[100] Between 1957 and 1977, more than twenty-five hundred local inhabitants would die from kuru, eighty percent of them from the Fore language group. In the early years, two hundred people, mostly women and children, were dying each year; this annual mortality amounted to more than one per cent of the population. Throughout this period the Fore continued to attribute kuru to sorcery. The local explanation of disease emphasized "malign human agents and disturbed social relations"; sorcery beliefs helped to define group boundaries and consolidate local communities at the same time as they worsened many social tensions.[101] Kuru sorcery seemed mostly directed at women, and the Fore were concerned that before long the women might all be dead, the victims of a few malevolent male sorcerers. Affected communities sought out "dream men" to treat the sorceries of the 1950s and 1960s. Dream men usually came from the border areas between language groups, and used the dreams that followed ingestion of psychotropic plants in order to disclose enemies. Like Gajdusek and the medical orderlies, they were one group among many candidate curers. But while the Fore quickly lost patience with purveyors of biomedical remedies, the dream men acquired considerable wealth and often became big men. "Fore express social relationships through reciprocal exchanges of goods and services," wrote Lindenbaum. "Without reciprocal exchanges, harmonious relationships cannot exist." The Fore had therefore provided Gajdusek and other outsiders "with territory, food and services, and they expected a reciprocal endowment of valuables." But on this occasion they soon had become disillusioned.[102]

Glasse and Lindenbaum proposed that some unidentified agent causing kuru might be passed on by cannibalism, an echo of similar speculations by Ann and J. L. Fischer, anthropologists at Tulane who had read the work of the Berndts. Yet no transmissible agent was identified at the time.[103] However, now that Gajdusek and Gibbs had proven that kuru was an infectious disease, the role of cannibalism had to be reconsidered.[104] As Walter Arens suggests, "the anthropological fixation on cannibalism in the field [had become] more compatible with laboratory experiments."[105] At first, Gajdusek was unhappy with the attempts to associate kuru and cannibalism, as he thought that the disease was already exotic enough. But Alpers and John Mathews independently came to the conclusion that the epidemiological evidence supported the association. Transmission by endocannibalism appeared to explain the age and sex distribution of kuru, its familial distribution, and the fact that the latest gen-

[100] Lindenbaum (1979, viii). Glasse was an American who received his Ph.D. in anthropology from the Australian National University; and Lindenbaum had trained in the Sydney anthropology program.
[101] Lindenbaum (1979, 72).
[102] Lindenbaum (1979, 111).
[103] Fischer and Fischer (1960, 1417–8).
[104] Glasse (1967). See Glasse and Lindenbaum (1992).
[105] Arens (1979, 109). Arens argues that an epidemiological association of kuru with cannibalism is just "a hypothesis based on circumstantial evidence," and the disease may also have been associated with European contact or other changes in lifestyle, p. 112. He quotes Burnet, who in 1971 warned that "it would be unfortunate if too easy acceptance of the cannibalism hypothesis should handicap further inquiry into the pathogenesis of kuru." See Burnet (1971, 5). Interestingly, Berndt had claimed that "people do not normally eat the victims of dysentery or guzugli sorcery," in *Excess and Restraint* (1962b, 270). However, Lindenbaum pointed out that the Fore would not eat those with dysentery or leprosy, but kuru victims "were viewed favourably, the layer of fat on those who died rapidly heightening the resemblance of human flesh to pork," in *Kuru Sorcery* (1979, 20).

eration of Fore children, born since the suppression of the practice, was with few exceptions growing up without succumbing to the disease.[106] Kuru had thus begun to disappear even before the scientific explanation for it had been assembled.

By the late 1960s, the science of kuru seemed more or less settled: its cause was a "slow virus" (to use Gajdusek's term) spread among the Fore if not strictly by endocannibalism, then by handling of the body in funerary rites.[107] For the discovery of the slow virus, a new etiology of human disease, Gajdusek was awarded a Nobel prize in 1976. His model of causation also appeared to explain Creutzfeldt-Jacob disease and, later, bovine spongiform encephalopathy (BSE); and it shaped the direction of research into the cause of AIDS. But Gajdusek's slow virus was chemically just a protein, a "virus" without DNA or RNA. The next generation of scientists preferred to call the agent a prion, a transmissible protein with peculiar stereochemical properties, and it was for this notion that Stanley Prusiner later received a Nobel prize.[108] At the end of the 1980s, the slow virus, for which Gajdusek had won his earlier Nobel prize, was as elusive as kuru.[109] But by then, Gajdusek had used his valuables to become a dream man in medicine, a big man in science.

5.7 Conclusion

In this essay I have tried to demonstrate the tension and confusion—and sometimes hybridization—between forms of appropriation ("cannibalism"), commodification, barter, and more reciprocal forms of exchange in colonial science. I have sought to understand what it meant for Gajdusek, and for Burnet and Smadel, to get their hands on kuru brains. In describing just one example of the complex, and often ambiguous, process of creating value in modern medical science, I also wanted to trace, more generally, the outline of a moral economy of scientific exchange, with its characteristic ways of assigning intellectual credit and recognizing social debt. But there are, of course, other issues raised by this story that I have not considered here. What, for example, was the role of scientific and colonial bureaucracies in regulating these transactional orders? How did middlemen—such as medical orderlies—influence exchange relations? In what way was the gender of the gift—most of the dead were female—significant? Most importantly, what did the Fore really make of all these investigations? On such issues the historical record is still confusing or opaque, or simply unavailable.[110]

It appears that for Gajdusek, in particular, the alienability of kuru brains, whether from the Fore or from himself, became a crucial issue in fieldwork and laboratory practice. It was never certain who rightfully possessed kuru material, or even what it might mean to possess it, or more importantly, how to give it away and still keep it. But if Gajdusek were to earn scientific credit in his exchanges with senior colleagues, if he were to imprint his name on kuru research, it was necessary for him to alienate kuru material from the Fore,

[106] Alpers (1965); JMathews, Glasse, and Lindenbaum (1968).

[107] Gajdusek (1997). Kuru was not transmitted experimentally through the gastrointestinal tract, so Gajdusek suggested that the route of transmission might be skin contact with the contaminated brain. See Gajdusek et al. (1997, 1253).

[108] Prusiner (1982).

[109] Gajdusek estimated that more than twenty-five hundred Fore had died of kuru between 1957 and 1982, by which time it was rare. For a review, see Gajdusek (1990).

[110] I hope to address many of these issues in a larger work on kuru exchanges, but some of the limitations of the historical record may mean that many of these questions are never answered satisfactorily.

to take possession of their body parts, and then to circulate them as gifts within a scientific network. No matter how variable Gajdusek's understanding and representation of his transactions with the Fore, one feature is constant: the need to make kuru material his own, or appear to be his own, even if previously it was out of circulation altogether, even if it, or he, was still tied to the Fore through a gift relationship. Among the Fore, such willful misrecognition of exchange—whether the illicit circulation of valuables or the denial of reciprocity in transactions—would have put the perpetrator in moral peril.[111] But Gajdusek remained a scientist, a member of a different community. Thus, having oversimplified the transactions that had taken place between himself and the Fore, Gajdusek proceeded to rework and exchange "his" kuru goods for recognition in science. Once the material had been carefully demarcated from its "conditions of possibility," Gajdusek was readily identified as the primary author of kuru, and given scientific credit and rewards for his discovery.[112] He was able to insert his valuables into a complex moral economy of science.

The changing economic articulation of scientific research—commonly expressed as a tension between authorship and ownership—will no doubt be more profitably explored only as we begin to piece together the material cultures of a greater range of recent scientific transactions. It would, for example, be helpful to compare kuru transactions with the contemporary, and perhaps more commodified, global traffic of genetic material.[113] Gajdusek's own career offers hints of a new transactional order emerging in science. In the 1990s, he was the director of the Central Nervous System Laboratory of the National Institute of Neurological Disorders and Stroke when it applied for a patent on a cell line from a Hagahai man, and he was named as an inventor in a similar claim on a cell line from a Solomon Islander. In May 1989, Carol Jenkins, a medical anthropologist affiliated with the Papua New Guinea Institute of Medical Research (led by Michael Alpers), had drawn blood from Hagahai men and women infected with a retrovirus known as HTLV-1. The virus was common in the region, but unlike elsewhere, in Papua New Guinea it rarely seemed to cause leukemia. The infected T-lymphocytes were extracted in Goroka and sent to the National Institutes of Health, where scientists suspected that the cell line, infected with the variant virus, might prove useful in diagnostic testing and vaccine development. Given the precedent of kuru, it is perhaps not surprising that no one consulted the Hagahai or the government of Papua New Guinea before applying for a patent on the cells.[114] But the difference in the transactional order of science is remarkable. In the 1950s and 1960s Gajdusek had circulated Fore material within the reward system of science, while in the 1990s he was participating directly in the market commodification of Hagahai cells. Once an author, Gajdusek had become a

[111] Parry (1989). Gajdusek regularly brought children back from his research trips and sent them to school in the United States. By the 1990s, more than fifty children had lived in his house. In 1996, Gajdusek was charged with the abuse of one of them, and he pleaded guilty. Although it may be tempting to seek facile analogies between the collection of children and of body parts, it seems that the exchange relations were quite different in character—as was the appreciation of moral peril.
[112] Biagioli (1999).
[113] On the changing transactional orders of science, see Nelkin (1984); Zuckerman (1988); Haraway (1997, 244–53); Krimsky (1999); Lock (1999).
[114] Jenkins maintained good relations with the Hagahai, and promised them half the royalties from the patent. Patent No. 5,397,696—for the DNA sequence "PNG human T-lymphotrophic virus (PNG-1)—was later dropped after protests from the Papua New Guinea government and activists who alleged "biopiracy." See Lehrman (1996a, 374, 1996b, 500); http://www.cptech.org/ip/rafi.html (July 12, 1999); Lock (1999). The controversy is covered in a special issue of *Cultural Survival Quarterly* (1996). On Carol Jenkins' research see "Medical Anthropology in the Western Schrader Range, Papua New Guinea" (1987).

patent-holder. Scientific objectification of the bodies of indigenous people has occurred for centuries, but generally any collected material has either gone out of circulation (often into museums) or, as in the case of kuru, become part of a scientific exchange regime. Now, however, governments and corporations—the new medical-industrial complex—can designate brains, blood, cells, and DNA as intellectual property, and having thus "immortalized" these body parts, they can trade them as commodities in a global market.Implicit in this essay is a methodological argument. I believe that we need to develop more locally specific models of the scientific exchange of gifts and commodities, to consider further the social life and moral weight of scientific things, and to document the cultural differentiation of scientific artifacts, rather than generalize about the global economy of science. But an emphasis on local knowledge should not be taken to deny the importance of global structures and systems. Instead, it challenges us to try to understand global science as a series of local economic accomplishments.[115] We need multi-sited histories of science which study the bounding of sites of knowledge production, the creation of value within such boundaries, the relations with other local social circumstances, and the traffic of objects and careers both between these sites and in and out of them.[116] Such histories would help us to comprehend the situatedness and mobility of scientists, and to recognize the unstable economy of "scientific" transaction. If we are especially fortunate, these histories will creatively complicate conventional distinctions between center and periphery, modern and traditional, dominant and subordinate, civilized and primitive, global and local.[117]

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[115] See also MacLeod (1987) and Chambers (1987).

[116] Marcus (1995). Multisited history is not the old comparative history, which tended to produce more systemic (and less interactive) comparisons. I am suggesting a series of microhistories connected by the passage of objects and careers. See Levi (1991, 95).

[117] See Nader (1996).

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Chapter 6
Knowledge in Transit

James A. Secord

What big questions and large-scale narratives give coherence to the history of science? From the late 1970s onward, the field has been transformed through a stress on practice and fresh perspectives from gender studies, the sociology of knowledge, and work on a greatly expanded range of practitioners and cultures. Yet these developments, although long overdue and clearly beneficial, have been accompanied by fragmentation and loss of direction. This essay suggests that the narrative frameworks used by historians of science need to come to terms with diversity by understanding science as a form of communication. The centrality of processes of movement, translation, and transmission is already emerging in studies of topics ranging from ethnographic encounters to the history of reading. Not only does this approach offer opportunities for crossing boundaries of nation, period, and discipline that are all too easily taken for granted; it also has the potential for creating a more effective dialogue with other historians and the wider public.

Halifax is just about the perfect place to hold this meeting. Not because of its eighteenth-century reputation as the wickedest town in North America, nor even because of the warm hospitality extended by our hosts, but, rather, because the city epitomizes so well the conference theme. As the site of several universities and a locus for exchange between different continents and traditions, Halifax is all about "circulating knowledge." The earliest evidence of migration to this region dates back over ten thousand years, and the Vikings probably visited about a thousand years ago. The area was explored by Europeans from the early seventeenth century, and the city itself was founded in the mid-eighteenth century. The first regular steam crossing of the Atlantic was inaugurated from Halifax in 1837, so that all news between Paris, London, and New York came through the town. Although the early Atlantic telegraphs went through Newfoundland, in 1925 Halifax became the anchor for all transatlantic messages. Today it is possible to travel easily from Europe to Halifax, precisely because the town remains a center for transportation and communication.

Conference themes, especially when the meeting is international and the participants are many, have a notoriously capacious quality, so that almost anyone can give a paper on almost any topic. "Circulating Knowledge" would seem at first glance to fit this bill. Historians of science may not agree about much, but I think they would all claim to share a concern with knowledge, and everyone recognizes in some way or other that it is not the sole property of individuals—that it "circulates." Indeed, every academic conference ever held could well be said to exemplify the theme of knowledge in circulation.

In this case, however, the theme does much more. It highlights an issue of real analytical significance—in fact, the central question for our field. How and why does knowledge circulate? How does it cease to be the exclusive property of a single individual or group and become part of the taken-for-granted understanding of much wider groups of people? In important respects, this involves issues of the social nature of knowledge, taking seriously the consequences of philosophical perspectives that are widely accepted by historians of science. In other ways, it is part of the history of education, ranging from the scribal schools of the ancient Near East to the modern university. In still others, it is a question of the passing on of tacit skill, as Harry Collins has stressed in his studies of attempts to transfer knowledge of how to make gravity-wave detectors. For students of literature and science, understanding the circulation of a variety of forms of writing has been a central concern. As Gillian Beer has said, this may involve conflict and transformation as much as mutual understanding and reconciliation.[1] In one of its most familiar aspects, the theme of circulation is represented by the history of book production and reading. In still others, it is part of the interaction between different cultures, as the blossoming of work on imperial encounters suggests.

