

THE MENDEL NEWSLETTER

Archival Resources for the History of Genetics & Allied Sciences

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A SPLENDID SUCCESS

As promised in this newsletter last year, the American Philosophical Society Library hosted the October 2004 conference, "Descended from Darwin: Insights into American Evolutionary Studies, 1925-1950". In total, eighteen speakers and over thirty participants spent two days discussing the current state of scholarship in this area. Some papers focused on particular researchers and their theoretical projects. Others worked to place work from the period into larger historical contexts. Professor Michael Ruse delivered the keynote address, a popular lecture on the differences in emphasis when evolutionists present their work in public versus professional spheres. It was a capacity crowd and a roaring success. Thanks to the 'Friends of the Library' for the grand reception.

This conference had a real buzz about it. I had the sense we scholars are on the brink of significant developments in our understanding of the period. Moreover, considerable progress is being made on how we might relate this period to research underway in the decades before and after. New archives, new ideas, new opportunities.

As organiser, I'd like to express my thanks to the participants for the hard work done to prepare. Also, I'd like to thank the Library's staff, especially Ms. Sandy Duffy. She's a gem. This conference was made possible through the generous support of the Barra Foundation in honor of Professor Frederick H. Burkhardt. Additional support was provided by the American Philosophical Society, Florida State University, and University College London.

The conference programme and some abstracts remain on-line: www.ucl.ac.uk/sts/cain/synthesis.

A proceedings volume, *Descended from Darwin*, with contributions from speakers and additional workers in the subject, is in preparation.

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Attendees at the Burkhardt Conference on the steps of Library Hall. October 2004.

The Correspondence of the Tring Museum at the Natural History Museum, London

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When Walter Rothschild (1868-1937) turned twenty-one, his parents gave him a plot of land on the family estate in Tring, on which was built two small cottages and a larger building to house his growing collection of natural history specimens. The museum that resulted was developed into both public galleries and a private collection, with an emphasis in the latter on butterflies, moths, and birds. By the time of Rothschild's death, his museum housed some two million specimens of Lepidoptera and thousands of mounted mammals and birds (this after most of the bird collection - 280,000 specimens - was sold to the American Museum of Natural History in New York in 1932). Today Rothschild's entomological collection and the public galleries are property of the Natural History Museum, London (NHM). Although the entomological collections are now in London, the NHM transferred its ornithological collection to Tring in the early 1970s, and both new and original buildings now house the NHM's Bird Group. The public galleries are open to the public and provide a fascinating example of a nineteenth century private natural history museum (<http://www.nhm.ac.uk/museum/tring/>).

During the "heyday" of the Tring Museum, as it became known, Walter Rothschild and his two curators, Ernst Hartert (1859-1933 - ornithology) and Karl Jordan (1861-1959 - entomology) amassed the largest private natural history collection in Britain and produced systematic works that still astonish biologists for their quality, comprehensiveness and the rapidity with which they were produced. Ernst Mayr has cited the "Tring triumvirate" as having completed brilliant examples of the so-called "new systematics" due to their conscientious emphasis on geographical variation, their use of trinomial nomenclature, and their dedication to the study of systematics as a foundation for evolutionary biology (Mayr, 1955. Also see the studies listed below by Haffer and Stresemann). Indeed, Karl Jordan often insisted that the collection and the work completed using its specimens provided critical data with which to check the, at times, overly-enthusiastic claims of the rising field of genetics during the first half of the twentieth century.

To amass this collection and carry out the excellent systematics for which they became known, Rothschild and his curators relied on a network of correspondents that included fellow ornithologists

and entomologists in the museums of the capitals of Europe and the Americas, natural history agents, colonial administrators, hobbyist collectors from the farthest reaches of the Empire, and biologists intent on bringing natural history safely into the twentieth century. Much of the correspondence that resulted is focused on the central aim of compiling the best and most scientific collection of those animals of particular interest to Rothschild. Within these letters and exchanges is the story of how the raw material of systematics - and in turn biology more generally - was amassed and in what context. The correspondence provides a window into the natural history network of another world, in which a wealthy collector could still use the infrastructure of Empire to bring back thousands of specimens.

The Tring Museum Correspondence is currently housed in London in a large series of engraved archival boxes. The correspondence begins in 1892, and ends within the Karl Jordan Correspondence, at Jordan's death in 1959. Miriam Rothschild calculated that the correspondence would take two years to read, working a normal 8-hour day (Rothschild, *Dear Lord Rothschild*, p. xv). Currently, volunteers are cataloguing the collection electronically. Amazingly, this extensive collection is in fact only a portion of the original museum records. The Tring Museum diaries and ledgers have been lost, as have all but four of the museum's books of out-letters written by the curators between 1892 and 1908. Despite these losses, the preservation of the incoming museum correspondence since 1892, and out-letters after 1908, provides a wealth of information on the activities of collectors, their relations to naturalists back home, the influence of Empire, international conflicts, and nationalism on the natural history network, and the sheer breadth of the naturalist community, from natural history agents to the conscientious pastors sending specimens home from their parishes on the banks of the Amazon. The collector Albert Meek alone wrote 500 letters, for example, describing his collecting activity in the Solomon Islands. Some of the more well known correspondents include E.B. Ford, E.B. Poulton, Ernst Mayr (who once collected for Rothschild), the ornithologists Erwin Stresemann and Otto Kleinschmidt, and the German entomologist Walther Horn. The task of those searching for specific collectors and correspondents' is made easier by the fact that the correspondence is alphabetized for each year, including those letters

not yet electronically databased.

Miriam Rothschild's entertaining, insightful, and very personal biography of Walter Rothschild, *Dear Lord Rothschild: Birds, Butterflies and History* (Balaban Press, 1983), is one of the few sources available on the Tring Museum, although a study of systematics in the twentieth century based largely on the life of Karl Jordan is in the works. Aside from these studies, the Tring Correspondence remains a largely un-mined source for the history of natural history and Empire, collectors, taxidermy, and the entomological and ornithological networks in Europe at the turn of the century. Letters are primarily in English and German, with some in French and Spanish. The collection can be supplemented by the manuscripts and archival materials still held at Tring (Warr, 1996), as well as the Natural History Museum's own institutional archives, not to mention the dozens of archives of natural history museums throughout the globe with which the Tring naturalists corresponded. Anyone interested in expanding our understanding of the naturalist tradition in the late-nineteenth century and early twentieth century will find fascinating and important insights within this extensive, highly cosmopolitan correspondence.

