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Managing Life: Human Biology 1918-1945

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Abstract

In the interwar period between 1918 and 1945, before the programmable computer and information theory were mobilized by biologists and economists as heuristics and instruments, the study of "man the animal" as a biological and social being was a managerial and bureaucratic pursuit. This pursuit was informed by changes in organization, the work process, and other institutions then taking place across wide swaths of American society. Coming as it did from such diverse sources, the field of human biology was always a loosely organized project, whose elements were in dynamic tension with each other. Human biology's research and popularizations would also necessarily be in tension with earlier eugenic arguments about heredity, even as they shifted the focus of concern onto the fields of human population growth, human variability, and social order. Two of the biggest recipients of human biology funding in the 1920s were the research groups led by Raymond Pearl at Johns Hopkins University and Lawrence Henderson at Harvard, particularly its business school. Henderson and Pearl were not only interested in solving social problems but also in establishing themselves in their fields. This consideration influenced their choice of audiences away from reform-oriented intellectuals and towards those they most directly needed to convince of their project's efficacy: university administrators, government officials, and business managers. For Pearl the problem of population growth and the differential rate of reproduction between native whites and immigrants would resolve itself through the natural action of the population's self-regulating capacities. Henderson on the other hand, and his allies at Harvard Business School Elton Mayo and Wallace Donham, saw an organizational and social world thrown badly out of equilibrium by the rapid changes of the early 20th century. They prescribed an elite cadre of manager-administrators to play a leading role in the key institutions of American life in order to reestablish equilibrium through their knowledge of "man the animal." What united Pearl and Henderson politically was their elitist conceptions of citizenship and science, and their animosity for progressive social reform, "uplift" and the New Deal.

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MANAGING LIFE: HUMAN BIOLOGY 1918-1945

My deep and heartfelt thanks to all my family and friends, chosen and not.

ABSTRACT

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In the interwar period between 1918 and 1945, before the programmable computer and information theory were mobilized by biologists and economists as heuristics and instruments, the study of “man the animal” as a biological and social being was a managerial and bureaucratic pursuit. This pursuit was informed by changes in organization, the work process, and other institutions then taking place across wide swaths of American society. Coming as it did from such diverse sources, the field of human biology was always a loosely organized project, whose elements were in dynamic tension with each other. Human biology’s research and popularizations would also necessarily be in tension with earlier eugenic arguments about heredity, even as they shifted the focus of concern onto the fields of human population growth, human variability, and social order. Two of the biggest recipients of human biology funding in the 1920s were the research groups led by Raymond Pearl at Johns Hopkins University and Lawrence Henderson at Harvard, particularly its business school. Henderson and Pearl were not only interested in solving social problems but also in establishing themselves in their fields. This consideration influenced their choice of audiences away from reform-oriented intellectuals and towards those they most directly needed to convince of their project's efficacy: university administrators, government officials, and business managers. For Pearl the problem of population growth and the differential rate of reproduction between native whites and immigrants would resolve itself through the natural action of the population's self-regulating capacities. Henderson on the other hand, and his allies at Harvard Business School Elton Mayo and Wallace Donham, saw an organizational and social world thrown badly out of equilibrium by the rapid changes of the early 20th century. They prescribed an elite cadre of manager-administrators to play a leading role in the key institutions of American life in order to reestablish equilibrium through their knowledge of “man the animal.” What united Pearl and Henderson politically was their elitist conceptions of citizenship and science, and their animosity for progressive social reform, “uplift” and the New Deal.

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CHAPTER 1: THE BIOLOGY OF LIBERALISM

What was human biology?

In post-World War II American universities, the term human biology refers to a subfield of biological anthropology. The journal *Human Biology*, founded by Raymond Pearl in 1929, passed under the editorship of Gabriel Lasker at Wayne State University in 1953. During Lasker's tenure the journal's content (biometry, human physical anthropology, human genetics, and comparative physiology) became part of the new biological anthropology. Lasker also participated in the post-war debates concerning the legitimacy of race science and racial typologies, and for the most part human biology followed genetics into a de-emphasis of race without ceasing to study the biology of human difference.

But between the First and Second World Wars, according to Sharon Kingsland, "there were several related attempts to encourage a biological approach to human biology that was distinct from both medicine and the social sciences." One of these "related attempts" at a human biology in the interwar period was a short-lived program at the Rockefeller Foundation. Edwin Embree, then in charge of the Division of Studies at Rockefeller, had an idea for a science of man to solve social problems. Embree penned the Introduction to cytologist Edmund V. Cowdry's edited volume Human Biology and Racial Welfare (1930), which included the writings of authors as diverse as Walter Cannon, Alexis Carrel, Raymond Pearl, Charles Davenport, and John Dewey. Embree described human beings' debt to its "simian" past for mankind's "love of talk" and "compulsion for action." He also observed that modern communication technology and population growth were bringing the human community closer together than ever before.

Preventative medicine had recently made stunning advances and now "knowledge applicable to man's welfare is now coming in other phases of biology" Embree closed by calling for "biologists with a background in statistics," anthropologists, sociologists, political scientists, psychologists and economists to apply the lessons of physics and chemistry to the study of "man the animal" so that the volume of writings he was introducing would "serve as a record against which to measure the rapid and significant advance that may be just ahead."

Coming as it did from such diverse sources, the field of human biology was always a loosely organized project, whose elements were in internal dynamic tension with each other. Human biology's research and popularizations would also necessarily be in tension with earlier eugenic arguments about heredity, even as they shifted the focus of concern onto the fields of human population growth, human variability, and social order. Two of the biggest recipients of human biology funding in the 1920s that sustained their research well past Embree's departure from Rockefeller were the research groups led by Raymond Pearl at Johns Hopkins University and Lawrence Henderson at Harvard, particularly its business school. In contrast to Embree's paternalistic progressivism (he later went on to a career as President of the pro-civil rights and pro-integration Julius Rosenwald Fund) Pearl and Henderson had other plans. First, they were less interested in solving social problems than they were in establishing themselves in their fields. This consideration influenced their choice of audiences away from reform-oriented intellectuals and towards those they most directly needed to convince of their project's efficacy: university administrators, government officials, and business managers. Second, Henderson and Pearl differed strongly with Embree on what constituted the most pressing social problems of the day, and how to best go about solving them. In fact Henderson and Pearl, though personally close friends and academic allies, were united more by what they opposed than what they supported.

For Pearl (under whose influence Embree had come to the idea of a program in human biology), the pressing problem of population growth and the differential rate of reproduction between native whites and immigrants would resolve itself through the natural action of the population's self-regulating capacities. Henderson, and his allies at Harvard Business School Elton Mayo and Wallace Donham, on the other hand, saw an organizational and social world thrown badly out of equilibrium by the rapid changes of the early 20th century. They prescribed an elite cadre of manager-administrators to play a leading role in the key institutions of American life in order to reestablish equilibrium through their knowledge of "man the animal." What united Pearl and Henderson politically was their elitist conceptions of citizenship and science, and their animosity for progressive social reform, "uplift" and the New Deal.

In *Who Wrote the Book of Life,* Lily Kay (2000) argued that molecular biology in the post-war period was an information science, inflected by the figure of the code and deeply marked by code-breaking technologies.[1] Philip Mirowski (2001) showed how, in the same period, economics became a cyborg science, indebted to models of feedback and human-machine equivalence.[2] However, in the interwar period between 1918 and 1945, before the programmable computer and information theory were mobilized by biologists and economists as heuristics and instruments, the study of "man the animal" as a biological and social being was a managerial and bureaucratic pursuit. This pursuit was informed by changes in organization and the work process, changes in ways of thinking about things and doing things, taking place across wide swaths of American society at this time.

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[1] Kay, Lily *Who Wrote the Book of Life?*

[2] Mirowski, Philip *Machine Dreams*

Power Over Life

In his book The History of Sexuality, an Introduction. (1979), Michel Foucault proposed that, since the 17th Century in Western Europe, the concepts and techniques of governance have undergone a deep and complicated shift. He characterized this shift as moving away from concern with the power of death and towards an enduring interest in the power over life. A whole series of institutions, including those of incarceration, education, and medicine were pivotal to this shift. The power of the state became less invested in the right to kill, and devoted more and more effort into the power to work on life: to govern it, and make it productive. This power over life took two primary forms. The first to develop were the institutions with disciplinary power over the human body and its anatomy, functions, and capabilities. Military barracks, hospitals, alms houses, and prisons developed sets of tools, ideas, and objects of study oriented towards controlling bodies, forging important modern subject identities (madmen, sexual deviants, vagabonds, criminals) in the process. The second aspect of the modern power over life emerged soon after the disciplinary institutions of the 17th century but then developed in tandem with them. This second variety of power over life focused on the human population, its characteristics, its health, and the dynamics of its growth. Examples of institutions for power over populations were granaries and bread price control, city planning, population control or natalism, public health, and sanitation measures. Foucault called the second kind of power over life biopower, or biopolitics, and he spent much of his later work characterizing its emergence, development, and the relationships of its institutions.

Foucault's other major concern in his later lectures was the relationship between biopower and the varieties of techniques of governance he called "liberal." Liberalism in the economic sphere taught that government should treat the economy as an autonomous field with its own laws and its own regularities, separate from the desires of the ruler. The development of liberalism in the West was bound up with the developing institutions of biopower, and they both operated according to similar strategies of action. In the field of population as well as that of the economy, the role of the governor was to encourage growth and productivity. The confluence between the development of biopower and the development of liberalism provides the conceptual links for the theme that I will return to repeatedly in this dissertation: the connections between the organizational and institutional structure of business and government in early 20th century America, and human biology as a field of knowledge and practice.

Population researchers Raymond Pearl and Alfred Lotka's work moved back and forth between John Hopkins University, federal government bureaus, and the life insurance industry. Their tools, concepts, and concerns did not strictly belong to one world or another, but were articulated with work in all three. Their common uses of mathematic models, empirical data, techniques of calculation, and organization of work became more tightly related to each other as biostatistics, insurance, and liberal governance developed in distinct but tandem tracks.

At the same time, an alliance of physiologists, social theorists, and administrators at Harvard University centered around Lawrence Henderson responded to the effects of contemporary social change by modeling worker fatigue and political disorder as two symptoms of one underlying cause: disequilibrium. This "Harvard nexus" also attempted pedagogical reforms at the Business School in order to train a new generation of elite managers whose skilled judgment would restore economic growth, worker productivity, and political stability.

Both of the actors at the center of their respective human biology research worlds, Lawrence Henderson and Raymond Pearl, were concerned with the human body as well as human populations. Both of these research streams would be important resources for the development of diverse post-war research streams. Pearl and Lotka's mathematical description of the self-regulating dynamics of population growth directly inspired systems biologist Ludwig von Bertalanffy work on growth models. Henderson's work on the chemical dynamics of the blood influenced Walter Cannon, and through him, the development of cybernetic thought in the Macy Group.

The Problem of Complicated Things

What if an object of study cannot be easily decomposed or reduced or to its component parts? What if the object of study is either the whole phenomenon or it is obvious that there are significant interactions within the whole not easily analyzed in a “billiard ball” approach? What methods and perspectives are appropriate to study such complicated things?

Two common approaches developed in the early 20th century. One was to redefine the problem all together and invoke a vital energy or organizing principle. This was the approach of vitalism and entelechy (such as in the work of the later Hans Driesch.) The other was to use the tools developed by people who had already studied complicated things and apply them to the study of the objects in question. One of the approaches to studying complicated things in the world of the early 20th century was the newly-emerging model building practices of economics. Another was to adopt methods designed for addressing problems of organization in the managerial firm. At the same time, the insurance industry refined its techniques for gathering data and evaluating risk in all of these areas. The tools developed to study the economy, the firm, and society were re-purposed and applied to evaluating and describing human population dynamics and the regulation of the physiology of the human body. And the knowledge of the function of the human body and the dynamics of the human population were fed back into strategies for studying and intervening with the administrative power over life.

But this raised the related problem of ends. If a complicated social or biological phenomenon could not simply decomposed into many small pieces for analysis, if there was a special guiding principle or energy, then what was that energy? How could it be studied? One of the answers to the problem of ends in human biology in this project was to fold old concerns about ends into the new concerns about form. The question of ends became the same as the question of form, which is to say, of organization. For example, to pose the question of fatigue physiology as one of the equilibrium allowed Lawrence Henderson to bring the tools of statistical thermodynamics to bear on it: "Just as Newton first conclusively showed that this is a world of masses, so Willard Gibbs first revealed it as a world of systems."

A notion I will be returning to repeatedly throughout this project is the development of a particular technology of rationality. Foucault wrote that a technology is "...a matrix of practical reason": a series of techniques, practices, approaches, and perspectives that related to one another more or less tightly. The term technology could refer to ensembles of techniques of production, systems of governmental thought and action, or sets of practices of self-cultivation. I am getting at a something like technology in Foucault's sense, but I want to be more sensitive to the way that the technology is articulated in practice and how it can become the basis for alliance-building and collective social action. To do this I will be using an armature borrowed from interactionist sociology: the notion of commitment. A commitment is a bet that a given action will be the right one in a given situation, or at least will not result in a bad outcome. Individuals can have commitments, and so can organizations. Commitments are conventions, regularized ways of doing things. Conventions combine to make institutions, collective capacities for carrying out a line of work. As parts of institutions, commitments are useful categories up and down the scale of social action. Commitments can nest within one another, as in a commitment about a commitment, which is called a virtue. Virtues are commitments that a particular commitment will be a good one, for example the virtue of curiosity makes the proposition that habitually looking for new experiences is a good way to spend one's time.

Commitments (and virtues and higher orders of nested commitments-about-commitments) form the basis for carrying out collective work such as research, curriculum development, and organizational reform. Actors recognize one another and enter into alliances of varying kinds on the basis of their common, or compatible, commitments. This does not mean that an alliance entails a strong bond, or protects allies from betrayal. An alliance simply means that two or more actors agree to act as audiences for each other, or at least they do not object so strongly that they will act to prevent what is going on. By focusing on commitments and alliances I will be able to show more clearly how research in human biology interacted with other fields of organizational life.

Institutional Rationalization and the Managerial Revolution

Between the 1880s and 1920s, a profound organizational and structural shift took place in the field of American business. Starting with textiles and railroads, and extending into heavy industry, other manufacturing, and finance, the landscape of modestly sized entrepreneurial family-owned companies increasingly became populated by large and multidivisional managerial corporations. The proliferation of the large multidivisional corporation with an internally segmented labor force developed alongside what has come to be called 'the managerial revolution' in the administrative practices of those institutions.

Firms got big partially by adopting the multidivisional form and partly by changing their work process. At the same time, some of the physical sites of the labor process (i.e. factories and foundries) grew as well. Late nineteenth-century steel plants like those at Lakawana Iron and Steel employed 3,000 workers under one roof. By 1924, the Ford River Rouge plant employed 68,000, the largest single-site employer in the United States, and probably in the world at that time. Increased size, the multidivisional form, and decomposing the work process into sub-tasks were all strategies comprising a particular kind of institutional rationalization. Max Weber called it zweckrationalität: efficiency or means-oriented rationality. Means-oriented rationalization was a process of bringing means and ends into tighter relationship; that is, increasing efficiency, getting more output from fewer or less inputs, and making things work more smoothly and predictably. Rationalization was a set of approaches to solving problems, and not an inevitable outcome or a phenomenon affecting the whole of U.S. society at once. While some institutions underwent rationalization at this time, others did not.

As firms grew, they encountered diseconomies of scale that prevented further rationalization along those lines. In particular, problems of communication and coordination between the different parts of a supply chain, and between the lower and upper levels of management became pressing problems. Business historian Joseph Litterer characterized one mode of solving these problems within the firm as "systematic management." , Systematic management shares some features with scientific management (also known as Taylorism), in that both are rationalizing approaches to institutions within an organization. The main differences are found in the fact that while scientific management sought greater control of the work process by separating planning from execution and by decomposing labor practices into smaller and more routinized tasks, systematic management was primarily aimed at rationalizing the supervision of the labor process, through developing standardized ways of performing managerial duties, but also by optimizing the recording, communication, storage, and retrieval of information within the firm. The wider use of the vertical hanging file system, the typewriter, the mimeograph, and the internally circulated memorandum corresponds to big firms' solution to the problems of communication and control. Already existing methods of keeping financial records like cost accounting and financial accounting became more widely practiced. At this time firms like DuPont and the Illinois Central Railroad adopted new forms of visual representation of organizational data like performance graphs and the Gantt chart. The style of institutional re-organization occurring in the management of data and records under systematic management can be characterized as 'coordinative' rationalization. This kind of rationalization streamlines relationships between things by removing extraneous parts, or optimizes processes by selecting and supporting the best functioning parts of that process.

During the period of organizational and institutional rationalization of 1900-1920, the multidivisional corporate organizational form spread to become the structurally dominant organizational structure in the United States, and many other organizations outside of the business world adopted it as well. But why this should be is not intuitively obvious. During the early 20th century in the United States, a subset of organizations like corporations that manufacture or transport commodities, or the banks that provide them with capital, provided an organizational template for other kinds of organizations. Corporations in particular were highly prestigious institutions, and their forms were more likely to be adopted by others. For example, universities and philanthropic foundations are two examples of organizations that did not necessarily have a functional reason for adopting the form of the multidivisional corporation, but there is nevertheless good evidence that they did so anyway. By adopting the organizational forms of successful firms like those in the steel industry and the railroads, research universities and philanthropic foundations could stake a claim to the powerful normative appeals of efficiency and professionalism. Mimicry of organizational form also implies the adoption of the techniques and ideas associated with those forms. In the case of the managerial revolution, American universities and philanthropic foundations took on the style of office work, record keeping, and communications that had come to characterize more "outcome-oriented" organizations.

Organizational sociologists DiMaggio and Powell argue “that there are two types of isomorphism: competitive and institutional. Competitive isomorphism, they argue, "…is more relevant for those fields in which free and open competition exists.” But they did not see this style of isomorphism formation as being the predominant one "in the modern world of organizations" in which "organizations compete not just for resources and customers, but for political power and institutional legitimacy, for social as well as economic fitness." By the beginning of the 20th century, the process of modernization and bureaucratic rationalization had been substantially completed in the U.S. commercial world, and elite organizations in philanthropy, education, and research were undergoing a similar transformation.

DiMaggio and Powell (1983) offer a three-category typology for institutional isomorphism: coercive, mimetic, and normative, with the understanding that in most real-life cases these categories exist as mixed, partial, and multiple causes. Coercive isomorphism occurs when dominant fields or organizations are able to impose their standards of rationalization onto their subordinates. The state is the most common source of coercive organizational isomorphism in the modern institutional settlement, but similar relationships of dependency resulting in isomorphism may obtain among subsidiary companies and their parent corporation, between service infrastructure providers and their clients, and between community organizations and their donors.

Mimetic institutional isomorphism takes hold in conditions of uncertainty and ambiguity, when less powerful actors actively imitate the forms and norms of their more powerful neighbors, even absent direct ties of dependency between them. Modeling successful organizations and institutions leads to imitative behavior, but also depends on a process of interpretation and local re-negotiation that often results in the imitative institution differing substantialy from its target of imitation. This also means that organizations with a looser relationship between its activities and its continued operation (for example education, scientific research, artistic production, and social movement work) will tend more strongly to model successful institutional forms than organizations with a strong coupling between its "bottom line" and its continued existence.

Finally, normative institutional isomorphism "stems primarily from professionalization.” By controlling who gets to become a member of a given profession, the already-existing institutions of a professional association or accrediting body insures that successful and aspiring members of a professional body come to resemble each other to a surprising degree in background, opinion, taste, and behavior. This in turn promotes institutional isomorphism within fields.

Scientific Anti-Democrats

In Science, Democracy, and the American University (2012), Andrew Jewett applied the label "scientific democrats" to "the large and varied group of American thinkers who contended that science, as they understood it, offered the basis for a cohesive and fulfilling modern culture." Jewett took care to define his democrats as having a particular set of concerns not immediately recognizable to partisans of democracy in the later 20th century. They were not focused, for example, on securing civil rights for women and oppressed peoples, and they did not have much faith in a redistributive state. Rather, the scientific democrats promoted democracy by promoting science. Their goal was "...to articulate and disseminate what they took to be the social meaning of modern science, above all its revelation of the need for citizens to adopt a greater sense of social obligation and mutuality." This did not mean the actual active participation of citizens in the democratic process. Instead, "simply assuming that public opinion mattered centrally in American governance, they focused on making an impact on the minds of citizens." The pragmatist philosopher John Dewey stands near the center of Jewett's concentric circles of scientific democrats. Moving out from Dewey's central position, there were also "those who argued, beginning in the 1880s and accelerating after the turn of the century, that the development and popularization of the human sciences would turn Americans away from competitive capitalism rather than towards laissez-faire ideals."

But as Philip Mirowski reminds us "[t]he yoking together of science and democracy was not such an obvious winning combination in the early twentieth century." If Columbia University gave

John Dewey a home in this period in New York, then Cambridge was the home turf of "a prevalent intellectual current that framed the duel as incompatible in structure and content." Here scientific democrats found their opposite: let's call them scientific elitists. They were a network of biologists, social scientists, administrators and businessmen held together by two common commitments. First, a belief in the basic irrationality of humans as individual and in groups. Second, deep pessimism about the compatibility of irrational humans with political democracy in a rapidly changing society. Their common intellectual project was to find a way to manage the institutional and organization re-structuring that emerged from the serial crises of World War I, immigration and demographic change, urban problems, labor unrest, the Great Depression, and the New Deal. Their answers to these questions varied in that some favored a laissez-faire approach to economics, politics, and civil society, while others argued for the necessity of a strong managerial elite to act as an autonomous nervous system for an acephalous society (for more on the differences between Lawrence Henderson and his friend and ally at Johns Hopkins Raymond Pearl see Chapter 6.) However, they were united in their mistrust of popular notions of democracy and in their scorn and contempt for intellectuals who associated with progressive social movements.

At Harvard University there was a network of scientific elitists, (Joel Isaac called these the Harvard Paretans), who occupied key positions within Harvard's Society of Fellows, the Law School, and the Business School. They did not dominate any particular department, but rather existed as an "interstitial academy" in the spaces between the established departments and power centers. The program in human biology at Harvard was an integral part of the Harvard Paretan's interstitial academy.

A deep reorganization of the Rockefeller Foundations under the supervision of Raymond Fosdick began in 1928. The programs of the Laura Spelman Rockefeller Memorial Fund, the General Education Board, the International Education Board, and the Medical Education Board were transferred to newly created Divisions. In 1929 a Division of Social Sciences was created along with the Divisions of Natural Sciences, Medical Sciences, and Humanities. Edmund Ezra Day, former dean of the University of Michigan and future President of Cornell University, was made Director of the Division of Social Sciences. The Rockefeller reorganization reduced the power of the entrepreneurial "barons" who had hitherto had final control over their programs, and instituted a system of middle managers more compatible with contemporary managerial practices.

In Making America Corporate (1990), Olivier Zunz' makes the point that because large corporations grew to include so many areas of American life "[t]hus their influence entered American life through a variety of channels." From 1900 to 1920, the managerial revolution in American corporations came to universities and foundations as well. Rockefeller's General Education Board and the Carnegie Foundation for the Advancement of Teaching pushed for the rationalization of the administration of colleges and universities, so that the use and management of foundation funds might be more effectively monitored. Without the accompanying new forms of financial accounting and management, foundation donors would have no way of evaluating the effectiveness (and efficiency) of their donations.

University reforms increased the professionalization of academic training and specialization of research, which had the effect of increasing the number of fields and sub-disciplines within each academic department. This in turn seemed to threaten the idea of the unity of knowledge upon which many administrators thought the institution of higher education depended. Harvard's president A. Lawrence Lowell put it like this in 1909: "We must construct a new solidarity to replace that which is gone. The task before us is to frame a system which, without sacrificing individual variation too much, or neglecting the pursuit of different scholarly interests, shall produce an intellectual and social cohesion." To this end Lowell introduced undergraduate concentration and distribution requirements, and it was on the basis of this hoped for "system" that Lowell recognized the value of the Harvard scientific elitists.

Other scientific elitists associated with the Harvard Paretans include Professor of Industrial Relations Elton Mayo, Dean of Harvard Business School Wallace Donham, and President of the New Jersey Bell Telephone Company Chester I. Barnard. But of course the most important Harvard Paretan was Lawrence J. Henderson, Director of the Harvard Fatigue Laboratory, organizer and central personality of the seminar on the General Sociology of Vilfredo Pareto, founder of the History of Science as a discipline at Harvard, and co-founder of Harvard's Society of Fellows.

Thorstein Veblen, one of the keenest observers of social and organizational developments in the early 20th century, doubted whether Law Schools had a place in Universities at all, and he was characteristically scathing about what we would now call business or management schools. He commented in 1918 in Higher Learning in America that a “college of commerce” was incompatible with a university’s real task, that of educating the community. Because education towards the end of private gain was against the interests of the greater social good “the support and conduct of such schools at the expense of the universities is to be construed as a breach of trust.”

Description of Chapters

In Chapter 2, The Human Biology of Raymond Pearl. I provide a new perspective on some of the most salient episodes of Pearl's career. First I examine Pearl's dissent from eugenics in the 1920s in the light of his friendship with Baltimore journalist Henry Louis Mencken, and their shared political and philosophical commitments. Next, I relate Pearl's faith in the self-regulating and self-limiting behavior of population phenomena to his work on the logistic, or S-shaped population growth curve. Finally, I tell the story of the rise and fall of Pearl's influence in the International Union for the Scientific Study of Population Problems (IUSIPP) during the turbulent decade of the 1930s, as the Rockefeller foundation withdraw support from the project in the face of increasing influence from European nationalism on population science. This chapter situates Pearl more accurately within the political and scientific controversies of his time than previous accounts.