That the spread of knowledge, its global ubiquity and circulation, should be a problem for the history of science is a great irony. For the positivist founders of the discipline assumed that this was the one issue they had cracked. Scientific knowledge spread because it was true; any failure of diffusion could be explained by resistance due to false beliefs and irrational commitments. Although this view would now have few supporters, historians have yet to take on board the full consequences of abandoning it. We have come to realize the centrality of knowledge in circulation—of science as a form of communication—only gradually and from diverse perspectives.

At the same time, the majority of the general frameworks and big pictures for history of science are worn-out inheritances from the origins of our discipline. The most persistent of these has been the concept of the Scientific Revolution. Criticized for positing a one-time shift toward modernity, it looked ripe for replacement a decade ago; but if anything it has expanded in significance, as areas as diverse as natural history and alchemy have been placed under its umbrella. The problems posed by the continued use of the concept of the Scientific Revolution, both for understanding the period in its own terms and for dealing with the surrounding centuries, have neither gone away nor been neutralized. It has, to an unfortunate degree, defined the task of charting long-term changes in knowledge as one of pinpointing comparable epistemological breaks, most notably the Chemical Revolution of the late eighteenth century and the so-called Second Scientific Revolution of the early nineteenth. Most of the narratives used to tell the stories of specific disciplines are looking even more threadbare, as their origins in partisan accounts written by victors in scientific debates are revealed. The Darwinian Revolution is a 1950s by-product of the modern evolutionary synthesis, given new life by the rise of sociobiology; the notion of "classical physics" and an Einsteinian revolution overturning it marks the triumph of relativity theory in the 1920s; the discovery of the DNA structure in 1953 is hailed as the founding moment for molecular biology, but only because of events in the 1960s.[2]

More seriously, revisionist accounts by historians of science tend to rely for their power on the continued dominance of older frameworks. We offer critiques rather than explanations or competing alternatives. My students sometimes complain that they have to learn everything twice: once to understand the old story, then again to learn why it is wrong.

[1] Collins (2004) and Beer (1996).

[2] On the problems of using the Scientific Revolution as an organizing concept see Jardine (1991). For the complications noted in the cases of the other so-called revolutions see Oldroyd (1984); Staley, "The Co-creation of Classical and Modern Physics," paper delivered at the BSHS/CSHPS/HSS meeting (2004) and de Chadarevian (2002).

We study "popular" science, "subaltern" or "indigenous" knowledge; but to varying degrees these categories are too easily framed through a contrast with an assumed story about elite Western knowledge.

There is a strongly felt need for replacements; and if these are to gain any currency outside specialist circles, they need to be simple and clear. Those of us who teach need them for our courses; those of us who write need them to combat what might be called the "Sobelization" of the history of science, with the subject being carved up into heroic sound-bites modeled on Dava Sobel's phenomenally successful *Longitude: The True Story of a Lone Genius Who Solved the Greatest Scientific Problem of His Time* (1995). We need unifying narratives and a sense of large connections, even if they are not the old-style Cinemascope stories offered in the confident days of the Cold War.[3]

Toward that end, I want to do three things in this talk. First, I will suggest that although an understanding of practices of communication, movement, and translation is becoming central to specific aspects of the way that the history of science is conceptualized, to go further we should think much more consistently about the problem, from the kinds of analytical resources we apply to the specific kinds of narratives we write. Second, I will argue that too much of our current work (my own included) has remained limited by unconceptualized geographical and disciplinary boundaries: we choose, as graduate students, to study eighteenth-century French natural philosophy or twentieth-century American physics, without knowing enough about what goes on around and just beyond the limits. And third, I will suggest that sustained attention to knowledge as communication can draw together a variety of approaches, while encompassing an understanding of the specialized, esoteric aspects of science that must remain central to what we do.

6.1 Knowledge as Practice

As will be evident to anyone who has looked over publishers' catalogues in recent years, historians of science have developed superb techniques for placing science in local settings of time and place. A standard model for historicizing science is to locate specific pieces of work in as tight a context as possible, binding them ineluctably to the conditions of their production. As it has usually been read, the classic work in this tradition is Steven Shapin and Simon Schaffer's *Leviathan and the Air-Pump* (1985),the most influential text in our field since Thomas Kuhn's *Structure of Scientific Revolutions* (1962).[4] In demonstrating the mutual relevance of disputes about the power of kings and the composition of cement, *Leviathan and the Air-Pump* revealed the founding moment of the new experimental philosophy as an outcome of specific circumstances in Restoration England, challenging those who assumed the value-free transcendence of science. My point is not that all historians subsequently adopted a local approach, nor even that the concerns of *Leviathan* necessarily tended in this direction. It is, rather, that such work became the focus of the key debates.

That an approach based on the close study of knowledge in context should have come to dominate controversies in our field is not surprising. The same thing was happening in the humanities much more generally, and especially in the history departments where most historians of science (at least in North America) actually teach. Microhistories modeled on

[3]On these issues see the essays in "The Big Picture," a special issue of the *British Journal for the History of Science* (1993). David Philip Miller discusses "The Sobel Effect" in Miller (2002); the work itself is Sobel (1995).

[4]Shapin and Schaffer (1985) and Kuhn (1962).

Clifford Geertz's anthropological notion of thick description became the foundation for the new cultural history. What Lawrence Stone called the "revival of narrative" has had particular appeal for historians of science, for it offers a way of bringing the transcendent power of science down to earth, locating it in specific times and places.[5] Moreover, these trends in general historical debate joined up with those within the developing field of sociology of knowledge, especially as practiced in the Edinburgh Science Studies Unit and then by Harry Collins and his students at Bath. Here specific passages of scientific activity provided "case studies" in the social character of the making of knowledge. The methods demanded an attention to circumstance and situation broadly compatible with what was being pursued in history. There were of course differences, including those of scale: at least some of the sociologically informed work tended to assume that the immediately relevant context was within the boundaries of relatively well-defined communities of practitioners—as in Trevor Pinch's *Confronting Nature: The Sociology of Solar-Neutrino Detection* (1986) and Andrew Pickering's *Constructing Quarks* (1984). Other works, notably Leviathan and the Air-Pump itself, looked to wider debates in politics and religion.[6]

As became clear in the mid 1980s, these approaches were converging toward a view that considered science as a practical activity, located in the routines of everyday life. Knowledge itself came to be seen as a form of practice. It is in this respect that the wider shifts associated with feminism and gender studies had their greatest (though often unacknowledged) shaping effect on the field. Science was, as Donna Haraway memorably put it, "situated knowledge."[7] The move to study practice has, in my view, been the single most significant transformation in our field during the past twenty years. It breached old boundaries between "internal" and "external" and opened up a view of science as a process, including inquiries into experiment, fieldwork, and theory making. Most fundamentally, it broke down old distinctions between words and things, between texts, books, instruments, and images.

It was in 1988, in the immediate wake of these upheavals, that the first of the joint British/North American gatherings was held in Manchester. For those old enough to have been there and young enough to remember it, that was a great meeting, offering for many whose framework had been defined by the intellectual parameters of the1970s Cambridge History of Science Series a huge sense of liberation and possibility. The meeting was notable on many accounts. For those studying the early modern period, it brought together a range of historians on both sides of the Atlantic who were interested in craft knowledge and the role of women. It revealed the range of work being carried out on twentieth- century science, especially in relation to military technology and popular audiences. And in some ways it was the high-water mark of integration between the sociology of knowledge and the history of science. Almost all the main historically inclined sociologists were there. And the meeting was also notable for a wonderful closing dinner in one of Manchester's best Chinese restaurants.[8]

Things have moved on in the past sixteen years, but I think it is fair to say that the core analytical issues that were under discussion at the Manchester meeting have remained central

[5]Geertz (1973) and Stone (1979).

[6]Pinch (1986); Pickering (1984). These controversies are apparent in Biagioli (1999), and esp. in Pickering (1992).

[7]Haraway (1988).

[8]The program is recorded in *Brit. J. Hist. Sci.* (1989, 502–512). Roy Porter (1990) discusses the contemporary state of the field.

to the field just about up to the present time. We can now see this exemplified (as indeed Kuhn himself would have predicted) in textbooks and works accessible to nonspecialists: the Science*Culture series edited by Steven Shapin for the University of Chicago Press, the innovative surveys published by Icon Books, and the amazingly useful (if frustratingly slow to appear) eight-volume *Cambridge History of Science* (2003–). The underlying questions involved are well summed up in Jan Golinski's *Making Natural Knowledge* (1998), which introduces historians to social theory and certain varieties of philosophy.[9]

I am not a big fan of labels, so the designations currently used for work being carried out in this tradition are worth examining. One, usually discussed in connecting history to sociology of knowledge, is "constructivist." This, in my view, tends to raise hackles unnecessarily and (after a certain point) tends not to do any work. As Margaret Jacob has said, "To speak about the social construction of science should be just another way of saying that people make science."[10]Probably the most widespread designations of the approach are "contextualist" and "cultural." These words, however, are now being so variously used that they scarcely have any meaning. As is suggested by the contents of the journal *Science in Context*, "context" can refer to anything from specific philosophical resources used in science to accounts of science in war and economic development. There is much to be said for this diversity, but there is not a good case for identifying it under a single rubric. Again, "contextual" starts to mean nothing much more than "historical." Many anthropologists would disown the term "culture" entirely. It has been important in history of science not so much for its analytical power, but as an identifying marker of an approach. From this perspective, if "science in context" is vague and implies unwanted distinctions between foreground and background, "science as culture" offers enticing possibilities of organic unity and integration. Science, understood through cultural history, can be seen as part of a distinct world of symbols, whose meaning is determined by a network of relations with other symbols. The danger, of course, is that such cultural systems are then seen as consistent, integrated, clearly bounded, and resistant to change. Moreover, the relation of cultural analysis to more traditional forms of social and economic history, with their emphasis on issues of access and power, can too easily be obscured. I suspect that the utility of "contextual" and "cultural" is pretty much exhausted, not only in history of science but in the humanities more generally.

If labels are useful in identifying emerging schools, they can also encourage new approaches to harden into orthodoxies. In this regard, the diversity and empirical grounding of most historical work has been a saving grace, especially compared with literary and cultural studies. But there are difficulties everyone has had to grapple with in practicing, reading, or challenging this form of history. One is the tendency to see the localizing of a piece of scientific work as a worthwhile end in itself. The difficulties of dealing with science as an object of inquiry have required attention to epistemological and ontological issues—a necessary ground-clearing that has been easy to mistake for actual history. The process of situating knowledge ends up as a conclusion rather than a method: the same implicit epistemological lesson, that knowledge is ineluctably local and variable, is hammered home again and again.[11] A second danger is that an emphasis on the local contexts of science can lead to parochial antiquarianism. We think we are making grand epistemological conquests, when in fact we are studying a few practitioners of a relatively esoteric activity, whose wider im-

[9] Lindberg and Numbers (2003–) and Golinski (1998).
[10] Jacob (1999, 115).
[11] Kohler (1999).

portance is assumed rather than demonstrated. The best work in our field is valued for its methodological sophistication and exploration of fresh topics, but it is often seen as being exceedingly narrow.

The final danger is that in focusing on locating the core aspects of scientific practices within broader situations, we may be depending too much on the willingness of other historians to take account of our work in general surveys. It certainly would be nice to think that we are showing how textbooks might include the history of science, beyond the ritual nod to Copernicus, Newton, and Darwin. Accounts of courtly patronage could include Galileo's telescope; histories of commercial culture in colonial New England could discuss the ways in which Benjamin Franklin's theory of electricity emerges from double-entry bookkeeping. Histories of postwar Britain could show how the rise of molecular biology depended on computers and other technologies whose development depended on the war. But in my experience, this kind of integration is happening primarily in those cases (and there are several distinguished ones) in which historians of science are involved as the coauthors of textbooks.[12] Moreover, the assimilation of history of science into general history, although highly desirable from many perspectives, is potentially at odds with an aim of creating big pictures focused on science itself.

So the field remains fragmented. The problems are, paradoxically, a by-product of the extraordinary success we have had in placing science in context, however that is defined. The more local and specific knowledge becomes, the harder it is to see how it travels. We have gained a breadth of connections and relations, but these are limited by the boundaries of a specific ethnographic field. The significance of this issue was, in fact, predicted well over a decade ago, in the justly celebrated paper in *Science in Context* by Adi Ophir and Steven Shapin entitled "The Place of Knowledge." In announcing a program that involved situating knowledge, they identified what they termed the "successor project" it generated:

> How is it, if knowledge is indeed local, that certain forms of it appear global in domain of application? Is the global—or even the widely distributed—character of, for example, much scientific and mathematical knowledge an illusion? If it is the case that some knowledge spreads from one context to many, how is that spread achieved, and what is the cause of its movement? Is its distribution a strong indication of its correspondence with reality, or is it properly read as reflecting the success of certain cultures in creating and spreading the very means and contexts of application? … Perhaps the days in which ideas floated free in the air are truly nearing an end. Perhaps, indeed, what we believed to be a heavenly place for knowledge we will come to see as the result of lateral movements between mundane places.[13]

Tellingly, this paragraph was the last in their paper —it raised the question but was not the heart of their argument. And it is telling that the main (and mostly beneficial) effect of Ophir and Shapin's intervention has been to spawn studies of science in a huge variety of places, from clubs and pubs to lecture halls and laboratories and playing fields. It has highlighted the significance of scientific architecture, encouraged studies of domestic spaces, and given

[12]Notable examples include Marvin Perry, Myrna Chase, James R. Jacob, Margaret C. Jacob, and Theodore H. Von Laue (2004) and Pauline Maier, Merritt Roe Smith, Alexander Keyssar, and Daniel J. Kevles (2002).
[13]Ophir and Shapin (1991, 16).

new life to studies of science in the city and the field.[14] It has, also, however, tended to legitimate the move toward local specificity—a trend that is seriously at odds with wider trends toward global and comparative history. The result is that we end up with a rich array of research that somehow adds up to less than the sum of its parts.