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The Cyril Dean Darlington Papers

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Until recently, the name of British cytogeneticist, evolutionary theorist, and all-round controversialist Cyril Dean Darlington (1903-1981) elicited the kind of reaction reserved for subjects of gravity or importance not entirely remembered or understood. Historians of biology were aware somehow of Darlington's impact and centrality, but few really knew what had been his life's work, much less the circumstances which fashioned him one of the deepest, but most resisted scientific thinkers of his day. Darlington's career and life spanned much of the twentieth century, and his activities were profoundly connected to the momentous shifts and turns in biology and to the story of the social relations of science both before and after World War II. Whether it be Darlington's beginnings at William Bateson's John Innes Horticultural Institution when chromosome theory was still struggling to find a foothold in England; his eventual contributions to cytology, genetics and evolution, accomplished at a staggeringly young age and almost entirely without guidance; his involvement with the Lysenko Affair at a time when a political and ideological battle was being waged in the West over the proper role of science in the modern world; or his early and controversial sociobiology, attempting to understand the rise and fall of nations, social stratification, class and race through the guise of biology, Darlington both reflected and impacted upon the paths of science taken by his times, and, consequently, upon the generations that followed after him.

Something of the impact of Darlington's work can be gleaned by consulting his obituaries, which referred to him as the "Copernicus" or "Newton" of cytology, or by referring to Bill Hamilton's comments about the enormity of Darlington's 1969 book *The Evolution of Man and Society*, and to the publisher's Allen and Unwin's judgment that it ranked alongside Darwin's *On the Origin of Species*, Toynbee's *A Study of History*, and Russell's *The History of Western Philosophy* as a publication event. Darlington was a major scientist who turned from the miniscule, silent world of the chromosomes and their movements to the bigger and wider field of human affairs, titillating and challenging many minds in the process. Striving to make connections unseen or uninvited by most, he also made many enemies, or worse, suffered the harsh penalties born of being branded irrelevant and reactionary. Among other reasons, this played a role in his disappearance to history: besides a Royal Society Biographical Memoir written by Darlington's old John Innes colleague Dan Lewis two years after his friend's

death, no account of Darlington's life and career had been produced until now.

I mention my recent book, *The Man Who Invented the Chromosome: A Life of Cyril Darlington* (Harvard, 2004) not only to refer readers to its hero's fascinating and forgotten tale, but also to his collection of papers housed in the Bodleian Library of Oxford University, which served as the main source for the book. This vast resource has been consulted sporadically over the years, since it was deposited in the Modern Papers Room in 1985, primarily by researchers interested in Darlington correspondents, but not primarily in Darlington himself. This is a shame, for I have seldom come across a more complete and rich set of papers belonging to a scientist. Darlington literally kept everything, and when consulting his papers this author, at least, got the feeling that his subject had known that someone would someday come along to tell his tale, and wanted to provide all the necessary materials for the task.

Like the papers of other Fellows of the Royal Society, the collection has been handsomely and efficiently indexed by the Royal Commission of Historical Manuscripts, reproduced for the Contemporary Scientific Archives Centre in two neatly bound blue volumes. Its nine sections, divided into hundreds of folders, are easily accessed and easy to use, and I would like in what follows to present a somewhat more detailed description of their contents for the use of prospective scholars.

Biographical and Personal

In this section are to be found Darlington's more personal materials. These include a vast collection of revealing and highly personal diaries and jotters, often in small red leather notebooks; reports from elementary school, high school, and university; an extensive family correspondence (minus two letters from his son Oliver written after the suicide of his brother, and Darlington's son, Andrew, in 1970, access to which has been blocked by Darlington's literary executor - and stepson - Professor Paul Harvey); photographs, including many of his youth and family; and a substantial collection of press cuttings that provide a wonderful chronicle of Darlington's interests throughout his life.

John Innes Horticultural Institution

Darlington joined Bateson's institute as a volunteer unpaid worker at the age of 20 in 1923, a recent graduate of the South Eastern Agricultural College at Wye, and an even more recent reject

of the Empire Cotton Growing Corporation, who found his application for a scholarship to go as a farmer to Trinidad unworthy. Unsure of his path, and scientifically naïve, Darlington fell under the spell of another young researcher, Frank Newton, who was the only person looking at chromosomes at the Innes, Bateson having expressed his disapproval of their alleged role in heredity. Here, within five short years following both Bateson's and Newton's death in 1926 and 1927 respectively, Darlington rose to become the world's leading cytologist, producing the very bible of the field, *Recent Advances in Cytology*, in 1932. This section of the archive includes all the materials relevant to Darlington tenure at the institute, from 1923 through 1939, when he became its Director, and until 1953, when he left to become Sherardian Professor of Botany at Oxford University. These include both scientific notebooks and drafts, playful and not so playful correspondence between the John Innes colleagues (including J.B.S. Haldane, Kenneth Mather, Daniel Hall, Dorothea de Winton, Pio Koller and many others), and administrative materials pertaining to the institute, including minutes of meetings of the Council. There is an extensive collection of papers pertaining to the decision to move the John Innes from its former location at Merton to the location at Norwich where it resides today, a move which brought with it a complete physical and scientific restructuring of the institution.

Oxford

At Oxford Darlington fought innumerable battles both in his own department, which had yet to incorporate genetics, and with the university administrators and heads whom, Darlington felt, failed both to integrate science successfully into the general curriculum, and to integrate the various science departments around the common theme of genetics. This section includes extensive documentation of the Franks Commission in 1964, an inquiry whose aim it was to reform higher education in Britain. It also includes minutes of meetings pertaining to Darlington's battles at the university, and will be useful to researchers interested in higher education in England in the period 1950-1970 in general, and in Oxford in particular.

Research

Darlington's collection of reprints, some 300 boxes worth of material, alphabetized by subject and author, does not exist in this collection, but rather behind glass windows attached to large oak cases, at Magdalen College, in the beautiful room of Professor of Genetics David Roberts facing the deer park. The collection had been housed in the halls of the Genetics Department for years, until Roberts offered to save it from a planned, historically unfriendly, departmental cleanup.

Access to the collection can be accomplished via Roberts directly. The "Research" section at the Bodleian, on the other hand, includes substantial files devoted to the history of science and scientists, notably William Bateson, Nikolai Vavilov, Russian genetics, and the Lysenko Affair.

Publications

Darlington published widely on chromosomes, genetics, man, education, politics, botany, and history. This extensive section conveniently orders almost all of Darlington's publications chronologically by year. It includes correspondence with publishers and pre-publication readers, rough drafts (and the telling differences between them and the final publications), and reviews. The most interesting parts pertain to Darlington's three books on man, *The Facts of Life*, *The Evolution of Man and Society*, and *The Little Universe of Man*, all of which encountered both praise and strong condemnation.