Chapter 3, Calculating the Population, is about the shared styles of work at Raymond Pearl's Institute for Biological Research (IBR) at Johns Hopkins University and the Metropolitan Life Insurance Company of New York (Met Life.) It follows the career of Alfred J. Lotka at the IBR and at Met Life, and connects his work in both organizations through his use of mathematics and his conceptualization of life as a self-renewing aggregate. One way of framing this story is that Alfred Lotka brought the tools and approaches of the life insurance industry to bear on the problems of population growth in human biology. Another way is to say that Raymond Pearl brought the tools and practices of his earlier work at the US Food Administration to bear on the problems of population growth. As I have argued in the Introduction and in Chapter 2, new kinds of rationality that were developed in the early 20th century for coordination of the work process were appropriated by human biology. The IBR and Met Life were both early adopters of mechanical calculators, punched-card tabulators, and a gendered division of labor. I also elaborate the role that the gendered division of labor had on the economy of knowledge at both organizations. Finally, I explain Lotka's insider-outsider status in human biology as the result of his blocked trajectory in the U.S. university system.

First, I explore the similarities of work process, instrumentation, and organization employed at these different sites, especially the role played by highly skilled feminized computational work, all of which lead to institutionalized similarities in the gendered division of labor and the allocation of intellectual credit. Second, I note the similarities in the conceptual models brought to bear on the respective problems of these institutions, including mathematical models. I suggest some ways that biological research work was influenced by bureaucratic and managerial fields on the basis of a shared style of rationalization applied to problems of populations. The instruments and structures of that rationalization were ultimately derived from business firms' competition with one another, and from the US government's desire to know more about its population.

In Chapter 4, Alliances in Human Biology: The Harvard Committee on Industrial Physiology, I describe the formation and maintenance of an alliance between Lawrence Henderson, Elton Mayo, and Wallace Donham at Harvard in the form of the Committee on Industrial Physiology (CIP). The Harvard Fatigue Labortatory was conceived by Henderson as a link at the Business School between his earlier blood physiology work and Elton Mayo's efforts in studying the human factors in industrial work. The construction of the Fatigue Laboratory's new location was paid for by a large grant by the Rockefeller Foundation to the CIP. The CIP was maintained by the alliance between Henderson, Mayo, Donham, the Division of Social Sciences at Rockefeller, and the administration of Harvard University. The CIP shifted its discourse around its reasons for existing from inter-departmental coordination to training managers and government experts in response to changes in personnel at Rockefeller and Harvard, and to the volatile funding environment of the run-up to WWII.

Chapter 5, Instrumental Rationality is my attempt to use Hans-Jörg Rheinberger's notion of the epistemic thing to follow the trajectory of fatigue through the Harvard Fatigue Laboratory and into the wider institutional environment of Harvard University. By doing this I hope to respond to some of the recent literature that has applied the tools and perspectives of science studies to 20th century American intellectual history. The Fatigue Lab's practices, instruments, standards, and perspectives developed alongside a particular style of institutional rationalization and how those styles were expressed in the social theory of Fatigue Lab director Lawrence Henderson. The Fatigue Lab's package of rationalized practices were associated with a program of training at Harvard Business School oriented towards preparing executives and business school graduates to be elite generalist managers, who would fill organizational positions in government, business, the academy, and in philanthropic foundations.

A concluding note on studying research programs in human biology

Sharon Kingsland (1991) noted the difficulty of detailing the history of interwar human biology, given its short trajectory, its lack of a disciplinary home, and its important but misunderstood role as a resource for the post-war social and systems sciences. She wrote that

> To reveal the pluralistic nature of the new human biology during these years, we would need several parallel histories of individual research strategies. The present essay is only one of these stories. The larger story is of interest no only for what it might tell us about the institutional and social politics of science in the interwar years, but also for what it would reveal about the variety of American attitudes toward "man the animal," attitudes which helped to shape the fields of psychobiology and sociobiology that emerged after the Second World War. (Kingsland p. 196-197)

There are methodological reasons for hesitating to present to an overly coherent or compact story in the history of human biology, or at least to be self-conscious of the limitations of such an approach. For my story, the interesting parts happen when the research veered out of the original track envisioned for it by its first funders and framers. In this case the trajectory of the research work bends sharply in a way that does not lend itself easily to a narrative arc, and to attribute such an arc to its content obscures as much as it reveals.

Another problem in assembling a coherent narrative in a field without a discipline, such as human biology, is that by the early 20th century in the US the deluge of paper had begun. Business firms grew to unprecedented size and complexity, government agencies and bureaus reached out to study and intervene in more areas of life, the universities began an expansion that would reach exponential proportions in the second half of the century. The thick tangle of interrelating institutions and organizations that characterize the 20th century's technoscience had already begun to take hold.

One way around this problem is to trace a thread that runs throughout the networks of organizations and institutions as they change over time. I have not followed this practice. Instead, I have tried to characterize the moving and changing alliances between researchers, their work, and their audiences.

CHAPTER 2: THE HUMAN BIOLOGY OF RAYMOND PEARL

> "In his professional life and work Pearl was chiefly a Human Biologist. He liked the term, gave it to more than one of his Journals, applied it to himself, and I think did more than any other man to fix the ideas, or at least many of the ideas hitherto neglected by anthropologists and sociologists, that seem indispensable for the growth of the sciences which he foresaw"[3]
>
> --Lawrence J. Henderson, 1940

"…Mencken ain't done right by our Pearl"

In November 1927 Raymond Pearl received a letter from his colleague Edward M. East of the Bussey Institute at Harvard University. East had included for Pearl a copy of another letter he had received from an un-named friend. The anonymous third party was irritated with Pearl over an article he had written for the magazine *The American Mercury* entitled *The Biology of Superiority* and had written to East to complain:

> …Pearl seems to believe that the eugenicists always expect "like to produce like." If this were so, of course no eugenist would ever have bothered with a "coefficient of correlation" which is mathematically sound, even if Pearl does not understand it… His thesis that we cannot predict any effect of the differential fertility of various classes within a population because the somatic characteristics of the offspring cannot be predicated from the somatic characteristics of the parents, is on the same intellectual plane as denying validity to life-probability tables because we can never know when a man will die.

[3] Henderson, Lawrence J. Raymond Pearl (Obituary Notice) *American Philosophical Society Yearbook*.1940.

> What has happened to Pearl lately? Not many years ago, I heard him introduce [noted eugenicist Edward Albert] Wiggam to a Baltimore audience… Such a sudden conversion from one spectacular extreme to another is always, in spite of St. Paul's precedent, suspicious.

> The appearance of such an offspring as "The Biology of Superiority" makes is evident that 'Mencken ain't done right by our Pearl.' But it's done now…[4]

Whatever the identity of the author of the letter, he was clearly familiar with the terms of the debate then going on in the fields of genetics, biometry, and eugenics. The letter mentions coefficients of correlation, the pure line garden pea experiments of Wilhelm Johannsen, and speculates as to the effects of selection on populations. Interestingly, the letter also brings up actuarial life-tables, a statistical tool used in the business of life insurance. The life table, and other tools of actuarial statistics, will re-appear later in this story. (See also Chapter 3. *Calculating The Population*).

Pearl was at that time Research Professor and director of the Institute for Biological Research at Johns Hopkins University, at the end of the second year of a big five-year grant from the Rockefeller Foundation. He was also Professor of Biology at the School of Hygiene and Public Health and Chief Statistician at Johns Hopkins University Hospital.[5] But who was this "Mencken" mentioned in the above letter, and what had he done to Pearl that had turned him against the eugenic orthodoxy of his day?

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[4] Raymond Pearl papers Box 7 Folder # 6, 1927 APS

[5] Kingsland, Sharon E. Raymond Pearl: On the Frontier i the 1920s. *Human Biology, February 1984, Vol.56, No. 1 pp.1-18.*

In fact, we can find the answer to this question in the unlikely location of a Baltimore social club called The Saturday Night. Here, Raymond Pearl and Baltimore journalist, author and critic Henry Louis Mencken played music and drank beer together weekly for over twenty years. In Mencken, Pearl found a kindred spirit, a lover of music (Mencken on the piano, Pearl on reeds or brass) and a fellow elitist and anti-democratic conservative. Mencken was a skeptic and a contrarian and loved to puncture the closely-held beliefs of those who he saw as hypocrites or moralists, whether of the political or religious kind.

Mencken was a bundle of contradictions. One puzzle in his life was the combination of intellectual and journalistic audacity with personal aversion to risk. He spent virtually his whole career working for the *Baltimore Sun*, and lived most of his adult life on-and-off in the same brick row house with his mother, and, after her death, with his bachelor brother.[6] In this as in many other things Raymond Pearl seemed to match Mencken's temperament as a ferociously confident scholar who could turn on a dime to become exquisitely cautious and sensitive to social propriety when he thought it might be warranted.[7]

But perhaps a more contradictory thing about Mencken was that, in spite of his dedication to satire, free-thought and the bursting of moralistic bubbles, he was highly sensitive when it came to his own cherished beliefs. The easiest and fastest way to attract his journalistic opprobrium was to offend his intellectual or, even worse, aesthetic, sensibilities. The merest suggestion that a representative democracy with universal suffrage was anything other than an excuse for the

[6] Teachout, Terry. *The Skeptic: A Life of H.L. Mencken*. 2002. Harper Collins, New York.

[7] See especially Pearl's relationship with Walter White of the NAACP, below.

corrupt and oppressive rule by an inferior class of men was enough to send him into high dudgeon. When he wrote against the social integration of Black people or Jews it was always from the bald position that one simply shouldn't try to mix with one's betters.[8] His disdain for Black people and poor whites was surpassed only by his contempt for the preachers, politicians, and intellectuals who he saw as trying to lead them.

Mencken was also a snob, though a self-confessed and contented one. In this, his philosophical sympathies were heavily influenced by his early reading of Friedrich Nietzsche, which confirmed his faith in the unbridgeable gap between great men and inferior ones, and his mistrust of any universal morality.[9] But Mencken's political sensibilities were not simply an easily caricatured white supremacy. He was an intellectual aristocrat who looked down on most kinds of people, white or Black. He associated vulgar racial prejudice with the Ku Klux Klan, the rural South, poor whites, populism, and religious fundamentalism; all of which were his sworn enemies. After the Second World war his views were so out of step with the times that he softened a little, even writing in favor of integrating public recreation facilities in Baltimore, on the grounds that Black peoples' taxes had paid for them as much as whites'.[10]

Pearl's aethetic and political commitments must be read as being in constant dialogue with those of Mencken, and, as we will see, Mencken's positions on the relationship between science and

[8] For a good summary of many of Mencken's views, see his excerpted notebooks published as *Minority Report: H.L. Mencken's Notebooks.* H.L.Mencken. 1956. Knopf.

[9] Mencken, Henry Louis. *The Philosophy of Friedrich Nietzsche.* 1908.

[10] Teachout, Terry. *The Skeptic: A Life of H.L. Mencken.* 2002. Harper Collins, New York.

society should be read in the same light. Mencken had this to say about his friendship with Raymond Pearl and their long companionship at the Saturday Night Club.

> In Baltimore I seldom saw anyone save a few of the executives at the *Sun* office and the members of the Saturday Night Club. To the latter Raymond Pearl, professor of biology at Johns Hopkins, was added in 1919, and thereafter, until his death on November 17, 1940, he was my most intimate Baltimore associate, and indeed almost my only one, save for Paul Patterson. We have a common bond in music, but we were both also interested in many other things, ranging from good eating to the congenital infirmities of the human race. I got a great deal out of Pearl, first and last, and I believe that he was appreciably influenced by his association with me. His death was a great blow to me, and no one else has taken his place, or will ever do so.[11]

One of Mencken's biographers went so far as to describe Pearl as "…the nearest thing he had to a soul mate."[12] Pearl, like Mencken, was essentially a contrarian. He enjoyed criticizing other positions to such a great extent that it sometimes overdetermined his own position on an issue- he was against whatever was the popular wisdom. This was especially true of issues that he saw as being contaminated with moralism, reform, or 'uplift.'

Mencken's mark can be found on many of Pearl's ideas.[13] They were both conservative on social questions and bordered on libertarianism (or "liberalism" as it was then known) in economics and statecraft. They both believed in a natural aristocracy of superior men, and were suspicious of unearned distinction. Mencken never went to university, and Pearl, while a product of Dartmouth College, did his graduate work at the University of Michigan and had spent a decade at the

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[11] Mencken, Henry Louis. *Thirty-five Years of Newspaper Work*. Hobson, *et. al.* (eds.) The Johns Hopkins Press. 1994. p.121

[12] Hobson, Fred. *Mencken, A Life.* Radom House New York. 1994. p. 337

[13] Pearl dedicated his 1927 book for young college men, *To Begin With, Being Prophyaxis Against Pedantry,* to Mencken. It is written in a 'Mencken-esqu' terse style, and consists mainly of lists of books that young men should read to become fully acquainted with the work of great minds.

University of Maine at Orono, away from the most prestigious centers of the American University system.[14] Both saw themselves as outsiders even thoguh they were near the top of the American pecking order. In this too, both men are shot through with the same contradictions: a mixture of boldness and self-consciousness, elitism and dislike of pretension, political conservatism and faith in the progressive power of free trade and unrestricted commerce. Echoes of this mixture of elitist conservative liberalism can be found in the early days of the Mt. Pelerin Society, and the 1938 Colloque Walter Lippmann in Paris. The closest contemoporary political comparison for Mencken and Pearl would be those of the Austrian liberal/monarchist Ludwig von Mises. They are truly out of place in the 21s century political landscape, where the presumed bond between liberalism and democracy has become much more intuitive. Mencken and Pearl's closest political analogues in the later 20th century would have been the individualist science fiction author Robert Heinlein, and perennial US Presidential candidate Congressman Ron Paul.

Pearl's Early Life and Career

Raymond Pearl was a big man with a big personality and an appetite and ambition to go with it. He was prolific in his scientific and popular writings and in his correspondences with his friends and family.[15] His undergraduate and graduate teacher, and his life-long mentor and colleague Herbert Spencer Jennings said of Raymond Pearl that

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[14] Technically 12 years but it seems that Pearl spent at least some of these last years in Washington D.C. Kathy Cooke has it that Pearl left Orono in 1916. (See Cooke, 1997)

[15] Bentley Glass. The Raymond Pearl Papers. *A Guide to the Genetics Collections at the APS.* http://www.amphilsoc.org/guides/glass/pearl.htm

> He was a man of unusual height and weight, physically an impressive figure. His was a masterful personality, of extraordinary resourcefulness and initiative, of wide knowledge, astonishing power of work, remarkable versatility and scope, and strong ambitions. His interest in biology was encyclopedic. In his contributions he touched upon most aspects of the subject. This was not a matter of merely the extent of scattered interests, but rather of the kind of interest, and of the kind of man that he was.[16]

And his close friend and academic ally Lawrence J. Henderson agreed:

> There are two kind of men of science whose interests and, activities greatly contrast. One kind, the orthodox, today very numerous, proceed by a kind of orthogenetical development and do not often step aside from a straight and narrow path. The other kind, rare today though often met with three or even two centuries ago, feel that their intense interest in all things — their *philosophical* interests, in an older sense of the word philosophical that has been preserved in the name of our Society — is a safe guide. Such a man was Francis Galton and another, in some measure a disciple of Galton's, was Raymond Pearl, who was elected to membership in the American Philosophical Society in 1915.[17]

Pearl was born in Farmington, New Hampshire in 1879, the only son of Frank Pearl and Ida Mae McDuffee.[18] The Pearls were from a respectable old New England Yankee lineage that had come to the Massachusetts Bay Colony a hundred years before the founding of the American Republic, and, going back before that the family claimed ancestors that had come over to England with the Norman conquest. His father worked at a grocery store and also as the foreman at a shoe factory,

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[16] Jennings, H.S. 1942. Raymond Pearl. *Biographical Memoirs of the National Academy of Science, USA.* 22:295-347.

[17] Henderson Lawrence J. Raymond Pearl Obituary Notice. *American Philosophical Society Yearbook 1940*. Press of the American Philosophical Society. Philadelphia. 1941

[18] Parker, Franklin. Raymond Pearl. Dictionary of Scientific Biography.

but the family was highly educated, including in the classics, and Raymond was expected to study Greek and Latin and attend college.[19]

He was schooled in his home town until he was 14, and then sent to nearby Rochester to improve his Greek and Latin in preparation for his matriculation at Dartmouth College two years later. There he scandalized his family by switching from Classics to Biology, under the instruction of Herbert Spencer Jennings. Pearl also began his life-long love affair with listening to and performing music at Dartmouth.[20] He did his PhD at the University of Michigan (also under Jennings, having followed him to Ann Arbor), graduating in 1902, staying four more years as an instructor, and meeting Maud Mary DeWitt from the Department of Biology, who he married in 1903.[21]

In 1905 both Pearls left Michigan for Europe, where Raymond did a year of post-doctoral finishing work. He visited the University of Leipzig and the Naples Maine Zoological Station, but spent most of his time in at the Galton Laboratory at University College, London with the eminent biometrician and eugenicist Karl Pearson. Pearl absorbed Pearson's methods of statistical biometry, and became an associate editor of Pearson's journal *Biometrika* until a falling out in 1910 ended the working relationship (although Pearl wrote an obituary memoir about Pearson in 1936.[22]) Pearson's influence on Pearl's working methods in statistics and his general outlook on

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[19] APS Biographical note. Raymond Pearl.

[20] Jennings, H.S. 1942. Raymond Pearl. *Biographical Memoirs of the National Academy of Science, USA.* 22:295-347.

[21] Incidentally, Pearl and his wife's collaborations began in 1901, when Pearl was studying at the University of Michigan. Pearl and (then) De Witt co-authored a paper entitled "Certain reactions of the common slug *Agriolimax campestris*".

Little, Michael A., and Garruto, Ralph M. Raymond Pearl and the Shaping of Human Biology. *Human Biology* 82:1. 2010. pp. 77-102.

[22] Memoir of Pearson: Karl Pearson, 1857-1936. *Journal of the American Statistical Association,*

science were considerable, even if the two men eventually parted ways on eugenics. Pearl read Pearson's book *The Grammar of Science* in his first year of graduate school, and later wrote that "it produced at that time such an effect on my intellectual outlook as no other book I have ever read."[23]

On his return to the United States Pearl worked as an instructor at the University of Pennsylvania, and then took a position as head of the Maine Agricultural Experiment Station at Orono in 1907, and stayed there until war work called him away in 1918.[24] The Hoover administration appointed Pearl to be Chief of the Statistical Division of the new United States Food Administration in 1917. There he headed a big government office and managed biologists and statisticians' work under him. The administrative experience that Pearl acquired there prepared him to run the Institute for Biological Research when it opened in 1925.[25]

In 1919 Pearl accepted a position in the Department of Hygiene and Public Health at John's Hopkins University, and was made Chief Statistician of Johns Hopkins Hospital, a title he held until 1935. Shortly after moving to Baltimore, a fire destroyed most of Pearl's books and reprints of papers. The fire marks a kind of half-way point in Pearl's career, and was an opportunity for him to make a new start. He was 40 years old, at the height of his intellectual powers, and newly arrived at one of the most prestigious universities in the country – certianly one of the only ones

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v. 31, pp. 653-664.

Referenced in Jennings, H.S. 1942. Raymond Pearl. *Biographical Memoirs of the National Academy of Science, USA.* 22:295-347.

[23] Raymond Pearl. *To Begin With, Being Prophylaxis Against Pedantry*. 1927 Knopf. p.37

[24] Jennings, H.S. 1942. Raymond Pearl. *Biographical Memoirs of the National Academy of Science, USA.* 22:295-347.

[25] Jennings, H.S. 1942. Raymond Pearl. *Biographical Memoirs of the National Academy of Science, USA.* 22:295-347. P. 298

willing to invest serious money and space in basic scientific research in the German model.[26] Pearl had spent his young adulthood as an extended journeyman, training with Jennings and Pearson, teaching, and developing the skills of a scientist-administrator in Maine and Washington. Now he had the opportunity to be a master, and he was going to take that opportunity.

Now we can return to the question of the anonymous critic writing to E. M. East regarding Pearl's dissent from eugenics. What was Pearl dissenting from? The term eugenics (latinate for "well born" or "well bred") was struck by Charles Darwin's cousin, the polymathic Francis Galton, in 1879. Galton's definition for the new field was "the study of agencies under social control which may improve or impair the racial qualities of future generations."[27] Eugenics was articulated by its proponents as a comprehensive scientific program to seize control of human heredity and bend it to the improvement of humanity. The program of eugenics was two-fold. The first part was research into inheritance, especially the inheritance of traits considered harmful. The second part focused on education and legislation aimed at limiting the reproduction of those persons known to possess harmful traits.[28] Acting as it did on the inheritance of 'traits' such as intelligence, alcoholism, insanity, and epilepsy, eugenics was always the prosecution of the goals of social

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[26] Chicago and Columbia also being notable, with Bryn Mawr in the unique position of a women's college pursuing the same goal.

[27] Diane Paul. "The Rockefeller Foundation and the Origins of Behavioral Genetics." in *The Expansion of American Biology.* Keith Benson, Jane Maienschein, Ronald Rainger, eds. Rutgers University Press. pp. 266-267

[28] This does not do full justice to the world-wide scope and variety of eugenics. I am dealing here mostly with what was called "negative" eugenics. There was also a pro-natalist "positive" eugenics, as well as a variety of neo-Lamarckian eugenic thought that emphasized improving human breeding through improving environmental factors such as sanitation, education, and moral reform. For more on eugenics see Daniel Kevles *In the Name of Eugenics: Genetics and the Uses of Human Heredity*. Harvard University Press, 1998. and Mark B. Adams *The Wellborn Science: Eugenics in Germany, France, Brazil, and Russia.* Oxford University Press, 1990.

management and control by other means. For this reason, it was popular among a wide spectrum of elite American society in the first half of the twentieth century. Conservatives adopted the language of eugenics to support punitive laws against the poor and to limit immigration. Progressives linked eugenics with efficiency, Prohibition, and birth control as the keys to uplifting the dominated sectors of the United States and to re-make them into more productive and respectable citizens.[29]

Despite the fact that genetics as a discipline owd much of its early success to eugenic ideas, eugenics as scientific practice began to pull away from eugenic social and political programs. In the 1920s eugenics came under attack from leftists like Hermann J. Muller and liberal centrists like Thomas Hunt Morgan and Raymond Pearl's mentor Herbert Spencer Jennings. By the thirties many geneticists were anxious to distance themselves from eugenics as a political program, and from its association with the extreme racial biology of Nazi Germany. In 1939 the Carnegie Institute of Washington withdrew its funding from the Eugenics Record Office at Cold Spring Harbor and the organization closed.[30] Still, many geneticists, anthropologists, legislators, and judges remained faithful, and laws requiring compulsory sterilization of those deemed unfit remained on the books well after the end of the Second World War.[31]

Pearl dissented from eugenics for two reasons. First, he saw it as increasingly untenable scientific position to hold in the light of new research in genetics and his own re-analysis of studies of

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[29] Garland Allen. 1997. "The social and economic origins of genetic determinism: a case history of the American Eugenics Movement, 1900–1940 and its lessons for today." *Genetica.* 99 pp. 77-88.

[30] Allen, *Ibid.* Mark B. Adams, personal communication

[31] Paul Lombardo *Three Generations, No Imbeciles: The Supreme Court, Eugenics, and Buck v. Bell.* 2008, Johns Hopkins University Press and Paul Lombardo (Ed.) *A Century of Eugenics in America: From the Indiana Experiment to the Human Genome Era.* 2011. The Indiana University Press.

inheritance. Second, Pearl was happy to criticize eugenic orthodoxy in his role as gadfly and exposer of the pretensions of reform and uplift. Though his training was in vital statistics and biometry, Pearl was receptive to Wilhelm Johannsen's study of inheritance in peas, and to pay attention to the 're-discovery' of Mendel's laws of inheritance, which was then the topic of intense interest. He also saw that the two approaches, biometry on the one hand and genetics on the other, were not necessarily so incompatible as was commonly held at that time.[32]

Pearl's practical experience with the genetics of chicken breeding at the Main Experimental Station gave him practical grounds on which to question the validity of eugenic theories of inheritance early on. Pearl had applied Johannsen's "pure-line" breeding of garden peas to chickens. By 1914 he had concluded that "being a little acquainted with the frailties of both poultry and poultrymen, I am not too optimistic as to the outcome of trying to breed chickens by formula." Genetics was not going to lead to a systematic method of 'eugenic' improvement in chicken breeding, due as much to the human obstacles to such a program as to scientific ones.[33]

Pearl aired his views on the subject of *human* breeding for racial betterment in a paper he delivered at the Fifth International Conference on Genetics in Berlin in September of 1927, and in November of the same year, he wrote a popular essay on eugenics for H. L. Mencken's magazine *The American Mercury.*[34] In it, Pearl separated out two distinct trends within eugenics since its foundation by Galton. The first trend was the entirely sober and objective study of human heredity, the second the inescapably emotional and subjective project of human improvement. Pearl upheld Galton's first project as noble and good and placed himself in its lineage. But he also

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[32] Kingsland "Raymond Pearl on the Frontier"

[33] Kathy J. Cooke. "From Science to Practice, or Practice to Science? Chicken and Eggs in Raymond Pearl's Agricultural Breeding Research." *Isis* 88:1 (March 1997) p.84

[34] Donald G Patterson and Edmund G. Williamson. "Raymond Pearl on the Doctrine of "Like Produces Like" *The American Naturalist* v. 63 no. 686 p. 265. 1929.

noted that Galton's work was done in ignorance of either Gregor Mendel's experiments that established the independent assortment and segregation of traits, and the work of Johannsen, which showed that even "pure-line" peas exhibited significant variation in phenotype. As Pearl put it:

> "A large bean may throw uniformly smaller offspring than a smaller bean. As we now know, the relation between the bodily characters of parent and offspring depends, not upon what the bodily character of the parents were, but instead upon their genetic contributions- the genes which they carried in their germ cells."[35]

Pearl criticized the social and political project of eugenics for claiming that the best kinds of people have the best children, and pithily observed that in this case "the best" was simply code for "*My* kind of people" or "People *I* happen to like."[36] Pearl contended on the other hand that genetics showed that like did *not* produce like, that famous men did not produce famous offspring at a particularly high rate, and that by claiming otherwise eugenicists did themselves a disservice. Pearl chided eugenicists that teamed up with reform-oriented legislators and moral uplifters that "for their public teaching, their legislative enactments, and their moral fervor are plainly based chiefly on a pre-Mendelian genetics, as outworn and useless as the rind of yesterday's melon."[37]

Mencken had written an opinion piece in the *Baltimore Sun* in May of the same year where he laid out many of the same principles that Pearl would echo with more scientific rigor and less satire. Mencken derided eugenics and birth control as "mainly blather." He went on to note that distinguished men often have sons that fail to live up to their father's mark, and that in any sensible society there should be many ways for a talented youngster to find his way to the top. He

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[35] Pearl, R. (1927). "The biology of superiority." *The American Mercury* 12: 257-266. p. 258

[36] *Ibid.* p.261

[37] *Ibid.* p.261.

pointed out that many truly great men, such as his much admired Friedrich Nietzsche, were not great material eugenically speaking, being often physically weak, congenitally deformed, afflicted by illness, or mentally unstable. Furthermore, great men as often as not died bachelors, thus not reproducing their traits and failing to succeed on the basis of the Darwinian notion of the struggle for existence. And in any case this was just as well, because "…when the relatively superior and distinguished class ceases to be fecund… natural selection comes to the rescue by selecting out and promoting individuals from the classes below." This is also a very good summary of Raymond Pearl's views on the matter.[38]

This is not to say that Mencken gave Pearl this opinion. In fact it was almost certainly the other way around. Pearl had begun questioning the validity of some aspects of eugenics since his days at the Maine Agricultural Station in Orono. In a January 1908 letter to his mentor Herbert Spencer Jennings, Pearl declared his skepticism toward the evidence of any strict hereditary rules, based on the results of his research with poultry.[39] And in 1919 Pearl wrote in *The Eugenics Review* that "the difficulties, both social and genetic, which beset sterilization as a remedial eugenic measure are so considerable as to make on doubtful of its accomplishing much."[40] Pearl's broadsides against the scientific basis of eugenics were taken up in the social science literature as well as the popular press. For example, a criminology study (granted, published in Pearl's house journal *Human Biology*) noted that "to be sure, Pearl, who we are quoting, speaks of the vegetable

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[38] Mencken, H. L. (1927). "On eugenics." *Baltimore Sun*, May 15, 1927.