6.2 Literary Replication

I cannot promise you historiographical salvation, and even if it exists there is certainly more than one way to it. But I am sure that we need to think much more explicitly about the problem of the movement of local knowledge. Fortunately, as this conference shows, this involves not a new approach but developing a more explicit sense of some important current trends within the field.

There are lots of ways of tackling this issue, but we need first to recognize that the issues are fundamental, involving the need to rethink the way in which the program of the cultural history of science was originally set out. That agenda has, as will be clear from what I've said already, encouraged a view in which science is created locally but then, by other processes, is transferred outward toward more general contexts.[15] To escape this, we need to shift our focus and think about knowledge-making itself as a form of communicative action. There are good precedents for taking such a view. Many of the philosophical issues most debated by historians of science in recent years give interaction between agents a central role in epistemology. Questions of trust, testimony, and communitarian objectivity are simultaneously questions of how knowledge travels, to whom it is available, and how agreement is achieved. "As a shared form of knowledge," Scott Montgomery argues, "scientific understanding is inseparable from the written and spoken word. … Communicating is the doing of science."[16]

To do real historical work, this perspective needs to be not only explicit but also foundational. This means thinking always about every text, image, action, and object as the trace of an act of communication, with receivers, producers, and modes and conventions of transmission. It means eradicating the distinction between the making and the communicating of knowledge. It means thinking about statements as vectors with a direction and a medium and the possibility of response. The most important task is to make our understanding of science as a form of communication—which is a commonplace in the theoretical literature—really work within the narratives we write. This sounds simple, and in many ways historians of science have devoted a huge amount of attention to identifying the audiences for science and the rhetorical strategies used to reach them. Yet we still regularly write as though people read authors rather than books. We speak of reading Einstein, when what we really mean is reading an article of 1905 in the *Annalen der Physik* on the electrodynamics of moving bodies. We speak of the reception of Descartes, or (worse still) of an essence called "Cartesianism," when what we mean is the debates that took place after the publication of a series

[14] Much of this work is summarized in Livingstone (2004).

[15] Since the days of the "strong programme" in the 1970s, the transmission of knowledge has always had a place in studies of science, but it has often been a secondary one. Thus, when Barry Barnes first introduced English-speaking historians of science to the work of the German social theorist Jürgen Habermas, it was through interest theory rather than the ideas of communicative action that were actually more central to his thought.

[16] Montgomery (2002, 1). See the special issue on testimony in *Studies in History and Philosophy of Science* by Kusch and Lipton (2002a), see esp. (2002b) with bibliography. The two most influential historical works have been Shapin (1994) and Daston and Galison (1992).

of printed books. We write, moreover, as though the author speaks to us directly ("Einstein says," "Descartes says"), when we know perfectly well that what we are actually reading is a narrative voice aimed at a particular horizon of expectations.

These points have been a commonplace of critical theory in the humanities for decades, and there is much to be learned from Hans Robert Jauss, Wolfgang Iser, and other exponents of reader-response theory. However, we need to use this approach much more consistently than is usually done in literary and philosophical studies, which have tended to develop theory rather than explore its application. The issues are especially vital when scientific works are being examined, for these more than any others gain their power through a claim to objective transparency, so that authors appear to speak directly for nature. We cannot get to the core of the problem without reading our most traditional sources—words and images—a lot more closely than we usually do. As Jonathan Topham stressed recently in *Isis*, the study of practices related to printed works has lagged far behind those dealing with experiment and fieldwork. As historians, we are in a good position to combine careful readings of texts, images, and objects with the evidence, often fascinating and diverse, of actual readers.[17] Thus although many historians of science have referred to the brilliant discussion of techniques of literary persuasion in *Leviathan and the Air-Pump*, fewer have followed the authors further in this direction or explored the extensive literature on prose rhetoric and genre.

The point I am making is a semantic one, but not merely so, for its consequences involve profound assumptions about the politics of knowledge. Traditionally the consequences of eliding author, narrator, text, work, and readers have been avoided by analyzing situations in which the distance between these elements is relatively limited and subject to convention. We have tended to assume that the works we study are universally available to all relevant readers and that all those who read them have access to knowledge of the author's person. But this is also to assume a highly specific model of the community of practitioners, in which practices travel relatively freely and modes of communication are relatively transparent. Now, it has been recognized for a long time that this is rarely the case: every act of communication excludes as well as includes. Yet the approach most historians of science take to transmission has tended to be piecemeal, after the real work of explanation is done.

Part of the issue involves recognizing that history of science, even more than most historical fields, has focused on origins and producers. Even when we are not explicitly studying discovery and innovation, we are obsessed with novelty and the places in which novelty begins. The further we move away from sites of the production of new knowledge, the vaguer our descriptive categories tend to become. Rather than saying that an idea was "popular," a "best seller," or a "sensation," we need to analyze audiences and readerships closely and carefully, with the same awareness of cultural nuance we might bring to an account of life in the laboratory. Otherwise we are simply reproducing the notion that science passes from highly individualized sites of production to an undifferentiated mass public.

Take the literature on Michael Faraday, which exhibits all the features of current best practice in the field. We have wonderful discussions of the way in which Faraday developed his experiments for presentation on the stage of the lecture room. The significance of his work in relation to the politics of the Royal Institution and the role of his lecture demonstrations in establishing his career have been brilliantly studied.[18] But we know less about his auditors (other than that they were genteel) and their reasons for attending. What made

[17] Jauss (1982); Iser (1978); Topham (2004).
[18] Morus (1998) and Gooding and James (1985).

chemistry and natural philosophy fashionable? How and why did certain newspaper and periodical editors report the lectures, and which ones did not? In relation to Faraday's audience among fellow practitioners, much has been written on how he made his experimental arrangements convincing, but less on how he addressed his readers and the role of publishing in journals such as the *Philosophical Transactions* and the *Philosophical Magazine*. There is no discussion, in relation to Faraday's work, of where and how such periodicals could be read, how many copies were printed, and how they were made available in other countries.[19] By default, such publications become universal multipliers: they take us from Faraday's immediate context to an international knowledge of what he was doing. In consequence, we have only a rather vague idea of how Faraday's unparalleled reputation actually developed over time. Until more of the perspectives that have broadened our understanding of Faraday's experimental practice area applied to his immersion in the world of print—through communicative actions largely carried out by others—we unwittingly enhance his heroic status. Readers are led to picture a Faraday who was supremely good not only at things that might justify his title of genius (such as experimental skill and conceptual innovation) but also at things he was unable to do—such as making his name known to everyone in the land or traveling instantaneously between continents. Moreover, our stress on origins and producers has led to inadequate attention to the structures of time itself in the stories we tell. François Furet has argued that all narrative history is a succession of origin events, as any narrative is dominated by its end and beginning. In telling stories, we are inevitably drawn toward a teleology. I am far from thinking that we should cease to write narratives, but I would suggest that we need to stop using time unreflectively. What is needed is not less attention to time, but more: a history in which notions of time are not taken for granted. As the American historian Thomas Bender has said, "A history liberated from origins would … historicize the axis of time itself, emphasizing structure, transformation, and relations."[20]

One of the few works in history of science to discuss such issues is Martin Rudwick's *Great Devonian Controversy* (1985), which explicitly attends to the relation between different scales of time involved in a scientific controversy. In that case, the highly publicized meetings of the British Association for the Advancement of Science offered very different opportunities for debate than those presented by the fortnightly discussions at the Geological Society of London. The temporal sequencing of communication has also been prominent in the essays produced from the SciPer (Science in the Nineteenth-Century Periodical) project at the universities of Leeds and Sheffield. These bring out important questions about the periodicity of knowledge as presented to readers in dailies, weeklies, monthlies, quarterlies, and annuals. Serial reading offered ways of creating and reinforcing individual identity, religious faith, and social cohesion.[21]

So the first of my suggestions would be to think, at every point in our work, about science as a form of communicative action—to recognize that questions of "what" is being said can be answered only through a simultaneous understanding of "how," "where," "when,"

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[19] An exception is a short article on the *Athenaeum* and the *Literary Gazette*: see James (2004). There are a number of studies that focus on the posthumous reputations of scientists such as Faraday, notably Cantor (1996). It is telling, however, that none of the literature I have read on Faraday's science cites Brock and Meadows (1984), the standard history of the firm that published and printed almost everything he wrote. On the market for Faraday's work the main source remains Berman (1978), though this can be updated by several of the essays in James (2002).
[20] Furet (1984, 69) and Bender (2002, 8).
[21] Rudwick (1985). For publications from the SciPer project see Henson et al. (2004); Cantor and Shuttleworth (2004); Cantor et al. (2004).

and "for whom." The "successor problem" identified by Ophir and Shapin needs to be part of the original formulation of what historians think they ought to be doing. It is not so much a question of seeing how knowledge transcends the local circumstances of its production but instead of seeing how every local situation has within it connections with and possibilities for interaction with other settings. If the slogan for much history of science in the past twenty years was "science in context," we could do a lot worse than to think now about "knowledge in transit."

6.3 Conventions of Circulation

At the time of the Manchester meeting, the one author whose works were really putting issues of the movement of knowledge on the agenda was Bruno Latour. His writings, especially on Louis Pasteur, proved exceptionally helpful in taking studies of scientific practice beyond the microsocial, embedding science in networks of translation and appropriation. In the end, however, Latour's conclusions have proved too ahistorical and too concerned with unstable hybrids to offer the practical resources historians need for interpreting the past. Concepts such as "centers of calculation," "immutable mobiles," and "obligatory passage points" proved to be better suited to thinking through the relation of single centers to a periphery (Pasteur's lab and French farmers) than for elucidating competing or multiple ones (Pasteur's lab and that of the German bacteriologist Robert Koch). Most fundamentally, historians of science have resisted Latour's call to give equal agency to nonhumans and humans. Giving agency to microbes and doors would seem to require recourse to the latest findings of biological and physical science, a move that goes against the most basic precept of the field as it has developed during the previous twenty years: the principle of symmetry in treating evenhandedly scientific findings that have proved true and those that have not.[22] Even so, Latour's writings have been of signal importance in stressing the need to examine knowledge as an activity occurring in time and space. Historians have adopted his emphasis on process, reception, and audiences; and they have done so in a way that has recognized the relative stability of many of the networks that Latour tended to believe were infinitely flexible. Not least, this has made the networks amenable to historical analysis.

Latour's work is thus only the most radical of several attempts to refocus the study of science around practices of entanglement, translation, and border crossing. Here we can identify part of the reason why the investigation of what Peter Galison has called "trading zones" has been so fruitful and why James R. Griesemer and Susan Leigh Star's work on boundary objects has been so widely cited.[23] Concentrating on sites of exchange is not enough, however, for these are often on the margins and involve practices developed for dealing with relative outsiders. It is in those fields where the study of contact zones has been combined with an understanding of relatively stable patterns of practice that we have begun to develop some of the most effective new big pictures. In this way, we are beginning to understand the generic regularities involved in the circulation of knowledge —and how these change according to time and circumstance.

The key to creating this history is our new understanding of scientific knowledge as practice. All evidence from the past is in the form of material things. This is (or, rather,

[22] Schaffer (1991) and Bloor (1999). Among Latour's influential works, see (1987, 1999b, 1999a).
[23] Galison (1997, 803–844) and Star and Griesemer (1989).

has become) obvious in the case of experimental instruments, natural history specimens, and three-dimensional models.[24] But it is equally true of pamphlets, drawings, journal articles, notebooks, diagrams, paintings, and engravings. Whether they study Newton's graffiti on Grantham schoolhouse or tape recordings of the discovery of pulsars, historians are inevitably chroniclers of the material world.[25] Robert Westman put it perfectly in his talk at this conference: "books and letters, not 'isms,' passed hands." It is in tracing the patterns of circulation of these "things-in-motion," as the anthropologist Arjun Appadurai has called them, that we can create a history that goes beyond particular instances. And because practices are often persistent and relatively stable, we are thus in a position to trace not just individual objects, but larger classes and genres of things. The new orientation thus offers the potential—as yet only partially realized—for histories that span long periods of time and different countries. It is a view that has already gone much further in transforming the histories of medicine and technology, cognate fields in which the material world has been harder to ignore.[26]

There are many resources to draw on for developing this approach. One of the best-established traditions of work is in art history, which since Michael Baxandall's *Painting and Experience in Fifteenth Century Italy* (1972) has dealt extensively with the transmission of material practices in both the making and the viewing of paintings. Pamela H. Smith's *The Body of the Artisan* (2004) brings these perspectives to bear in demonstrating the role of practical men in the transformation of knowledge in the sixteenth and seventeenth centuries. We can also see the significance of skill, training, and apprenticeship in Myles Jackson's work on Joseph von Fraunhofer and precision optics in Bavaria and England. Apprenticeship of a different kind is explored in Andrew Warwick's *Masters of Theory* (2003), which shows how mathematical physics in nineteenth-century Cambridge was transformed by coaching and examination. Transmission, innovation, and skill are bound together in pedagogy. As Warwick points out, education has received considerable attention from historians of science, but its potential for redrawing the larger contours of an understanding of knowledge as practice remains surprisingly underdeveloped.[27]

A related focus on material forms of knowledge transfer is available in work on the history of print and the sociology of the mass media. Roger Silverstone, Ien Ang, and other students of modern media consumption have been exceptionally helpful in opening up new questions about this field; some of the most suggestive studies of audiences, from which historians have much to learn, are based on empirical studies of television watching as an example of domestic technology in use.[28] Janice Radway's *Reading the Romance* (1984), an in-depth empirical study of a group of women romance readers in the midwestern United States, offers many insights into how to study a particular literary genre in relation to its readers. The greatest impact of studies of print has been in late medieval and early modern histories, notably the work of Ann Blair, Anthony Grafton, Adrian Johns, Nancy Siraisi,

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[24] For examples see Gooding, Pinch, and Schaffer (1989); Jardine, Spary, and Secord (1996); de Chadarevian and Hopwood (2004).

[25] For examples see the essays in Gumbrecht and Pfeiffer (1994) and in Daston (2000).