Lectures and Broadcasts

The broadcasts are often of controversial topics: evolution, heredity, Russian genetics (including a 1948 B.B.C. broadcast on Lysenko in which Haldane painfully evaded criticizing the Russian charlatan), and the conflict of science and society. The collection includes the texts, and, in some instances, reactions.

Societies and Organizations

This is the least rich of the sections. Darlington hated societies and soirees, and perfected the art of declining to join their ranks. Much of the material in this section comprises declinations of one kind or the other. Two exceptions are the materials pertaining to the Society for the Freedom of Science, founded by John Baker and Michael Polanyi in 1940, and the Tots and Quots Club, a society of scientists led by Solly Zuckerman that was active during World War II.

Visits and Conferences

Darlington traveled extensively during his career, and kept a record of all of his meetings, talks, and impressions. Of particular interest is the trip to Persia in 1929, ostensibly in search of chromosomes, but in fact more substantial as the fount of Darlington's initial interest in human diversity and custom; and visits to Japan, India, Australia, New Zealand, South Africa, and the United States as a Rockefeller Fellow in 1932. During this last mentioned visit, Darlington encountered the aging Wilson, Morgan and his group, Babcock, Beadle, Dobzhansky, Haldane, and many others.

Correspondence

This is the section which has been most consulted by researchers until now. Darlington's correspondence was abundant, and, like Charles Darwin's, remarkable for its breadth and variety. Alongside scientific colleagues and friends such as Karl Sax, C.H. Waddington, Henry Dale, J.B.S. Haldane, R.A. Fisher, Julian Huxley, E.B. Ford, H.J. Muller, Michael Polanyi, Theodosius Dobzhansky, G. Ledyard Stebbins, Ernst Mayr, Walter Bodmer, Russian, Indian, Japanese and continental colleagues of note, there are also correspondence with philosophers (Karl Popper), plant and animal breeders, historians, archaeologists (Vere Gordon Childe), educationalists (Cyril Burt), psychologists, linguists and racialists (Coon, Baker). Not all of the return letters exist, but many do, as do jottings on the incoming letters themselves, indicating the response.

Scholars familiar with the Modern Papers Reading Room at the Bodleian will know that Colin Harris and his staff are immensely helpful, and make working in the room a joy. It is my hope that a growing number of researchers will begin to consult Darlington's rich and important collection of papers, and continue the work I have only begun on this fascinating scientist and historical figure.

Joseph Henry Woodger (1894-1981) Papers at University College London

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Some years ago I searched for archival materials related to Joseph Henry Woodger, philosopher, biologist and advocate of logical positivism (Cain 2000). The hope was to assess the extent of informal contact with those pursuing synthesis in evolutionary studies, 1920s-1940s. I found little connection to these evolutionists when searching their personal papers, then went in search of an archives for Woodger. I did not find such a collection. Worse, I was told by reliable informants that Woodger's papers and correspondence had been destroyed.

I now can report those informants were wrong. A significant deposit of Woodger's papers and correspondence was made to the Library of University College London in 1990 by Woodger's family. Twenty-five boxes (approximately 12.5 linear feet) were deposited. A box level inventory is held by the Library. The collection contains material between 1922-1980, covering Woodger's professional career. In general terms, this archive contains research notes, published and unpublished manuscripts, and correspondence. Most correspondence is incoming, focusing largely on research and administrative matters. For close collaborators, some information on personal and period issues also can be found.

Currently, the Woodger Papers require additional processing, so access is restricted. A box is available from UCL Library Special Collections. Researchers interested in using the collection are strongly advised to contact the archivist for an update. The current URL is: < www.ucl.ac.uk/Library/special-coll/ >

I'd like to thank the staff of UCL Library Special Collections for their generous assistance in my examination of these papers. This note is not associated with their processing of this collection.

Three phases to his career

These papers give the impression of three broad phases in Woodger's scholarly career. The first phase was defined by his employment at the medical school of The Middlesex Hospital in central London (Thomson 1935; Ranger 1985). Woodger (1924) established, in English medical circles, his reputation as a skilled and thoughtful instructor. He was known as a skilled histologist and preparator. This textbook also presented him as someone with important ideas for larger educational goals in the medical curriculum. Supporting him

were those wishing medical students memorised less and critically reflected more.

The second phase has Woodger (1929) as a landmark. This book offered an analysis of dichotomies supposedly structuring fundamental problems in biological research. This grew from Woodger's interest in experimental embryology and from interactions with similarly inclined developmental biologists in the Cambridge-Oxford-London triangle, especially Joseph Needham and Gavin de Beer (Abir-Am 1991, 1987). His principal professional organisation along these lines seems to have been the Society for Experimental Biology. Woodger (1929) gained wide attention and was extensively reviewed (many published reviews are in this collection). Commentators focused particularly on Woodger's analysis of the mechanism versus vitalism polarity. In addition to complaining about imprecision in language, Woodger rejected vitalism but demanded more from mechanism. Woodger called for a return to fundamental principles. His notoriety at this point was largely English, though a few other biologists of the same proclivities (e.g., Raymond Pearl) befriended Woodger as a result of this work.

The third phase began in the early 1930s, as Woodger sought tools for increasing the precision of language and reasoning. He became fascinated with Whitehead and Russell (1910-1913) in the early 1930s. Pursuing the logical and symbolic expression of biological theories became Woodger's main research activity thereafter. This led him into the international activities of the Vienna Circle, logical positivism, and the Unity of Science movement (Woodger 1939; 1937). In biological subjects, Woodger followed his interests in development and growth in the 1930s. He became increasingly focused on psychology and logic thereafter. Woodger was especially proud of his invitation to present the Tärner Lectures, 1949-1950 (Woodger 1952). More general writing continued after Woodger's retirement in 1959. He chose to abandon most research in 1978. Woodger died in 1981.

These Papers include several valuable secondary items that seem to have been lost to interested scholars. Marshall Allen's (1975) masters thesis provides an informative biographical introduction to Woodger. Martin (1954) briefly introduces Woodger's axiomatic method. Both collaborated with Woodger.