[39] Michael Mezzano. "The Progressive Origins of Eugenics Critics: Raymond Pearl, Herbert S. Jennings, and the Defense of Scientific Inquiry"

The Journal of the Gilded Age and Progressive Era

Vol. 4, No. 1 (Jan., 2005), pp. 83-97

[40] Raymond Pearl. *The Eugenics Review* 1919.

bean, but, to venture an atrocious pun, his statement is undoubtedly true of the human bean as well."[41]

However, some biologists disagreed with the accuracy and appropriateness of Pearl's arguments in *The Biology of Superiority*. Donald Patterson and Edmund Williamson of the University of Minnesota criticized Pearl's interpretation of Galton's data, questioned his understanding of genetics and statistics, and opined that a popular publication like *The Mercury* was really no place for a respectable biologist to weigh in on scientific matters.[42] This last seems unfair in the light of the fact that Pearl was basically arguing the same thing: that the science of genetics should keep out of social policy-making and stop giving scientific imprimatur to social engineering dressed up in eugenics' clothes.

Some of Pearl's allies privately questioned his brashness but remained personally and academically close to him. Harvard botanist E. M. East (see the first section of this chapter) would go on to join Pearl's International Population Union (the IUSIPP) despite his differences with Pearl over the question of eugenics. Their letters indicate the level of intellectual confidence that Pearl felt on the topic. His response to East's questions in 1927 was firm:

Dear East, …

[41] — J.V. DePorte and Elizabeth Parkhurst: Homicide in New York State, A Statistical Study of the Victims and Criminals in Thirty-Seven Counties in 1921-1930. Human Biology v. 7 no. 1 pp. 47-73.

[42] Donald G. Patterson and Edmond G. Williamson. "Raymond Pearl and The Doctrine that Like Produces Like" *The American Naturalist* 63: 686 May-Jun 1929.

> I cannot entirely agree with your position problem and the relation of your work to it if I correctly understand what your position is…

> You go on to say that naturally no modern geneticist believes in the extremely high correlation between characters and characteristics of parents and offspring. But that is exactly the point of my whole article— that no modern geneticist does believe in such a high correlation. That is precisely why I wrote the article; to emphasis the fact that no qualified geneticist does believe in it. It is only the brash eugenic boys who take that position, and they are the people I am after, not the geneticists. Could anything be plainer than this in the whole article? You seem to take the position that I am attacking geneticists. On the contrary, what I thought I was doing was defending geneticists from its [*sic.*] friends.

> …I appreciate very much your writing as you did. It seems to me that it is very much better to discuss such things when two people are old friends, rather than to keep them bottled up and let them rankle.

And in a follow-up letter on the 2nd of December, Pearl re-iterated his point. "There is one point in the whole matter upon which I am afraid we do fundamentally, and even violently, disagree. You seem to think that eugenics and genetics are synonomous [*sic.*] terms. <u>I do not.</u>[43]"

Pearl's commitments against eugenics extend to his skepticism about race science as well. But hi stance on race and the color line were characteristic of the stances he took in other areas of life. He was fiercely defiant of tradition and convention when it came to what he saw as populist bigotry, but was capable of breathtakingly quick reversals when he came up against elite prejudice, and the deeply ingrained racial divides of American life. Pearl's relationship with Walter White, the first Black leader of the NAACP, is instructive. From 1928 on, Pearl and White were at least slightly personally friendly, and visited each other socially in New York and Baltimore, though Pearl often did not make it to his appointments and White was sometimes too

[43] Raymond Pearl papers Box 7 Folder # 6, 1927 APS

busy with emergencies. They seem to not be close friends, but Pearl does seem at least intermittently interested in White, and offers him help and the use of the Institute for Biological Research's library for White's research into the secondary literature on skull suture fusion in black and white children. Pearl responded enthusiastically to White sending him a copy of *Rope and Faggot*, his 1929 study of lynching in the American South, but Pearl would not write a review of it, nor consent to his letter to White being excerpted in place of a review. While it seems that Pearl had some interest in and affection for White, he did not want to be publicly associated with his work; however their relationship continued into the 30s. At White's request, Pearl participated in a committee set up to study allegations of misconduct at Harlem Hospital. Pearl and his wife went to the Cotton Club one night in May 1934, scheduled to meet White and his wife, but White was unable to make it that night. Still, Pearl reported to White in a subsequent letter that he had a nice time. Pearl wrote to White in 1936 that he had looked into the differential fertility rates of black and white women, and, as far as he can tell, barring differences due to the usage of birth control, there does not appear to be significant differences in "innate fertility" between the two races.[44] It seems that Pearl was not a rigid ideological racist, but was sensitive to the perception that his iconoclasm had passed beyond acceptable boundaries. In a sense, though Pearl's human biology did not lead him to take a hard position for or against the relative superiority of the different races, he was quite interested in the biological basis of racial difference. And in the end, it is instructive to know that racism in America does not depend on scientific imprimatur for its legitimacy. And seemingly for Pearl, and the color line in the Progressive Era, scientific racism was hardly necessary at all social inequality being quite sufficient for him.

[44] Pearl Papers, National Association for the Advancement of Colored People folders #1-3

Garland Allen has documented Pearl's dissent from eugenics in his essay *Old Wine in New Bottles: From Eugenics to Population Growth in the Work of Raymond Pearl*. Allen demonstrates Pearl's growing disenchantment with eugenics, and his move throughout the 1920s and '30s away from the problematic of eugenics as a field and towards what Pearl called "the problems of the population."[45] I substantially agree with Allen's treatment of the subject but differ on two points. First, Pearl was not a liberal or a progressive, as Allen and Michael Mezzano imply, but rather a political conservative who mistrusted progressivism politically and scientifically.[46] Second, while Pearl tried to re-constitute the research program of eugenics as the scientific study of populations, he rejected concerns about overpopulation in terms of absolute numbers, and constructed a model of population growth that was self-correcting and would naturally arrive at a steady state. Pearl's signature conceptual tool and symbol for the self-regulation of the population was the logistic curve.[47]

The Logistic Curve

The statistician Lowell J. Reed was a fellow New Englander and long-term collaborator with Raymond Pearl. Reed completed his PhD in 1915 from the University of Pennsylvania and Reed and Pearl probably began their relationship there while Pearl was an instructor. Reed moved to

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[45] Garland E. Allen. "Old Wine in New Bottles: From Eugenics to Population Control in the Work of Raymond Pearl." in *The Expansion of American Biology*. Keith Benson, Jane Maienschein, Ronald Rainger, eds. Rutgers University Press. pp. 231-261.

[46] Michael Mezzano. "The Progressive Origins of Eugenics Critics: Raymond Pearl, Herbert S. Jennings, and the Defense of Scientific Inquiry"

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Vol. 4, No. 1 (Jan., 2005), pp. 83-97

[47] See below for more on the logistic curve

Maine to work with Pearl during his tenure at the Maine Agricultural Station at Orono. He also followed Pearl into government service in Washington during World War I (see below), where he was Chief of the Bureau of Tabulation and Statistics of the War Trade Board. Reed came to Johns Hopkins in 1918 and spent the rest of his career there as a biostatistician and later as an administrator and finally (for three years) as President of the University and Hospital before his death in 1966.[48]

Reed and Pearl's most important work together was their landmark 1920 paper in which they developed their mathematical model for population growth: the logistic curve. In it, Reed and Pearl made the argument "that growth of population is fundamentally a phenomenon like autocatalysis," i.e., that a growing population would, like an enzymatically catalyzed chemical reaction, have a slow start which then would speed up exponentially before slowing down again as it consumed its remaining substrate. Warning against predicting population growth on the basis of extrapolation from arithmetic or geometrical series, or according to a higher-order parabolic curve, Reed and Pearl argued that an S-shaped curve best fit empirical data for the growth of population under a variety of conditions.Their paper concluded that:

> "As the population becomes more dense and passes into a phase where the still unutilized potentialties of subsistence, measured in terms of population, are measurably smaller than those which have already been utilized, all of these forces tending to the increase of population will become reduced."

[48] Lowell J. Reed, 1886-1966 Author(s): Iwao M. MoriyamaReviewed work(s):Source: The American Statistician, Vol. 20, No. 3 (Jun., 1966), p. 50

Lowell J. Reed (1886 - 1966) Author(s): Clyde V. KiserReviewed work(s):Source: Population Index, Vol. 32, No. 3 (Jul., 1966), pp. 362-365

The population of the United States, like flies in a crowded jar, would grow until there was no more room. The death rate would rise as the length of life and birthrate fell, leading to an equilibrium or steady-state population.[49]

Reed and Pearl discovered that a Belgian mathematician named P. F. Verhulst had described the same curved path of population growth in 1838, which Verhulst called the 'logistic curve,' A name that Reed and Pearl adopted and generalized to describe a wide variety of biological phenomena. In 1953 Reed made a point about the insufficiency of analysis for solving problems. This may be related to the approach that Pearl was taking along with Lotka away from simple partitioning and towards an approach that factored in the interactions among elements within the system. He wrote

> Analysis is always easier than synthesis. In any field of human activity it is simpler to obtain individual bits of knowledge than it is to put those bits together into a general pattern of human behavior. So also in the various fields of science, it is easier to obtain specific pieces of scientific observation than it is to put those pieces together to form a general scientific law. The process of reasoning involved in integration or synthesis is, however, statistical in character, and I should hope that the statisticians of the future would take synthesis as well as analysis as a part of their responsibility and would play a leading role not only in the planning of broad programs, but also in the development of scientific generalizations.[50]

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[49] On the Rate of Growth of the Population of the United States Since 1790 and itsMathematical RepresentationAuthor(s): Raymond Pearl and Lowell J. ReedReviewed work(s):Source: Proceedings of the National Academy of Sciences of the United States of America,Vol. 6, No. 6 (Jun. 15, 1920), pp. 275-288

[50] Man as a Planning AnimalAuthor(s): Lowell J. ReedReviewed work(s):Source: Journal of the American Statistical Association, Vol. 47, No. 257 (Mar., 1952), pp. 1-5

Though my reading of Pearl's work and the work he supervised is that it is substantially a break with the eugenic tradition it emerged from, there is a more complicated question when it comes to the legacy of Malthusianism. The logistic model of population growth and stabilization promoted by Pearl and Reed do not corroborate the stereotypical view of Thomas Malthus's demographic crisis and collapse. The logistic curve flattens out and stabilizes; it does not drop precipitously after the limits of the resources of a population have been reached. Malthus's influence on eugenics in the form of concern with catastrophic population collapse and "racial suicide" have been documented (**ref**). But it is also true that Pierre Verhulst's development of the logistic curve owed much to his reading of Malthus.

Pearl's contribution to the debate lay in his early acceptance of the particulate model of genetic inheritance, and his adaptation of the mathematical tools developed by the biometric school to the problems of the analysis of inheritance. Pearl still worked with the tools of the pedigree and spoke language that American and British eugenicists would have been able to understand, but he did so partially to criticize them with their own methods, but also to introduce his own tools and concerns into the debate. In a 1928 publication in the collected papers of the Institute for Biological Research, Pearl carried out a detailed study of a working class family of seven in Baltimore whose members were ravaged by tuberculosis and related "Breakdown of the Respiratory System"[51] He comapred the pedigree of the family in question to the pedigree of a closely-related family of cousins from a similar stock and in a similar circumstance of life, who did not suffer from the same high rate of active and devastating tuberculosis. The difference between the two families was neither their genetic inheritance, nor their social or physical

[51] Pearl, Raymond. The Constitutional Factor in Breakdown of the Respiratory System. *Annals of Eugenics*. Vol. 2, pp. 1-24. 1927.

environment. The difference was that the healthy family had few children, while the sick family had many. The sick family's parents had married early while their average fecundity was still high, and had consequently had five children in a small house supported by the modest wages of their father. The differences in health were actually differences in natality.

This is where Pearl's half-way Malthusianism comes into play. Referencing some of his own work on the population dynamics of bacteria in a petrie dish, Pearl intimated that a similar set of forces were at play. When bacteria run out of room and food, their death rate goes up and their birth rate goes down until a rough equilibrium is achieved. Rather than running up to a dramatic crash in numbers, a balance of misery is reached where the population does not overshoot its resources, because every individual bacterium is living close to the limits of its life at all times. The ramifications of Pearl's study of the proletarian family in Baltimore was the same: as density increased and as available resources were consumed, the mortality rate, especially that of children increased, and the average rate of reproduction fell. Thus a rough equilibrium was maintained, as poor people just squeaked by, while being encouraged by their tough circumstances to marry later, have fewer children, and 'naturally' check their own previously uncontrolled fecundity.

Pearl concluded his piece by pleading for more mutual understanding and cooperation between medicine and bacteriology, under the larger supervision of "the biometrician" who "feel that the point of view and evidence which the crucial experimenter presents is often narrow and inadequate, because it neglects all but one aspect of an obviously broad and complex biological problem." And Pearl reiterated this plea in another paper in the IBR's bound collection of transactions:

> The experiments here reported also suggest that there is probably a limiting asymptote to the effect upon duration of life which can be produced by increasing the degree of crowding of the flies in the bottle. After a density of 200 flies per one ounce bottle is reached further increase in density of population produce but slight further reductions in mean duration of life.[52]

So the population, though self-regulating in the long-run, functioned as an ever-increasing scale of misery in the short term. This is in fact rather close to Malthus's argument in his *Essay on the Principle of Population*, where he cautioned that population would grow until it was limited, not by catastrophic failure and collapse, but rather that "… the actual population kept equal to the means of subsistence, by misery and vice."[53]

The Institute for Biological Research

Pearl established himself as the Director of the Institute for Biological Research at Johns Hopkins on the basis of a generous grant from the Rockefeller Foundation. From 1925 to 1930, this made Pearl one the highest paid[54] professors at Hopkins, and gave him complete autonomy to pursue his work in "health, longevity, population growth, and human genetics" which Pearl called either

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[52] Pearl, Raymond. *American Naturalist*. Vol. 61, p. 316. (Reprinted in *Collected Papers of the Institute for Biological Research*, volume 1. 1928. Princeton University Library)

[53] Malthus, Thomas Robert. 1798. *An Essay on the Principle of Population.* Oxford World Classics. *Ch 6. p.61*

[54] $15,000/year in 1925 dollars.

"general biology" or "human biology".[55] At one point in 1929 Joseph Ames, one of Pearl's supporters and President of Johns Hopkins University, suggested that Pearl's Institute for Biological Research be re-named the Institute for Human Biology, though Pearl demurred.[56]

In describing the use of computing machines in office work in the 1920s and '30s, historian of computing James Cortada quoted C. Wright Mills in saying that "machines and social organization had begun to interact and … it is a true mark of the 'era of scientific management in the office'."[57] The availability of mechanical calculators was not in itself decisive— it merely facilitated the real change, which was the move to a ramified multi-unit work process relying heavily on the feminized labor of secretaries and computers. The reciprocal interplay between tools and social organization in the white-collar workplace was reproduced by Raymond Pearl in his plans for the Institute for Biological Research.

Sharon Kingsland characterized Pearl's work on Drosophila at the Institute for Biological Research as doing "… for the flies what the actuary did for human populations, drawing up tables of vital statistics and survivorship curves for all manner of flies: fed, starved, crowded, and mutant flies."[58] Pearl described the research program of the Institute for Biological Research in a

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[55] Kingsland, Sharon. 1985. Modeling Nature: Episodes in the History of Population Ecology. University of Chicago Press. pp. 62-63

[56] APS Pearl Papers. Johns Hopkins University, Correspondance with Joseph S. Ames. Box 16. Folder 1.

[57] James Cortada "Before the Computer: IBM, NCR, Burroughs and Remmington Rand the Industry they Created 1865-1956. Princeton University Press. 1993. p. 186

[58] Kingsland, S. 1984. Raymond Pearl: On the Frontier in the 1920s. Raymond Pearl Memorial Lecture, 1983. *Human Biology* 56(1) 1-18.

letter to the President of Johns Hopkins where he characterized his planned work as following directly in the pursuit of those areas he had laid out seven years earlier to the Department of Hygiene and Public Health in 1918:

> It was developed around the idea that duration of life, the central problem of vital statistics, was really a problem of general biology, and that the attack upon it was likely to be most fruitful if it proceeded in two parallel lines of study. Along one of these lines man would be used as material, and the method of approach would be purely statistical— the application of modern biometric and genetic technique to human material. Along the other line of approach animals lower in the evolutionary scale than man, and plants would be used as material, thus giving the opportunity to bring to bear upon the problem the experimental as well as the biometric technique…
>
> The development of the department's research program, originally confined exclusively to the problem of life duration **[underline mine]**, demonstrated that this was an even broader biological problem than it had first been conceived to be. Natural death and senescence were realized to be but parts of the whole life cycle, inseparably integrated with its earlier parts. In consequence we were led into the study of growth, both of individuals and populations. Again, with the same inevitability, the adaptive character of the responses of organisms to all parts of the life cycle demanded consideration and investigation. All the varied branches of modern biological technique and methodology were increasingly called into operation, including biochemistry, biophysics, animal behavior, genetics, etc.…
>
> The central idea behind its intellectual organization, faithfully reflected in its physical structure and personnel, is that within its walls investigations in general biology and human biology shall go hand in hand. The ultimate goal of all biological studies may fairly be held to be a sound and comprehensive understanding of human life. But the basis for such an understanding must be built upon general biology.[59]

This long passage encapsulates the research program of the Institute marvelously. The intellectual and organization work (largely the labor of women, as we will see below) was focused on solving "the central problem of vital statistics," whose object of study is conceived of as "man"! Irony aside, Pearl himself confirms what I have been arguing: that "the intellectual organization" of the Institute for Biological Research was "faithfully reflected in it physical structure and personnel."

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[59] APS Pearl Papers. Institute for Biological Research. 1926-1929.

In 1925 the Institute for Biological Research filled the entire fourth floor of the Hunterian Laboratory for Experimental Biology, a part of the Johns Hopkins Medical School. In 1925 the staff of the Institute was 13 full time employees, plus a consulting chemist, a consulting Mathematician, and a consulting pathologist and his (female) assistant. There were also three graduate students engaged in research. In 1927, the Institute expanded and took over space on the third floor of the Hunterian Laboratory as well.[60] With the expansion of the Institute, Pearl hired new research and administrative staff, and, as he put it "the enlargement of the staff necessitated a more formal organization into Divisions…"[61] remaking the organization of the Institute along the lines of a corporation in miniature, with six departments: Administrative, General Biology I, General Biology II, Biometry, Human Genetics, and Biochemistry. The staff grew to twenty-five. Data at the Institute was coded by hand into punched cards to be counted by tabulating machines, and graphic representation of the data were drawn with drafting tools to fit the computer values.

Joanne Yates differentiates systematic management of firms' organization and communication and supervision from the scientific management of F.W. Taylor and the time-and-efficiency study of individual bodies of workers and work processes. Though managers in the business world experimented, sometime enthusiastically, in rationalizing the work process of office workers, they encountered many of the same problems that their peers did in the machine shop and the factory floor. Workers and foremen resisted their loss of autonomy and protested that their work process did not lend itself well to this kind of abstract rationalization. But work rationalization was a

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60 *Ibid.*

61 *Ibid.*

powerful ideological and cultural trope, which allowed managers, investors, and regulators to coordinate their respective roles in production, distribution, and regulation.[62]

Pearl's lab did not clearly follow either of the above sytles of workflow rationalization, the work itself being too small-scale and artisanal to be managed in such ways. It is unclear that the adoption of multiple units of work was a necessary measure for efficiency and control, or if it was just a familiar and comfortable structure that Pearl (and his employees at the IBR) had experience with.[63] While many of the particular lines of task work like care for the animal colony, filing and data management, and correspondence and dictation could be routinized to an extent, it is an open question as to how useful this approach was to performing experiments and analyzing data. It is more likely that the reorganization of IBR along the lines of a managerial corporation or a bureau in the federal government had as much to do with the adoption of a prestige form granting increased intelligibility to patrons and allies[64]. Nevertheless, the work environment at the IBR must have felt a great deal like an office for a big firm or a government bureau. The sounds of people at their desks; womens' and mens' voices; the rustling of sheets of paper; cards being punched; the soft clicking of the mechanical Monroe and Brusviga calculators; the intermittent clatter of a punched card tabulator working though a stack of cards; bundles of documents being sent from subordinate departments to the center[65]. Pearl's deployment of the life table, combined

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[62] See Margery W. Davies (1982) *Woman's Place is at the Typewriter: Office Work, and Office Workers 1870-1930* for more on gender and the rationalization of office work, especially chapter 6, "Scientific Management and the Office"

[63] Joanne Yates *Control Through Communication:The Rise of Systems in American Management*. 1989. Johns Hopkins University Press.

[64] DiMaggio and Powell (1983), *The Iron Cage Revisited: Institutional Isomorphisms and Collective Rationality in Organizational Fields.* The American Sociological Review 48: 147-160.

[65] Pearl replies, October 26th, 1921

> …About calculating machines, I should say that unless you are flush with money, I would get the Monroe instead of the electrically driven Millionaire. The latter is undoubtedly a better machine,

with the rationalized, multi-unit organization of the IBR, and the computational techniques of the insurance industry and the government planning office was an innovative combination because it offered new approaches to solving problems in population research. It made the IBR an intelligible organization to actors in other fields such as the Rockefeller Foundation, the administration of Johns Hopkins University, and organizations within the US government like the National Research Council[66]. In 1923 the Fleischmann Yeast Company had offered to make Pearl the director of their research laboratory in New York. Clearly there was significant overlap between the techniques of academic biostatistics and those of commercial and industrial research and development.[67]

> but I doubt whether from a practical view point, it is enough better to warrant the large difference in price. The Monroe people have a new twenty hole machine at $400 which they demonstrated to me the other day, that is just coming on the market, and is much superior to the old Monroe, both because of its larger capacity and other reasons as well. I would certainly advise getting this rather than a large electronically driven Millionaire. The only other machine of the same class as these two is the Brunsviga. Personally I like it better that either, but that merely means that I was brought up on it, and am prejudiced. Every on else here in the laboratory prefers the Monroe. Either will do the work equally well. I do not know whether it is possible to import Brunsviga now or not. If you should get one, consider nothing but the largest 20 hole Machine…

APS: Raymond Pearl Papers.

Henderson, Lawrence J. Folder #1, 1914-1922

[66] Pearl was initially hostile to the aims of the NRC as a coordinating body for research work, but this did not stop him from approaching it for support for the IUSIPP in the 1930s.

[67] And as Alfred Chandler pointed out in *Scale and Scope*

> "Processors of food and consumer chemicals were among the very first American enterprises to organize research units that did more than test or provide quality control… During the 1920s the majority of food and chemical firms that ranked among the top two hundred made equally substantial investments in laboratories and set up separate departments to administer them."
>
> p. 162

Raymond Pearl's Center of Calculation in World War I

Pearl's training in the techniques of statistical analysis were parallel to his training in the techniques of organization and co-ordination of the work process of a large office, as well as the use of tools and machines borrowed from government and business statistical work. His work process and organization at the IBR closely resembled the style of work he acquired at his former place of employment during World War I at a center of calculation in the US Government, where he served in the from June 11, 1917 to February 28, 1919[68]. Called to Washington by Herbert Hoover, Pearl ran the statistical office at the United States Food Administration, employing as many as 35 people. Pearl's group took in information on the supply and price of food commodities from all over the country, analyzed it, and produced reports on the U.S.'s agricultural production, which was then used by the Hoover administration to monitor the national food supply during the war. This was part of a broader national mobilization of technical and managerial expertise during the war, which consolidated and spread the trend towards the rationalization of national institutions as well as the spread of punched-card technology[69].