[26] Robert Westman, "Circulating Theoretical Knowledge: Kepler and Galileo in the Years of Public Silence," paper delivered at the BSHS/CSHPS/HSS meeting (2004); Appadurai (1986, 5). For a stimulating example of how models from medical history can be used to create a "big picture" account of science see Pickstone (2000).

[27] Baxandall (1972); Smith (2004); Jackson (2000); Warwick (2003).

[28] Marris and Thornton (1999) offers a comprehensive anthology; Schiller (1996) is a helpful introduction to the main debates.

and others.[29] But related areas continue to receive less attention, especially the history of scientific periodicals, journalism, and book production after about 1850.

Why has it taken so long for the histories of education and communication—among the most promising avenues for developing a history of knowledge practices—to gain a significant position in academic debate? It a curious relic of disciplinary hierarchies that important aspects of these fields were for many years kept apart from the rest of the humanities. The history of primary and secondary education was usually taught in specialist teachers' colleges; the history of all but the most elite forms of publication was limited to journalism schools. Book history meant bibliography, which was taught primarily to librarians. These were vocational subjects, related to professional training, and although the work done was often of high quality, it remained low in status compared to scientific sociology, the history of ideas, and abstract philosophy. In art history, too, study of the material qualities of paintings was, for many years, seen as subsidiary to (and largely separate from) analyses of iconography and attribution. This situation is now changing, but it has taken an immense effort (and much administrative reorganization) to acknowledge the significance of these subjects. Even museums, which from the start played a founding institutional role, have only in the past two decades achieved a central position in defining the intellectual agenda.

Studies from these new directions, dealing with paper, parchment, ink, brass, steel, rubber, and glass, are grounded in the material world, and as such they are deeply rooted in ecological history. This relation has been brought out most explicitly in Robert Kohler's work, which examines the boundary between the laboratory and the field using tools modeled on those developed in William Cronon's *Nature's Metropolis* (1991) for understanding the relation between the city and the country.[30] As the writings of ecological historians show, attaining a global picture is not a question of transcending or erasing local practices but of giving more attention to practices of circulation on a wide variety of scales. Writing a global history of knowledge primarily as doctrine and ideology is probably impossible; writing a history of knowledge as circulating practices would not be easy, but at least it is possible to see how it might be done.

An approach grounded in communication opens up the possibility of integrating accounts of technical, specialist aspects of science with their wider uses. When Claude Bernard jotted down in his notebook his results on the physiological effects of curare, he was bridging what he was doing in the lab and what he would eventually report to the Académie des Sciences. The particulars of this bridging practice were so much taken for granted that they are unlikely to be made explicit; rather, they need to be teased out from the practical ways that specific passages from the notebooks were recycled for later use in publications. Even pencil jottings made in the laboratory were targeted toward potential audiences, and notebooks have conventions and a history of their own within a cycle of communication.[31] It is often thought, for example, that the history of the scientific book involves looking at publishers, binders, readers—anything but the actual words on the pages being produced. But this is simply not the case, or at least certainly should not be. Everyone knows Marshall

[29] Radway (1984); Blair (1997); Grafton (2001); Johns (1998). Also helpful are Frasca-Spada and Jardine (2000) and the discussion between Adrian Johns and Elisabeth Eisenstein in "How Revolutionary Was the Print Revolution?" (2002).
[30] Kohler (2002). See Cronon (1991) and also (1995).
[31] Grmek (1973); Holmes (1974, 1987).

McLuhan's famous slogan, "the medium is the message"; but it is simultaneously true that messages are the medium: they are what defines a communications technology.

So we need accounts of the generic development of the field notebook, the experimental register, the museum catalogue, and other documents of practice, as bridging studies moving between specific passages of technical work and their wider settings. It is amazing that we lack a good general history of the protocols and procedures for announcing a discovery in different periods. We know a lot about theories, but not nearly so much about theorizing as an act of communication. We have only a limited number of studies of scientific letter writing, note taking, habits of journal reading, technical drawing, close observation, lecture attendance, and lab talk. We have paid little attention to local attitudes toward different forums for publication and to specific practices for producing words and images in relation to education, textbooks, and translations. There are only a handful of accounts of the conventions of natural philosophical travel, scientific museum going, and the experience of attending conferences. Recent works, however, have begun to take border crossing as their main subject. Jean-Paul Gaudillière has shown how the travels of French biologists to the United States shaped the development of biomedicine in postwar France. "Rather than being simple transfers," he writes, "the transatlantic exchanges nurtured processes of adaptation, tinkering, and mobilization of outside resources for local purposes."[32]

As such studies suggest, the aim is not just to append accounts of some new aspects of science to our existing analyses. Part of the difficulty has been in thinking of communication as something that is involved in all aspects of science, not something that occurs only when scientists write for publication. Many historians of science will know Robert Darnton's communication circuit, which shows how a work passes through a cycle of production from author, to printers and publishers, to readers, and back to the author. However, this model, at least in its main outlines, is too focused on the production of printed materials to have had much impact outside the history of the book. Readers—surely of the greatest significance to most historians—play a role in the circuit primarily in terms of their feedback to the authors and the subsequent publication process. Unless carefully used, the communication circuit tends to produce accounts in which histories of publishers, printers, broadcasters, and so forth are inserted into an already-known story.[33] Adding a brief account of the publishing firm of Macmillan to a study of a laboratory group that regularly publishes in *Nature* is unlikely to be illuminating. What we need to know more about are patterns of circulation and use in the appropriate local settings.

Concentrating on conventions of circulation is especially important if we are not to end up just adding further particulars to a story already heavy with detail. It may seem challenging enough to explicate the contents of a particular piece of scientific writing or to characterize a passage of experimental activity—without the additional burden of explaining whom it was for, by what means it was communicated, and how it was received. To undertake a close investigation of the context of every statement would be insufferable. Writing a history based around changing practices for knowledge, however, is much more feasible. For all their faults and inconsistencies, we could do well to look to Raymond Williams's *The Long Revolution* (1961) and *Culture and Society* (1958), which examined the creation of literature in England in terms of changes in the audience and mechanisms of authorship,

[32] Gaudillière (2002, 413).

[33] This point is briefly developed in Secord (2000, 126). In his recent writings, Darnton's model is considerably more complex, though in practical terms it has remained centered in the world of publishing. See Darnton (2000).

reading, and publication. Or we could look again at Friedrich Kittler's *Discourse Networks 1800/1900* (1990), which (although difficult to read, at least in translation) shows what a history of writing in the machine age might look like.[34]

Perhaps the biggest challenge is creating a history that keeps the virtues of the local but operates at a unit of analysis larger than a single country. Much of the founding work in the social history of science in the 1970s was concerned with national styles in science: French, Scottish, Canadian, American, and so forth.[35] In identifying national "styles," historians challenged universalist notions of science, but they also tended to align their work with a certain kind of nationalism—an alliance made all the more potent by problems of language and the traditional association of history writing with the rise of the nation- state. As a result, it now appears to require a huge scholarly investment to move one's research outside the boundaries of a single country. There is, of course, nothing wrong with a geographical focus, as long as it not simply taken for granted—or, what is worse, assumed as a kind of global microcosm. This has notoriously been a problem with studies of Britain, where accounts of the origins of the Royal Society or the reception of Darwin's *Origin* are often taken as possessing an automatic international applicability.

A good example is the remarkable gulf between studies of science in Victorian Britain and the antebellum United States. For scholars of the seventeenth and eighteenth centuries, Atlantic history has been a commonplace for at least fifty years. But the situation is very different for the nineteenth century—although communication was during most of this period far better than it had been before. As a result of the widening of the Atlantic in the nineteenth century, we have two sophisticated bodies of secondary literature on two closely connected national cultures—but little cross-citation between those who study them. In part, this is because of general issues of exceptionalism in the writing of American history; in part, it is because of British parochialism and the long-standing dominance of literary scholarship within accounts of the Victorian period. But whatever its causes, the result is that some of the most relevant and best work in one field is simply not used in analyzing closely similar situations in the other. For example, the most revealing works on scientific fraud and hoaxing are not about English showmen, but about P. T. Barnum.[36] Yet, bizarrely, almost no one in Victorian history generally (let alone in studies of science) ever refers to these works. In effect, we have been even more nationalistic than the people we study.

One way of getting beyond national histories has been to undertake comparative studies. But as has often been pointed out, comparative work can all too often end up reaffirming national boundaries, as the nation becomes the standard unit of comparison. Volumes such as *The Scientific Revolution in National Context* and *The Comparative Reception of Relativity* have brought out the complexity and particularity of specific national situations, but they have done less toward creating a global picture.[37] If you want a history that truly does the job, the answer is not to invite one contributor to discuss each country separately but to find people willing to study different kinds of interactions, translations, and transformations.

[34] Williams (1961,1958); Kittler (1990).

[35] For characteristically illuminating thoughts on the issue see Rosenberg (1970); Morrell (1974), and the editors' introduction to Levere and Jarrell (1974). There were of course many exceptions: notable ones dealing with transatlantic relations include Fleming and Bailyn (1969) and Rossiter (1975).

[36] Harris (1973); Reiss (2001); Cook (2001).

[37] Porter and Teich (1992) and Glick (1987).

More promising has been the outpouring of work on imperial and postcolonial science during the past decade. An early fascination with Latour's actor-network theory has given way to a fully historical understanding, often informed by anthropological perspectives, with divisions between center and periphery replaced by patterns of mutual interdependence. The consequences are clear in the new history of disease and germs, which goes beyond the laboratory to interpret the forging of bacteriology as part of the processes of imperial exchange. Most strikingly, accounts of standardization, measurement, and public exhibition have transformed the history of the physical sciences. The result has been a dramatically new picture of the origins of field theory, energy physics, and statistics in relation to telegraphy, economic development, and modern accounting practices.[38] These have been such exciting sites for research, I would argue, because they raise issues so clearly implicated in political struggles over global empires and industrial capitalism.

In situations where domination and conquest are less obvious, the significance of communication and acting at a distance has been easier to miss. This has certainly been the case in many local studies, whether by professional historians or not, in which scientists are shown to interact with those immediately around them, with other audiences and competing centers of practice remaining in the background. At the other extreme, the writers of general histories tend to imagine that modern scientific inquiry is the closest thing to a perfectly globalized system that we possess. This surely remains a dominant view among scientific practitioners and the public at large. International conferences, international journals, and international visitors are taken for granted, thus making fields such as nuclear physics or molecular biology appear at times to be without boundaries at all. Here the assumption that knowledge simply travels by itself seems easier to make, for the work that has gone into making this appear to be the case is so pervasive and institutionalized that it has become hard to see. Struggles for access and control, however, are always at stake in any form of communication: to make knowledge move is the most difficult form of power to achieve.

6.4 Conclusion

Historians have a tendency to neutralize fundamental challenges by creating new subdisciplines that allow their advocates room to work while minimizing their impact. They add sidecars to a vehicle that continues to travel in the old way toward the old destination. So I should stress that I am not recommending that historians of science pursue the creation of a separate discipline of the "history of the book" or of "print culture." At one level, book history has been concerned with publishers, editors, printers, and so forth, aspects of production that are important but need not occupy the attention of more than a minority of historians of science. Book history, in that sense, has been too narrowly about print to capture the full range of what historians of science ought to be interested in. If science really is an activity pursued by people, the study of communicative practices should be something that we all do all the time. So there are lessons to be learned from book history, just as there are from translation studies and accounts of the laboratory–field boundary; but the label is not really appropriate for the range of things that need to be done.

Similarly, I am not advocating the creation of a subfield within history of science devoted to the study of popular science. Indeed, at this stage it would be best if "popular

[38] Much of this work is conveniently surveyed in Nye (2003).

science" as a neutral descriptive term was abandoned. As a descriptive category, "popular science" and its cognates suffer from serious disadvantages. First, they have an exceptionally rich and multivocal history. Studying these meanings is eminently worthwhile, but it is hard to see how together they refer to a coherent entity. To dump Johann Amos Comenius's *Orbis sensualium pictus* (1658), Camille Flammarion's *Astronomie populaire* (1879), and Stephen Hawking's *Brief History of Time* (1988) in a single genre surely conceals more than it reveals. "Popular science" is not a thing that comes into being at a particular moment or period; it is not appropriately seen as an emergent category.[39] The second disadvantage is the diffusionist baggage that the term "popular science" has carried since the mid-nineteenth century. To label something unequivocally as popular science can be seen as tantamount to saying that it is "not science" or even a kind of pseudoscience parading as the real thing. Above all, it prejudges the boundary that Ludwik Fleck long ago identified between expert, esoteric knowledge and the exoteric knowledge found in textbooks and simplified redactions. In any historical study of science, that boundary ought to be a critical site for investigation.[40]

These are not easy times for history of science. In Roy Porter, Stephen Jay Gould, and Susan Abrams, we have lost too early some of the most effective public advocates for our field. As everybody knows, getting an academic book into print is much more difficult than it was four years ago, when Jan Golinski spoke so eloquently to the last three-societies meeting about historical narratives and the wider public.[41] These days even university presses seem reluctant to take on titles unless they promise abroad appeal. The real running in the past decade seems to have been made by journalists whose writings bring past science to a large general readership. Many of these works are excellent, but many also do little more than reinforce existing attitudes toward heroic genius, the inevitable progress of science, and the triumph of narrowly defined conceptions of national character. What these books do make clear is that there are large audiences for history of science, which a number of our colleagues have begun to reach with different and more challenging messages.

Perhaps this is just my own experience, but I think it is fair to say that the field of history of science, compared to any time since its founding in the 1950s, has experienced a loss of direction. I suspect this is because, as in other parts of the humanities, a certain kind of engagement with theoretical perspectives is coming to an end, and it is not clear what the replacement is to be. At Manchester, for example, all the main figures in sociology of knowledge gave papers; here we are largely on our own, with our links and collaborations more likely to be with general historians of the periods we study. It is no longer possible to look to Paris, Edinburgh, Bath, or even Cambridge fora unified, programmatic notion of what is to be done. That is probably a good thing, for the subject has always thrived on diversity, but it is also a challenge.

It is, of course, always possible that history of science will seamlessly merge into cultural history, philosophy, the natural sciences, or the fields on which it borders in science studies. Last year's president of the History of Science Society, John Servos, once pub-

[39]Comenius (1658); Flammarion (1879); Hawking (1988). For exploration of the rich history of "popular science" and its cognates, the special issue on "Science Popularization" (1994) remains a good starting point, as does Whitley (1985).