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Where to Look Next? Agricultural Archives as Resources for the History of Genetics

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Historians in general, but historians of science perhaps even more particularly, are accustomed to archival repositories organized along the rhetorical structures of “great man” history: if one can find archival collections named for the great man (or, occasionally, the great woman) under consideration, one has in one’s hands the crucial basis for historical analysis. But is this really so? In a world of dramatically expanding bureaucracies—those associated with government agencies, private research universities, public universities, funding agencies—in other words, the world of genetics and geneticists in the twentieth century—isn’t it possible that administrative archives—the workaday records of departments and programs in genetics—hold, within their solemn and unsuspected vaults, information crucial to the indefatigable researcher? Especially for those historians whose interests extend beyond intellectual history to the social and institutional context of disciplinary development, such resources are invaluable.

Within the last several decades, the importance of agricultural institutions in the advancement of American genetics has been established beyond question. The archives of these institutions hold records of potential interest to disciplinary historians as they are replete with discussions of who to hire, who to fire, in which fields or programs to expand institutional strengths, which internal programs to dismantle, often along with the personal, intellectual, and financial bases for such decisions. Yet such archives remain under-used by historians of genetics. Here I will offer examples from my own research of how they can enrich and fine-tune one’s analysis.

Perhaps the strongest and most legitimate excuse for neglecting the archival repositories of agricultural institutions is the remarkable richness of other major holdings in the history of genetics. Historians of American genetics are most familiar with two absolutely crucial repositories of archival material: the extensive manuscript collections of the American Philosophical Society in Philadelphia, and those of the Rockefeller Foundation in Tarrytown, NY. In both cases one can find correspondence organized by name or institution, and quickly hold in one’s hands the records of the targeted subject. These archives also house substantial collections of correspondence from researchers located at agricultural institutions. In quite a number of cases—Raymond Pearl and Albert Francis Blakeslee at the APS come to mind—these

researchers moved from agricultural institutions to major research institutes or universities, and the collection of their correspondence bridges these institutional moves. Of what advantage might be a further search in the institutional archives of the agricultural institutions that first employed these individuals, especially given the richness of the APS and Rockefeller collections?

A personal anecdote will make clear the relationship between these two equally valuable kinds of repositories. In a study of many years that I recently concluded (soon to be published in the *Journal of the History of Biology*) addressing the early career of botanist and geneticist Albert Francis Blakeslee, I made extensive use of the Blakeslee Papers at the American Philosophical Society. The letters I found there allowed me to explore his job search after he obtained his Harvard doctorate, his successful pursuit of a job as botanist at the Connecticut Agricultural College at Storrs, the turn of his interests towards genetics, and his decision to remove himself from Storrs to C. B. Davenport’s Station for Experimental Evolution at Cold Spring Harbor. Satisfied with my effort, I sent off the manuscript for consideration. One among the several anonymous referees wondered why, since I was focused on Blakeslee’s career at Storrs, I had not consulted the manuscript collections at that university. Eager to pursue this advice (to say the least), I hastened to explore those archives. There I found, in letters written by Blakeslee’s colleagues to one another and to administrators within the agricultural college, substantial information concerning Blakeslee’s relationships with other agricultural professors with whom he collaborated on applied projects. The difficulties, challenges, resentments that these letters revealed had not been evident in the materials I had consulted at the APS. These letters were inevitably missing from the collection organized under his name at the APS for the obvious reason that they were written neither by him nor to him, but about him. However, even those letters at Storrs actually written by him addressed not his work and professional interests, but rather concerned the number of reprints he was to receive, how and to whom they would be distributed, etc. In other words, these materials did hold information directly relevant to the content of Blakeslee’s scientific work. They were nonetheless extremely helpful to me in evaluating Blakeslee’s professional relationships at Storrs,

and in teasing out reasons why he might have been persuaded to leave.

Similarly, the work and career of E. B. Babcock can be most fruitfully explored by dividing one's time between the collection of Babcock Papers at the APS and the institutional archives of the College of Agriculture at Berkeley (where California's "official" agricultural college was located prior to its relocation to Davis). At the APS one can find Babcock's outlines for courses in genetics, and letters to colleagues about questions genetical. But one learns little from these papers about how Berkeley's and Babcock's histories intersected prior to the founding of the Department of Genetics within the agricultural college in 1913. Indeed, agricultural archives are most crucially helpful to the historian of genetics who is interested in disciplinary development and in the day-to-day tactics and manipulations that are part and parcel of institution- and discipline-building. Babcock was not the earliest advocate of Mendelism and genetics at Berkeley; the institutional archives offer information on the prior institutional initiatives that produced the Department of Genetics, and Babcock's early career at Berkeley. Through these records one can reconstruct how advocacy of Mendelism and genetics at Berkeley provided a catalytic ferment through which economic, cultural, and scientific interests together provided crucial support for the establishment of a department of genetics. These archives show how genetics provided a substrate through which scientific disciplinary development exploited and was exploited by intersecting sets of forces at the levels of the department, university, and state.

These early institutional transformations are precisely those that agricultural institutional archives are well placed to illuminate. Having once identified individuals such as R. A. Emerson and E. M. East, who proved important in the early disciplinary development of American genetics, investigating the records of their home institutions early in their careers—the agricultural colleges of the University of Nebraska for Emerson and of the University of Illinois for East—reveals the institutional pressures, constraints, and opportunities that turned these young men towards careers in genetics. In the case of Emerson, being placed in charge of a horticultural program by Charles Bessey (who preferred to focus on botany as administrative duties at Nebraska pressed deeply into his time) allowed Emerson to explore the application of Mendelism to the enhancement of yield for beans. The Nebraska institutional archives suggest that Bessey, in liberating himself from the demands of administering a horticulture program, in effect liberated Emerson from Bessey's profound commitment to sustaining a uniquely American botany, permitting Emerson to take advantage of intellectual and professional patronage and opportunities proffered by the

USDA and the American Breeders' Association. Emerson, attempting the improvement of bean varieties in the early 20th century, was introduced through these agricultural organizations to Mendelism, and quickly turned his investigations in that direction. Through using in tandem the letters of both Emerson and Bessey at Nebraska, it was possible to piece together the institutional and intellectual priorities of both men, and learn how these institutional circumstances permitted Emerson's shift of interest.

Similarly, investigating the very early career of E. M. East, who turned from his original interest in agricultural chemistry to plant breeding and genetics, requires a visit to the institutional archives at the University of Illinois, where East worked with chemist Cyril Hopkins on corn selection experiments exploring the extent to which selection could increase or decrease oil, sugar and protein content. Certainly, this is the route which diverted East toward the work of Darwin and Mendel. East's career at this time is documented almost exclusively through agricultural materials, published and unpublished. Once he joined the project, the publications of the Illinois Experiment Station concerning these investigations included East's name, but it is through the letters of Hopkins and others among East's colleagues that I learned about the extent of East's contributions to these publications, and about his growing dedication to field work. I also learned from these materials that in 1904 East staffed the Illinois contribution to the agricultural experiment stations' exhibit at the Louisiana Purchase Exposition at St. Louis, at which the "anthropological" exhibits included living colonies of peoples from the recently acquired Philippines. Could this have been one source of East's eugenic interests?