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[68] History of the US Food Administration 1917-1919. p.378

[69] The task of mobilizing men and society for warfare was approached according to the experience of the various nations in governmental regulation and in organizing business. To a large extent, this tasks was accomplished by blowing up to a national scale known ways and tools of organizing big production and distribution. A key tool was operational statistics processed by using punched cards.[1]

The national economy was mobilized through the introduction of the command economy**.** To control the economy the government in the summer of 1916-- before entering the war-- carried out a census of the production capacity of about 80,000 industrial establishments. This census was managed by a special administrative body, the Industrial Preparedness Committee of the Naval Consulting Board, and was processed using punched cards. Notably, it was not carried out by the Census Bureau. Later several special administrative bodies pushed to control the economy, such as the Food Administration, the Fuel Administration, the Railroads Administration, and the War Industries Board.[2]

> Calculators… went to war" wrote historian James Cortada, "but the dramatic examples of data processing at work were punched card gear."[3]

Historian of computing David Alan Grier described Pearl's office at the Food Administration as a busy center of machine and human calculation:

> At the center of his organization was a punched card processing room with a staff of several dozen machine operators. Surrounding this facility were several smaller offices, each charged with preparing statistical reports on a specific aspect of the agricultural economy. One office handled sugar production, another food storage, and a third retail prices… The volunteers, who were almost exclusively women, in contrast to the male computing staff, were given a package of forms and instructions to collect the prices of specific items"[70]

The Food Administration addressed itself to two problems of wartime food production: "the assurance of an adequate supply of foodstuffs" for the U.S. and its allies, and "the protection of our people from prices which threatened… to disrupt the entire social and commercial life[71]." The policy tools the Food Administration took to be proper and acceptable ones were mainly to focus on encouraging conservation at the consumer level, to reduce waste and discourage hoarding by distributors, and to coordinate government purchases with other large buyers. The Food

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[1] *Punched Card Systems and the Early Information Explosion*. p..63

[2] *Punched Card Systems and the Early Information Explosion*. p.64

[3] Grier. p. 145

Cortada, James *Before the Computer* p.81

[70] David Alan Grier. *When Computers Were Human.* Princeton University Press. 2005. p.147

See also Raymond Pearl and Magdalen Burger, "Retail Prices of Food during 1917 and 1918," *Publications of the American Statistical Association*, vol. 16, no. 127, September 1919, pp.411-39.

[71] History of the US Food Administration 1917-1919. p. 60

Administration used its power to purchase large orders of wheat, other coarse grains, and sugar to stabilize fluctuations in the price of these commodities, either by counter-cyclical purchases, or through setting an effective ceiling on prices based on what the government was willing to pay[72]. The price-setting function of supply and demand was still held up as an ideal, but it was recognized that "under war conditions it either did not operate at all or operated so slowly that damage was done[73]." Explicit price controls, quotas, and other aspects of a "command economy" were avoided as un-American, and hostile to business interests.

These techniques of governance and knowledge over national population were developing rapidly in the U.S., the Soviet Union (before the purges of the 1930s), in the interwar Wiemar Republic, and somewhat later in Britain and France.[74] But the ways in which wartime food policy was enacted varied from country to country. Herbert Hoover's approach to food scarcity during the war was not to impose rationing, as in Europe, but instead to "mobilize the spirit of self-denial and self-sacrifice in this country.[75]" The federal government did intervene in food prices and supply: through strategically purchasing wheat, flour, and sugar, through prohibiting "unnecessary resale" in order to discourage hoarding, and through the "stimulation of hog production."[76] Somee administrative measures were also enacted,: hoarding and the destruction of

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[72] History of the US Food Administration 1917-1919. p.60

[73] History of the US Food Administration 1917-1919. p. p.60

[74] Tooze, Adam (2001), *Statistics and the German State 1900-1945*: The Making of Modern Economic Knowledge. Cambridge University Press.

[75] *Food: Control and Distribution of Food Supplies.* United States Senate Committee on Agriculture and Forestry.(1917) p. 12

[76] See also: *Reference handbook of Food Statistics in Relation to the War* (1918) by Raymond Pearl and Esther Pearl Matchett, Statistical division, U.S. Food administration. Published 1918 by Govt. print. off. in Washington . Written in English.

foodstuff for the purpose of raising prices was outlawed, and government bodies were empowered to requisition food if necessary, though this power was almost never used.[77]

Foucault classified this style of food supply regulation practiced by the Food Administration as belonging to the category of governmental practice he called "security." The strategy of security, applied to public health, food prices, and other questions of the administration of life, does not rely primarily on the power to forbid or prevent. Instead "…the essential function of security, without prohibiting or prescribing, but possibly making use of some instruments of prescription and prohibition, is to respond to reality in such a way that this response cancels out the reality to which it responds."[78] The key thing here is that the regime of security does not try to prevent famine, starvation, or disease. Security takes these things as a given, as part of reality, and tries to respond to them in order to counteract them. This entails gathering detailed information about the phenomena of interest, which in turns requires an apparatus for knowledge collection, storage, and processing, and a set of tools to analyze and interpret findings. Foucault argued that the regime of security was a key part of the development of the notion he called biopolitics. Biopolitics was the set of practices, concepts, and technologies which established a relationship between government and living populations, a relationship of knowledge and power oriented towards maximizing the productivity of life. Here again we see the relationships between the tools and concepts of government, management, and those of the life sciences, especially those concerned with populations. Raymond Pearl's work at the US Food Administration, his vision for

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[77] *Food: Control and Distribution of Food Suppliess.* United States Senate Committee on Agriculture and Forestry.(1917) pp. 4-6

[78] Foucault, Michel. *Security, Territory, Population.* pp. 41-42, 56,

the IBR, and as we will see in the next chapter, his colleague Alfred Lotka's passage between the IBR and the life insurance industry bears this out.

The International Union for the Scientific Study of Population Problems

In January of 1926 Pearl received a letter from Clarence Cook Little,[79] then the president of the University of Michigan. Little tried to recruit Pearl to attend an international conference on population problems in Geneva, Switzerland. The conference was to be held in the Summer of that year, "under the auspices of the Neo-Malthusian League."[80] Pearl accepted, and over the next few months he began to play a greater and greater role in the planning and direction of the conference. But first he made sure that he was being represented as an expert on scientific matters of population study, instead of with the "ethical division of the conference," as had been first proposed.[81] He then suggested that the whole project be postponed for one year, and that it be held in Berlin instead of Geneva, so that it could be co-located with the International Genetics Conference and thus increase the likelihood that "prominent" scientists would attend. The correspondences between Pearl and Little over the next months discussed the various challenges that they thought would come up for their new project.

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[79] Little was a geneticist and cancer researcher, eugenicist and birth control advocate, and director of the Jackson Laboratory in Bar Harbor, Maine. He cofounded the Birth Control League with Margaret Sanger in 1921.

Crow, James F. "C.C. Little, Cancer, and Inbred Mice." *Genetics*, 161: 2002 pp. 1357-1361.

[80] Little to Pearl. January 15th, 1926. Pearl Papers

[81] Pearl to Little. February 1926. Pearl Papers.

The three biggest concerns the League were, first of all, coordination of so many persons and institutions over large spans of space and time, secondly securing funding for the conference, and thirdly establishing and maintaining the highest scientific standards and credibility in the persons and investigations associated with the conference. The men immediately began strategizing over which philanthropic foundations and government agencies would most likely support the population conference, quickly settling on the Laura Spelman Rockefeller Foundation and the National Research Council as likely sources of funds. At the same time, they discussed how to sideline the non-scientist Margaret Sanger from the proceedings, despite the conference being convened under the aegis of one of Sanger's own organizations![82] Pearl also stated baldly that he would refuse to have anything to do with the conference if it did not ban the powerful eugenicist judge Harry Olson, partially because Olson's participation would repel funders, but also because:

> "Mr. Olson… is, in the first place, not a scientific man and this was supposed to be a scientific conference, and,… there is no reason to support that he knows anything about, or has anything to contribute, to the population problem, and… he is an outstanding representative of a body of propaganda which has no place in this Conference."[83]

It is telling that these were the concerns at the beginning of the project that would become the International Union for the Scientific Study of Population, the IUSIPP. The three problems laid out in Pearl's letters to Little, coordination, funding, and credibility, would dog the Union throughout the 1930s.

The IUSIPP could have been a major institution-building project for Pearl, one that would bring him from national prominence to international leadership in the fields of population studies and

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[82] Pearl had been consulting for Sanger's American Birth Control League when it planned an exhibition on population and food supply. IUSIPP folder #1. January-June 1926. Pearl Papers.

[83] Pearl to Little. April 27. 1927. Pearl Papers.

See Paul A. Lombardo's *Three Generations, No Imbeciles: The Supreme Court and Buck v. Bell.* 2008. John's Hopkins University Press. pp. 81-82 physical anthropology, perhaps under the umbrella of his own human biology. But the dream of the at the IUSIPP failed, for a number of reasons. First, Pearl and his American allies like Alfred Lotka and Louis Dublin were always a minority within the Union, even though Pearl had warm personal relationships with many with whom he disaggreed. Second, and probably more importantly, in the tight funding environment of the 1930s, the politically charged nature of population science and Pearl's weak position within the IUSIPP kept the big American foundations at a leery arms-distance. This is turn opened up opportunities for the national governments of Europe, particularly Germany, to step into the gap left by the more independent foundations. National funding led to the perception that politics were being insinuated into science despite Pearl's protests and reassurances. German national funding under the Nazis also emboldened the racialist and right-wing members of the IUSIPP, further alienating the liberals and the Americans. This self-reinforcing destructive spiral continued until WWII and Pearl's death stopped the IUSIPP as a project until it was re-started on a new footing in after the war 1947.

When the World Population Conference met (in Geneva as it turned out) on August 31st , 1927, Pearl chaired the committee that chartered a new organization: the International Union on Population. The purpose of the Union was to:

> "Promote the scientific study of problems relating to the various aspects of population… to initiate and encourage research that… requires the cooperation of different countries as well as that which is best carried out by individual countries, [and] to study methods of research, to promote the standardization of methods and to encourage the collection of more accurate and comprehensive data relating to problems of population in different countries."[84]

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[84] IUSIPP folder #3 January-June 1927. Pearl Papers

The founding committee of the Union was serious about the international aspect of the work they hoped to carry out. But colonial and imperial realities defined what the nature of that international relationship would be. Self-governing settler colonies like Australia, New Zealand, and South Africa would be eligible for membership in the Union under their own countries, but the only non-European country at the founding conference was Japan. The other American members of the provisional organizing committee included Pearl, Dr. William H. Welsh, and Edward Murray East of Harvard's Bussey Institute. Bernard Mallet, President of the British Eugenics Society, and the geneticist F.A.E. Crew, represented the United Kingdom; and Leon Bernard for France. The demographer Corrado Gini represented Italy, and the plant geneticist Erwin Baur represented Germany.[85] As was only appropriate for an organization dedicated to studying populations, member countries were expected to contribute monies, and were entitled to votes, on the basis of their number of inhabitants, including colonies and dependancies.

In the published document announcing the formation of the provisional Organizing Committee, Pearl wrote:

> "The plans for financial support of the Union set forth… is to be understood as an ultimate rather than an immediate scheme… Additional funds will be necessary…. I may confidentially say to the Committee that there is good reason to hope that a substantial donation may be obtained as soon as the Union is organized"[86]

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[85] Corrado Gini's work was adopted by the Italian Fascists and Baur, along with race hygenists Eugen Fisher and Fritz Lenz, contributed to Nazi theories of racial superiority and race struggle.

[86] IUSIPP folder #3 January-June 1927. Pearl Papers

Pearl was presumably referring to the money he hoped to receive from the National Research Council, but as we shall see this was more difficult to accomplish than he had expected. After the World Population Conference, Pearl wrote to his friend at Harvard Lawrence Henderson from vacation in France. He was elated; indeed, he was barely able to contain himself:

> The thing was a great success. We have completed the organization of an International Union on Population, with the utmost harmony established between representatives of a very unharmonious lot of nations. All the Europeans give credit to me, but I think this is largely politeness. Anyhow the thing came off and is organized entirely as I wanted it, and I believe the prospects for its future usefulness are excellent. Unfortunately, they insisted on electing me President, but there is to be a General Secretary who will, when we once get started and get our funds, do the actual work.[87]

Sadly for Pearl, none of the things that he crowed about in this letter to Henderson would come to pass. From the very beginning the Union was shot through with conflicts of personality and politics, and never managed to acquire the kind of money that would have allowed it to overcome those conflicts.

The First General Assembly meeting in Paris, July 4th 1928: "I am pretty sore about the whole matter, but there is no use crying over spilled milk"

In January 1928, the Winter before the first General Assembly of the IUSIPP was planned to be held in Paris, Pearl confided to E. M. East that the funding he had been so sure of getting were not guaranteed after all. Pearl was betting that a successful General Assembly of the IUSIPP, would help his case by starting some reputable scientific work and proving the organizations

[87] Pearl to Henderson. Lawrence J. Henderson Folder #5 July-December 1928. Pearl Papers.

worth.[88] By early in the new year it was clear that Pearl had made a bad bet. Although he was able to secure $10,000 a year for three years from the Milbank Memorial Fund, the Executive Committee of the Laura Spelman Rockefeller Foundation declined to match those monies. Pearl's International Union was left with half the support it had expected, and Pearl did not feel comfortable going back to his contacts at the Milbank Fund to ask for more.[89] Eventually a compromise solution was found, where the Rockefeller Foundation,[90] through the Social Science Research Council, would allocate another $15,000 per year to the IUSIPP. However, this too fell through, and by February of 1929 Pearl has bad news for his friend Henderson:

> The Rockefeller people have turned us down again on the project, and this time I think it is chiefly to be attributed to just plain bad luck. In this matter of {illegible} for the International Union, God has frowned on me instead of smiling… Now the matter is, so far as I can see, settled from their point of view. Certainly I shall not make any further attempt to review it with them… This means that I have now got to gun around and see if I can find some other foundation that will give us the desired money. I am sick and discouraged about the whole business, chiefly because I have spent so much time on the matter which I might much more usefully have employed in other ways.[91]

At this time Pearl made plans to step down as president of the IUSIPP in case by the removal of his person the Union could stand to attract support without his presence serving as a lightning rod. He needn't have worried, however, as he was not the most difficult personality on the

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[88] "Now as to the question of funds, this is a delicate matter. As a matter of fact I may tell you confidently that the whole thing has been carried as far, I think, by Merriam, Kellog, and myself, as it can be at this moment with the Laura Spelman Rockefeller Memorial, and I think it would only make the future more uncertain to attempt any pressure on them at the present time. They are much interested, and, in point of fact, are ready, when the present organization is actually on its feet, to make a very substantial contribution towards its first five years. But, as I say, the whole thing is in a fairly delicate position, and in my judgment had best be let alone as it stands until after our meeting in Paris"

Pearl to E. M. East Folder #7 1928. Pearl Papers

[89] Pearl to E. M. East. Folder #8 1929. Pearl Papers

[90] Not to be confused with the Laura Spelman Rockefeller Foundation

[91] Pearl to Lawrence J. Henderson. Folder #6 1929. Pearl Papers

Executive Committee of the IUSIPP. Compared to Corrado Gini or George Henry Lane-Fox Pitt-Rivers, Pearl was a charismatic and skillful diplomat. Nor the biggest political liability: Gini, Pitt-Rivers, and Eugen Fischer were all openly pro-fascist. However, Pearl also had personal and scholarly conflicts with influential people, like Edwin Bidwell Wilson of the National Research Council, and this clearly had something to do with his bad results.[92] Further, the Rockefeller Foundation had re-organized its internal divisions in 1926, with an eye to folding its research support for Human Biology into the Division of Medical Education. In 1928 Pearl's sponsor at Rockefeller Edwin Embree had left his job as foundation Vice President in charge of the Division of Programs. Finally, the Rockefeller grant for Pearl's Institute for Biological Research at Johns Hopkins was not renewed in 1929, and at this point Pearl stopped approaching them for money.[93]He seems to have spent most of 1930 concentrating on other projects such as his new journals *Human Biology*, and *The Quarterly Review of Biology*. But by 1931 the second General Assembly of the IUSIPP was scheduled to be held in London, and Pearl was once again the bearer of bad news.

The Second General Assembly in London, June 15th-18th 1931: "An Already Sufficiently Harassed World."

[92] Kingland, Sharon. *Modelling Nature*. University of Chicago Press. 1994

Ramsden, Edmund. "Carving up Population Science: Eugenics, Demography and the Controversy over the 'Biological Law' of Population Growth" *Social Studies of Science.* 32:5/6, 2002. pp. 857-899

[93] Kohler, Robert E. *Partners in Science: Foundations and Natural Scientists 1900-1945*. University of Chicago Press 1991. pp.125-129, 260.

At the London meeting of the General Assembly of the IUSIPP the problems of funding and the marginal legitimacy of population investigations (at least from the perspective of American foundations) were matters of record. The conference had been purposefully scheduled to not coincide with the International Congress on Population, which Margaret Sanger would be attending, in order to make it clear that the IUSIPP was a scientific organization of scientific men concerned with strictly scientific questions. IUSIPP General Assembly attendees openly speculated about how to attract money from the Scripps Institute or the Carnegie Foundation, both known for funding eugenics and population-related research. Pearl formally resigned his position as president of the IUSIPP, but remained as Vice-president and took over as Chairman of the American National Committee of the IUSIPP, taking over from Louis I. Dublin.[94]

If the poor financial health of the Union wasn't obvious, Pearl brought the matter home in an address to the membership. In it he quoted a letter he had received from the Milbank Memorial Fund. It indicated that the Fund intended to cut off its support of the IUSIPP unless member countries other than the United States started to contribute substantively. After reviewing the options that lay before the Union, Pearl gave an impassioned speech, which bears going into more closely because of how very uncharacteristic it was of his style. It may be that Pearl meant to rally his colleagues and give them some hope for the future, which did look bleak at the time. It is also possible that he was playing to the choir a little, but in doing so he was far more blunt than he usually was in public:

> "It requires no expert to perceive that the evergrowing [*sic.*] hordes of people on the face of the earth are, by their mere numbers, constantly and increasingly adding to the economic and social difficulties of an already sufficiently harassed world. Standards of living are slowly but surely

[94] IUSIPP Folder #7 June 1931. Pearl Papers

> lowering among great portions of mankind. Unemployment has come to be widely accepted as a permanent and intrinsic element of the social structure"[95]

But the world was waking up to the problems of population, said Pearl, and the time was right to press for their solution. He predicted "in the course of the next fifty years- and perhaps in a much shorter time— population questions will become the primary and basic elements in determining political policies and actions throughout the world." This sounds confoundingly like the kind of mixing of politics and science, policy and scholarship, that Pearl denounced as being wrong-headed when practiced by eugenicists. But here Pearl is actually being quite consistent. He did not object to policy decisions being made on the basis of science; he welcomed it. The trouble with eugenics for Pearl was that it was *bad science* as well as being tainted with association with Progressive Era social reform movements, like Prohibition, that he abhorred. If the IUSIPP could constitute itself on firm and scientific grounds Pearl would have had no problem with it influencing policy. He makes this clear:

> "The Union is an organized body of scientific students of population problems. In the face of the situation as it exists the Union plainly has not only an opportunity but a duty. If it continues to function as it ought to function, it can take a pre-eminent position in these matters, and play a part in substituting rational action, scientifically grounded, for the policies of the demagogue and the mob."

Pearl concluded…

[95] IUSIPP Folder #7 June 1931. Pearl Papers. APS.

> We should not be pessimistic as to the future. It is my conviction that additional financial support will come if the Union proceeds steadily on its way, on however temporarily small a scale financially, maintaining at all times its scientific dignity and adhering firmly to its announced purposes. The gravest danger which confronts the Union at this moment is that nationalistic political ambitions, relative both to affairs internal as well as external to the Union, may be permitted to exert an influence upon the conduct of the Union itself. **If anything of this sort happens it will destroy any possibility of getting substantial external support for the Union, certainly in America.** If we can avoid this danger, and keep our ship on an even keel, so that potentially large donors may continue to have confidence in the management of the Union, I personally am confident that we shall see the Union continue to grow in usefulness, influence, and prestige. (Emphasis mine)

However the skepticism of American philanthropic foundations regarding population research was growing.[96] Pearl's speech indicates that he saw that the rising nationalism of some of the European members of the IUSIPP was making the whole institution increasingly un-fundable. It may be that Pearl recognized the danger but was unable to do anything, as his allies in the IUSIPP, like Pitt-Rivers, were the very ones whose work was making the American funders nervous. Perhaps Pearl hoped that his friends would moderate their stances. Without private foundation support there was little money to be had for scientific research in the depths of the Great Depression, except for from the governments of European states. And as we shall see this is exactly what happened.

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[96] But see Edmund Ramsden (2002) for the competition between biologically minded and social-scientifically minded population researchers for monies from the National Research Council, Social Science Research Council, and foundations such as Carnegie and the Scripps Institute.

Ramsden, Edmund. *Social Studies of Science*. 32:5/6 PP. 857-899.

The Third General Assembly in Berlin, 1935: "…he who pays the piper has a right to call the tune."

After the 1931 conference the IUSIPP was no longer Pearl's central project, and he never went to another General Assembly. He continued to participate in the business of the Union but he also carried on the relationships that he had developed through the founding of the Union in his journal *Human Biology* (see section on *Human Biology*.) The financial problems that had beset the Union from its foundation did not go away, and the governments of European states stepped in to fill the gap left by the American foundations. Pearl tried to moderate the increasing 'nationalization' (both structural and scholarly) of the IUSIPP, but never took a strong stand against it.[97]

In November 1933 Pearl asked Eugen Fischer (then the president of the Executive Committee of the Union,) to delay the next meeting of the General Assembly of the IUSIPP for a year because "…just now, population questions have become somewhat political, and it is most important that our Union as a purely scientific body, should keep its deliberations free from politics…" Pearl also hoped that the world economic situation would have improved somewhat, thus re-opening the question of funding.[98] A little over one year later, in December 1934, Pearl tried to move the 1935 conference from its planned location in Berlin to one of the "…more neutral countries…" such as the Netherlands or Belgium – but his reasons for doing so were cynical:

> There is a very strong and wide-spread feeling amongst university men in this country against the policy of the Hitler Government relative to German universities. Personally I may say that I do not share this feeling quite as completely as do some of my colleagues, because I am predisposed to believe that there was at least some measure of justification for the policy upon which the German government embarked. This view, however, means no more than I am constitutionally

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[97] Indeed, Pearl was friends and allies those who, like George Pitt-Rivers, welcomed this trend!

[98] Raymond Pearl to Eugen Fischer. IUSIPP folder #10 Pearl papers

> predisposed to the view that every question has at least two sides, and I am also predisposed to look at and give consideration to all the sides of a question about which I can get information. But the feeling among university men of which I have spoken is, I gather, not confined to the United States. The columns of Nature during the past year have contained, as you are aware, many expressions of the same opinion from some of the most eminent scientific men in England, including some who are very directly interested in the work of the International Union.[99]

In other words, Pearl himself had no objections to holding the conference in Berlin, but he was worried that it might negatively affect the reputation of the IUSIPP. It is possible that Pearl was only correctly taking the temperature of the international scientific community. A few months earlier the IUSIPP had been rejected from its application for membership in the International Council of Scientific Unions.[100] In any event, Pearl's suggestions were not followed and the Executive Committee voted to hold the meeting in Berlin.[101]

Pearl did not attend the Berlin meeting of the IUSIPP, and Close did not write to him about it until February of the next year. From Close's perspective, the conference had been "well organized," aside from there being too many speeches in German, but he was concerned about the effects that the conference may have had on the IUSIPP's reputation. Close was concerned that Professor C.G. Campbell, "an ardent supporter of the present regime," had not been a very good representative for the American delegation to the General Assembly. Even worse, Close feared that the IUSIPP had been upstaged by its host country and its national committee. In Closes' own words:

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99 Raymond Pearl to C.F. Close. IUSIPP folder #10. Pearl papers

100 C.F. Close to Raymond Pearl. IUSIPP foder #10. Pearl papers

101 Close to Pearl. IUSIPP folder #10. Pearl papers

> Our Union was put a little too much in the background. But [Eugen] Fischer had not, I believe, a very free hand. He was personally most kind and agreeable. Of course it is not only fair to say that all the expense of the Congress was born by the German authorities, and that the German Government contributed a considerable sum, and he who pays the piper has a right to call the tune.[102]

After the Berlin conference Pearl faded even further from the management of the affairs of the IUSIPP. The project that he had led and shaped had slipped away from him with his ability to attract foundation support. His only subsequent substantive participation was during a small scandal in the Union that centered around the behavior and political position of Pearl's friend and ally, "Captain" George Henry Lane Fox Pitt-Rivers.

In May 1937 Close wrote to Pearl that Pitt-Rivers had "run-amuck." Pitt-Rivers had been Honorary Secretary of the Executive Committee of the IUSIPP since its inception, and had written an "Interim Report" on the status of the IUSIPP that he wished to have published and distributed to the membership of the Union. The report recommended that countries more than two years in delinquency of their dues to the Union be expelled, and that more attention be paid to standardization of methods the research of population problems. It also denounced the treatment of the German-speaking minorities in Czechoslovakia, and proposed that the IUSIPP send a fact-finding mission to that country to investigate.[103] Close recommended that Pitt-Rivers be prohibited from presenting his report, as it would only harm the IUSIPP's reputation to be

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[102] C. F. Close to Raymond Pearl. IUSIPP folder #11 1936. Pearl Papers.