[40]On the diffusionist baggage borne by the term "popular science" see Secord (1994). For Fleck's boundary see Fleck (1979).

[41]Jan Golinski, "Tall Tales and Short Stories: Narrating the History of Science," available online at https://www.academia.edu/9271123/Tall_Tales_and_Short_Stories_Narrating_the_History_of_Science.

lished an essay in *Isis* on "a disciplinary program that failed" in physical chemistry.[42] In my more pessimistic moods as a graduate student, I sometimes wondered if the last article in the journal might be a similar obituary for the field I was just then entering. My sense these days is much more optimistic, if not always about jobs then certainly about the underlying intellectual enterprise. There are many indications that we are beginning to tackle, from a fundamentally historical perspective, knowledge not just as abstract doctrine but as communicative practice in a range of well-integrated and closely understood settings. My sense also is that this transformation is more advanced in some fields, such as imperial science and the sixteenth and seventeenth centuries. Moreover, there are encouraging signs of an appreciative audience for our work among general historians, historians of art and literature, and the public at large. Historians of science have been influential beyond their numerical strength in pursuing new topics, from the history of the book to the history of the body, in ways that have attracted interest throughout the humanities.

For this to continue, we need to grapple with the circulation of knowledge at the right scale. Here there really are abundant opportunities. It is only in the past few years that we have begun to realize just how constrained the frameworks for understanding the larger narratives of science really have been. But the great advantage now is one of perspective beyond that of the inherited stories. We have a way to move toward larger narratives made by historians of science and specifically tailored to serve historical purposes. The words Roy Porter quoted in 1975 from the geologist Charles Lyell, at the first history of science meeting I ever attended, are still to the point: "the charm of first discovery is our own, and as we explore this magnificent field of inquiry, the sentiment of a great historian … may continually be present to our minds, that 'he who calls what has vanished back again into being, enjoys a bliss like that of creating.' "[43]

2004 is the year of the transit of Venus, and this surely is a heavenly sign of the ascendancy of the forms of historical practice I have been discussing. The transit of Venus has never been primarily about discovery but, rather, about determining the basic astronomical unit, the distance from the Earth to the Sun; it thus underlines the significance in science of measurement, standardization, and ordinary practice. It is a local event—to be seen by specific observers in specific places—that has sparked national rivalry, global exploration, and wide inquiry. It is an event that has caused both astronomical observers and historians to think about time, from the scale of the personal equation of individual observers in seeing the notorious "black drop" to the scale of years and centuries when the transit recurs. At every stage the transit has underlined the integration of new forms of communication and how these have been transformed, from its observation by Jeremiah Horrocks in a Lancashire village in 1639 to its appearance in early June of this year, when I saw it both through the early Victorian Northumberland equatorial telescope at the Institute for Astronomy in Cambridge and on my laptop computer at home. Moreover, the transit has been a huge public success, not only for astronomical science but also for interest in its history. The transit of Venus will be visible again in eight years, just in time to herald what will be the seventh of these three-society meetings. I'm looking forward to seeing where historical studies of science are headed in that time.

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[42] Servos (1982).

[43] Porter (1976, 100). Lyell was quoting the pioneering German historian Barthold George Niebuhr, whose *History of Rome* had been translated into English in 1828.

Acknowledgments

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Chapter 7
Peasant Friendly Plant Breeding and the Early Years of the Green Revolution in Mexico

Jonathan Harwood

The agricultural programs collectively known as the "Green Revolution," initiated in the 1940s with funding first from the Rockefeller Foundation and later from the Ford Foundation, were remarkably successful in some respects but disappointing in others. On the one hand, within a relatively short time various developing countries increased cereals production several fold so that imports were no longer necessary. On the other, after three decades it was clear that the programs' declared aim to alleviate world hunger had not been realized; in some regions where Green Revolution programs had been in operation, rural poverty (effectively synonymous with hunger) actually increased. The explanation for this outcome, advanced by a number of critics during the 1970s, was that the high-yielding plant varieties and intensive cultivation techniques produced by the Green Revolution were adopted primarily by large commercial farmers. Peasant farmers, in contrast, lacked both the capital and the appropriate growing conditions, such as irrigation, necessary to take advantage of the new technology.[1]

Why did the Green Revolution's agricultural scientists pursue such an inappropriate—peasant unfriendly—form of plant breeding? Several explanations are conceivable. In view of the sharp decline in small farms in both Western Europe and the United States, especially since the 1950s, some observers might be tempted to conclude that the revolution developed a technology suitable for large commercial farms because there was simply no alternative. That is, there was no advanced technology available in the 1940s and 1950s that could have boosted the productivity of small farms. This explanation is untenable, as technology suitable for peasant farms was developed during the green revolutions that started in Western Europe during the late nineteenth century. For example, in German speaking Europe several states established plant-breeding stations around 1900 that explicitly aimed to bring the benefits of advanced breeding technology to the peasant farmers who predominated in those regions, and the evidence indicates that these efforts were successful in invigorating regional agricultural economies. One consequence of such policies in Germany was that the only size-category of farms to increase (both in number and in proportion of the total acreage) between the late nineteenth century and the 1960s was that of ten to twenty hectares (twentyfive to fifty acres). During the 1940s and 1950s when the classic foundation-sponsored

[1] In Mexico maize yields nearly doubled from the late 1940s to the late 1960s, while wheat production rose eightfold, but the increase was mainly generated on large irrigated farms. Wellhausen (1976); Tuchman (1976); Stakman et al. (1967).

Green Revolutions for the developing world were being planned, therefore, a successful European model for promoting small-farm productivity was available.[2]

Were the Green Revolution's designers simply unaware of European policies, perhaps seeing them as irrelevant for American conditions where farms were on average far larger? Or were they familiar with European developments but took the view—sometimes expressed by US secretaries of agriculture during the 1950s and 1960s—that small farms were not worth bothering with since they could never be as efficient as large ones? Were they aware of the European approach but preferred to take the path of least resistance, thinking that it would simply be easier to work with large farmers who tend to embrace new cultivation methods? Or were foundation officials worried that targeting peasant farmers for assistance might be seen by host governments as an unwelcome "political" intervention?[3]

These questions can be answered by looking at the early history of the MAP, the first of the Green Revolution programs undertaken by the Rockefeller Foundation. Most historians of the MAP tend to emphasize the extent to which the program's advisors and staff relied upon an American model of agricultural development that was largely inappropriate for Mexican conditions or have questioned the MAP's commitment to alleviating rural poverty and hunger. While this is probably an accurate characterization of the program in the 1950s, it does justice neither to the original design of the program in the early 1940s nor to the initial aims of its maize-breeding work. Thus, the program underwent a substantial shift between the 1940s and 1950s.[4]

[2]Koning (1994); Harwood (n.d.); Boelcke (1995). The farms in question were similar in size to those in the developing world. In Bavaria, 60 percent of the farms around 1900 were less than five hectares (twelve acres) in size, Kiessling (1906). In the West Punjab in the 1950s, 79 percent of farms were less than four hectares, Griffin (1974, 20). In the central highlands of Mexico during the 1970s, where half of Mexico's farmers lived and rural poverty was pronounced, the average amount of arable land per farm was about six hectares, Wellhausen (1976, 136).

[3]Billard (1970). According to one account, by the late 1960s Ford Foundation officials had concluded that Mexican peasant agriculture would have to disappear if the country's agriculture were to be fully modernized, Perkins (1997, 114). On large farmers' enthusiasm for new methods, see Paarlberg (1981).

[4]Fitzgerald (1986, 459); Alcantara (1976, 20–23); Jennings (1988, ch. 3). Doubting its commitment to poverty reduction, Olea-Franco (2001, 721) writes that the MAP was "in no way a philanthropic enterprise to end hunger in the world": Although Cotter's book is a study of Mexican agricultural scientists during the twentieth century rather than an analysis of the MAP, he acknowledges that some of the MAP's work during the 1940s sought to improve peasant farmer wellbeing, Cotter (2003, 198–322). On balance, he takes the view that the "foundation wanted to push Mexico through the transition from an agrarian to an industrial society … and thus tried to create commercial farmers, not vibrant, autonomous communities of peasant corn growers" (2003, 322, 188). In Jennings's (1988) discussion of the 1940s, he recognizes that there were initially competing visions as to how the MAP might operate, but he underestimates the extent to which alternative approaches were taken seriously by the program's Advisory Committee as well as foundation officials. Moreover, his argument that the MAP decided to try to increase productivity without regard to social consequences ignores the concern among advisors and officials from the mid-1940s about the need for more attention to developing the extension system. Much the same can be said of Stephen Lewontin's "The Green Revolution and the Politics of Agricultural Development in Mexico since 1940" (1983). Karin Matchett recognized that the early breeding program was in fact more appropriate for Mexican conditions than was the work being done by Mexico's own breeders, Matchett (2002).

Because I am interested in the extent to which the founding vision of the program rested upon earlier European experience, I have drawn primarily upon archival materials that shed light upon the aims and approaches of the foundation during the planning stage. Other historians of the MAP have been concerned with the work that its staff actually did, usually during a later period. Among the existing histories, only Fitzgerald and Matchett have looked closely at the 1940s, and the latter's analysis focuses heavily upon the MAP and Mexican maize-breeding programs from the 1930s to the 1960s rather than upon the general strategy underlying the MAP and its transformation during the early years of the program.

In fact, the foundation's agricultural officials were almost certainly aware of European developments, and some of them, as well as advisors to the MAP, also had first-hand experience of the problems faced by small farmers in the United States and elsewhere during the interwar period. Furthermore, perhaps surprisingly, the foundation's declared aim of alleviating rural poverty was not just posturing by an organization anxious to be seen as an agent of philanthropy; some foundation officials and advisors appear to have been genuinely concerned to improve rural living standards through increasing the productivity of peasant farms. In addition, the program's early work in maize breeding indicates that rather than just trying to develop American-style hybrid maize varieties for Mexican conditions, the breeders used quicker methods to produce varieties more appropriate for the circumstances of poor farmers. Once the MAP had been running for a few years, however, it became apparent that the task of getting new varieties and cultivation practices to small farmers, though urgent, was going to be difficult, not least because the MAP possessed neither the facilities nor the formal authority to undertake this task on its own. Faced with the need to make some kind of impact quickly, MAP staff chose to concentrate upon projects that were likely to find a ready audience. This meant setting aside the needs of peasant farmers to develop high-yielding varieties especially suited to large commercial farms. In effect, the program was abandoning its original aim of alleviating rural poverty.

The Rockefeller Foundation's interest in agricultural development did not begin with Mexico nor were its agricultural advisors unaware of the problems of peasant agriculture. Before the First World War, for example, the General Education Board (GEB) had funded extension programs in the American South, and during the 1920s the International Education Board's (IEB) support for European higher education had included agriculture. Among the agricultural experts associated with the IEB the most important was A. R. Mann. Dean of Cornell University's School of Agriculture, Mann was the board's director of agriculture from 1924 to 1926; and during the 1920s he visited the major European experiment stations on its behalf, alert to the possibility that American agriculturists might have something to learn from them. Among the sites he visited were German plant-breeding stations, and although his diaries do not single out their peasant-friendly orientation for comment, he seems to have been aware that European peasants had different needs from most American farmers.[5]

Unlike some of the IEB's other advisors, Mann was not single-mindedly in favor of supporting "fundamental research" in the agricultural sciences. Foundations, in his view, could pursue two quite different paths toward improving production. One of these was to fund work aimed at longer-term improvement, the other was to support "practical measures" that would convey best practice to farmers, as the GEB had done before 1914.[6]

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[5] *Mann Diaries* (Mann 1924–1927, Log 10 (Dec. 1924–Jan. 1925) on Germany; Mann 1926–1927 and 1940–1942, Log 34 (Dec.1927) on Finland, 1924b, 1924a). Mann notes the views of Asher Hobson of the International Institute of Agriculture that the rapid growth of cooperatives in Europe was due to the "extreme need" to husband small savings, a point that "possibly [had] greater significance than in the US." C. B. Hutchison, director of agriculture for the International Education Board Archives (IEB) for 1926–28, also visited the principal German stations; see Hutchison Papers (Hutchison n.d.).

[6] One of these advocates for fundamental research was Hutchison, see Hutchison (1928); numerous documents in Folder 334, Box 23, Ser. 1.1, IEB, Rockefeller Archive Center (RAC). The same conception of agricultural research is evident in the survey of US agricultural education conducted for the IEB by Whitney Shepardson (1929). On two paths toward improvement, see Mann (1925). In an annual report, the IEB drew a similar distinction between "economic" and "scientific" perspectives in agricultural research. The former developed empirical methods of

This practical emphasis was also evident in the foundation's support for agricultural development in China. In 1924, for example, Mann agreed to provide IEB funding to support a "Cooperative Plant Improvement Program," which would send breeders from Cornell University to the University of Nanking. The program was tailored in a number of respects to the needs of small farmers. The Cornellians, for example, ignored the requests of Chinese cotton-mill owners and chose to focus instead on the major staple crops, among them sorghum because of its ability to withstand drought. And they chose to use selection rather than hybridization because it was cheaper and faster. Part of the program entailed funding Chinese students to study plant breeding, but an American agricultural scientist advised the board that it would be a bad idea to send young Chinese to study in the United States because they would come back with "an American training that doesn't fit Chinese conditions." The conditions in question included the fact that average farm size was only 3.5 acres, and the breeders realized that this made Chinese peasants necessarily cautious about adopting new varieties (unlike their American counterparts who had land to spare for risky trials). This meant that extension was crucial—a point that the breeders eventually recognized. Though the program's breeding work managed to acknowledge peasant needs, however, in one historian's view breeders never fully solved the dilemma of extension. To what extent Rockefeller agricultural advisors learned a lesson from this experience is not clear, but it is interesting that in its subsequent China Program between 1935 and 1937 the foundation took the view that the program's agricultural work should focus upon the application of existing knowledge and its dissemination rather than upon research.[7]

This emphasis upon "practical measures" and extension may have characterized the GEB's Southern Agriculture Program, which Mann directed from 1936. Before he took over the program, it was concerned with public education, "negro education," and (from 1933) with southern white colleges and universities. In 1936, however, the program's declared aim was to strengthen those areas of education and research that dealt with the economic and social problems of rural areas.[8]

After the end of the IEB in the late 1920s, Mann continued to play a major role in Rockefeller agricultural programs, initially as director of the Southern Agriculture Program and then as vice-president of the foundation. It was natural, therefore, that he was consulted on the design of the Mexican program, and from 1946 he served as part-time officer in the Division of Natural Sciences with responsibility for the MAP. It is thus largely through him that the foundation's pre-war experience in agriculture was brought to bear upon the MAP. As

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production and distribution and was local in orientation, while the latter dealt with general principles and was international in scope, see *Annual Report of the International Education Board, 1925–26* (1926, 18).