From the above, it is plain that institutional archives answer particular kinds of questions about particular kinds of historical issues. Most profoundly, they reveal the intimate relationship between institutional factors (whether opportunities or constraints) and disciplinary development, on the one hand, and individual careers, on the other. The story of John Detlefsen at Illinois offers the most compelling example I have come across of the trials and tribulations of a young geneticist at an agricultural college. Detlefsen's was one of the earliest doctorates in genetics completed in the United States, earned through research conducted under William Castle at Harvard's Bussey Institution. Detlefsen, therefore, was not one of those botanists, horticulturists, plant breeders, zoologists, chemists, entomologists, and microbiologists who metamorphosed themselves into geneticists. He was a geneticist from the start, a fact that may have contributed to his downfall when hired as a geneticist within the Animal Husbandry department of the University of Illinois. The Dean of the Illinois Agricultural College at the time of Detlefsen's hiring was none other than the

former head of the Animal Husbandry department, Eugene Davenport, whose strong interest in the role of heredity in animal breeding had not produced an equivalent interest in Mendelism and genetics. The hiring of Detlefsen was an acknowledgement that by 1912 the university “needed” a geneticist, but the administrative papers of Davenport’s deanship document the growing tensions between Detlefsen and most of his more practically oriented colleagues at Illinois. They reveal in fascinating detail how and why decisions, first to reward and encourage Detlefsen, and then to confront him with their growing dissatisfaction, were made by his dean and his colleagues.

Finally, I must mention a crucial institutional archive haunted by historians of genetics, housed at the Carl A. Kroch Library of Cornell University. These records can hardly be said to have been neglected by current historians of genetics, but up and coming scholars might benefit from knowing, for example, that the bulk of the institutional holdings of letters to and from R. A. Emerson can be found not within the collection formally titled the Emerson Papers (in which one can find a number of pamphlets, photographs, Cornell publications and interviews, and a few letters), but rather are located within the Records of the Plant Breeding Department, which Emerson headed for decades beginning in 1916. It is within this collection that we read letters, departmental memos, and university reports that reveal the house-cleaning Emerson indulged in upon his arrival, through which he successfully divorced the Cornell genetics program from its early commitment to eugenics. It is here that we learn, through Emerson’s letters to colleagues like East, L. J. Stadler, and former Cornell graduate students E. W. Lindstrom and E. G. Anderson, of Emerson’s efforts to place the Plant Breeding Department’s graduate students in advantageous positions likely to advance their career. Here we can find the letters of those graduate students as well, commenting on Emerson, other Cornell professors, and the Cornell program in general, not to mention other geneticists. But these letters also contain significant scientific discussion; an historian primarily interested in Emerson’s scientific work might have to wade through a great deal of internal administrative trivia, but in the long run would not be disappointed. Plenty of these letters begin with chats about family and friends, and conclude with detailed discussion of the heredity of aleurone color. There is enough to please historians of a wide range of historiographic approaches in this large and incredibly rich archive. Indeed, Cornell University’s archival holdings of the Plant Breeding Department offer exceptional resources for the intellectual as well as institutional history of American genetics, documenting not only the work of a strong institutional leader and researcher, but also how through the work of many such individuals one of the strongest genetics programs in the country was built.

The beauty, as well as the difficulty, of these institutional archives is precisely their extraordinary number and their unpredictability. Having used the Cornell records first, I have frequently found myself disappointed after traveling long distances to archives and finding comparatively little. This, however, is an occupational hazard, and in general, even in such cases a little went a long way toward filling out gaps in personal histories left by larger repositories. There were plant breeders interested in (or rejecting!) Mendelism at virtually every agricultural institution, and in later years geneticists as well, and the archival holdings of one after another of these institutions are treasure troves of information about how their careers developed. The most crucial thing to remember when making creative use of these archives is that one might find as much vital information about the individuals at the center of your research in materials written about them as you might in materials written by them. Just one little hint—the next time you use such archives, be guided in your requests by the following consideration: the extensive and fascinating materials available about you in personnel records, letters, and departmental reports housed in your dean’s file cabinets!

Contact information for archives mentioned in this article:

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Archives & Special Collections
405 Babbidge Road, Unit 1205
Storrs, CT 06269-1205

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University of California, Berkeley
Berkeley, CA 94720-6000

R. A. Emerson and Charles Bessey
University Archives
University Library
University of Nebraska at Omaha
Omaha, NE 68182-0237

R. A. Emerson
Carl A. Kroch Library
Cornell University
Ithaca, NY 14853

E. M. East and John Detlefsen
University of Illinois Archives
University of Illinois at Urbana-Champaign
Urbana, IL, 61801

Foundations of Genetics at the Electronic Scholarly Publishing Project

Michael R. Dietrich
Dartmouth College

In 1995, Robert Robbins began putting classic papers in genetics online (<http://www.esp.org>). From its beginning with Hardy's 1904 letter to *Science*, Robbin's Electronic Scholarly Publishing Project has grown to now include over 90 articles and books concerning the Foundations of Genetics. As an extensive collection of primary source documents, the ESP project is a valuable resource for anyone interested in the history of genetics or the history of biology in general. As this site expands to include material from other sciences and contributed analysis of the history of genetics, it promises to become an even richer source for scholars and students.

Although Robbins began developing a website devoted to classic papers in genetics in 1995, the ESP project took its great leap forward in 1997 when it was funded as part of the US Department of Energy's Human Genome Project. Robbins' goal was to facilitate users' appreciation of the findings of the human genome project by first helping them appreciate the foundations of classical genetics. With funding from the DOE, the ESP project moved from its original home at Johns Hopkins University to its own server and URL. The still growing collection of original papers and books forms the Foundations of Genetics series at the core of the ESP site. Robbins himself was trained as a biologist and now serves as Vice President for Information Technology at the Fred Hutchinson Cancer Research Center in Seattle, Washington. He developed the ESP site and the Foundations of Genetics collection out of personal interest in the history of genetics.