[103] IUSIPP folder #10. Pearl papers.

associated with it. Pearl blandly parried Close's alarm about Pitt-Rivers, and opined that the General Assembly of the IUSIPP could be the judge of the suitability of the interim report.[104]

Pitt-Rivers, who came from an aristocratic English family, was a political reactionary who penned anti-semitic conspiracy theories about the origins of the Russian Revolution and the discontents of modern industrial life. He was also a supporter of Oswald Mosley's British Union of Fascists and during World War II he would be interned by the British government as a risk to internal security. Pearl had been friendly with Pitt-Rivers since at least 1930, and the two men shared an appreciation for high culture and a dislike of social reform, which Pearl referred to in a letter to Pitt-Rivers as "…the uplift."[105] It is true that Pearl did not share Pitt-Rivers' enthusiasm for eugenics, but was he content to agree to disagree on the topic.[106] This was not the first time Pearl had to smooth things over between Pitt-Rivers and another central member of the Union. In 1931, when Pitt-Rivers had a petty conflict with the America statistician and life insurance executive Louis I. Dublin, Pearl advised the Englishman to apologize "because I am sure that if the Union maintains Dublin's good will he will raise a great deal of money for us."[107]

Returning to Pearl's bosom friend H. L. Mencken sheds a little light on the matter. Mencken derided the Adolf Hitler as a "boob" and "a Babbit run amok" but he absolutely *hated* President Roosevelt, and he warned that giving in to the "professional kikes" in the United States would

[104] Pearl to Close. IUSIPP folder #11. 1937. Pearl Papers

[105] Pearl to Pitt-River. 15 October 1931. Pitt-Rivers folder. Pearl papers

[106] A Pearl put it to Pitt-Rivers regarding eugenics: "I shall not press the matter or nag you, but insidiously undermine your position." *Ibid.*

[107] Pearl to Pitt-Rivers. IUSIPP folder #8 July-December 1931.

only increase Hitler's appeal. As late as 1933 Mencken glibly claimed to a friend that "Certainly the Germans are not beating up Jews, as such. It simply happens that a good many Communists are Jews."[108]

In 1939 Pearl mentioned in a letter to the demographer Pascal Whelpton that his wife was seriously ill and he couldn't be expected to do any business having to do with the IUSIPP at the moment.[109] That is the last record of Raymond Pearl's involvement with the International Union for the Scientific Investigation of Population Problems. Pearl's plans for the IUSIPP failed for two reasons. He was unable to control the allies he had enlisted into his endeavor in Europe. They simply differed too strongly from him in terms of their scientific goals and approaches. The second reason was that Pearl's inability to discipline the IUSIPP as an organization meant that he was unable to reassure his primary funding source, the Rockefeller foundations, that he could guarantee a 'good' population science that did not cross unacceptable lines into racial science and politics.

The vacuum left by the withdrawal of Rockefeller support was filled by the national governments of an increasingly fractious and crisis-ridden Europe. The Nazi-dominated German government's sponsorship of the Union's 1936 conference only reinforced the growing rifts between the organization, its funders, and its one-time leader. Pearl never chose to take a stand against the co-optation of the IUSIPP, and in any case by that time he had let the problem slip out of his hands.

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[108] Teachout, Terry. *The Skeptic: A Life of H. L. Mencken.* Harper Collins Publishers. 2002. p. 266

[109] IUSIPP file #13. Pearl Papers.

CHAPTER 3: CALCULATING THE POPULATION

This chapter is about the shared styles of work at the Institute for Biological Research at Johns Hopkins University and the Metropolitan Life Insurance Company of New York. As I have argued in the Introduction and in Chapter 2, new kinds of rationality that were developed in the early 20th century for coordination of the work process were appropriated by human biology. And in this chapter I show how the work of Alfred Loka brought the tools and apporoaches of the life insurance industry to bear on the problems of population growth in human biology.

I will be using the notion of strategic action fields (or just 'fields'), as elaborated by Neil Fligstein and Doug McAdam in their 2012 book *A Theory of Fields*. The concept of strategic fields illuminates the nested systems that organize social action across seemingly disparate areas of concern. A strategic action field is a space of organized social action organized on the basis of a shared understanding of the rules and of what is happening in the space. The rules of a field are called institutions, which are in turn composed of ensembles of routinized capacities for action, called conventions. Fields are internally stratified by status hierarchies, which act according to a negotiated social order. Fields interact strongly or weakly with other fields in relationships of dominance and subordination, competition, or cooperation. Many fields contain other fields, or are in turn embedded within other fields. The state, for example, is a nested hierarchy of fields embedded within other fields. A large firm is another nested hierarchy of this type, though with a more limited scope of action. Actors who work in more than one field may bring conventions and strategies from one field to another, resulting in a process of transference with modification, and adapting the new suite of techniques to a situation with different stakes and different rules of the game.

This chapter deals with the work of Alfred Lotka and Raymond Pearl in three fields: biological research work on populations, government economic statistics, and the life insurance industry. A fourth field, that of philanthropy, is an important audience for the field of biological research work. I examine philanthropy in more depth in Chapter 4, which follows the arguments and appeals of the Committee for Industrial Physiology at Harvard University for continued support from the Rockefeller Foundation's Division of Social Sciences before WWII. I am interested in two aspects of the institutions in these fields: first, the processes by which they came to resemble one another, and second, their dependence on similar conceptual models. First, I explore the similarities of work process, instrumentation, and organization employed at these different sites, especially the role played by highly skilled feminized computational work, all of which lead to institutionalized similarities in the gendered division of labor and the allocation of intellectual credit. Second, I note the similarities in the conceptual models brought to bear on the respective problems of these institutions, including mathematical models. I suggest some ways that biological research work was influenced by bureaucratic and managerial fields on the basis of a shared style of rationalization applied to problems of populations. The instruments and structures of that rationalization were ultimately derived from business firms' competition with one another, and from the US government's desire to know more about its population.

Mathematical biologist Alfred J. Lotka's main claim to scientific fame is his half of the name of the Lotka-Volterra equation, which described the population dynamics of a two-species predator-prey interaction. What is less well known is that Lotka's single longest stretch of employment was actually with the Metropolitan Life Insurance Company of New York (Met Life). While at Met Life, Lotka collaborated with vice-president Louis Dublin also a statistican with graduate training in biology. He also continued to publish and work with biostatistician Raymond Pearl at Johns Hopkins University's Institute for Biological Research (IBR).

Lotka and Pearl attacked the project of calculating the population through a particular kind of rationality: they took a population to be made up of individuals with measurable characteristics. The overall behavior of the population could be modeled by aggregating density-dependent effects produced by the members of a population bumping into one another like billiard balls. Between the IBR's founding in 1925 and the beginning of the Second World War, Pearl, Lotka, and Metropolitan's Vice President Louis I. Dublin developed an approach to biostatistics that understood the behavior of a population to be a function of the aggregate qualities (length of life, rate of reproduction, incidence of injury) of the members of that population. This suite of concepts, organizational commitments, and instruments was also in use in several organizations of government bureaucracy, where Pearl had worked during World War I as chief of the Statistical Division of the United States Food Administration. The actual "laboratory" spaces in which these data were analyzed and produced at the IBR and at Met Life resembled each other in their gendered division of labor and their use of instruments and techniques for handling data and records, like punched-card tabulators and mechanical calculators.

Lotka's Mistake

On June 19th, 1921, statistician Alfred J. Lotka realized that he had made a mistake: a manuscript he had hoped to see published in the *Proceedings of the National Academy of Science, USA* contained an error. Lotka's article had been communicated to the National Academy by his friend and patron, Raymond Pearl, professor at Johns Hopkins University. Lotka dashed off a letter to Baltimore to fix the problem before it was sent to the printer. "Dear Dr. Pearl," Lotka wrote:

> In looking over the carbon copy of my paper on Economic Conversion Factors of Energy I have just discovered a perfectly ridiculous error in transcribing from my notes…. Page 1 line 10 reads, **in the copy I sent you, "(the transfer) of energy from one portion of a physical system to another etc." This should be "the transfer of property by sale or purchase".** I am sorry of this blunder. Will you kindly forward to Dr. Wilson the enclosed page and ask him to substitute it for the original."[110] (emphasis mine)

Here we have what appears to be a case of mistaken identity corrected at the last moment. Energy is confused with commodities, and the impact of one inelastic body on another is mistaken for the buying and selling of property. But though his was a typographic error for Alfred Lotka, it was not an error of categories. He was interested in both of these fields, and he was conversant in the mathematics that described them both. For the next 24 years he would inhabit two worlds simultaneously: the world of academic research and publication in biology, and the world of actuarial computation in the insurance industry. Lotka's letter to Pearl is a good introduction to the institutional and conceptual borrowings between the practices of managing, governing, and scientific research in the field of human biology. These are a series of interesting family resemblences between a set of different institutions that produce knowledge about different matters of concern: between war-time bureaus of the federal government, insurance companies and their setting of rates for employee casualty insurance, and the work on biometry, biostatistics, public health, race science, and population growth that Raymond Pearl called general biology or human biology.

[110110110] Lotka to Peal. June 19, 1921. Raymond Pearl Papers. American Philosophical Society.

Lotka had been corresponding with Pearl since 1920, and began contributing to the work at Pearl's Institute for Biological Research the summer of 1921.[111] Lotka had been trained as a chemist in Europe, and had graduate training in physics and mathematics from Cornell. He had worked as a industrial chemist at the US Patent Office and Bureau of Standards, and had edited a popular scientific journal. He is remembered in biology for his work on the Lotka-Volterra equations that describe the dynamics of a two-species predator-prey interaction. But his longest stretch of employment was with the Metropolitan Life Insurance company of New York from 1924 to 1947, when he retired with the rank of Assistant Statistian.[112] He worked for the US government, he worked for a big insurance corporation, and he worked at the Institute for Biological Research. Taking into account all the social worlds he participated in together characterizes the way that the rationalization then taking place in the world of business and government was mutually conditioned by the production of knowledge about human biology.

Early life, training, and career

Lotka was born in 1880 in the Austro-Hungarian Empire in the city of Lemberg (now Lvov, Poland.) His parents were Americans, giving Lotka American citizenship from birth.[113] Lotka went to high school in Birmingham in the United Kingdom, and took a degree from the

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[111] Kingsland, Sharon E. (1985) *Modeling Nature: Episodes in the History of Population Ecology*. University of Chicago Press. p. 30.

[112] A modest title but he was 'assistant' to Louis I. Dublin, the 3rd Vice President of the whole of Metropolitan Life and subsequently President of the entire corporation.

[113] Lotka wrote at the bottom of his C.V. Letter to Pearl that "My father was a naturalized American when I was born. I have indicated this as I have no desire whatever to pass as an Austrian."

Raymond Pearl papers. Correspondences with Lotka. Box 17, Folder marker April-June 1921

University of Birmingham. In 1901-1902 he attended lectures in physical chemistry at Leipzig University.

Between 1902 and 1924 Lotka had a mixed career in the United States. He pursued graduate training at Cornell, and took a non-PhD doctorate (a D.Sc.) from Birmingham University on the basis of his published work. He worked as a chemist for the General Chemical Company, as an assistant in physics at Cornell, as an examiner at the U.S. Patent Office, and as an assistant physicist at the U.S. Bureau of Standards. For three years (1911-1914) he was an editor for the popular science magazine *Scientific American*. During this period of Lotka's career, he published articles in demography, geometry, chemistry and statistics, and worked on his book *Elements of Physical Biology* (1925).

In 1914 Lotka went back to work for the General Chemical Company, and remained there until the end of World War I. He was formally associated with Raymond Pearl's Institute for Biological Research at Johns Hopkins from 1922-1924, through an unpaid courtesy appointment, and joined the Metropolitan Life Insurance Company in 1924. There he was Supervisor of Mathematical Research (1924-1925), General Supervisor (1933-1934), and then Assistant Statistician (1934-1947), a non-executive rank roughly below Vice President. While at Met Life, Lotka continued to publish in demography, epidemiology, population biology, and mathematical statistics. Together with Vice President of Metropolitan Louis I. Dublin[114], Lotka authored *The Money Value of a Man* (1930), *Length of Life, A Study of the Life Table* (1936), and *Twenty-Five Years of Health Progress* (1937).

[114] (1882-1969) PhD at Columbia in biology under Edmund B. Wilson, and statistical training under Franz Boas (Bouk, p. 245). Vice-President of Metropolitan Life and President of the American Statistical Association.

Despite the fact that Lotka worked primarily in industry and government, and never had a stable academic appointment, he published in academic journals and took part in the life of several professional associations. He was President of the Population Association of America (1938-1939), President of the American Statistical Association (1942), and a fellow of the American Society for the Advancement of Science and the Royal Economic Society.[115] He was also active in Raymond Pearl's International Union for the Scientific Study of Population Problems (the IUSIPP). Lotka married late in life (1935, at the age of 55) to Romola Beattie of Red Bank, New Jersey. He died in 1949.[116]

Lotka's approach to biology: life as a mathematical system.

Lotka was influenced in his use of quantitative models for biological problems by his reading of the work of Canadian entomologist William Robin Thompson. Thompson had applied mathematical models to the study of parasite control in agriculture, and was himself influenced by reading D'Arcy W. Thompson's 1917 book *On Growth and Form.*[117] This led Lotka to the

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[115] Also the Econometric Society, the Institut International de Statistique, the Inter-American Statistical Institute, The Institute of Mathematical Statistics, The American Mathematical Society, the Swiss Actuarial Society, the American Public Health Association, and the Washington Academy of Science.

[116] (Refs Kinglsand 1995, Dublin a Family of 30 Million, Pearl Papers Lotka Correspondences APS, Lotka Papers Princeton University)

[117] Sharon E. Kingsland. "Mathematical Figments, Biological Facts: Population Ecology in the Thirties" Journal of the History of Biology, Vol. 19, No. 2, Reflections on Ecology andEvolution (Summer, 1986), p.241

See also Kingsland's *Modeling Nature* p.103 …When Thompson's articles appeared, Lotka accordingly brought the host-parasite example into his analysis as well. He did not fail to notice that Thompson's fluctuations provided the empirical support for the rhythmic oscillations that Herbert Spencer had deduced as the necessary outcomes of the balance between the forces of increase and decrease in populations. Lotka's method differed from Thompson's in that he used a continuous-time scale, study of economists like Leon Walras and William Stanley Jevons who modeled the behavior of economic systems with mathematical tools, "especially of calculus."

> The search for an exact expression of these economic principles naturally led Lotka to the mathematical school of the nineteenth-century economists represented by Augustin Cournot, Leon Walras, Hermann Heinrich Gossen, and William Stanley Jevons. These were a school not in the sense of following the same program, but in the that they independently explored the use of mathematics, and especially of calculus, in economic analysis. All but Jevons were unsuccessful in their attempts to popularize their techniques, and it was to Jevons's *The Theory of Political Economy* that Lotka turned for his principal model, adding a few modifications culled from the early work of Vilfredo Pareto.

> Lotka's use of Jevons reflected not so much his agreement with the conclusions of that branch of economic theory, as the fact that the mathematical treatment made it easy to transfer the analysis to a general biological context. Jevons's economics was rooted in the Benthamite "hedonistic principle" which related action to the increase of pleasure and lessoning of pain. Viewing economics as analogous to the physical sciences dealing with statics and equilibrium, Jevons tried to develop a program of scientific economics from Bentham's doctrine, creating out of the combination a "calculus of pleasure and pain"[118]

Before this, in 1911, Lotka had encountered the work of Sir Ronald Ross, the 1902 Nobel prize winner in medicine who discovered the cause of malaria. Lotka integrated Ross's notion of density-dependent relationships between two species (in this case, host and parasite) into his general theory of mathematical biology[119].

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rather than the discrete-time scale with the generation as the unit of time which Thompson had used. The change gave Lotka greater flexibility but was less realistic for insect populations.

[118] Kingsland, pp.41-42. Kingsland continues to discuss parallels between political economy and ecology. Spencer, Jevons, Marshall. See also pp. 103-104.

[119] Lotka seized upon Ross's research as soon as it appeared in 1911 and incorporated the malaria example into this general study on evolution[?1].

Kingsland. p.102

Lotka's training in physics and mathematics, and his interest in the great mathematizers of economics led him to treat problems of life (disease epidemics and population growth) as if they were problems of kinetics. He used the same equations to describe the behavior of two interacting species. This model assumes an equilibrium. Either the system runs up to it and then settles (an S-shaped curve) or else the system swings up and down around the level of the equilibrium (epidemic outbreaks and the two-species model of population dynamics, whether predator-prey or host-parasite)

Lotka calculated the rate of growth of a population on the basis of the size of a population, its survival rate, and its rate of reproduction as determined by age. Again returning to the proposition that the dynamics of a population are analogous to the dynamics of atoms and molecules, Lotka compared the death of an individual living organism in a population to an atom's radioactive decay or "molecules of a chemical compound decomposing by a monomolecular reaction."[120] He had acquired data on the lifespan of humans from different sources, including the U.S. Census and papers published by Raymond Pearl. But the tools of mathematical analysis he took straight from the actuarial toolkit.[121]

Lotka's views on biological evolution were progressivist and somewhat orthogenetic, a position that was common among biologists in the 1920s and 1930s.[122] His view of evolution's dominant

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[120] Alfred J. Lotka, *Elements of Physical Biology* (Dover Publication, Inc. New York,)1956. p.106.

[121] "The function *p* (a) is that commonly denoted by 1(sub)x in actuarial notation, and tabulated in the principle column of a "Life Table." Alfred J. Lotka, *Elements of Physical Biology* (Dover Publication, Inc. New York,)1956. p.102. (Originally published in 1927 as *Elements of Mathematical Biology)*

[122] Bowler, Peter J. (19830, *The Eclipse of Darwinism: Anti-Darwinian Evolution Theories in the Decades around 1900*. Baltimore. The Johns Hopkins University Press.

tendency was a development from simple to complex, from small to large, and from less perfect to more. He did not deny the existence of natural selection but believed that "orthogenesis, whether creative or destructive, must accelerate the pace of evolution."[123] So there was room in Lotka's philosophy for teleology of a kind. He thought that purposiveness in a system could be explained on the basis of a definite mechanism, and that such "teleological mechanisms" could be due to the characteristics of a physical system, or due to conscious volition on the part of a human being.

Lotka's comparison the death of an individual living being in a population to the decomposition of a radioactive isotope was not metaphorical, nor was it a pragmatic application of a tool that seemed to work. Rather, it was based on his metaphysical commitment to the idea that all natural systems, from cosmological to biological and social, were governed by the same laws of interaction and development. Hence Lotka used the formula for describing the decay of radioactive isotopes to calculate what he described the "force of mortality" at work in a population.[124]

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[123] P.380 Lotka appears to subscribed to a variety of orthogenetic theories of evolution. He does not deny the existence of natural selection but believed that "…orthogenesis, whether creative or destructive, must accelerate the pace of evolution."

Lotka, Alfre J. (1925) *Elements of Physical Biology* Baltimore. William and Wilkins. p.380

Also see Lotka's article *Biassed Evoltution* in Harper's Magazine, May 1992.

[124] *Ibid.*

Calculation by Machine

Lotka's commitment to the idea of life as a mathematical system, or as a "self-revewing aggregate" as he put it (see below), would prove to be compatible with the style of work he moved into a Met Life. Conceptualizing the population and its characteristics as self-renewing mathematical system made it amendable to mathematical analysis. Not just empirical numerical number-crunching, but projection, modeling, simulation. One key to this was computation, both mechanical and through human computers. The other was new ways of organizing numbers thought filing systems and central statistical offices in government bureaus and insurance companies.

Computation by machine was by no means a new practice in 1920. Mechanical calculators based on the designs of Blaise Pascal and Gottlieb Leibniz had been available in Western Europe since the 1700s, though they were unreliable and expensive. The real mass adoption of mechanical calculating tools did not occur until the great flood of information generated by late 19th century statecraft and commerce presented itself as a problem.[125] The U.S. Census Office adopted Herman Hollerith's mechanical tabulating machine for its 1890 census. In the United States, engineer-entrepreneurs like William Burroughs (grandfather of the writer) and Frank Baldwin started companies to manufacture adding machines in the industrial cities of the Midwest.

These new calculating machines were developed for use by, and mostly purchased by, big industrial and commercial firms in railroads, insurance, engineering, surveying, and government.

[125] Hacking, Ian. *The Taming of Chance*. Cambridge University Press. 1992.

The very availability of the mass of population and demographic data upon which population study was based depended on tabulating and recording machines like those developed by the Computing Tabulating Recording Company (which merged with Hollerith's Tabulating Machine Company in 1911). This genre of data computation, as much as the standardized form of data entry made possible by the punch card. Even so, mathematical computing was a labor-intensive work process, requiring skilled and disciplined workers to spend long hours completing repetitive tasks with high levels of precision. The human computers thus employed worked for variety of employers great and ordinary, from the Harvard Observatory, to the United States Coastal and Geodesic Survey, to the U.S. Census Office, to the offices of industrial, commercial, and financial firms. Often they were educated young women who had struggled to obtain university education or technical training, only to find that there was no place for them in the managerial or professional hierarchies of the time. Marriage was not an option for these women if they wished to keep their jobs, which is to say they were typically fired if they got married, and the pay was typically low.[126]

Karl Pearson used human computers in the Biometrics Lab—first only volunteers and family members, and later hired employees.[127] They used German-made Brunsviga mechanical calculators to analyze data and compile tables for reference. The scope of the work at the Biometrical Laboratory was quite wide; in addition to problems in biometry and eugenics they tackled agricultural production, the weather, and the construction of ballistics tables for

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[126] Grier, David Alan. *When Computers Were Human*. Princeton and Oxford, Princeton University Press. 2005.

[127] Including the wonderfully-named sisters Beatrice and Frances Cave-Browne-Cave

artillery.[128] The Metropolitan Life Insurance Company was an early adopted of punched-card technology, and started using Hollerith card tabulating machines for actuarial purposes in its Ordinary Insurance Department after 1910, but kept its in-house methods of hand-sorted cards and bound volumes of data for its more voluminous industrial insurance work.[129] Met Life used a cost accounting system (another common entrance point for punched cards and tabulators) for internal management, but this was not part of Lotka's department, the actuarial division.[130] In 1893, 400 of the 650 clerks employed at Metropolitan were women.[131] And by 1914, the total number of clerks in the actuarial division alone was almost 500.[132]

Punched cards and tabulating machines for scientific calculation were very rare in the 1920s, but Pearl's earlier (1918) plan for a Department includes the rental of a tabulating machine, and his Department at the U.S. Food Administration had a punched card processing room.[133] His early ambitions for a Department of Biometry and Vital Statistics included plans for a tabulator machine operator and the rental of tabulating machines.[134] It is not clear if card tabulators were used at the Institute for Biological Research, as it seems that there were instances where

128 *Ibid*.

129 Yates p. 49 (refers to Douglass North, "Life Insurance and Investment Banking at the Time of the Armstrong Investigation of 1905-1906." *Journal of Economic History* 14 (1954): 209-228.)

130 Yates, p. 22

131 Louis I. Dublin, *A Family of Thirty Million: The Story of the Metropolitan Life Insurance Company* (New York: Mutual Life Insurance Company,1943,) pp. 230-231 from Yates, 2005

132 Zunz, Oliver. 1992*. Making America Corporate 1870-1920*. University of Chicago Press. pp.113-116

133 Martin Campbell-Kelly."Punched Card Machinery" in *Computing Before Computers* (Edward Asprey ed.) p. 147

134 APS Pearl Papers. Johns Hopkins University — Correspondence with Frank J. Goodnow 1919-1924. n.d.

mechanical calculators were used, and instances where tabulators were used, but the use of both of these together would have been unusual for the time.[135]

When Lawrence J. Henderson and Raymond Pearl corresponded about calculating machines they mentioned three models; the Monroe, the Millionaire, and the Brunsviga. The first and third were standard respected brand names in mechanical computation, while the aptly-named Millionaire was an expensive, electrically-driven luxury model. These new calculating machines were developed for use by, and mostly purchased by, big industrial and commercial firms in railroads, insurance, engineering, surveying, and government. The fact that Pearl was considering purchasing one speaks to his use of mechanical calculators, as well as to the size of the Institute's budget.[136] Additionally, Pearl's obituary in the Royal Statistical Society mentions that he carried a full-size Brunsviga calculator around with him as early as 1905.

The first punched-card systems were designed by American engineer Herman Hollerith in the 1880s. His Tabulating Machine Company (later International Business Machines) won a contract to process the United States Census in 1890, and from then on punched-card technology was an integral part of government projects to study population.[137] Starting in 1910, the Tabulating

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[135] Computers before computing book?

[136] Pearl Papers APS

[137] The punched cards applied to these administrative systems in the United States, France, and Germany in the early decades of the twentieth century found their origins in more modest ambitions half a century earlier in the 1880s. The first punched-card systems had been built buy the engineer Herman Hollerith to process the United States population census in 1890, a job requiring only counting, not calculation… To reach a broader market, during the 1890s and 1900s, the original numeric punched-card system was developed for other kinds of statistics requiring addition, in private business and in public organizations. Hollerith incorporated his business as the Tabulating Machine Company, which later became International Business Machines.[1]

Machine Company and its competitors developed punched card-systems that printed as they tabulated, contributing to the need for more sophisticated data handing procedures.[138] In the 1930s, punched-card applications grew to include the production of records, and thus became a storage mechanism as well as a calculative one. Big firms and government agencies integrated this new capacity into their record management systems[139].

The Metropolitian Life Insurance Company of New York:

From the late nineteenth century, American life insurance companies were driven to a strategy of growth and competition which had the tendency to winnow the industry down to a handful of very big firms. These big firms competed with each other on the basis of market share, thus maintaing increasing size as a virtue. As the size of the firms grew, so did costs associated with the internal management and organization of those firms. Growth no longer reliably provided efficiencies of scale, but there was really no easy way to opt out of the strategy of growth. If a firm did not keep up with its competitors, no matter the cost, it would be driven out of business

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Punched Card Systems and the Early Information Explosion. p.3

[138] In the 1910s, when the first bookkeeping systems using punched cards were designed, punched-card machines were launched that that printed both the calculations computed by and the information stored on punched cards, and challengers to the Tabulating Machine Company emerged in the United States.[1]

Punched Card Systems and the Early Information Explosion. p.3

[139] In the 1930s, the scope of punched-card applications started to expand from business statistics and bookkeeping to include record management. Until then, punched cards had been a data-processing tool to be discarded once the process was completed. Punched cards became a storage medium. Several insurance companies, public utilities, and other businesses introduced registers of customers and wage earners in their punched-card-based bookkeeping systems. Various national governments adopted and developed this concept to make record keeping the core of the system.[1]

[1] *Punched Card Systems and the Early Information Explosion*. p.4

through loss of market share. The owners and managers of the life insurance companies internalized this lesson, and it became a part of their habitual approach to work.[140]

Large and highly-capitalized firms rationalized their operation to deal with these internal problems of organization and coordination. Rationalization refers to institutionalizing practices that increase the efficiency of a system or a process.[141] Rationalization can be with regard to material, time, money, or other resources, but it is always oriented towards getting more for less, or at least getting more for the same inputs. Nor is there only one way to rationalize an organization. One rationalizing strategy, say, a finer division of labor in one department, may create problems for another department, *e.g.,* increased accounting and communication costs. Social scientists have characterized a tendency towards the rationalization of institutions as one of the hallmarks of modern industrial societies[142] And finally, rationalization can serve as a powerful ideology, a relatively coherent set of ideas and conventions that can serve as the necessary conditions of mutual intelligibility between allied actors, or as ways for weaker actors to attract attention and legitimacy from stronger ones.