[7]On crops, see Stross (1986, 152, 157, 201–202, and ch. 6); on selection and farm size, see Love and Reisner (1964, 11–34). These two Cornell breeders observed that "in some ways this is the most difficult part of a crop improvement program" (1964, 33). The director of the program, Selskar Gunn, was an expert on public health who had been unhappy with the fact that most of the Rockefeller's support for medicine in China (since before World War I) had been directed at the Peking Union Medical College. Although this promoted high standards in medical science, he believed it had too little impact on rural public health. As a result, he called for the China Program to take a "fully integrated" approach to improving educational, economic, and social conditions in rural areas where the vast majority of the population lived. The aim of the program, Gunn argued, was to "coordinate several fragmentary native efforts into a united movement to improve the lot of the Chinese peasant," see Gunn cited in Thomson (1969, 149).

[8]*Annual Report of the General Education Board* (New York: General Education Board) for the years 1932–33 through 1938. Mann may have seen extension as a more important task for the MAP than has been suggested, see Matchett (2002, 80).

planning for the program got underway, a Survey Committee (later the Advisory Committee for Mexican Agriculture) was also set up late in 1941–consisting of a plant pathologist (Elvin Stakman), a plant breeder (Paul Mangelsdorf), and a soil scientist (Richard Bradfield). These three, along with Mann, effectively designed the MAP.[9]

Although the publicly declared aim of the MAP was to alleviate poverty and hunger, this does not mean that it was necessarily prompted by humanitarian concerns. Indeed, officers and advisors on occasion attributed a strategic political significance to the program:

> Communism makes attractive promises to underfed peoples; democracy must not only promise as much, but must deliver more… . Asiatic and other underprivileged people attribute their present plight to the domination of the capitalist colonial system… . In this struggle for the minds of men the side that best helps satisfy man's primary needs for food, clothing and shelter is likely to win… . Appropriate action now may help [people of developing nations] to attain by evolution the improvements, including those in agriculture, which otherwise may have to come by revolution.

Additionally, foundation officials were aware that the early 1940s was a good time in which to launch an aid program in Mexico since the newly elected Mexican government appeared more interested in cooperating with the United States than its predecessor (which had nationalized the oil industry without compensation).[10]

But how serious were officials and advisors about reducing rural poverty and hunger in Mexico? Considering the general aims of the original MAP, it appears that those involved agreed that it was important, not simply to boost agricultural production overall (which could have been achieved by targeting assistance just to large commercial farmers), but also to alleviate hunger and low standards of living among Mexican small-holders. Indeed, the original suggestion to establish an agricultural program in Mexico came from staff in the Rockefeller's International Health Division, who thought that it would usefully complement their existing public health program in Mexico. This idea of integrating public health with agricultural development—an early example of what was later regarded as best practice in development policy—found favor with the Survey Commission in 1941, with Warren Weaver in 1946, and with Stakman in the early 1950s although it seems not to have been implemented.[11]

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[9] *Annual Report of the General Education Board, 1936–37*; Memo from Warren Weaver (1946a). On the establishment of the Advisory Committee (later renamed the Advisory Committee on Agricultural Activities), see Folder 56, Box 9, Ser. 323, RG 1.1, RF, RAC. This committee grew out of the Agricultural Survey Commission established early in 1941, consisting of Stakman, Mangelsdorf, Bradfield, and Richard Schultes – whose task was to visit Mexico, assess the state of its agriculture, and make recommendations on what a Rockefeller program might be able to achieve, see Folder 70, Box 11, Ser. 323, RG 1.1, RF, RAC.

[10] Advisory Committee for Agricultural Activities (1951). Weaver employed the same argument the following month to persuade Chester Barnard, president of the foundation, of the need for a separate division for agriculture, see Weaver (1951a). On US-Mexican relations, see *Correspondence* for 1941. Lewontin argues that the foundation's plan for an agricultural aid program was supported by US officials because it fit well with the administration's policy toward cooperation with Latin American states after 1938, Lewontin (1983, ch. 4).

[11] In the 1960s Stakman claimed that the seriousness of this problem had been brought home to the advisors by food riots in 1942 and 1943 and "real distress," *Interview with Elvin Stakman* (n.d.). On the Int. Health Division, see Jennings (1988, 189). This combination of measures aimed at public health as well as agricultural development was also a feature of the foundation's program in China during the 1930s, see Thomson (1969, ch. 6). On best practice, see Staatz and Eicher (1990, 20–21). "As the improvement of agriculture and rural life involves not only progress

In addition, program designers were aware that alleviating rural poverty would mean addressing the specific needs of peasant farmers and would require more than just the use of commercial inputs. Both scientists and officials were clear, for example, that it would not be possible merely to apply established American cultivation methods to Mexican farming. In its report to the foundation following the first exploratory visit to Mexico in 1941, for example, the Survey Commission noted that they had assessed the state of Mexican agriculture: "Not solely by American standards but in the light of the history and traditions of the Mexican people. It would be wholly impossible, even if desirable, to impose the modern American culture upon Mexico; any improvement that is to be brought about must come within the framework of Mexican culture." When officials consulted Carl Sauer, a social scientist with extensive experience of Mexico, they got the same message:

> A good aggressive bunch of American agronomists and plant-breeders could ruin the native resources for good and all by pushing their American commercial stocks… . Mexican agriculture cannot be pointed toward standardization on a few commercial types without upsetting native economy and culture hopelessly. The example of Iowa [i.e., where the maize crop was based almost entirely upon a few hybrid varieties] is about the most dangerous of all for Mexico. Unless the Americans understand that, they'd better keep out of this country entirely.[12]

While the advisors' awareness of the complexity of the challenge facing the MAP is significant, perhaps more striking are the specific measures that the program adopted in order to improve Mexican agriculture. To be sure, it does not seem that Warren Weaver (as director of the Division of Natural Sciences that oversaw the MAP), Mann, or the Advisory Committee consciously considered European models in designing the MAP, although Stakman's ties with German plant breeders meant that he would almost certainly have heard about the peasant friendly stations there. Nevertheless, several features of the initial breeding program were peasant friendly in the sense that they were designed with the needs of small farmers in mind, in particular resource-poor farmers who could not afford to purchase new seed annually. In view of the criticism later directed at the MAP for giving wheat breeding such a high profile, it is significant that at the outset the advisors were agreed that maize was the crop to focus upon. Since it was central to most Mexicans' diet and was grown by the great major-

in techniques of crop and animal production but also amelioration of living and health conditions, it seems obvious that the proposed commission (MAP] should be intimately associated with the local office of the International Health Division, thus promoting economy and efficiency," *Summary of the Survey Commission's report* (1941); Cobb (1956); *Minutes of meeting of Mexican Agricultural Commission* (1941); Weaver (1946b); Stakman (n.d.). According to Cotter (2003, 189), in 1945 the foundation considered a cooperative arrangement between the MAP and IHD, but nutrition never became a major issue for the MAP.

[12]Following his visit to Mexico in 1945, Mann (1943) drew attention to the "urgent need" to develop methods of maintaining soil fertility that would not require expensive commercial fertilizers. Members of the Agricultural Advisory Committee were familiar with the nature of peasant agriculture. Stakman claimed that the MAP was not unprepared for the problems in Mexico because there had been "a lot of peasant farming in the US when we were young," Stakman (1971, 945); *"Report on the Status of Agriculture in Mexico"* (n.d., 144). As the MAP's director recalled, MAP breeders learned that not all improved varieties of beans were welcomed by peasant farmers; acceptance required that beans had the right color. "So we tried to meet this requirement. Sometimes their preferences are bound up with bitter experience, so unless you know that experience, it is better not to try to interfere too abruptly with customs or habits," *Interview with J. George Harrar* (n.d., 52); RBF to ARM, AJW, and JAF, excerpt of comments by Carl Sauer on Wallace's proposal (1941). Sauer, a cultural geographer from the University of California-Berkeley, was not the only Latin American specialist who conveyed this message to the foundation at the outset, see Cotter (2003, 143).

ity of small farmers, maize offered "the greatest opportunity for improvement in Mexican agriculture." Equally important, advisors were not single-mindedly bent upon developing new high-yielding varieties (as had been done in the United States). The most pressing problem, they argued in 1941, was to improve cultivation practices (because of serious erosion and depleted soils). Improving varieties was the next most important issue, but the advisors envisioned these being provided via "introduction, selection *or breeding* [my emphasis]." Accordingly, the MAP initially devoted considerable effort to testing existing Mexican varieties of maize to identify the best one for each locality. Simply by redistributing these to the most suitable locality, one advisor recommended, the quality of maize cultivation could be improved quickly without having to wait years for the development of new varieties. This approach was richly rewarded when it was discovered that yield in some regions could be increased by 20 to 30 percent by introducing a variety native to another region.[13]

In addition, as Karin Matchett's study of the MAP has shown, the program's advisors took into account the economic circumstances of peasant farmers. As the Survey Commission observed in 1941, the problem with US-style hybrid maize was that seed had to be purchased each year, "and the small farmer in Mexico has neither the cash nor the initiative to do this." The breeder on the commission, Mangelsdorf, pointed out that hybrid maize varieties had not been successful in the American South where conditions were similar to those in Mexico. His experience in East Texas had been that it was hard to get small farmers there to purchase hybrid maize seed every year instead of saving seed from the previous harvest. Where farms were small and maize was grown for subsistence rather than for sale on the market, therefore, it was necessary to develop improved varieties that could be replanted year after year. Open-pollinated varieties—where crossing occurs spontaneously in nature rather than under the control of the breeder—he thought, met this need while hybrid maize did not.[14]

[13] During the 1920s Stakman became acquainted with Theodor Roemer. Professor of plant breeding at the University of Halle, Roemer had visited the University of Minnesota in 1925, after which the two men set up a student exchange between their universities, and Stakman was visiting professor at Halle in 1930, *Interview with Elvin Stakman* (n.d.); Stakman (1971). Stakman's knowledge of the German plant-breeding stations may also have been based on his experience with the IEB's agricultural work from about 1926 when he became an advisor to Hutchison, see Hutchison to Rose (1926); Shepardson to Hutchison (1927). For the focus on maize, see *"Agricultural Conditions and Problems in Mexico"* (1941). Wheat, unlike maize or beans, was not central to most Mexicans' diet; the demand came from the wealthier urban sector of the population. Moreover the number of farmers who grew wheat was only 2 or 3 percent of the number who grew maize, and wheat farms were larger and better irrigated. Within a few years, the program devoted increasing attention to wheat, much to the annoyance of Sauer, who dismissed it as a food consumed by "the privileged fraction of the population," see Sauer (1945). Between about 1950 and 1970, the MAP allocated similar levels of research funding to wheat and maize, see Myren (1970). This "wheat bias" has been cited by historians - correctly - to illustrate the large farm orientation that the MAP eventually acquired, see Lewontin (1983, 127). The advisors' support for improved techniques did not entail dismissing all native cultivation practices. In their work on beans, MAP breeders took into account the traditional practice of intercropping beans with maize since they thought it was likely to continue for generations to come, see Cotter (2003, 188). The quotation is from *Summary of the Survey Commission's report* (1941). *"Agricultural Conditions and Problems in Mexico"* (1941); Mangelsdorf (1943b); Harrar (1946a).

[14] Matchett (2002. 2006). The quotation is from *"Agricultural Conditions and Problems in Mexico"* (1941). Mangelsdorf (1943b); *Interview with Paul Mangelsdorf* (n.d., 69). Mangelsdorf envisioned a two-track breeding program in which the MAP should develop US-style hybrids since he believed that large Mexican farms could make good use of them. To argue that a high-input model suited to commercial farming "was ultimately the only … plausible model to which these scientists could refer" thus fails to recognize the extent to which MAP staff were sensitive to the needs of Mexican peasant farmers (Fitzgerald 1986, 463). Similarly, Cotter's claim that "the MAP did not create seeds to solve the problems of peasant farmers" (Cotter 2003, 188) is not true, at least for the 1940s.

According to Matchett, although Edwin Wellhausen's maize-breeding program at the MAP included constructing conventional hybrids using the double-cross method worked out in the United States, most of his efforts through the 1950s used quite different ways of improving maize. Some of the work relied on a simple and traditional method (mass selection) to improve Mexican landraces, but the majority of it was dedicated to making synthetic varieties, a kind of quick-and-dirty hybrid that was relatively high-yielding and whose seed could be replanted each season. Part of the rationale for this was agro-ecological. As a MAP progress report in 1944 noted, conventional hybrid maize varieties in the United States were so heavily tailored for a particular region that "they are complete failures elsewhere while inferior open pollinated lines are more adaptable and can be grown in various regions." But the MAP's breeding strategy was also pragmatic; it was much quicker to produce synthetic varieties than double-cross hybrids (which could take over ten years), and, as Mangelsdorf emphasized, Mexico was a good place to turn out something in a hurry because almost anything would be a substantial improvement. The initial results were promising; the first synthetic varieties released by the MAP in 1948 yielded about 30 percent more than the benchmark variety.[15]

Despite the MAP's promising start with a peasant-centered approach to development, it soon became evident that improving the state of peasant agriculture was not going to be easy. By the late 1940s there were signs of disagreement—among advisors as well as within the foundation—on the most effective strategy for transforming Mexican agriculture. And by the 1950s the MAP was no longer pursuing some of its original aims.