Beginning with a set of Mendel's papers, the Foundations of Genetics series includes a number of major works in late nineteenth century heredity. These documents can be browsed by author, title, and date. An annotated content list is also available. Complete copies of Darwin's *The Variation of Animals and Plants Under Domestication* and Francis Galton's *Natural Inheritance* are provided along with August Weismann's *Essay Upon Heredity* and William Bateson's *Materials of the Study of Variation*. Mendel's rediscovery is represented with English translations of papers by Correns, de Vries, and Tschermak. The cytological foundations of genetics can be found in copies of E. B. Wilson's *The Cell* and classic papers by C. E. McClung, Walter Sutton, and Nettie Stevens on chromosomes. Painter's 1934 papers on salivary gland chromosomes in *Drosophila* are also available. The dispute between Mendelians and Biometricians is represented by a series of papers and books by William Bateson and W. F.

R. Weldon.

Thomas Hunt Morgan's theory of the gene and the rise of *Drosophila* genetics constitutes the largest number of entries in the Foundations of Genetics collection. The landmark text by Morgan, Thomas H., Sturtevant, A. H., Muller, H. J., and Bridges, C. B. *The Mechanism of Mendelian Heredity* is available, but so are entire copies of books by Hugo DeVries and William Castle. This part of the collection also contains an interesting group of papers on crossing over, including papers by Barbara McClintock and Harriet Creighton, Richard Goldschmidt, and Calvin Bridges. H. J. Muller's papers on induced mutation and Morgan's and East's books and papers on the nature of the gene round out this group of sources.

Robbins has also made available the proceedings of the 1932 International Congress of Genetics and a collection of contemporary documents on the Human Genome Project itself. The gap between the 1930s and today is a product of both the intent to capture classical genetics and the limitations imposed by copyright restrictions.

The ESP project includes major texts in evolutionary biology as well. The full texts of Malthus' *Essay on the Principle of Population*, Vestiges of the Natural History of Creation, Darwin's *The Voyage of the Beagle* and *Origin of Species* are all available. Sewall Wright's classic 1931 paper "Evolution in Mendelian Populations" is also included.

Not all of the items in the Foundations of Genetics collection are scientific publications. The collection has a fascinating group of papers and books devoted to geneticists' history of their field, including the centenary celebration of Mendel and Galton in 1923 with articles by E. M. East, T. H. Morgan, J. A. Harris, and G. Shull. A. H. Sturtevant's book, *A History of Genetics* (1965), is also available in its entirety.

These original papers and books are complemented by a comparative timeline called Genetics in Context. This feature contains links to the original documents in the Foundations of Genetics collection, but places them in a chronology with other events from both the history of science and world history in general.

With assistance from a newly established advisory board, composed primarily of historians of genetics, the ESP site will be adding more original material as well as more historical analysis. Biographies of some geneticists are already a part of the site, but more will be contributed. The site is also seeking historical essays and bibliographies that address the Foundation of

Genetics collection.

You may visit the site at www.esp.org. Your contributions, suggestions, and use of the ESP site are always welcome. If you would like to contribute to, or share thoughts about, the Electronic Scholarly Publishing Project, please contact Robert Robbins (rrobbins@fhcrc.org) or any member of the advisory committee.

The Advisory Committee for the Electronic Scholarly Publishing Project includes:

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A Cultural History of Heredity

Jenny Marie
Max Planck Institute for the History of Science

There are two aspects to the Cultural History of Heredity project jointly run by Staffan Müller-Wille and Hans-Jörg Rheinberger. The first is a series of workshops, which are held biennially on the subject and the second is a small research group. This report looks at both aspects of the project and the resources that it provides for studying the history of heredity.

What, though, is a cultural history of heredity? The clue is perhaps in the word 'heredity'. The project does not aim to study the science of heredity, which in the twentieth century was genetics. Instead, we study the history of ideas about inheritance, whomever they were held by. Understanding heredity this way makes it almost necessary for the history to be cultural. However, the project's participants are particularly interested in the crossing of cultural domains by this hereditary knowledge (e.g. from science to popular culture and vice versa) and more specifically, in the way that practices can act as vectors by which these ideas cross cultural domains.

There have now been three workshops run by the project and another two are planned for the foreseeable future. The first workshop concluded that there was no general concept of heredity during the seventeenth and early eighteenth centuries. However, hereditary ideas existed in some specific domains, such as in the practices of plant hybridisation, explanations of disease and the organisation of colonial societies on racial characters. Approaches to the question of generation were highly diverse, and not just guided by the preformation / epigenesis debates. A report of the conference can be obtained from <http://www.mpiwg-berlin.mpg.de/HEREDITY/REPORT1.html> and some of the papers given at the conference are available from <http://www.mpiwg-berlin.mpg.de/Preprints/P222.pdf>.

The second workshop focused on the period from Kant's publication of *Von den verschiedenen Rassen der Menschen* in 1775 to Darwin's publication of *The Variation of Animals and Plants under Domestication* in 1868. It concluded that by the end of the period heredity was a central concept of the life sciences and, as it became so, certain shifts occurred in the understanding of heredity that existed in medicine, breeding, natural history and anthropology. Heredity became the phenomenon of parental and filial relationships and it became mosaic rather than a phenomenon of the general morphology of species. A report of this conference is available from <http://www.mpiwg-berlin.mpg.de/HEREDITY/REPORT2.html> and some of the papers presented are available from

<http://www.mpiwg-berlin.mpg.de/Preprints/P247.pdf>. Results of the first two workshops have been brought together by Staffan Müller-Wille and Hans-Jörg Rheinberger at <http://www.mpiwg-berlin.mpg.de/Preprints/P276.PDF>.

The third workshop focused on the period from Galton's *Hereditary Genius* of 1869 to Wilhelm Johannsen's "Experimente über die Vererbung des Samengewichtes bei *Phaseolus-Bohne*" of 1903. The workshop investigated the different contexts in which heredity was investigated during this period. These included statistics, evolutionary theory, physiological theories, sociology, psychology, genealogy and anthropology. Abstracts of the talks are available at http://www.mpiwg-berlin.mpg.de/HEREDITY/Abstract_Reader_2005.pdf.

The next workshop in the series will focus on how molecular biology and conceptions regarding it came about. In particular it will focus on how molecular biology brought together ideas that had existed in a wide variety of domains during the first half of the twentieth century. This workshop is currently planned to be held at Exeter University circa January 2007.

The final workshop in the series will focus on how the advent of gene technologies has impacted many areas of our daily lives, from the food we buy to the evidence offered in law courts. It will also consider how genetics could develop when genome sequencing becomes routine.

The workshops are just one part of the 'A Cultural History of Heredity' project. The other half is a working group of researchers, mainly based at the Max Planck Institute for the History of Science. This working group currently consists of five members, two of whom are postdoctoral fellows. The Max Planck Institute funds one of these fellowships and the Liechtenstein Government funds the other through the institute. The project thus has a limited supply of funding available to support postdoctoral fellows. Below I describe the current research being undertaken by this group so that potential applicants for such positions can better understand the type of interests that the project encourages.