And so firms like Metropolitan Life Insurance Company adopted and developed techniques and instruments like the vertical folder filing system, the centralized storage of data and records, and the internal memorandum, into technologies of organizational management towards the ends of controlling costs and supporting growth. It was also one of the main customers for calculating and

[140] Yates, p. 27

[141] Gerson in Ackerman (2007).

[142] Weber, Giddens, but see Brumo Latour's *We Have Never Been Modern* (1994) for a partially dissenting view

tabulating machines. Met Life was one of the three biggest firms in the US insurance sector by 1905, holding policies in excess of $1 billion. By 1913, it was the biggest insurance firm in the United States.[143] Its headquarters, the Metropolitan Life Insurance Company Tower on New York's Madison Avenue, was the tallest building in the world when it was completed in 1909. Its clock tower, modeled on the bell tower in Venice's Piazza San Marco on a larger scale, could be seem from a mile away.[144]

Although it contained Ordinary and Industrial divisions, the bulk of Met Life’s business was in industrial, rather than personal, insurance. This means that it insured waged workers in industry, and covered work-related injuries and illnesses. Organized as a stock company as opposed to a mutual (policy-holder owned) company, Metropolitan’s reason for existence as an enterprise was to produce returns to its stock holders, but it limited its profits to less than the 7% legal limit on interest rates in the state of New York.[145]

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[143] Heide, Lars (2009) *Punched Card Systems and the Early Information Explosion.* p. 100

[144] Kwolek-Folland, Angel, (1994). *Engendering Business: Men and Women in in the Corporate Office 1870-1930*. p. 102.

> At the home office of Metropolitan Life Insurance Company in New York City… a new addition was being completed which included a tower in the manner of the campanile of St. Mark’s in Venice. The tower, then the tallest office structure in the world, held a clock by which a watch could be set a mile away.[1]

[1] Strom, Sharon Hartman, (1992), *Beyond the Typewriter: Gender, Class, and the Origins of Modern American Office Work, 1900-1930.* University of Illinois Press. p. 24.

[145] JoAnne Yates. *Structuring thr Information Age: Life Insurance and Technology in the Twentieth Century.* (The Johns Hopkins University Press, 2005,) p. 15

Punched cards at Metropolitan Life Insurance

The biggest life insurance firms like Prudential Assurance and Metropolitan Life hedged their technological bets by contacting with several different vendors of punched-card machines. If one supplier of computing tools failed, there would be another system to fall back on.[146] One of Metropolitan Life Insurance Company's punched card systems was developed by John Royden Pierce, an early competitor to Hollerith's Tabulating Machine Company. Pierce had secured a contract to provide Metropolitan with punched-card machines in 1913, and though they were only delivered in 1916, the big insurer contracted for additional Pierce machines. However, by 1926 Pierce had failed to deliver on his new orders, and he had folded his operation into that of the Tabulating Machine Company. There, Pierce's "double-deck 86-column card" was used as part of Metropolitan's system of policy administration into the late 1920s.[147] The Powers firm also produced tabulating machines for Metropolitan in the 1920s, but by 1927 they merged into Remington Rand, along with several other small competitors.[148]

The life insurance industry was receptive to the ideology of efficiency and systematic management. One of punched-card technology's first uses in the insurance sector was in office management, but it also moved into actuarial work early on.[149] Tabulating technology's path of

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[146] Yates, JoAnne, (2005). *Structuring the Information Age:Life Insurance and Technology in the 20th Century.* Johns Hopkins University Press. p. 76-77

[147] *Punched Card Systems and the Early Information Explosion*. pp. 99-102; 118.

[148] Yates, JoAnne, (2005). *Structuring the Information Age:Life Insurance and Technology in the 20th Century.* Johns Hopkins University Press. p. 91

[149] "Insurance intestes in the tools and techniques of sstematic management was clearly rising at this time and would encourage firms to adopt and use tabulating technology in various areas from actuarial to operational.□"

uptake and spread was first through the actuarial valuation of insurance policies, and then into the operational management of the office and corporate bureaucracy.[150] Metropolitan Life had intended Pierce's machines to be used in their Actuarial Department as early as 1914.[151]

Punched cards could cut the time and improve the accuracy of certain classes of routine calculation, but they were no help at all for others, and for some operations (like calculating least squares analysis) they were helpful with one step of the process but not with another.[152] This meant that in lines of work that employed mechanical calculators and punched card tabulators, there was a correspondingly great need for reliable and mathematically skilled human calculators.

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[1] Yates, JoAnne, (2005). *Structuring the Information Age:Life Insurance and Technology in the 20th Century.* Johns Hopkins University Press. p. 55-56

[150] "It seems to have been developed at the outset for handling valuation and insurance account records, and gradually has been extended until at the present tim it takes care of many tasks of an accounting and statistical nature."

See R.W. Leib "Committee Report: Punched Cards in the Life Insurance Office" Proceedings of LOMA (1930): 257-311.

(in Yates, JoAnne, (2005). *Structuring the Information Age:Life Insurance and Technology in the 20th Century.* Johns Hopkins University Press. p. 97.)

[151] J.D. Craig, (1914) *Installation of a Perforated Card System," TASA* 15:414

(in Yates, JoAnne, (2005). *Structuring the Information Age:Life Insurance and Technology in the 20th Century.* Johns Hopkins University Press. p. 61.)

[152] Grier, David Alan () *When Computers Were Human*. p. 162

Alfred Lotka at Met Life

Lotka began working for Metropolitan Life in 1924, and by 1928 he was the Supervisor of Mathematical Research at the Statistical Bureau, with Louis I. Dublin as Head Statistician. Lotka continued to publish scholarly articles in journals of biology, public health, and demographics, and participated in the professional life of the American Statistical Association and the International Union for the Scientific Study of Population Problems. But all the while he was also supervising a team of statisticians, actuaries, clerks, and secretaries as they compiled life-tables and applied those tables towards projecting insurance rates for workers' casualty insurance. Lotka's work in the life insurance sector influenced his approach to biostatistics in more ways than the use of life tables and business computing. He also adapted the concept of amortization of capital goods to the problem of population dynamics. Lotka found that models used to determine "the replacement of equipment in manufacturing plants and in public utility establishments, as this equipment becomes worn out or obsolete" were similar enough to the models used in Raymond Pearl's 1928 paper "The Progeny of a Population Element" to warrant comment to Pearl in a letter:

> … Since writing you last I have found that the problem dealt with in this paper has close affinity to certain practical problems arising in connection with the replacement of equipment in manufacturing plants and in public utility establishments, as this equipment becomes worn out or obsolete. I have added a sentence to the introductory paragraph bringing out this point. I am inclined to think that this angle of the subject adds very greatly to its importance, also I am informed that some work in this direction is in progress…[153]

[153] **Folder 1922-1928** Alfred J. Lotka, Supervisor

Dear Dr. Pearl,

This letter provides evidence of the directionality of Lotka's thinking, showing that he directly borrowed ideas from population science to solve "practical problems" in his commercial work in insurance companies. [I would replace the following sentence with something like this]

Here of course is the always difficult (but always interesting!) question of the directionality of borrowings between Lotka's population science and his work in industry and commercial insurance. There is ample evidence that the traffic went both ways, and that Lotka's conception of a mathematical biology was the foundation on which he built his later career as a statistician for a life insurance company. But it is equally important to recall that Lotka based his biology on a reading of the mathematical economics of Alfred Marshall, Stanley Jevons, and Leon Walras; and that between 1902 and 1921 he was employed as an industrial chemist, an editor of scientific periodicals, and in the U.S. Patent Office and Bureau of Standards. Lotka's practical outlook, and habits and tools and skills that he brought to biological work, were acquired and solidified throughout his long journeyman period in the world of American business, and government. And

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I am now sending you the manuscript on, "The Progeny of a Population Element." Since writing you last I have found that the problem dealt with in this paper has close affinity to certain practical problems arising in connection with the replacement of equipment in manufacturing plants and in public utility establishments, as this equipment becomes worn out or obsolete. I have added a sentence to the introductory paragraph bringing out this point. I am inclined to think that this angle of the subject adds very greatly to its importance, also I am informed that some work in this direction is in progress…

Very truly yours,

Alfred J. Lotka

Supervisor, Mathematical Research

Pearl's work on the longevity of life, the rate of living, and the biology of death, conceived of as "problems of general biology" grew out of his adaptation of the life table to the biological science of the human population.

The Life Table

> "I like the suggestion in your friend's letter… that I do not understand a correlation coefficient, or the meaning of a life table.[154]"
>
> -Raymond Pearl, to E.M. East of Harvard University, referring to an anonymous critic. 1927

Lotka and Louis I. Dublin described a life table as "a document which reflects… the course of mortality of vitality of a population." They further described the elements of a life table: "The table may relate to a population taken as a whole, or, what is more appropriate, to a subdivision thereof distinguished as regards certain fundamental characteristics such as sex, color, etc., or their combinations." The life table was composed of a number of columns. Each column was compared across by rows that indicated the value of cell of each column by age. The columns commonly included were the Survivor column, the column of Deaths, the column indicating Mortality Rate, and the Expectation of Life column.[155]

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[154] Pearl to E.M.East. Raymond Pearl papers Box 7 Folder # 6, 1927 APS

[155] Louis I. Dublin and Alfred J. Lotka. *Length of Life*: *A Study of the Life Table.* pp 12-29 The Ronald Press Company, New York.1936.

Life tables were only available for large parts of the population of the United States after 1901, as figures were generated by each state and could not be obtained from the 1890 census.[156] The large numbers on which reliable life tables were based needed to be gathered, checked, and compiled by small armies of volunteers, clerks, and statisticians. This kind of labor was prohibitively expensive before the substantial automation of the U.S. Census in 1890 using Hollerith's card-tabulating machines. Thus, the data that went into making life tables was only available after the development of a quantitative statistics and the desire of the governments of the industrializing states to know more about the characteristics of their populations.[157] From this perspective, the kind of biological knowledge produced at the IBR would have been impossible if not for the techniques of statecraft and governmentality on which the IBRs life tables were modeled.[158]

Lotka was quite ambitious in his conception of the uses of the life table. He and Louis Dublin observed

> "…We have considered only some of those applications of the life tables which involve what might be called the purely biological and demographical aspects of population. There is another extremely important field of application to problems in which not only human lives, but sums of money and the satisfaction of human wants enter prominently, in other words, economic problems. Out of a wide range of applications of this kind, we shall here consider only a few:
>
> 1. To selected problems in life insurance.
>
> 2. To a certain problem in social relations, namely, the economic value of a breadwinner to his family or dependents.

[156] Ibid. p.59

[157] See Hacking, Ian *The Taming of Chance* for an earlier qualitative statistical science that would not have been usable for generating life tables.

[158] Foucault, Michel. *Security, Territory, Population.*

3. To the replacement of industrial equipment"[159]

This may seem at first to be a heterogeneous list that combines incompatible elements into an incomprehensible hodge-podge, but all three were deeply intertwined. And indeed, the techniques of analysis that depend on a "life table for things" is still used in the modeling of product lifecycle management in the design and planning of new products.

The compilation of life tables in the IBR allowed the statisticians and biologists working under Pearl to model the growth of fly populations under a variety of environmental and genetic conditions, to fit that growth to the idealized logistical curve, and to drawn conclusions about the behavior of populations as a whole, the behavior of cohorts and segments within those populations, and the interactions between them.[160]

The work process in the Statistical Division of Met Life appears to have been quite similar to that of the Institute for Biological Research and the US Food Administration: compiling data on the length of life and rate of death into life tables, and using those tables to project likely outcomes for workers of a given race, sex, age, and occupational category. Both organizations made extensive use of feminized technical labor to collect, compile, analyze, and communicate data, and this speaks to the cost-saving measures that were employed by both organizations. Women

[159] Dublin 278

[160] It is clear, then, that birth rates and death rates alone (even in the absence of migration) cannot give us an adequate measure of the powers of growth inherent in a population. To obtain such a measure a special plan must be followed, in which the life table plays a dominant role." (Dublin, *The Length of Life* p.243)

made good computers because they were less expensive to hire, easier to fire, and regarded as especially suited to repetitive and detail-oriented work.

Interrupted Trajectories

Lotka's interdisciplinary scope, or rather his disinclination (or inability) to practice within one discipline, was one of the reasons for his failure to find a stable academic appointment. His ambition to providing the tools for a 'science of everything'[161][162] made him a poor fit in any one particular field. But this was not all: Lotka's career trajectory in the academy was also blocked because he was Jewish. Socially sanctioned anti-Semitism was the coin of the realm in the early 20th century American university. Talented and credentialed Jewish graduates of American doctoral programs in the biological sciences found themselves shut out of biology departments and their enrollment capped at elite universities and medical schools.[163] Louis I. Dublin, Lotka's boss at MetLife, and fellow member of the IUSIPP and American Statistical Association, earned his PhD at Columbia under famed cytogeneticist Edmund B. Wilson but spent his career, like Lotka, as an academic insider-outsider.[164] Both Lotka and Dublin did yeoman's work as officers in professional associations and published in academic journals, but they were dependent on their relationships with more established allies in the academy, who were mostly non-Jews. To make matters more complicated, Raymond Pearl, Lotka's patron at the IBR and in the IUSIPP, was a

[161] Kingsland, p.

[162] Lotka's situation was probably not helped by the fact that his training was in physical chemistry and mathematics, not biometry, morphology, or genetics, though this handicap was not decisive for differently situated actors. For example J.B.S Haldanes undergraduate studies were in mathematics and Classics, and he never took a doctorate.

[163] **Ref?**

[164] Bouk, unpublsihed

genteel anti-Semite himself, and gleefully confided to Lawrence J. Henderson the informal mechanisms by which Johns Hopkins Medical School kept Jewish enrollment down. Pearl smugly denied any "racial prejudice" concerning "the Jewish Question," but justified anti-Jewish discrimination on the basis of a tongue-in-cheek comparison to evolutionary competition and survival:

> Whose world is this to be, ours or the Jews? If you give equal opportunity, my bet is that it will be their world. Perhaps it ought to be, but it is distinctly unbiological [*sic*.] to suppose that we are going to lie down and let that situation come about. In other words, if we are not prepared to look after our own survival, we are indeed in a bad way.[165]

Pearl thus played both roles in Lotka's blocked trajectory. He took part in the old boys' network that formally and informally excluded and marginalized Jewish scientists, while simultaneously providing a limited avenue for his participation at the IBR. Lotka and his wife Romola were on socially friendly terms with Pearl and his wife Maud DeWitt Pearl and they sometimes dined together over the course of their twenty-year professional relationship.[166] Lotka was good enough

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[165] (letter from Pearl to Henderson. Pearl Papers. Henderson Correspondences. June 22nd, 1922. APS.

[166] **1938-39**

Pearl addresses Lotka as "Jim" (From Alfred James Lotka) in letters that he seems to have typed himself in these later years, and Lotka returns the favor by calling him "Raymond" in some of his handwritten replies.

In a letter dated April 13, 1939, on Population Association of America letterhead, Lotka (Now the President of the Board of Directors of the Population Association) Addresses in Pearl's eyes to co-author papers with, and perhaps to have dinner with, just not good enough to support for a permanent position, or to have his work funded.[167]

In *Engendering Business: Men and Women in the Corporate Office 1870-1930,* Angel Kwolek-Follard argued that the changing organization of white collar office work in the early 20th century produced new patterns of social life, and that these changing conventions also reproduced the existing relationships of the gendered organization of work space and domestic space:

> "Changes in technology and consumption patterns encouraged new temporal and spatial partitions of work, domestic life, and leisure. In this context, the commercial buildings of service industries— department stores, theaters, hotels, insurance, and baking— reintegrated the domestic and commercial functions of older urban neighborhood patterns in order to appeal to middle-class consumers as gendered people.[168]"

At Raymond Pearl's IBR, seven of the thirteen full-time employees were women, all but one of them secretaries, technical assistants, or assistant biologists. This meant that the bulk of the

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Pearl as "Dear Raymond" and says that he and "Romola" (Lotka's wife) would be pleased to accept the invitation to visit Baltimore after the meeting of the Population Association, and that "Romola wishes to join me in kind regards to you and Maud"

167 Kingsland on not paying Lotka

168 Kwolek-Folland, Angel (1994) *Engendering Business: Men and Women in the Corporate Office 1870-1930.* Temple University Press p. 105

administrative and biometric work at the Institute was being done by women.[169] In an earlier attempt to get funds for a similar project that never came to fruition, Pearl noted that

> Most of the statistical research or teaching lies in strictly routine copying of figures, computing, etc. It would be a sheer waste of money to expect a competent statistician to perform these clerical operations himself.[170]

After the 1927 expansion of the IBR twelve of the twenty-five staff at the Institute were women, now including Pearl's wife Maud DeWitt Pearl as a "Volunteer Associate." The administrative staff remained feminine, but the new "multi-unit" organizational structure of the work led to changes in the gender balance of decision-making. The Administrative Division, the Division of General Biology I, and the Division of Biochemistry were now headed by women ("in charge" underlined after their names in reports) and in all of those cases there were men operating under those women's authority. But there was also the high turnover associated with feminized office and technical work. Three of the women who had been with the Institute in 1925, Hilda Barnes, Margaret McConnell, and E. Marion Pilpel, had resigned within three months of each other in 1926.[171]

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[169] Executive Secretary- Blanche Poole; Biologist- Agnes Allen; Assistant Biologists- Florence M. Barclay, Hilda Barnes, Violet Hoff; Technical assistants- E. Marion Pilpel, Mrs. Margaret R. McConnell

[170] APS Pearl Papers. Johns Hopkins University — Correspondence with Frank J. Goodnow 1919-1924. n.d.

[171] Executive Secretary- Blanche Poole; Biologist- Agnes Allen; Assistant Biologists- Florence M. Barclay, Hilda Barnes, Violet Hoff; Technical assistants- E. Marion Pilpel, Mrs. Margaret R. McConnell

For more on the predicament of young educated women in the sciences see Margaret Rossiter *Women Scientists in America: Struggles and Strategies to 1940.* 1984. Johns Hopkins University Press.

Women's work

Many of the institutional and organizational forms I have examined here have to do with problems of coordination, either in terms of coordinating a work process, or the problems of coordination in calculation in biology and insurance. There was one more institution for solving coordination problems, and that is correspondence: letter-writing. This can take the form of the internal memorandum in multi-unit firms, either between different departments or between the head office and subsidiary offices. It was also the most important way for researchers to communicate with suppliers of reagents, animals, and instruments, with funders, and with colleagues. To the degree that the organization of a specialized line of research is impossible without a steady exchange of information, like news, employment openings, policy decisions, and even gossip, it was impossible in the early 20th century without letters and letter writing.[172] Most directors of institutes at the time did not physically write their own letters. Rather, they dictated them to their secretarial staff who were trained in shorthand. Secretaries would type letters afterwards, and make sure they were sent to the correct addresses. When writing official letters, secretaries typed onto carbon paper to preserve a sender's copy. Additionally, since publishing an article in a scholarly journal entitled the author to a set number of reprints, it was common practice to request a copy of an article or of a series of articles from its author, as the library work involved in a literature survey could be extensive.

With the more widespread adoption of telegrams, scientific correspondence could be sped up considerably, but this was usually only done when the message was time sensitive. And the duties

[172] This has been true since early modern natural philosophers formed the Republic of Letters. Se the literature on the Invisible College and the Circle Mersenne. (*e.g.* Shapin and Shaffer)

of secretarial staff were not limited to professional correspondence: Raymond Pearl would often send word to colleagues and friends when he was headed to New York so he could be sure to secure a spot as a dinner guest in colleagues' homes or at restaurants. This was all the work of secretarial staff, along with forwarding correspondence to summer homes and marine research stations, answering routine inquiries while the director was away, and booking trips by rail and steamship to conferences, retreats, and hearings in Washington, D.C.

The correspondence that came out of the Institute for Biological Research[173] usually had a small initialed signature of the secretary near the bottom of the page, below the signature. Some secretaries used lowercase letters, and some preferred uppercase. For example, at the Harvard Fatigue Laboratory, R/K/G indicated that the secretary who had prepared the document was Rebecca K. Gregg. The desired effect was for the reader to simultaneously feel as though they were in direct and personal communication with the person who had dictated the letter, but also to be able to hold the actual letter writer accountable should anything go wrong. The relative anonymity and transparency of Pearl's secretarial staff compared to the personal relationships between the investigators whose dictation they took was drawn into relief when sometimes correspondents mistakenly called a secretary by another woman's name, not knowing that the original secretary had left Pearl's employment.[174] This suggests the interchangeability of women

[173] As well as the Harvard Fatigue Laboratory, and other contemporary research organizations.

[174] 2 October 1937

Dear Doctor Lotka:

Doctor Pearl's address is as follows:

℅ Brown, Shipley, and Compnay,

office workers in the Institute's work process, and their status as subordinated human tools of organization, computation, and communication.

Not all of the women's labor at the IBR was menial, routinized, and low-status. After its 1927 reorganization, women headed the Administrative and General Biology I divisions, in some cases supervising men who worked under their direction. Women had had access to graduate training in science and mathematics since the turn of the century, so there was no shortage of highly credentialed female applicants for work in laboratories in government and the academy. Notably, biologist and environmental writer Rachel Carson worked in both, as a laboratory assistant at the IBR in 1930 and also a biologist for the U.S. Bureau of Fisheries from 1935.[175] But it was the bureaucratic rather than the scientific role that made heading a division at the IBR an option for women at the IBR like Blanche F. Poole ('in charge' at the Administrative Division) and

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123 Pall Mall

London, England.

Sincerely yours,

P.S. Miss Pooler is no longer connected with this office.

A.H.

Alfred Lotka Correspondence,Pearl Papers, APS

[175] Quaratiello, Arlene Rodda, (2010) *Rachel Carson, A Biography.* Prometheus Books. pp. 15-18.

(See also Rossiter Margaret, (1982). *Women Scientists in America: Struggles and Strategies to 1940.* Baltimore. Johns Hopkins University Press. pp. 20-32.)

Florence B. White (likewise 'in charge' at General Biology I). Female technician-adminstrators were on the move in big government and business organizations in the 1920s and '30s, as bookkeepers, efficiency experts, and in libraries and research departments.[176]

As I have argued, the management and organization of the insurance sector in the early 20th century was shaped by a pervasive trend towards institutional rationalization (see "Met Life", above). This rationalizing trend interacted with an already existing gendered division of labor. Secretarial, filing, and computational work became feminized at the same time they became increasingly rationalized.[177] Metropolitan Life, an early mover in the trend toward rationalization, was also a national leader in terms of its size and complexity. It was also a leader in the adoption

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[176] A relevant comparison would be the career trajectory of Millicent Pond, Bryn Mawr graduate, MS in mathematics, chemist, and industrial consultant:

> A college graduate turned business administrator, a department manager, a private secretary, a timekeeping clerk, and a personnel manager: these new office occupations lay at the heart of dramatic and far-reaching changes in American office work after 1900. Both government and business experienced staggering expansion…The gendered composition and size of the American office labor force was changing. Even with intense mechanization of the workplace clerical workers in the United States increased overall nearly two and a half times between 1910 and 1930…. Women were only 5.7 percent of the bookkeepers in 1880 but were 31 percent of them by 1910; by 1930 women made up 63 percent of all the book-keepers.[1]

Strom, Sharon Hartman, (1992), *Beyond the Typewriter: Gender, Class, and the Origins of Modern American Office Work, 1900-1930.* University of Illinois Press. pp.17-18.

[177] As more routine and mechanized clerical jobs were handed to women, rationalization continued apace.[1]

[1] Strom, Sharon Hartman, (1992), *Beyond the Typewriter: Gender, Class, and the Origins of Modern American Office Work, 1900-1930.* University of Illinois Press. pp. 242.

of mechanically-assisted computation and hiring of female white collar labor. At Met Life in the 1900s, "nearly 60 percent of the labor force (who numbered more than a thousand) were female."[178] And by 1921, Metropolitan, "gigantic, highly bureaucratized, yet projecting an image of corporate beneficence," employed *six thousand* clerical workers[179].

Sharon Hartman Strom has argued that the ideology of scientific management was seen as a solution to the serious organizational problems that emerged for business firms as they grew to unprecedented sizes. Hierarchical divisions of labor, specialized personnel and records departments, professionalized accounting, and "new methods for keeping workers under control" were developed and adopted. According to Strom, "These changes all relied for their existence on each other, and the language of cost accounting tied them together into an integrated system."[180]

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[178] Strom, Sharon Hartman, (1992), *Beyond the Typewriter: Gender, Class, and the Origins of Modern American Office Work, 1900-1930.* University of Illinois Press. p. 197. (References Priscilla Munro, "White-Collar Women: The Feminization of the Aetna Life Insurance Company, 1910-1930," Unpublished paper, American Studies Program, Yale University, 1982, 9: Joan Wallach Scott, "The Mechanization of Women's Work," *Scientific American* 249(Sept. 1983): 171.