At the center of this shift was the question of what role the MAP should adopt in relation to extension. Several months before the Survey Commission's visit to Mexico, Mann had recommended that a Mexican program should take a two-pronged approach, pursuing not only research but also extension, since the latter offered the promise of a relatively quick impact. Following their visit in the summer of 1941, similarly, it was clear to the Survey Commission that an immediate improvement to Mexican agriculture did not require the production of new practices or varieties. An enormous improvement in yield could be achieved just with the application of existing knowledge. During his visit to Mexico in 1943, Bradfield was struck by the fact that basic forms of good practice such as crop rotation or fertilizing with manure were rare, and the wooden plow was almost universal. The highest priority, he stated in his report to the foundation, should be extension. Following another visit two years later, he reiterated his concern; something had to be done to improve soil fertility, he argued, before the full fruits of the work on plant disease and new varieties could be exploited. The

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Although Lewontin recognizes that the original emphasis of the maize-breeding program was on open-pollinated varieties, he misses the significance of this fact, Lewontin (1983, 157–58).

[15] In the double-cross hybrid method, the breeder selects four inbred lines that have been derived from five or six generations of inbreeding. Lines A and B are then crossed with each other, as are lines C and D, and finally the AB hybrid is crossed with the CD hybrid in order to produce the desired variety. Landraces are mixed varieties (consisting of many distinct subtypes) that have been traditionally planted in a particular locality over many generations and are thus well adapted to it. On synthetic varieties, see Matchett (2006, 351–366, 2002, ch. 2, and 163). The potential value of synthetic varieties had been proposed a few years earlier for regions "where hybrid corn may not be economically feasible," see Jenkins (1940). De Alcantara attributes advocacy for synthetics primarily to Mexican maize breeders of the late 1940s, evidently unaware that the MAP had embarked on such a breeding program several years earlier, de Alcantara (1976, 37–38). The quotation is from the *MAP Progress Report* (1944, 5). On the need for speed, see *Interview with Paul Mangelsdorf* (n.d., 69). That MAP breeders placed a higher priority on speed in developing new varieties than did their Mexican counterparts is clear from Matchett (2002, ch. 6). On the yield of synthetic varieties, see Stakman (1971, 1071).

time was approaching, "if we have not already reached it, when we should begin to think about how the information obtained in the research program can be made most effective in Mexican agriculture." In another year or two, he thought, there would be enough knowledge upon which to base an extension program. At this point the other members of the Advisory Committee did not agree, arguing that the MAP should stick to research for the time being. But after their visit to Mexico the following year (1946), Mangelsdorf and Stakman concurred with Bradfield: the MAP's research had been so successful that it was time for the foundation to start pressing the ministry for the development of a properly designed extension program or perhaps even to become involved in extension itself.[16]

This initial emphasis upon extension is significant because it reflected the advisors' concern to reach small farmers. Large farmers, in Mexico as elsewhere, were much better placed to look after themselves. Unlike their smaller brethren, they could afford to take risks with new methods and had the capital to invest in them; many were thus keen to cooperate with the MAP by offering land for field trials and adopting new varieties. But to disseminate the requisite knowledge to small farmers required a functional extension service, which Mexico did not yet have. The existing service, Bradfield pointed out, was totally unsatisfactory. Since its staff had no means of transportation, they never came into direct contact with farmers and were thus reduced to distributing leaflets and answering letters. Given low levels of literacy among peasants, this was not a viable way of reaching the vast majority of farmers. Nor was the MAP ever likely to have either the resources or the manpower to take on such a huge job itself.[17]

By 1946 the advisors were not the only ones to have reached this conclusion. During their own visit to Mexico that year, Weaver and Mann met the minister of agriculture who expressed the view that it was time to begin extending the findings and new seeds to farmers. The minister was keen for the MAP to take the initiative in developing a Mexican extension service, and the Advisory Committee concurred, outlining how such a service–funded and administered by government–might be organized. Weaver and Mann endorsed this new emphasis: "a start should be made now in the difficult but absolutely essential business of introducing into the actual agricultural practices of Mexico the improved materials and methods being developed. We can't finance an extension system for Mexico, nor is Mexico ready for such, but extension must anyway be started." Within a few weeks of returning from Mexico, Weaver had drawn up a list of matters to discuss with MAP Director George Harrar, among them extension. Harrar agreed with the Advisory Committee that the program should have an additional staff member who would concentrate upon extension and liaise

[16]"Experience," Mann wrote, "justifies confidence that considerable improvement ineconomic and living conditions can be expected from such [extension] methods…They constitute the most direct approach to the relatively early introduction of changes" (1941). For the Survey Commission's view in 1941, see *Agricultural Conditions and Problems in Mexico* (1941). The other key issues cited by Bradfield (1943) were education and cooperative production and marketing. The quotation is from Bradfield (1945). On the change of heart in 1946, see Mangelsdorf (1946). In 1947 Mangelsdorf struck the same note. The technical progress achieved in four years had been amazing, but "whether these achievements can now be translated into … immediate improvement of Mexican agriculture," only time would tell, see Mangelsdorf (1947, 9). Among items suggested for discussion at the October 1946 meeting of the Advisory Committee, Stakman proposed what the foundation might do to compensate for the weakness of the extension system in Mexico. Should the foundation take responsibility for distributing seed of new varieties, he asked? Ideally, the Mexican government should do this, but there were practical difficulties. And if the foundation did decide to do this, the method of distribution might need to be different for *ejidos* and for landowners, Stakman (1946).

[17]*Interview with Bradfield* (n.d.).

with Mexican officials on the development of the service. And it was agreed that Harrar would get the ball rolling by writing to the minister and explaining precisely what, in the MAP's view, the Ministry would need to do in order to get the service underway.[18]

In the event, things developed slowly. In 1947 the MAP held farm demonstrations in various regions, and in 1948 the program appointed Mortimer Barrus to work with the Ministry on designing the extension service. But by 1949 Barrus was not receiving the cooperation he wanted from Ministry staff, and when the foundation declined to intervene on his behalf, he resigned. Thereafter formation of the service dragged along more slowly than foundation officials or experts had hoped, and the MAP's collaboration with the Ministry remained difficult. By the early1950s there was still concern in the foundation that the MAP's considerable technical advances were not being made available to the great majority of Mexican farmers, and some advisors and program staff were clearly frustrated. Reporting on his site visit in 1953, Bradfield noted that, while in principle the use of mineral fertilizers or crop rotation using alfalfa would enormously improve Mexican agriculture, in practice "to handle a system of farming of this more complex type…will require more managerial ability and more capital than many Mexican farmers have at the present time." But meeting those needs required an extension service and improved credit arrangements. The dilemma with irrigation was similar. As Wellhausen (who became director of the program in 1952) later remarked, in areas with adequate rainfall it was clear that by simply applying existing knowledge farmers could produce far more than they currently did. But since water was not being efficiently used, the (predominantly small) farmers who needed it were not receiving it, and how to increase yield in areas of low rainfall was much less clear. Although the MAP was adept at developing technical solutions, only government could provide the necessary infrastructure: "Mexico needs to do something about increasing production in these marginal corn-growing areas because it is in these areas where the population is also beginning to increase… The people from these areas are beginning to march on the cities and form the slums."[19]

Some historians have been critical of the MAP's failure to develop an effective extension program, arguing that its staff "seemed to feel that large grassroots [extension] campaigns were not part of their agenda," an attitude that allowed them to "bypass the vexing

[18]On Mann and Weaver's visit, see *"Topical Diary of Visit to Mexican Agricultural Program; WW and ARM, Sept. 12 to Oct. 6, 1946; JDR 3'd and WIM Sept. 28 to Oct. 6, 1946"* (1946); *"Visit to Mexican Agricultural Program, ARM and WW, Sept.12–Oct. 6,1946; Summary Conclusions"* (1946). In a memo. apparently from Weaver, the items to be discussed with Harrar included "Is there a Mexican who could be trained in the US to head up the extension work?," "How much have Mexicans themselves developed in knowledge, organization and personnel which could be used in the extension service?" and "To what extent have Mexican scientists collaborated with RF personnel? Are they sharing in the RF [projects]?" *"For Discussion with Harrar"* (1946). On Harrar's agreement, see *"Topical Diary of Visit to Mexican Agricultural Program"* (1946); *Minutes of meeting of Advisory Committee for Mexican Agriculture* (1946); *"Report on the Mexican Agricultural Program prepared by JGH"* (1946).

[19]Fitzgerald (1986, 471–72); Cotter (2003, 197–98). On collaboration with the ministry, see *Minutes of Nov. 1948 meeting of the Advisory Committee on Agriculture* (1948); Bradfield (1953). Among the advisors, Bradfield was most aware of the ways in which technical solutions had to be adapted to farmers' circumstances as well as of the role that extension and other non-research measures could play in agricultural development. This broader vision was again evident from the 1960s when, as head of the Multiple-Cropping Systems Division at the International Rice Research Institute, he was repeatedly critical of the institute's single-minded focus upon producing high-yielding rice varieties dependent upon irrigation. This ignored the needs of two-thirds of Asian rice farmers, he argued, whose land was neither irrigated nor suited to rice monoculture, see Anderson et al. (1991, 42–46, 86–88). On Wellhausen's career, see "Wellhausen" (1982). On water use, see de Alcantara (1976, 52–309). The second quotation is from *Interview with Wellhausen* (n.d., 56).

problems of rural poverty." One problem with this critique is that it is difficult to imagine how a program as small as the MAP during the 1940s—with relatively limited resources and half a dozen scientific staff—could have mounted anything but a token extension effort. More fundamentally, what the criticism overlooks is the extent to which MAP staff and advisors were aware during the early years of the program that extension was essential and that it would be necessary to assist the Mexican government in strengthening the existing service.[20]

The fact that the MAP's work failed to reach peasant farmers was not only due to the shortcomings of the Mexican extension system. For, although Weaver and Mann had called early on for more attention to be given to studying the "economic matrix in which the scientific agricultural studies are placed," the MAP was slow to move in that direction. One of those who backed Weaver and Mann's call was William I. Myers, professor of agricultural economics at Cornell and a trustee of the foundation since 1941. Soon after the MAP started, he had spoken with Harrar and Weaver about the need to bring in agricultural economics to supplement the biological sciences and make the MAP a more rounded research program, but with little success. For one thing, the natural sciences and social sciences were situated in different divisions of the foundation and had no experience of collaboration. For another, despite Weaver's verbal support for studies of economic context, he occasionally expressed the view-to Myers's annoyance-that farming was "just applied biology." The MAP staff had been trained in biological sciences with little exposure to either economics or rural sociology, and "they were suspicious of what they didn't know." Myers kept pushing. Responding to a MAP progress report in 1951, he remarked that although he liked the report, it was optimistic on what remained to be done. "We have not yet made even a beginning in the study of economic and social problems that are also important in improving the general level of well-being of the countries concerned." The foundation, he thought, could research such problems and suggest methods of solving them. By this time even Weaver was coming around to the idea that more work on economic questions was needed, writing to the Advisory Committee that he thought future grants for agriculture should include not only scientific work but "studies of a less specifically scientific nature but also addressed to long-range limiting problems of agriculture (water, land tenure, taxation, etc.)." But Weaver was either unwilling or unable to push this idea since nothing happened for several years. When Stakman recommended in 1954 that the MAP hire a rural sociologist and an anthropologist, one foundation official agreed but thought that such staff only needed to be added in the course of the next decade. A social science perspective on agricultural development was evidently not a priority within the foundation, and it was not until 1956 that an agricultural economist was finally added to the staff.[21]

[20] The quotation is from Fitzgerald (1986, 42–43). Matchett's view that the Advisory Committee's decision in 1941 to take a "top-down" approach was a "defining moment at which any immediate plan for an extension program lost most of its ground to a program built primarily around research" (Matchett 2002, 81–84) misses the lack of initial consensus among advisors, as well as the growing concern with it among both advisors and officers by the mid-1940s. Even by the mid-1950s, when the MAP had eighteen foundation scientists and over one hundred Mexican scientists, the program was not large enough to provide an extension service, see de Alcantara (1976, 21–86). What the MAP might have attempted was a manageable pilot program aimed at peasant farmers in one region. If well done, this might have stimulated farmer interest in the MAP's work and demonstrated to the Ministry how the new extension service should be designed. Whether this possibility was conceived or attempted, I do not know.

[21] The first quotation is from *"Visit to Mexican Agricultural Program, ARM and WW, Sept. 12–Oct. 6, 1946; Summary Conclusions"* (1946). Sauer voiced concern in 1945 about what he saw as pressure to introduce "American methods unsuited to the country. The same thing is true all over Latin America, where Argentina is the only country

Why, then, did the MAP give up its original aim of raising productivity on peasant farms? That is the crucial question, but the answer is undoubtedly too complex to develop in depth here. What is worth doing, however, is to outline a number of hypotheses that emerge from the archive and that merit further consideration by historians.

While the views and recommendations of foundation officials were important, the MAP's actions obviously depended as well upon the field decisions of its own staff, above all, Harrar. And there are indications that Harrar did not always see eye to eye with either officials or advisors. In the autumn of 1946, for example, when Weaver and Mann had returned from Mexico, persuaded that more attention should be given to the economic context in which scientific agriculture was practiced, Harrar objected that adding an agricultural economist to the program would be "dangerous." His view may have prevailed, since it was another decade—by which time Harrar was no longer director—before an agricultural economist was finally appointed. Similarly, although the MAP's original focus was on maize, beans, and wheat, by the spring of 1946 Harrar was interested in expanding the scope of the program to include crops with export potential such as fruit, vegetables, sugarcane, oil-bearing plants, pharmaceutical plants, and rubber. Whether he conveyed this proposal to the minister of agriculture is not clear, but it may be significant that when the minister met with Weaver and Mann later that year, he emphasized that the most important tasks for the MAP were maize, beans, and wheat; other crops could be taken up later. Why might Harrar have deviated from the general line adopted by officials and advisors? There are hints that personality may have played a role. Mangelsdorf, for example, said that "There is some danger that a man of Dr. Harrar's temperament, eager to get things done, and constantly confronted with immediate problems, will tend to lose sight of the long-time objectives of the program." There was certainly no shortage of "immediate problems." As Stakman later recollected, staff were inundated right from the start with people wanting them to work on particular projects that had nothing to do with the main food crops (e.g., improving limes, vanilla, and coffee). If the foundation granted Harrar considerable operational freedom–and there are signs that it did–his conception of what the program should do may have diverted its work away from the direction envisioned by the advisors.[22]

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that was designed to fit into the North Atlantic pattern of agriculture. Are these proper questions for social science? I think they are," Sauer (1945). In 1946 Myers recommended adding an agricultural economist, not only to the MAP staff but also to the Advisory Committee, see *"Topical Diary of Visit to Mexican Agricultural Program"* (1946) According to Jennings, in 1949 Myers urged Harrar to offer an institutional home to a sociologist who was investigating the extension process in Mexico, Jennings (1988, 122–23). The second quotation is from *Interview with Myers* (n.d., 56–82). Although impressed with the MAP's technical achievements, Myers's disappointment with what he saw as the program's "one-sided" character prompted him to set up a small foundation focused on the economic and social dimensions of development: the Agricultural Development Council. The third quotation is from Myers (1951); Weaver (1951b). Two years earlier the trustee, John S. Dickey, had called upon the foundation to add a social scientist to the MAP, see Cotter (2003, 205). For Stakman's recommendation, see Stakman (n.d.). That economics came so late to the MAP is cited by historians as evidence of the program's narrow conception of development; what nearly all of them have missed (the exception being Jennings) is the strength of the opposing view among advisors and officers.