Hans-Jörg Rheinberger is currently researching the material characteristics of different experimental systems that were used for hereditary research in Germany at the end of the nineteenth century and during the first half of the twentieth century. These systems were created by various scholars, who did so by drawing on their different disciplinary backgrounds. Hans-Jörg is focusing on six main experimental systems. The first area of focus is the pea and corn hybridisation work of

Carl Correns, which led him to rediscover the Mendelian laws. However, the study extends beyond this to Correns's work on variegation and his views on extra-chromosomal inheritance. The second focus is Alfred Kühn's group's work on the flour moth, which brought together considerations of transmission genetics and developmental physiology. The third is the Kaiser-Wilhelm Institute's work on the Tobacco Mosaic Virus, which combined biological, chemical and physical approaches to studying the genetic material. The fourth is Max Hartmann's protozoology work on fertilisation, reproduction and sexuality and the relation it had to genetics. Hans-Jörg also plans to research Richard Goldschmidt's research on sex determination and Fritz von Wettstein's research on cytoplasmic inheritance in mosses.

Staffan Müller-Wille, who is now based at Exeter University, has researched a variety of subjects for the project. These have included Linnaeus's ideas about heredity and how his classificatory system provided an important context for Mendel's hereditary work. Currently, Staffan is researching developments in anthropology and their relations to genetics at the start of the twentieth century. He is trying to understand the genealogy by which both questions of heredity in genetics and kinship systems in social anthropology became structuralist at the end of the nineteenth century and the start of the twentieth. He also seeks to understand why this occurred when this seemed to be incompatible with concepts such as evolution and adaptation and the idea of wholes, such as races, cultures and people.

Bernd Gausemeier also focuses on changes that occurred to ideas about heredity at the turn of the twentieth century. His interest is in how genealogy changed from being a practice that represented the social status of families into something that focused on the transmission of deviances in Germany. Bernd argues that genealogy structured concepts of human heredity and, since it provided a historical dimension, it also added questions of higher significance. Bernd therefore argues that this change in genealogy was important to the rise of eugenics.

My own work looks at the relationship between geneticists, animal breeders and gardeners in the first forty years of the twentieth century. I argue that animal breeders served as a link between geneticists, reproductive physiologists and husbandry men. This, among other things, encouraged genetics to remain a broad discipline in Britain. I also investigate the context that horticulture provided for genetics. I have noted that the International Genetical Conference series was originally a horticultural conference series, and many of the early genetic organisms were garden plants or insects that harmed them.

Christina Brandt is conducting a comparative study of the history of cloning in the latter half of the twentieth century. She investigates changes in German developmental biology and how this provided a background for the development of cloning techniques. She also looks at changes concerning scientific concepts of cloning. She explores how the term has been applied to plants that were propagated vegetatively, to cell cultures, gene cloning and finally to organisms. The final aspect of her work is the consideration of how clones have been represented in novels and scientific fiction, and how this has impacted public debates on cloning.

The project is thus fairly wide-ranging, as any project looking at the connections between genetics and other cultural domains would need to be. It provides the opportunity for scholars to discuss cultural histories of heredity at its conferences, and Internet access to the outcomes of these discussions. It also provides some support for postdoctoral and research scholars who work on cultural aspects of hereditary history.

BOOK REVIEW

Plants, Patients and the Historian. (Re)membering in the Age of Genetic Engineering
Paolo Pallodino. Rutgers University Press, 2003. (224 pp. 3 b&w illus.)

by: Vassiliki Betty Smocovits
University of Florida

Paolo Pallodino's new book is a synthesis of some 15 years of effort to understand the institutional contexts of genetics in Britain from its origins as an agricultural science concerned with plant breeding in the early decades of the twentieth century to its more recent biomedical turn. This synthesis between these two increasingly diverse areas is mediated by prolonged critical self-reflexivity that examines the practices of history and the role of the historian. Largely through this exploration, Pallodino argues for parallels between not only agricultural genetics and medical genetics, but also between genetics and history. To interrogate or to intervene with the genetic material which is bearer of past, present and future is analogous to the kinds of interrogations and interventions common to the historian. Both involve processes of re(membering) and both share the use of "archives" which they generally treat as a passive repository of available knowledge. Pallodino wants to bring to relief the active process of recollecting, the manner in which archival sources are used to "objectify" knowledge-claims that cannot be disengaged from the past, present, future, and to restore the "voices" usually silenced in the process, that of the plant, the patient and the historian (hence the title of the book).

Pallodino's book is the second in the new "Encounters" cultural histories series with Rutgers University Press whose aim is to "demonstrate that history is the hidden narrative of modernity (p. ii)." As such, the book has as its overarching concern a deep engagement with history and theory, which Pallodino lays bare in the scaffolding he provides in the introduction and first chapter of the book. Here many of the "usual suspects" of literary, cultural or critical theory (Michel Foucault, Walter Benjamin, Jacques Derrida etc.) are evoked directly or alluded to, as well as some minor figures and their concerns from science studies. These chapters—and the book as a whole—consciously defy historical conventions like full or clear consistent naming, detailed footnotes, documentation, or even accreditation to epigraphs or some quotations. Apologizing for "interpellating" his reader and violating the famous "pronouncements" of the "death of author" *pace* Roland Barthes and Michel Foucault, Pallodino points out those are the very kinds of problems he wishes to confront in his book.

Although it touches on a range of complex historiographic concerns, the introduction itself is brief, and only skims the theoretical literature. More importantly, I am not so sure the theoretical scaffolding is retained for most of the subsequent chapters (though most retain snazzy chapter titles like "genetics and the erasure of history," or "metaphor, desire and the historian."). Though it is clear Pallodino is rethinking his role as historian in these chapters, he still sounds pretty well much like an institutional historian of genetics. That is not all that surprising, since much of this material was already published as nine articles or essays previously (he accredits the publications early on in his acknowledgements). As such, this compact book really is best seen as a collection of essays (though there is some sense to seeing them assembled here and reconfigured with the theoretical introduction) that have been revisioned through a self-reflexive prism of historiographic concerns, though not always consistently.