[179] After the regulations that followed the Armstrong hearings of 1905, more restrained advertising established the legitimacy of life insurance as a savings mechanism and widened its apple to the middle class. Life insurance in force in the United States rose from $40,540 million in 1920 to $105,403 million in 1930. The Metropolitan Life Insurance Company moved past its competitor, the Prudential Life Insurance Company, by providing insurance to the armed forces through he Bureau of War Risk Insurance. By 1921 Metropolitian, "gigantic, highly bureaucratized, yet projecting an image of corporate beneficence," had six thousand clerical workers.[?1]

Keller, *The Life Insurance Enterprise,* 285-91; Marquis James, *The Metropolitan Life: A study in Business Growth* [New York: Viking Press, 1947], 199-27; Lawrence Washington, "Personnel Management of the Metropolitan Life Insurance Company," *IM* 62 [July 19210: 27.

[180] The attempt to find a solution to financial crisis and organization chaos was in the wings. The overarching ideology of the solution became known as "scientific management", and its implementation required extensive new methods and personnel for running organizations effectively. They included what

The role that the sex/gender system appears to have played in the division of labor and the allocation of intellectual credit at the IBR and MetLife was to separate out those whose paths to career advancement were clear, and those for whom forward progress was not possible. In practice this meant blocked trajectories for women with technical training and quantitative skills. This is not to say that a skilled female secretary or statistician was not valued. Pearl was happy to help the female secretaries, assistant biologists, and statisticians in his network, but he was careful to refer them to appropriate positions. For example, in 1927 Pearl answered a personnel query from Lawrence Henderson by recommending Eleanor Gregory, one of his former employees at the Food Administration, as a research librarian for the United Fruit Company. Henderson had asked on behalf of an acquaintance at United Fruit, and Pearl recommended Gregory as someone who "can read a variety of languages and who will dig up economic information for them from various sources."[181] As in his relationship to Lotka, Pearl's dealings

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Alfred Chander, Jr. described as new "strategies and structures" for managing large corporations; far more extensive government regulation at every level; the development of an independent accounting profession; the emergence of a hierarchy of office occupations ranging from chief executive to file clerk; and new methods for keeping workers under control. These changes all relied for their execution on each other, and the language of cost accounting tied them together into an integrated system[1].

[1] Strom, Sharon Hartman, (1992), *Beyond the Typewriter: Gender, Class, and the Origins of Modern American Office Work, 1900-1930.* University of Illinois Press. p. 24.

[181] Dr. L.J. Henderson

4 Willard Street,

Cambridge, Massachusetts

with his female technical staff was defined by its paternalism. Lastly, the credentialed staff at the IBR and at Metropolitan were generally American born, had some kind of post-secondary education, and were (presumably, at least) uniformly white.

Pearl's wife Maud DeWitt Pearl was herself a example of a woman with elite scientific credentials whose role in the IBR demonstrated her blocked trajectory. Trained as a biologist at the University of Michigan, DeWitt Pearl participated in the gendered organizational-scientific

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May

1927

Dear Henderson: I have just received a letter from Fredrick S. Dellenbaugh, Jr., who is on of the officials of the the United Fruit Company, 1 Federal St., Boston, asking me to recommend someone to them for a research statistician. This I have done. Also in the same letter, he asked me whether by any chance I know of anyone who might do for research librarian, a position which they propose to establish in the course of the next three or four months.

When I answered him I could not think of anyone to recommend as research librarian, but it has since occurred to me that this might be an excellent opening for Eleanor Gregory. I know exactly the kind of work because we had a good deal of it in connection with the Food Administration. They want someone who can read a variety of langueages and who will dig up economic information for them from various sources. The job is intrinsically much more interesting than straight library work, and it seems to me that Eleanor Gregory would be excellently equipped for it, and furthermore would enjoy it. They will pay, I am sure, a much larger salary than she can expect to get in straight library work.

My suggestion is that you go over to see Mr. Dellenbaugh and tell him that in thinking the matter over after I wrote him this suggestion occurred to me and that I asked you to come and see him about it.

I am in the last desperate struggles of getting ready to sail, which I do on the President Harding a week from next Wenesday, May twenty-fifth. When I get on the old craft I shall let down the tension by a very large amount.

Yours ever,

(Raymond Pearl)

Henderson, Lawrence J. Folder #4 1927-June 1928 work of the IBR. She served as editor of *Human Biology* and assistant editor of the *Quarterly Review of Biology*[182]. In this she was fulfilling the feminine roles of secretary, statistician, and amanuensis, but came to be a substantial part of the intellectual labor of Raymond Pearl's later career. After his death, DeWitt Pearl edited her late husband's last manuscript and posthumously published it in 1946 as *Man The Animal.*

Who says you can't put a dollar value on a human life?

The collective capacities for carrying out work in business, government, and biology were being rationalized along similar lines at the same time, and had reciprocal action on one another. One of the technologies that Raymond Pearl's IBR used to good effect, the life table, came there from the life insurance industry. Historian of the American insurance industry Dan Bouk has followed the use of the life table from its origins as a tool of government statistics in the 18th century, to its use in British life insurance companies, and then to its adoption by American mutual insurers in the middle of the 19th century.[183]

I understand Bouk to be addressing problems in Michel Foucault's category of *biopolitics*: the techniques, practices, tactics, and concepts of managing human populations and making them live more productively. Bouk's work demonstrates that it is possible for some lives to be worth more than others, even under conditions of juridicial equality. Therefore, death being the limit to the

[182] Jennings, H.S. 1942. Raymond Pearl. Biographical Memoirs of the National Academy of Science. USA 22:295-347.

[183] Bouk, Dan, (2011) *The Science of Difference Developing Tools for Discrimination in the American Life Insurance Industry, 1830-1930.* Enterprise and Society, v.12, no. 4. December 2011, pp. 717-731.

power over life, some lives were more easily allowed to die. In the US life insurance industry, racialized categories of persons and the calculation of their differential worth were part of a technology[184] that supported a whole array of techniques, approaches, and problematizations devoted to managing Black and white bodies differently.[185] Similarly, women were valued substantially less than men in insurance policies, usually calculated at around one-half the standard male rate, on the basis of the lower projected loss of wages to a female worker. Women workers made less money than men did, so they were insured at a lower rate and their policies paid out less.[186]

Lotka quoted Alfred Marshall that a purely economic valuation of individuals would

> …put the value of the male immigrant too high, and that of the female immigrant too low unless allowance is made for the services which women render as mothers, as wives and as sisters, and the male immigrants are charged with having consumed these services, while the female immigrants are credited with having supplied them[187].

Lotka admits this is a problem, but decides that it is not a problem to be solved in his book, as it is concerned with "the estimation of the money value of the breadwinner to his family…"

> "Man has much in common with the industrial aids, machines, manufacturing plants, and so forth, of which he makes use to conduct the business of life. Like them he has a "cost of installation," corresponding essentially to the period of his infancy and adolescence, during which he is prepared for service. During this period, there are to be considered running expenses, interest on capital invested, and the loss of a certain

[184] In Foucault's sense of the word I will use again: as a "matrix of practical reason" (See Foucault, *Technologies of the Self*)

[185] Bouk Dan "How Our Lives Became Numbered" (unpublished) (2014?)

[186] Bouk, Dan (), *How Our Days Became Numbered*.

[187] *The Money Value of A Man*. Dublin and Lotka 1930. The Ronald Press. New York.

p.15

> proportion of children that do not live to attain adult age, just as in manufacturing processes allowances for "spoilage" of material that never reaches the "finished" stage."[188]

Discriminatory insurance practices were endemic due to the special breaks afforded white policy holders on the basis of their whiteness (the "material and psychological wages of whiteness" according to W.E.B. DuBois[189]). But this discrimination was also the result of an empirical analysis of the different [life?] outcomes of white and black workers. This was of a piece with the class and gender stratification practiced by insurers as well, with different occupational categories being deemed suitable for different insurance regimes.[190] Foucault mentions both the institution of life insurance and the relationship between biopower and racism in his 1975-1976 lectures *Society Must be Defended*. In it, he asked,

> What is racism? It is primarily a way of introducing a break into the domain of life that is under power's control: the break between what must live and what must die. The appearance within the biological continuum of the human race of races, the distinction among races, the fact that certain races are described as good and others, in contrast, are described as inferior, all this is a way of fragmenting the field of the biological that power controls. It is a way of separating out the groups that exist within a population. It is, in short, a way of establishing a biological-type caesura within a population that appears to be a biological domain…. That is the first function of racism: to fragment, to create caesuras within the biological continuum addressed by biopower.[191]

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188 *The Money Value of a Man.* p. 44

189 Du Bois, W.E.B. *Black Reconstruction in America,* 1935

190 Applicants for life insurance by a natural process sort themselves into economic groups… Persons in comfortable circumstances, including not only the more prosperous groups, but also the more highly paid wage-earners, clerical employees and professionals, will as a rule elect Ordinary insurance… Industrial insurance has been developed for the convenience of wage-earners and their families, when the amount of insurance is small and the insured pays his premiums on a weekly or on a monthly basis[?1].

The Length of Life. pp. 299-300.

191 Foucault, Michel. *Society Must Be Defended*. p. 255

From this perspective, Bouk has identified one of the key institutions of the American racial order, one that would defy the formal legalization of civil rights. After all, is it necessarily discriminatory to point out that different populations behave measurably differently, have statistically different lengths of life and risk of death, and "objectively" constitute a greater cost to insurers and employers?

Conceptual Approach: Self-Renewing Aggregates

The common style of organization of the work process at the IBR and the Statistical Division at the Metropolitan Life Insurance Company was not the only thing they shared. Both the IBR and Metropolitan also used similar abstract ideas, models, and equations derived from mathematical economics. Alfred Lotka was quite self-conscious about this: a biographical note prepared for his obituary indicated that he had

> numerous publications in scientific and technical journals on mathematical analyses of population, mathematical theory of evolution, [and] actuarial mathematics applied to problems of population and industrial replacement.[192]

In a 1939 paper entitled *A Contribution to the Theory of Self-Renewing Aggregates, with Special Reference to Industrial Replacement* Lotka made the explicit connection, in terms of his conceptual object of study, that I have been making through the comparison of work process and

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[192] Alfred J. Lotka Papers. Mudd Manuscript Library, Princeton University. Box 1 Folder 8: Resumes and Biography 1939-1950 (see also digital photo taken) Lotka's employment history.

work place: the seemingly similar characteristics of problems in economics on the one hand and biology on the other.

> These... have been mainly of two kinds, namely, first, applications to population analysis with related problems in genetics on the one hand and actuarial problems on the other; and second, applications to industrial replacement. As the fundamental setting of the two types of problems is very similar, leading in each case to certain integral equations, it will be advantageous to consider together both problems, or both phases of the general problem.[193]

The dynamics of a population of flies, or the likelihood of injury of a worker in a steel mill, cashes out as the a aggregation of the characteristics of the individuals in the population, such as length of life, rate of birth, and rate of death or injury. This is also the model for the kinetics of a chemical reaction. Lotka called it "the model of encounter" for his two species interaction model. What are the chances that a predator will bump into its prey? The likelihood of an encounter is density-dependent, and density in turn depends on the respective rates of death and reproduction for each of the species.

Sharon Kingsland has suggested that Lotka attempted to institute a new 'science of everything' through his books, *Elements of Physical Biology.* Following Lotka's later career in the life insurance sector, it is conceivable that he succeeded in doing so. Insurance, and the calculation and provision of insurance, came to saturate every aspect of commercial and personal life in the United States. Work, injury, sickness, health, life, and death: all were mediated by their calculated worth on the part of estimated insurance policy values. Interestingly, by 1935 this applied even to poor people, the unemployed, and senior citizens. The Social Security Act of 1935 provided for unemployment insurance, retirement insurance, and disability insurance,

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[193] Lotka, Alfred J. "A Contribution to the Theory of Self-Renewing Aggregates, with Special Reference to Industrial Replacement." *The Annal of Mathematical Statistics*, Vol. 10, No.1 (March 1939) pp. 1-25.

bringing an increasingly large proportion of the population of the United States into the calculation and administration of like the ones that Lotka developed in his Physical Biology. As Lotka himself put it,

> One reminder, to the social body, of the value of its individual members, is the burden which the community may have to assume when an individual is removed from the productive force by unemployment, disablement, or death, and dependents are left without adequate means of livelihood. Poverty, in turn, is a fertile soil for the weeds of sickness, delinquency and crime. And so, if only in self-defense, society is wise to assist in providing for those whose resources have fallen below the subsistence level.[194]

The Economy of Nature

There were significant organizational isomorphisms among the high-prestige multi-unit form of the business enterprise, the powerful government bureaucracy, and some organizations within the academy such as the IBR. However, the character of these isomorphisms is not obvious. The institutions at work in biological research, insurance rate calculation, and technologies of governance used similar instruments, but not in identical ways. The IBR, MetLife, and the US Food Administration shared key personnel, and this encouraged these key actors to be able to speak, and act, in a manner that was recognizable to other key actors in the other organizations. But the decisive basis for shared intelligibility between the IBR, MetLife, and the US Food Administration was their common approach to the problems of the population. How to describe it? What were the important parts? What were the significant dependencies between those parts? All of these groups came to the same answer: that the characteristics of a group or a population

[194] *The Money Value of a Man.* p. 160

could be determined by calculating the averaged effects of the individual members of the population. Mortality, birth rate, and death rate were simple averages, and they were affected by density-dependent factors which were themselves also the result of the interactions of the individuals in the population. This "billiard ball" causality of group characteristics was amenable to analysis by the mathematics of mechanics that had been developed by Willard Gibbs and others in the late 19th century to describe the thermodynamics of gasses. Pressure, heat, and quantity were proportional given a constant volume of container. No subsystem-level interactions or dependencies were needed to get a reliable working picture of the physical chemistry of gasses (though it was usually necessary to work in a coefficient to correct for non "ideal" gas behavior that had to be worked out empirically). The same equations that Gibbs developed to model chemical reactions could also describe other gas-like compositions of independent but homogeneous objects equally well, whether in Lotka and Pearl's population studies, or Paul Samuelson's economic models (see below). In all of these cases, what we see is not the passive transfer of "influence" from one inert body to another. Instead, when Lotka deployed 19th century mathematizers, and when Paul Samuelson paid Lotka the compliment for the work done by Wilson and Gibbs before him, they were both active agents who retroactively cited certain theories and mobilized certain actors as allies.

The confluence of biology, business, and government in centers of scientific and bureaucratic calculation had significant effects and resonances in the post-World War II milieu of disciplines dealing with complex systems such as evolutionary biology, economics, and the post-war field of general systems theory (GST).

Geneticist and evolutionary theorist JBS Haldane's 1927 paper, *A Mathematical Theory of Natural and Artificial Selection, Part IV*, cites Lotka's data on the rate of natural increase in human population approvingly. Haldane presented Lotka's work as being both confirmed by his own mathematical work, and also as a key component of his approach to the genetics of evolution under natural selection. Haldane's mathematization of selection showed that it was compatible with Mendelian inheritance and was therefore a key component of the modern evolutionary synthesis.[195]

Austrian biologist Ludvig von Bertalanffy was inspired by Lotka's growth model. In 1954 Bertalanffy's General Systems Theory took root in the fertile soil of Stanford's Center for the Advanced Study of the Behavioral Sciences, from where it spread into post-war anthropology, economics, ecology, and psychology, bringing notions of equilibrium and self-regulation with it.[196] Finally, no less a luminary than Paul Samuelson, winner of the equivalent to the Nobel prize in Economics in 1970, said that Lotka's mathematical models had been "a crucially necessary condition" for the development of his economic work.[197] Samuelson was a central player in the

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[195] JBS Haldane's 1927 paper, *A Mathematical Theory of Natural and Artificial Selection. Part IV*,

[196] See Hammond *The Science of Synthesis*

- In M Drack, 2009 *Ludwig von Bertalanffy's early system approach* indicates that Bertanlaffy was influenced by Lotka's writings. (Systems Research and Behavioral Science, 2009- Wiley Online Library)

  For the realationship between Lotka, Bertalanffy, and General Systems Theory, see Kingsland, Sharon, *Modeling Nature* p.104

[197] "The conservative nature of the Lotka-Volterra model, whatever its realism, is a crucially necessary condition for the applicability of the variational formalisms, microscopically and macroscopically.

Paul A. Samuelson, 1974. *Proceedings of the National Academy of Sciences, U.S.A.*

integration of Keynesian macro-level perspectives into neoclassical economic theory, and arguably the single most influential economist in the post-war West.

While Lotka received credit for an economist's application of equilibrium concepts to his trade, I take historian of economics E. Roy Weintraub's point that Samuelson was not influenced by Lotka in a directly casual manner so much as he was retroactively deploying the work of a biologist who had also applied the mathematical tools of thermodynamics to a complex system. Weintraub argued that Lotka's *Elements of Physical Biology* did not "shape" Samuelson's economic views.[198] Instead, Samuelson was most decisively influenced by the thermodynamic physical chemistry of Willard Gibbs through the person of Gibbs' last PhD student, E.B. Wilson, Samuelson's "revered teacher of mathematical economics and statistics."[199]

A related point can be made regarding my account of the gendered division of work at the IBR and Met Life. The fact that women did all the low-status clerical work in these organizations doesn't mean that the outcomes of that work process were indelibly marked by it. Rather, the use of feminized technical labor, like the conceptual apparatus of the self-aggregating system, was one of the conditions of mutual intelligibility that allowed for the parallel and overlapping development of the capacities of these organizations for generating knowledge about populations, whether it was for the purpose of assessing risk, computing value, governing, or publishing academic journal articles. Interactions between individuals were considered to be collisions between inelastic billiard ball-type objects, whose attributes are contained within the objects themselves and are not dependent on the relationships between objects. This model of group behavior in economics and politics, based as it is on the aggregation of individual utility

[198] "However, it is not my claim that Lotka's book shaped Samuelson's." Weintraub, E. Roy. "Stabilizing Dynamics, Constructing Economic Knowledge." 1991. p.42.

[199] Weintraub, pp. 69-71 Quoting Sauelson (1972) p. 11

maximizing preference, has the characteristics of a the liberal individualist model of personhood, also implicitly masculine and male.[200]

La Pensée Bourgeoise

In his polemic against sociobiology Marshall Sahlins warned against efforts to explain society on the basis of some underlying biological substrate because "…the culturalization and the naturalization of culture" as he put it, obscure real understanding of both the organic world and of society.

> In the social sciences we exhaust our own symbolic capacities in endless utilitarian theorizing, some of it economic, some of it ecologic… All these efforts taken together represent the modern encompassment of the sciences, both of culture and of life, by the dominant ideology of possessive individualism.[201]

At the same time, Sahlins recognized that such movements between nature and culture, economy and biology, were characteristic of modern Western thought since the 1500s, or as Sahlins named it, *la pensée bourgeoise:* the concepts typical of bourgeois civilizations.

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200 See Anne Fausto-Sterling and Susan Oyama for the imllic gender identities of relational thinking as compared to atomistic/individualist.

201 We seem unable to escape from this perpetual movement, back and forth between the culturalization of the nature and the naturalization of culture. It frustrates our understanding at once of society and of the organic world. In the social sciences we exhaust our own symbolic capacities in an endless reproduction of utilitarian theorizing, some of it economic, some of it ecologic. In the natural sciences, it is the vulgar and scientific sociobiologies. All these efforts taken together represent the modern encompassment of the sciences, both of culture and of life, by the dominant ideology of possessive individualism… Give it its due: sociobiology is Scientific Totemism[1].

Sahlins, Marshall. (1976) *The Use and Abuse of Biology.pp. 105-106*

> One might say that if production reflects the general scheme of society, it is looking at itself in a mirror. But it is saying the same thing… to observe that in Western culture the economy is the main site of symbolic production… The uniqueness of bourgeois society consists not in the fact that the economic system escapes symbolic determination, but that the economic system is structurally determining.[202]

Indeed, it is exactly these continual passages back and forth between the artificially maintained poles of Nature and Society that distinguish modern bureaucratic, rationalizing, capitalist societies from other ways of organizing human affairs.[203]

This returns us to the point I made earlier about technical work in bureaucracies, centers of computation, and research laboratories being organized on the basis of a shared understanding of what is at stake and what constitutes legitimate action. Should we think of the commonalities between business, biology, and government in terms of "trading zones," "a limited domain of shared interest but divergent understanding,"[204] with Lotka's work being abstracted and transferred from one field to another? Certainly, the role played by mathematics and statistical procedures in Lotka's work made it easier to empty it of specific content and thus easier to pass

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[202] Sahlins, Marshall. (1976) *Culture and Practical Reason.*

[203] Latour, Bruno, (1994) *We Have Never Been Modern*.

[204] Porter. *Objectivity as Standardization*. p. 48

See also Galison (1997) *Image and Logic: A Material Culture of Microphysics.*

and

Starr and Griesemer (1989) *Translations and Boundary Objects.*

between fields lacking common understanding.[205] But in other ways, the figure of the trading zone is not useful, because the IBR, Metropolitan Life, and the US Food Administration *did* share certain important understandings. They inhabited different fields (bureaucratic, scientific, commercial) but ones that were mutually recognizable due to similar styles of rationalization of work and organization, and similar conceptions of populations as equilibrium-seeking collections of atomized and individualistic agents. The notion of strategic action fields is again useful here, because it allows us to think about the means by which different organizations and institutions come to resemble each other, without recourse to a theory of tracking the "influence" of one on the other, but also without having to simply stop at saying that there is a socially dominant field, and all other must somehow "reflect" its logic.

Conclusion

The human biology of Raymond Pearl and Alfred Lotka was working within a particular notion of what counted as an explanation for the behavior of a system. This pursuit was articulated by changes in organization and the work process, changes in ways of thinking about things and doing things, taking place across wide swaths of American society at this time. The common identification of this form of rationality amongst business enterprises, government bodies, and

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[205] The crucial insight here is to see objectivity as a way of forming ties across wide distances. It epitomizes the possibility of effective communication between parties whose goals, interests, and beliefs may differ fundamentally. It serves as a common ground, if only by leaving out much of what each party to the transaction regards as fundamental. In physics, it is an alternative to a shared understanding. In regard to politically charged questions such as those faced by cost-benefit economists, objectivity is a substitute for trust….a small, closed community can get by with much less objectivity. But when meanings are not shared, and face-to-face dialogue is impractical, a rigid and austere formalism may provide the best hope for settling contested issues.

Porter. *Objectivity as Standardization*. p. 48

scientific research work in the 1920s and 1930s. In the late 19th century, Alfred Marshall and Charles Darwin shared and took up in common notions of progress, equilibrium, and efficiency from contemporary political economy.[206] Lotka was quite aware of this identification when he enlisted Marshall's views on efficacy as a factor in progressive evolution into his own orthogenetically-tinged theories. And, as Kingsland has pointed out, Lotka integrated insights early in his career from 19th century mathematical economics, especially the early developers of general equilirium theory *i.e.,* Leon Walras, and his successor Vilfredo Pareto.[207]

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[206] The Mantra of Efficiency: From Waterwheel to Social Control. (2008) Jennifer Karns Alexander. Baltimore: Johns Hopkins University Press. pp.70-75.

[207] Kingsland, Sharon. (1985) Modeling Life: Episodes in the History of Population Ecology. pp.41-42.

CHAPTER 4: ADVICE TO THE PRINCE: THE COMMITTEE ON INDUSTRIAL PHYSIOLOGY

"Intelligent Solutions"

In 1946, Elton Mayo prepared an introductory lecture for a course on Human Relations in Industry. Mayo's lecture is in some sense a retrospective of the work of the Committee on Industrial Physiology, an ad-hoc group at Harvard that supported the work of Mayo's Hawthorne Studies, Lawrence Henderson's Fatigue Laboratory, and a shifting roster of other projects between 1930 and 1939.

Mayo's lecture captures several of the important perspectives that he brought to the work of the Committee, namely; the importance of synthesis, equilibrium, the interaction between parts and wholes, and the indivisibility of social and biological phenomena. Observing at the beginning of his lecture notes that "…in any given moment in the history of scientific development, there tends to be a ruling idea that determines the direction of many inquiries," Mayo described how "Fifty years ago the ruling idea in science was analysis." That is, science had achieved much by breaking phenomena down to their component parts. In physics, chemistry, biology, and psychology, analysis had paid out high dividends. But, Mayo cautioned, "There was one grave defect in all these theories." Analysis could not explain the difference in structure and function of all the subunits of a functioning whole, and it could not conceive of a phenomenon that behaved in a dynamic and responsive way over time, what Mayo called "the integrity of response of the system." After outlining the role of equilibrium in chemistry and biology, Mayo turned to the social sciences.

> Now the social aspects of equilibrium. This presents greater difficulty, for in this area we cannot take for granted an almost automatic adjustment to the process of living, as we can in the case of the physical organism. A useful analogy may nevertheless be made between the human body and a social group. The internal system of the organism is arranged to adapt as a unit involuntarily; equilibrium is maintained by physiological balances. The social equilibrium has not thus been contrived for us. But in primitive cultures, taboos and traditions function almost as a species of vegetative nervous system. Modern culture with its loss of routine and emphasis on intelligent adaptability, is in a position analogous to that of an organism that has lost its vegetative nervous system. Our society therefore is compelled to find intelligent solutions for every separate problem when faced with change.[208]

A social group for Mayo was a system tending towards equilibrium. Modern social groups and biological organisms were different in that modern societies lacked the equivalent of an automatic physiological or instinctual response to stimuli. Primitive and traditional societies had this characteristic, but it had been lost in the last century in the West due to industrialization and atomized urban life. In the absence of its "vegetative nervous system," society is forced to look to "intelligent solutions for every separate problem when faced with change."

What follows is an account of the "intelligent solutions" that Mayo and the members of the Committee on Industrial Physiology proposed in their communications with the Rockefeller Foundation and with the administration of Harvard University as the fortunes of the Committee rose and fell over the course of the 1930s. In 1929 the newly reorganized Rockefeller Foundation funded the work of a cross-disciplinary group at Harvard University called the Committee on Industrial Physiology (CIP). The committee's research and pedagogical work were oriented

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[208] *Noted on a Lecture on Equilibrium, 1946.* Elton Mayo Papers, Carton 2, folder 8. Baker Library Historical Collections Department.

towards different ends for different members of the alliance. The CIP program included a research component in the Harvard Fatigue Laboratory and Elton May's interpretation of the Hawthorne Studies; a pedagogical aspect as part of Wallace Donham's curriculum for Harvard Business School; and Lawrence Henderson's work with the Harvard Pareto Circle, his course Sociology 23, and the Harvard Society of Fellows.