[22] *"Topical Diary of Visit to Mexican Agricultural Program"* (1946). Three years later Harrar again declined to take up Myers's suggestion that the MAP take seriously the social dimensions of agricultural development, see Jennings (1988, 123). On Harrar's expanded program and the ministry's reaction, see Harrar (1946b). Despite its original peasant friendly intentions, the MAP was slow to get started with research on beans, beginning its work in 1949, and it was 1954 before expenditure on beans reached one-half of that spent on wheat, see de Alcantara (1976, 25). The second quotation is from Mangelsdorf, "Report on a Trip to Mexico" (1943a, 4). On limes, etc., see Stakman

The problems in implementing the original vision of the MAP, however, were not confined to matters of governance. For the program depended on the Agriculture Ministry's extension service to disseminate improved cultivation practices to the peasantry. And a change of government in 1947 brought a different policy on extension, with the result that Harrar's efforts to persuade the new leaders of its importance ran into "many obstacles and a good deal of resistance." A service was finally established in 1953, but even by the mid-1960s it had not developed as quickly or satisfactorily as he would have liked. The most serious obstacle from the program's point of view centered on the mechanisms for the distribution of seed from the MAP's improved maize varieties. As early as 1946 the Advisory Committee was concerned about the government's arrangements for multiplying and distributing the new seed. Although Stakman conceded that ideally a state institution should distribute the seed, in practice he and the others worried that the proposed new system was open to abuse and that seed would be made available to large farmers rather than to small ones. The solution, he suggested, might be for the MAP to distribute its own seed (although how its staff could have managed such a gigantic task is not clear). But his suggestion was not taken up, and by the spring of 1947 Stakman was disturbed to see that responsibility for distribution had been assigned to the newly established National Corn Commission. The MAP, he reckoned, would have to make the best of the situation and try to prevent the same thing from happening with seed from other crops.[23]

The resistance that the MAP encountered while trying to improve the extension service is not surprising, given the Mexican economic and political situation after 1940. As de Alcantara, Lewontin, and others have argued, the program of rural development championed by the left-inclined government of Lazaro Cardenas between 1934 and 1940 was abandoned by the center/right governments that came to power over the next twelve years. The latter's power base consisted of an alliance between urban businessmen and large landowners who agreed that public funding should be channeled toward "progressive" commercial farms

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(1971, 976). In 1949 the president of the foundation appears to have overruled Weaver in favor of giving Harrar such operational freedom, see Jennings (1988, 120).

[23]The first quotation is from *Interview with J. George Harrar* (n.d., 183). Wellhausen was similarly disappointed. The gains in yield through improved fertilization practices were largely confined to "better farmers." Only a few *ejidos* managed to do this, and there was a risk of rural riots among poor farmers unless the government provided more assistance for them via extension, *Interview with Wellhausen* (n.d., 169).

Lewontin finds it "remarkable" and "ironic" that Stakman et al. should have been so critical of the National Corn Commission since in his view "the Foundation … had come to Mexico with the intention of cooperating with just these groups [large farmers]," Lewontin (1983, 175–165). But this is remarkable only if one has missed the fact that the original conception of the MAP was far more peasant friendly than has yet been recognized. On Stakman's reluctant conclusion, see Stakman (1946); *Minutes of meeting of Advisory Committee for Mexican Agriculture* (1946); Stakman (1947). During a site visit in 1947, Mangelsdorf learned that the Corn Commission had been set up by a friend of the new president and two of this friend's associates. Opinion was divided among Mexican agricultural scientists on whether or not these three would appropriate the commission's funding but do nothing, Mangelsdorf (1943a). But the Corn Commission's vulnerability to corruption was not the only problem with the state distribution system. According to Matchett, the commission possessed inadequate facilities for multiplying the seed provided to it by the breeders, and its staff did not take sufficient care to maintain seed quality, Matchett (2002, 218). Moreover, the commission favored hybrid maize varieties and made little effort to multiply and distribute the open-pollinated varieties developed by the MAP, see Lewontin (1983, 166–74). Finally, in addition to the president's Corn Commission, the Ministry chose to setup its own rival organization-the National Commission for the Increase and Distribution of Improved Seeds-which sought to control the distribution of MAP seed. The result, as Harrar put it, was that "progress was not as rapid as it might have been," *Interview with J. George Harrar* (n.d., 96); *Interview with Wellhausen* (n.d., 102–104). On the chaotic history of the two corn commissions, see Fitzgerald (1986, 466–67); de Alcantara (1976, 74–75).

rather than "backward" peasants (through irrigation works, subsidizing wheat prices, and generous credit terms for big farms), and the large agricultural surpluses thus generated would provide capital for the industrializing economy. Since large farmers received the technical information and assistance they needed, only a rudimentary extension service was required. Thus "post-Cardenas governments gave extremely low priority to the kinds of programs required to support production within commercial *ejidos* [peasant communes]."[24]

Unfortunately the limitations of the extension service, as MAP staff saw it, derived not only from a lack of government enthusiasm; cooperation with Mexican scientists was also hindered by professional rivalry as well as a clash of cultures. On the one hand, Mexican agricultural scientists appear to have been sensitive about well-funded foreign scientists—especially from north of the border—arriving at the Ministry and questioning traditional ways of doing things. Some of the maize breeders, for example, did not want the MAP's new varieties to displace older ones. On the other hand, the Mexicans' notions of professional status hindered good working relations with peasant farmers. From the visitors' perspective, Mexican agricultural expertise was "book knowledge," not grounded in hands-on experience. Research that took the scientist into the field–something that MAP staff took for granted–enjoyed little status, and even younger Mexicans thought that fieldwork was beneath them. That most Mexican scientists had little sympathy with peasants and were inclined to lecture them only aggravated this problem.[25]

Finally, the MAP had to deal with political issues. As representatives of an American foundation-and especially one with connections to the oil industry–MAP staff had to tread lightly when offering advice that had policy ramifications. One such issue was land reform. Following the revolution of 1910, peasants had been given tracts of two to four hectares each (five to ten acres), and this farm size remained enshrined in land reform policy through the 1940s. But MAP experts believed that such small farms only could allow subsistence but no surplus production for the market and thus no purchasing power for the peasant. It would be better, they concluded, for land reform in future to parcel out larger tracts. But to voice such views publicly might have jeopardized the program. As Myers later noted, something of this kind had occurred in the Philippines where a western agricultural economist had criticized the landholding system for preserving many large farms, provoking an outcry that nearly shut down the development program. In Myers's view, the economist's analysis was actually correct, but: "You don't go into a country … no matter how much you know and

[24]The quotations are from de Alcantara (1976, 311–307); Lewontin (1983, 114–20). Possibly, extension was not the only arena in which MAP proposals fell upon deaf ears. In 1948, for example, the Advisory Committee agreed that the development of local crop improvement associations should be "cautiously encouraged," see *Minutes of Nov. 1948 meeting of the Advisory Committee on Agriculture* (1948). What happened to this recommendation is not clear, but according to de Alcantara, from the 1940s the formation of such cooperatives among large landowners was common, while among peasants in receipt of government credit it was illegal, de Alcantara (1976, 311–51). Such associations had also been promoted early in the twentieth century by staff at the Bavarian plant-breeding station who saw them as a way to facilitate the introduction of new cultivation practices on small farms.

[25]On resistance to new varieties, see Matchett (2006); Cotter (2003, 190). Mangelsdorf had the impression that neither of the two most important Mexican maize-breeding groups were prepared to coordinate their work with that of the MAP, Mangelsdorf (1943a). On Mexican aversion to field work, see *Interview with J. George Harrar* (n.d., 36); Olea-Franco (2001, ch. 6); Cotter (2003, 192–93). In his early maize-breeding work, Wellhausen found it easier to cooperate with farmers than with experiment stations whose staff struck him as jealous and overly concerned with their personal reputations, *Interview with Wellhausen*. Cotter judges Mexican agricultural scientists to have been more concerned with enhancing their professional status than aiding the peasantry, Cotter (2003, 156–57, 203–204, 324–26). One consequence was that in the 1930s Mexican experiment stations were giving more attention to export crops than to maize, see Matchett (2006, 353).

how bad their situation is–and tell them what's wrong and what ought to be done about it. . . . [The problem of large landholdings] *is* one of the basic problems of Latin America–but you can't solve it when you land in the country–[and] tell them how lousy their system is." Moreover, the history of US intervention, both military and economic, as Harrar recognized, made Mexicans suspicious of American motives, extending even to plans for the reform of Mexican agricultural education. Besides keeping their mouths shut on land reform, MAP staff tended to stay clear of the US embassy because, as Stakman recollected, "I think at that time the Foundation felt that they should retain their independence of action and avoid any appearance of being political."[26]

Faced with the scale and complexity of the problem of disseminating MAP results to small farmers, the MAP's lack of control over the extension service, and the need to steer clear of offering advice on basic questions of agricultural policy, there are signs that the staff began to concentrate upon problems where they thought they could make progress. In 1947, for example, Wellhausen argued that areas where a maize surplus could be produced should be addressed first; subsistence areas could be left until later. And in 1951 Bradfield recommended to the Advisory Committee that: "Since the improvement of agriculture is dependent upon improvements in education, health, transport and the availability of [inputs], priority should be given to the few situations where a well-rounded program of development seems most probably [sic] over situations which are not yet so ready." Similarly, when asked in the 1960s how maize yield could be increased in areas of low rainfall, Wellhausen replied, "I don't really know. These areas we have not been too concerned about. We've … concentrated our efforts on the areas which are more productive from the standpoint of corn." This tendency to take the path of least resistance may help to account for the effort eventually devoted to wheat breeding. For one thing, the Mexican government was keen to boost wheat production to meet a growing urban demand and reduce imports, but extension problems were also easier with wheat since growers were literate and had the capital to take advantage of intensive methods. As Myers later suggested, the rapid success of the wheat program was so spectacular—Mexican output trebled in only ten years and by 1958 the country was exporting wheat—that "probably they haven't given adequate attention to other things" such as farm management. It would appear, therefore, that work on easier problems aimed at large commercial farmers gradually displaced that designed to help peasants.[27]

To conclude, Rockefeller experts and officials believed during the early years of the MAP that the program could and should make an impact upon Mexican poverty and hunger. To this end, they endorsed a program of extension, varietal-testing, and breeding designed

[26]That the foundation was cautious about development programs that might be seen by the host country as controversial is evident from the mid-1930s when an official familiar with Mexican conditions recommended to the foundation's president that although education was a delicate issue, agriculture was relatively uncontroversial, see Lewontin (1983, 91–92). Weaver seemed to have absorbed this message as he remarked that assisting countries with elementary education was "obviously hot politically," Weaver (1946b). On land reform, see Mann (1943); Cotter (2003, 189). The first quotation is from *Interview with Myers* (n.d., 86). On suspicion of American motives, see *Interview with J. George Harrar* (n.d., 46); *Interview with Elvin Stakman* (n.d., 211). The second quotation is from Stakman (1971, 974).

[27]On Wellhausen's view in 1947, see Cotter (2003, 196). The first quotation is from Bradfield (1951). The second quotation is from *Interview with Wellhausen* (n.d., 56). A decade later he noted that there was still little effort devoted to the development of drought-tolerant varieties and that this task would be harder than the kinds of breeding that had been undertaken during the 1940s and 1950s, see Wellhausen (1976, 148–50); Lewontin (1983, ch. 6). On extension and wheat growers, see Dalrymple and Jones (1973, 16–26); Myren (1970). The third quotation is from *Interview with Myers* (n.d., 67).

to help the small farmer. By the 1950s, however, the program had taken on a rather different cast. Just why the original vision became derailed is not clear but may include factors such as the director's own convictions as to how the program should develop, difficulties in collaborating with Mexican experts, and a lack of control over key institutions such as the extension service. The resulting *modus vivendi* was a division of labor in which the MAP concentrated upon research and training, while leaving responsibility for extension to the Ministry. And, although the success of their research in alleviating hunger was ultimately dependent upon economic and political policies that established the framework in which the new technology would have to operate, this policy arena was one that MAP staff and foundation officials sought to avoid. As a result, the program's greatest impact, despite its original intentions, was upon large farms. For some this remained a disappointment. When the Advisory Committee returned to Mexico in 1962, one of the things that disturbed them was to see that, despite the useful knowledge and practices the MAP had generated, the lot of the Mexican peasantry remained largely unchanged. As Mangelsdorf remarked, "Some of this has [also] happened in the US … I don't know what the answer is."[28]

Demonstrating that the MAP was a dynamic entity, capable of shifting in response to circumstances, opens up a new area of inquiry for future studies, not only of the MAP but also of other green revolutions. As the first of these programs, the MAP was important as a model for subsequent programs in Latin America and elsewhere, and it remains to be seen whether the later programs succeeded in learning from the MAP's experience or blindly reproduced it.

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[28]Stakman et al. (1967, 214). According to one source, in 1960 over 80 percent of Mexican farmers' families were living at a subsistence level or worse, see de Alcantara (1976, 310). The quotation is from *Interview with Paul Mangelsdorf* (n.d., 110).

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