Historiography aside, the institutional contexts explored by Pallodino in his subsequent chapters are most welcome. Historians of genetics can always benefit from more diverse national contexts and those of us who work the American side can especially benefit from seeing parallel developments or differences between the US and Britain as well as the rest of the world. Pallodino's treatment charts the course of large scale trends like nationalism and privatization and its effects on both agricultural and medical genetics as he follows the fate of key genetics institutes like the Institute for Plant Breeding (as it was sold first to Unilever and then to Monsanto), and its chief institutional rivals, the Scottish Plant Breeding Station and the Welsh Plant Breeding Station. Pallodino's chapters also highlight the disputes and conflicts between applied or field workers and those committed to "pure" research; the field/clinic/ and laboratory dynamic playing itself out as both social and epistemic context is similarly featured prominently. In one especially useful section in chapter 3, Pallodino delves into the British response to Mendelism after its rediscovery and the disputes between breeders, botanists and geneticists. He explores the dispute between Sir Rowland Biffen, first director of the Plant Breeding Institute, and then first professor of agricultural genetics at the University of Cambridge, who as "pure" science advocate and academician blurred the distinction between

agriculture and biology as he promoted the new science, and his adversary Edwin Sloper Beaven, the “foremost plant breeder in Britain,” who as “self-made businessman” doubted the claims made by professional academics like Biffen, many of whom were funded by the state. Yet another figure, John Percival, trained much like Biffen in the “new botany” added yet another layer to the complexity of the response since he was appointed as professor at the University College in Reading, which had a different institutional understanding of the farmer-scientist relationship and therefore the agriculture-biology relationship.

The same tension between academic scientist/professional/laboratory worker and clinician/practitioner is even more acute in areas associated with humans, especially in biomedicine. Here Pallodino builds on well documented conflicts between modernizing medical researchers focused on laboratory medicine and those more conservative clinicians with their traditional focus on clinical medicine by framing his analysis with the polemical exchange between Sir Frederick Gowland Hopkins, President of the Royal Society of London and laboratory advocate, and Lord Moynihan, President of the Royal College of Surgeons of London, a more conservative clinician skeptical of medical knowledge emerging from laboratory contexts. In the context of these tensions, he then charts the institutionalization of cancer research in Britain, and follows the dispute between a lesser known figure, J. P. Lockhart Mummery, the senior surgeon at St. Mark’s Hospital and secretary of the British Empire Cancer Campaign in, his challenge to Georgiana M. Bonser in the Department of Experimental Pathology and Cancer Research at the University of Leeds over her use of specialized inbred strains of mice to study cancer; people were not mice, he charged, and experimental knowledge gleaned from cancer studies in them was not applicable to humans. Organismic systems were not all the same and the turn to model organisms as experimental systems for treating diseases of humans, was therefore one especially acute problem encountered in modernizing medical genetics.

The book is filled with other such overlapping concerns and unexpected connections. Because of these and because of the British contexts, historians of genetics will find this book valuable. The historiographical perspective might make it a stimulating discussion piece for graduate seminars in the history of biology. As an original work of scholarship, however, I am not so sure that the historiography is sufficiently developed or retained throughout the text to make it count as a “synthesis” or a “revisionist” history of the British context of genetics, nor am I convinced of the timeliness of the perspective (the “new” cultural history that relies on the same literary and cultural theory is now well over 16 or 17 years old). Still, this is a deep engagement with some

important historiographic concerns on a range of important problems concerning the politics of institutionalization in two critical areas of agriculture and genetics. That makes it worthwhile reading.

A Dunn (Re)Discovery

Charles Greifenstein
American Philosophical Society Library

Leslie Clarence Dunn urged the establishment of *Mendel Newsletter* and contributed to it thereafter in many ways until his death in 1974. He knew the value of preserving the papers of scientists, and actively helped to preserve them. In the first issue of *Mendel Newsletter* (April 1968), Dunn wrote, "The existence of [archival] records incites communication among persons of diverse backgrounds who are interested in historical questions and to them the location of documentary material is of primary concern" and noted that "libraries are also aware of the need for archival services for such records."

Given L. C. Dunn's work in preserving the legacy of his discipline, it is only fitting that the first collection preserved under a federal Save America's Treasures grant is his. The grant, whose primary activity is to produce preservation photocopies of collections with brittle and deteriorating paper, funds two full-time conservation technicians for two years. Over 400 linear feet of records will be copied. Besides Dunn, the other genetics collections are the papers of H. S. Jennings and

of Raymond Pearl. Researchers will use the photocopies, and the originals will be sealed in their boxes, though available if necessary.

The procedure for creating copies is straightforward: retrieve a box, remove a folder, copy the contents, return the originals, create a new folder, create a new box, label folder and box, file new folder, shelve the copies. However, one immediately runs into complications. Although the Dunn collection is processed and accessible, cataloging decisions are necessarily revisited as copying proceeds.

Original cataloging decisions can seem mysterious and in fact might not provide the easiest and most logical access to material. The project technicians, John Armstrong and Kelly McCabe, discovered that material relating to Dunn's study of blood groups in the Jewish ghetto of Rome needed a more logical arrangement.

Dunn, with his anthropologist-son Stephen, undertook the study in 1953 to determine, first, to what extent the community of the ghetto (which, by Dunn's time, had no imposed legal restrictions) is

Mendel Newsletter

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culturally different from its neighbors and, second, are the Roman Jews biologically distinct from other Romans?¹ L. C. and Stephen jointly published an article in *Science* in — , that discussed the history of the ghetto and the findings. 23 percent of Roman Jews were of blood group B compared with 11 or 12 percent Catholics. More significantly, an Rh gene called Cde was found in 5 percent of the Jews tested, a rate 5 to 10 times higher than non-Jewish Italians.²

The data and other documentation were preserved in the Dunn's papers, but the way in which the papers were organized complicated retrieval. To begin with, all of the material was filed in the Correspondence series. There was actually some correspondence, both in separate folders and interfiled with other material. Some of the data was filed under Blood Groups—Jews. Numbers were assigned to families and individuals for data collection. The names were associated with the numbers in separate notebooks. The notebooks were filed under Dunn, L. C., Notebook—Rome. Other material was filed under Dunn, L. C., Rome (Jewish Community). Some of the data was the most difficult material to locate, filed in the Correspondence series under C.A.R.E. Unless a researcher knew that C.A.R.E. had helped fund the study, there is no indication in the finding aid that the C.A.R.E. folders contain material related to the Rome study.

The SAT grant gives the APS an opportunity to both enhance the preservation of the Dunn papers and make them more accessible to researchers—something Dunn would have appreciated. The material related to the Rome study will be put into a new series, logically called Research.

Endnotes

1. Leslie C. and Stephen P. Dunn, "The Jewish Community of Rome," *Scientific American*, March 1957, p. 119.
2. Ibid, pp 121 - 122.