This is a story about the formation and maintenance of alliances within and between specific organizations carrying out scientific research work, philanthropy, university administration, and the management of industrial firms. These organizations became intelligible to each other and entered into alliances on the basis of the similarities that had developed in the ways they each carried out their own lines of work. Specifically, the style of research work being carried out through the Committee on Industrial Physiology were asking questions and using tools and techniques that seemed like good ways of going about organizing a line of work to its funders in Rockefeller and its allies in Harvard. At the same time, Rockefeller and Harvard were confronting organizational and administrative problems of their own, and were reforming their institutional practices according to a particular kind of rationalization. The Committee's alliances that allowed it to carry out its work depended on the mutually recognizable styles of work and kinds of rationalization that were going on at the same time within the institutions of its audiences in Rockefeller and Harvard. The CIP provided the financial and institutional support necessary for the work of the Harvard Fatigue Laboratory, the large-scale industrial sociology project of the Hawthorne Studies, and W. Lloyd Warner's "Yankee City" study of Newburyport, Connecticut (Trahair, 2004). The CIP was also imbricated in the development of the case-study system of education at Harvard Business School, the Harvard Society of Fellows, and the interstitial "Harvard Paretans" under the CIP's Lawrence Henderson.

I intend the story of the CIP in the '30s to provide insight into what Michel Foucault called governmentality[209] and the problems of government. In discussing Machiavelli and the anti-Machivellian literature of the 16th century Foucault claimed that in the art of government there were basically two strategies for intervention. The first strategy worked "upward": to govern the state the ruler must learn to govern himself. The second worked "downward: the policies (police) of the state must govern the lives of its citizens so that they well ordered, so that in turn the state will function well.

> There is upward continuity in the sense that whoever wants to be able to govern the state must first know how to govern himself, and then, at another level, his family, his goods, his lands, after which he will succeed in governing the state… It is the education of the Prince, therefore, that will assure the upward continuity of the different forms of government. Then there is continuity in the opposite, downward direction in the sense that when a state is governed well, fathers will know how to govern their families, their wealth, their goods, and their property well, and individuals will also conduct themselves properly. This descending line, which means that the good government of the state affects individual conduct or family management, is what begins to be called "police" at this time. The education of the Prince assures the upward continuity of forms of government, and police assures their downward continuity.[210]

The Harvard Committee on Industrial Physiology followed both of these forms: the upward form of elite training and the downward form of policy on industry, administration, and the organization of the state. The two pieces of the Committee's project could be conceived of as 1. Advice to the Prince (training of elites and young men who are being groomed to run

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[209] Foucault's use of the term is fairly labile. For the sake of this case study, I treat governmentality as the institutions of governance, including techniques, forms of knowledge, and sites of intervention. For more see *Security, Territory, Population.* Lectures at the College De France. 1979. P. 107

[210] *Security, Territory, Population.* Lectures at the College De France. 1979. pp.132-133

corporations, government departments, and universities). and 2. Policy ("police," in Foucualt's terms)

The primary actors within the CIP alliance shared a concern with training men for elite careers in government service, business leadership, and academic prominence. But the first communications between the CIP and the Rockefeller Foundation did not emphasize training in human biology. Instead, the CIP presented itself as a coordinating body that would be able to organize all the varied work going on at Harvard that did not fit easily into one department, and it was on this basis that the CIP became legible to the President of Harvard, A. Lawrence Lowell, and to Rockefeller's Division of Social Sciences. The members of the CIP alliance used the term human biology for this project of research, training, and institutional coordination.

Scientific Anti-Democrats

In *Science, Democracy, and the American University* (2012), Andrew Jewett applied the label "scientific democrats" to "the large and varied group of American thinkers who contended that science, as they understood it, offered the basis for a cohesive and fulfilling modern culture." Jewett took care to define his democrats as having a particular set of concerns not immediately recognizable to partisans of democracy in the early 21st century. They were not focused, for example, on securing civil rights for women and oppressed minorities, and they did not have much faith in a redistributive state. Rather, the scientific democrats promoted democracy by promoting science. Their goal was "...[t]o articulate and disseminate what they took to be the social meaning of modern science, above all its revelation of the need for citizens to adopt a greater sense of social obligation and mutuality." This did not mean the actual active participation of citizens in the democratic process. Instead, "[s]imply assuming that public opinion mattered centrally in American governance, they focused on making an impact on the minds of citizens."[211] The pragmatist philosopher John Dewey stands near the center of Jewett's concentric circles of scientific democrats. Moving out from Dewey's central position, there were also "those who argued, beginning in the 1880s and accelerating after the turn of the century, that the development and popularization of the human sciences would turn Americans away from competitive capitalism rather than towards laissez-faire ideals."[212]

But as Philip Mirowski reminds us "[t]he yoking together of science and democracy was not such an obvious winning combination in the early twentieth century."[213] If Columbia University gave John Dewey a home in this period in New York, then Cambridge was the home turf of "a prevalent intellectual current that framed the duel as incompatible in structure and content."[214] Here scientific democrats found their opposite: let's call them Harvard's scientific elitists. They were a network of biologists, social scientists, administrators and businessmen held together by two common commitments. First, a belief in the basic irrationality of humans as individual and in groups.[215] Second, deep pessimism about the compatibility of irrational humans with political democracy in a rapidly changing society. Their common intellectual project was to find a way to

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211 . Jewett, Andrew. "Science, Democracy, and the American University From the Civil War to the Cold War" Cambridge University Press. p. 9.

212 . Jewett, Andrew. "Science, Democracy, and the American University From the Civil War to the Cold War" Cambridge University Press. p. 17.

213 . Mirowski, Philip. Sleights of the Invisible Hand: Economists' Interventions in Political Theory.(Review Essay) "The Journal of the History of Economic Thought", 27(1) March 2005, p. 92

214 . Mirowski, Philip. Sleights of the Invisible Hand: Economists' Interventions in Political Theory.(Review Essay) "The Journal of the History of Economic Thought", 27(1) March 2005, p. 92

215 "Not all these biologists assigned the human mind a leading role in the creation of the cooperative commonwealth. A strikingly anti-rationalist account came from the pen of Harvard's William Morton Wheeler, a leading proponent of emergent evolution in the 1920s… Wheeler's system omitted the individual mind as a distinct phase of reality. An entomologist by speciality, he argued that human societies took their shape from exactly the same instinctual forces driving social insects such as ants and termites to form societies. " --Jewett, Andrew. *Science, Democracy, and the American University.*

manage the institutional and organization re-structurings that emerged from the serial crises of World War I, immigration and demographic change, urban problems, labor unrest, the Great Depression, and the New Deal. Their answers to these questions varied in that some favored a laissez-faire approach to economics, politics, and civil society, while others argued for the necessity of a strong managerial elite to act as an autonomous nervous system for an acephalous society (see Introduction the differences here between Lawrence Henderson and his friend and ally at Johns Hopkins Raymond Pearl). However, they were united in their mistrust of popular notions of democracy and in their scorn and contempt for intellectuals who associated with progressive social movements.

In order to provide a sense of what was at stake for the scientific elitists in the aftermath of World War I, I offer two short excerpts from the memoirs of prominent members of that network. Fritz Roethlisberger was born in 1898 on the Upper West Side of Manhattan to immigrant Parents. French mother of Swiss French parentage and a Swiss German father (Bern Canton.) His father and uncle were in the wholesale cheese import business, and prosperous. His father died when Fritz was five, a year after the family had moved to Staten Island. His mother remarried. He was baptized in the French Swiss Evangelical Church, and his step-father was Presbyterian. He went to private school in Staten Island, the Columbia College, then MIT. After two years traveling, working as a chemist in Texas and visiting in Mexico, Roethlisberger enrolled in a PhD program at Harvard's Philosophy Department, though he never finished his dissertation. He had early socialist leanings but seemingly grew out of them by the time he began working for Elton Mayo in September 1927 as an instructor at Harvard Business School, where he co-wrote the landmark analysis of the Hawthorne Study data, *Management and the Worker* (1939). Looking back on his career in 1977 and he had some reflections on the place of this career trajectory in the larger social changes then going on around him:

> …So to the questions of "Where were you, Charlie, when all these storm clouds were gathering?" and "What did you do? What peace marches and protest movements did you join to bring peace and justice to this troubled land?" my answer has to be I was on no firing line but on some ivory-colored cloud-- believe it or not-- of all places at the B-school, trying to understand who, if anyone, was going to administer all these changes and revolutions that were going on. I shall try hard not to justify this position, but neither do I want it to be discounted. I felt that in my experience there might be some clues, if not solutions, for the future. That is why I continued to write while all hell seemed to be breaking loose around me and when on many occasions I was ready to take the hemlock.[216]

Thought the above passage explicitly refers to peace movement of the 1960s, Roethlistburger's biographical concerns surely reached back into the the period of the Depression and the New Deal. He taught at Harvard Business School for 47 years, from 1927-1974 when he retired as the Wallace Brett Donham Professor of Human Relations, training thousands of young men (and eventually women) for positions of power and influence in America's worlds of business and government.

Another member of the Paretan interstitial academy was George Homans, co-author of *An Introduction to Pareto, His Sociology*. Homans' father was Robert Homans, graduate of Harvard Law school, and member of the Harvard Corporation, the seven-member governing body of Harvard, "The President and Fellows of Harvard College." Robert Homans, his brother Jack, and their father were all members of Harvard's Porcellian Club. George was not considered for membership in the Porcellian, but his brother Bob was (the Porcellian Club made Skull and

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[216] . The Elusive Phenomena: An Autobiographical Account of My Work in the Field of Organizational Behavior at the Harvard Business School

Roethlisburger, Fritz J., 1977. Harvard University Press.

p. 6

Bones look positively *arriviste*). Homans was a a protege of Lawrence J. Henderson, attendee of the Pareto Seminar, a Junior Fellow at the Society of Fellows, and served on Harvard faculty from 1939 to 1970. In his autobiography Homans looked back at his decidedly Brahmin upbringing in Boston and recollected the distinct sense of anxiety and uncertainty that members of his social class felt at the time.

> We youngsters did not articulate the social ideas we learned in the Back Bay. Had we done so, they might have sounded something like this: "We are a group, the Yankees, and we are different from other ethnic groups. Not only are we different, we are better than the others, especially in a lingering respect for intellectual attainments, even on the part of those of us who have not acquired them. We are perhaps worse than other groups in our reluctance to fight. Yet is our betterness really a betterness when it has done us so little good? We have lost political power in Boston and even our control of the State House is in jeopardy. It is only a question of time before we lose our other superiorities. Our financial power may be the last to go. Nor is it merely a question of our superiority: our very identity is at stake. We are a great group with a great history, but we are bound to disappear as surely as Cooper's Mohicans." We had also learned not to be quite so sentimental about the proletariat as some of our later, Marxist, friends affected to be. After all we had suffered at its hands.[217]

Homans' reminiscences set some of the tone for what the outlook of the interwar period was for the certain established Boston families. Their political power was in decline in the face of the Democratic party machine and the Irish, Italian and Eastern European Catholic interests then gaining ground in Boston and Massachusetts. The idea of an American way of life based on Protestantism, free trade, and good (*i.e.* limited) government seemed at risk. The remaining influence the Brahmins did posses was in the elite institutions- especially higher learning. Through the universities, in this case Harvard Business School and Harvard College, a large number of important reforms took place which stabilized the institutions that Homans was worried about disintegrating throughout the Depression, the New Deal, and WWII. So successful

[217] Homans, George. 1984. *Coming to My Senses: The Autobiography of a Sociologist.* Transaction Publishers, First Edition. p.19.

was this program of institutional reform and stabilization that Homans was able at the end of his life to look back on the events of his childhood and wonder what it was that had him and his family so worried.

> My later experience has reversed some of these inarticulate judgements. How could we have been so worried? In a strange way we Yankees have won, not lost, and won in a way I never conceived in my youth. Who would have believed then that parochial schools would be closing, not opening, that the once-conquering Catholic Church would be hard put to fill her seminaries, that her newer churches would look for all the world like colonial meetinghouses, and that in them the priests would repeat the Mass-- which I bet will soon be called Holy Communion-- in English and facing the congregation? Of course it is our culture not our numbers that has won. It is not the old Yankee culture, but then that too had been changing and was bound in any event to change further. Still, it is a descendent whose cultural genes have more Yankee in them than anything else. In the process much has been lost. For instance, the Irish have given up their lovely brogue for the most nasal of Yankee dialects. Perhaps all I am saying is that I should never have believed that we would all become so American, though our convergence still has far to.[218]

Other scientific elitists associated with the Harvard Paretans include Professor of Industrial Relations Elton Mayo, Dean of Harvard Business School Wallace Donham, President of the New Jersey Bell Telephone Company Chester I. Barnard.[219] But of course the most important Harvard Paretan was Lawrence J. Henderson, Director of the Harvard Fatigue Laboratory, organizer and central personality of the seminar on the *General Sociology* of Vilfredo Pareto, founder of the History of Science as a discipline at Harvard, and co-founder of Harvard's Society of Fellows.

Wallace Brett Donham (1877-1954) was Dean of Harvard Business School from 1919 to his resignation in 1942. Donham was a product of Harvard College, Harvard Law School, a vice-President of the Old Colony Trust Company, and a friend and confidant of Harvard's President A.

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[218] *Ibid.*

[219] After WWII Barnard was President of the Rockefeller Foundation, 1948-1952; Chairman of the National Science Foundation, 1952-1954; and one of the first members of the Society for General Systems Research.

Lawrence Lowell. He embraced and supported Mayo's work on "the human factor" in business and industry, and extended the use of a case-based system of instruction at the Business School (Elisséeff, 1955. Trahair, 1984 p.8).

George "Elton" Mayo (1880-1949) was born in Adelaide, South Australia, and trained there in philosophy and psychology after incomplete medical studies in Edinburgh and London. He had worked at the University of Pennsylvania's Wharton School from 1923 until 1926, at which point he moved to Harvard Business School. The work for which he was known in the 1930s was the Hawthorne Studies, also referred to as the Hawthorne Experiments. This was a longitudinal study of "human factors" in industrial sociology that tracked the productivity and attitudes of cable assembly workers at the Western Electric Company's Hawthorne Works in Cicero, Illinois, just outside Chicago's West Side. The Hawthorne Studies were one of the various projects funded through the CIP.

Lawrence Joseph Henderson (1878-1942) was a polymathic figure at Harvard. He was a Harvard AB, and trained in medicine at Harvard and physiology at the University of Strasbourg. Henderson moved his research in biological chemistry away from the Medical School in 1926 and into Harvard Business School, where he ran his Fatigue Laboratory from the basement of Baker Hall. The research of the Henderson group (Arlie Bock and David B. Dill, William and C. Consolazio, and their students and visiting scientists) into the dynamics of blood-gas equilibria was summarized in *Blood: A Study in General Physiology* (1928.) But their work on blood was conceived of differently than their later work on the physiology of fatigue, and carried out on a different, or rather, expanded, subject. In 1927 Henderson's lab moved from the its original home in Boston at Children's Hospital into custom-built facilities in the basement of Baker Hall in

Alston, on the campus of Harvard Business School. The subject of inquiry expanded to include respiration, metabolism, and circulation.

> From the blood the study has advanced to include also, simultaneously the circulation, respiration, and metabolism. Nomographic handling of all this is extremely complicated and not yet satisfactorily but the understanding involved in such problems, that has come from the use of the nomogram, has immensely assisted the study of this difficult question.[220]

Henderson had conceived of the Fatigue Laboratory as a link at the Business School between the earlier blood physiology work and Elton Mayo's efforts in studying the human factors in industrial work. Construction of the Lab's new location was paid for by a large grant by the Rockefeller Foundation to Harvard's interdisciplinary Committee on Industrial Hazards/Physiology. A report by the Committee in 1929 made the case that knowledge of fatigue was key to proper industrial organization.

> The relation of physiology to both industrial activities and psychiatric conditions is too obvious to need explanation....Industry has studied production and the economic organization of production sufficiently well; it has never been also to advance a single step in the direction of measuring the effect of working conditions (and industrial organizations) upon the human organism except by way of inference from differences in output. Necessary as this last is, it nevertheless will contribute little or nothing to our knowledge of the human aspect of industry until changes in output can be related to changes in organic and mental equilibrium.[221]

Dean of Harvard Business School, board member of the CIP, and interstitial Paretan Wallace Donham echoed this sentiment. The science of fatigue could explain why some employees worked happily and efficiently, while others dragged their feat and complained.

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220 . 1929. Committee on Industrial Fatigue grant application to Rockefeller Foudation. Page 3. RAC. Rockefeller Foundation Collection. Record Group 1.1, Series 200. Box 342 Folder 4081.

221 . 1929. Committee on Industrial Fatigue grant application to Rockefeller Foundation, appendix I. RAC. Rockefeller Foundation Collection. Record Group 1.1, Series 200. Box 342 Folder 4081.

> These fatigue investigations will inevitably find their sphere of importance in the industrial situation. In the meantime the indirect value of the Henderson approach has been shown to be immensely significant for industry. This can be put in several ways. One might say that high production (De Mar in Marathon running) occurs in those individuals who can increase their muscular activity without any significant change in the character of their organic equilibrium. In the factory this implies that any measure, however unsatisfactory, or organic equilibrium, should show a significant difference to exist between the worker who is producing well and easily and the worker whose production is low and accomplished with difficulty. This has been found to be the case.[222]

In addition to his work at the Fatigue Lab, Henderson ran a seminar on the sociology of Italian economist Vilfredo Pareto, founded the Harvard Society of Fellows, and was instrumental in bringing the study of the history of science as a discipline to Harvard. Henderson was a proud native New Englander, and several of his close colleagues noted his imperious and abrasive personality (Cannon, 1942. Homans, 1984 pp. 89-94).

Henderson's early approach to theory of knowledge was conventionalist and phenomenalist, somewhat in the vein of Ernst Mach.[223] He had a deflationary definition of truth, and quoted Henri Poincaré approvingly regarding the practical interchangeability of statements "'the external world exists,' or 'it is more convenient to suppose that it exists'"[224] For Henderson, the thing that separated real science from unscientific social science tying to trade on the authority of the natural sciences was this skepticism regarding concepts: "Today, clear-headed physicists no longer 'believe' their theories; but Marxists, Freudians, Fascists, New Dealers, and disciples of

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[222] . Wallace Donham. 1929. Committee on Industrial Fatigue grant application to Rockefeller Foundation. Page 2. RAC. Rockefeller Foundation Collection. Record Group 1.1, Series 200. Box 342 Folder 4081.

[223] . It is unclear whether Henderson got his "constructivism" with regards to concepts from the Machian positivists or from the Harvard pragmatists, especially William James and Charles Pierce.

[224] . Henderson, Lawrence J. "On the Social System." The University of Chicago Press.1970. p. 76 (Originally in

'laissez-faire' are in general believers in dogma rather than mere users of theory."[225] The difference between science and ideology was that ideologists believed in the reality of their own theories, something working scientists did not do. Henderson contraposition of science and ideology was intentional, as it corresponded to his interest in seeing what science could do to clarify social problems, but only from what he considered to be the proper scientific point of view.

Henderson's reliance on physics as the support for his sociology was methodological as well as conceptual (physicists were skeptics, sociologists were believers.) But Henderson was not above using the findings of physicists as a weapon against the sociologists when it suited him. The historian of equilibrium thought Cynthia Eagle Russert saw that one of the attractions of Pareto' thought for Henderson was the Italian's mechanical rationalism:

> In treating society as a system, Pareto was doing for sociology what Gibbs had done for chemistry, what Bernard had adumbrated for physiology. Pareto's social system was in important respects analogous to Gibbs' physiochemical system.[226]

But this was a problem. If Pareto's worldview and his economic science relied on assuming strictly rational and efficient phenomena, then how could this be squared with Pareto's aristocratic social[227] theory? The answer to this question formed the core of Henderson's appropriation of Pareto: social action was chaotic and irrational because

225 . Henderson, Lawrence J. "On the Social System." The University of Chicago Press.1970. p. 76

226 . Cynthia Eagle Russert "The Concept of Equilibrium in American Social Thought." 1966 Yale University Press. p. 115

227 Pareto's name is associated with a certain definition of optimal distribution in economics. A system is 'Pareto efficient' if none of its actors can be made better off without making at least one other member worse off.

human beings were not governed by rational principles. Instead they were animated by *sentiments*, the built up accumulation of beliefs, values, and habits of feeling that characterized a people in a particular stage of historical development. Pareto's sociology hinged on the notion of sentiment, "…unobservable mental states" which structured thought and action. The sentiments were expressed simultaneously in the actions and "derivatives" and "residues", two other Paretan terms. Malleable "derivatives" often took the form of verbal justifications and statement about beliefs; and the more stable "residues" manifested as values and deep cultural structures. The residues were considered good representatives of the characteristics of sentiments, even if the two were analytically distinct[228].

Like Pareto, Henderson's view of sentiments as applied to social change was essentially conservative in two senses. First, he viewed the built up sentiments of a people, time, and place (residues and derviatives in Pareto' phraseology) to be basically adaptive and beneficial.

> The social environment is what sentiments, routines and rituals make it. From the most perfect family in the world take away the sentiments, the routines and the rituals, and the residue will be unrelated individuals. No doubt the social environment... of a factory is in many ways less important and far less perfect than the social system of a good family. But in several respects it is the same kind of thing and, as experiment shows, it is in several respects so important that it cannot be neglected by anyone who wished to plan wisely, or even to know what he is doing.[229]

Sentiments built up over a long time served society as a reservoir of values regarding the rules of the game, what was at stake, and what counted as a legitimate actor or action. In the language of

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228. Shapin, Steven. Understanding the Merton Thesis. *Isis*, vol 79, No. 4. (Dec. 1988) pp. 594-605.

[229] ??? *On the Social System?*. 234

symbolic interactionist sociology, these sentiments could be translated as the conventions and commitments that comprise.[230] In the language of Pareto's countryman (and fellow theorist of elite circulation) Antonio Gramsci, these sentiments could be viewed as the "common sense" of a society, the unspoken legitimating rules and short-hands that constituted.[231]

> When societies are too unstable, individuals suffer. It seems probable that the psychoneuroses are most common in Durkheim's anomic people and that, for instance, disease of the coronary arteries, gastric ulcer and duodenal ulcer are most frequent in unstable societies.[232]

There was a second sense in which Henderson's perspective on social change was conservative, and this was his cyclical understanding of the larger processes of history. He did not deny that social change was possible, only that it could be dangerous and that in the long run its effects would almost always be undone. The social system was self-equilibrating, and was not an "orthogenetical evolution from the simple to the complex," as Herbert Spencer purportedly believed. According to Henderson

> "[Spencer] held that the evolution is largely determined by what he supposed to be the law of instability of the homogeneous. The meaninglessness of all this was long ago demonstrated by the physicist Tait in a forgotten conversation with Spencer. Perhaps a remark of Clerk Maxwell's on a postcard addressed to Tait will suggest the natural attitude of a man of science in these premises. Maxwell wrote "Have you (read) Willard Gibbs on Equilibrium of Heterogeneous Substances? ... Refreshing after H. Spencer on the Instability of the Homogeneous."[233]

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[230] Joan Fujimura on Anselm Strauss's use of the notion of commitment

[231] Gransci, Antonio. *Selections from the Prison Notebooks*. 1971.

[232] . Henderson, Lawrence J. "On the Social System." The University of Chicago Press.1970. p. 258 (originally in What is Social Progress 1941)

[233] . Henderson, Lawrence J. "On the Social System." The University of Chicago Press.1970. p. 252 (originally in What is Social Progress 1941)

While some things in human life were progressive in the sense of growing more complex, such as technology and organizations, other things became simplified and degraded, like grammar in language, culture, and in the vital sentiments that kept society functioning properly. Henderson approvingly quoted Machiavelli and Polybius to the effect that the virtue of religion in pagan Rome was the good effects that its cults had on the state and the people. That Roman polytheism was false was not the issue: it played a stabilizing and therefore salutary[234] role.

Cynthia Eagle Russet noted although Marx and marxist sociologists despised equilibrium notions in social theory as being conservative inasmuch much as they were "against progress," it was impossibly to simply diagnose someone's political positions on the basis of their opinions on equilibrium.[235] But in the case of Henderson the affinity for equilibrium and reactionary modernism coexisted comfortably, and it is clear that his politics and his metaphysics reinforced

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[234] See Garland Allen, 1975 for a contrasting perspective on Henderson's equilibrium as it related to his politics.

"Henderson's methodology started from the basic assumption that all living systems function according to the laws of physics and chemistry... But studying individual reactions was not enough; the blood buffer systems have could not be understood by simply reducing it to a list of separate parts. What was crucial in the new method was the focus on organization and on finding a quantitative means of describing it." (P. 99)

[235] But if the choice of an equilibrium model logically precludes a revolutionary ethic, it is not demonstrable that it legislates any more positive social philosophy than a willingness to operate within the established social system... the equilibrium concept was scarcely a conscious acquisition of social theorists at all. The scientific advances sketched in Chapter 2 had made it a part of the conceptual apparatus of anyone who aspired to theorize scientifically. Marx, after all, had to confront the concept if only to demolish it.

The very diversity of ends to which equilibrium was put would seem to indicate the controlling influence of social philosophy on choice of models, rather than an innate congruity between equilibrium and one or another viewpoint. The fact that equilibrium was used in conjunction with both a laissez-faire and a reform sociology does not establish it as a value-free concept. But it does at least establish its capacity to encapsulate quite different versions of Utopia. Cynthia Eagle Russert "The Concept of Equilibrium in American Social Thought." 1966 Yale University Press. pp. 43-54 each other in own estimation. He was so skeptical of progress that he declared the very word to be “meaninglesss,” and he despised reformers and "intellectuals" who deluded themselves into thinking they could make social progress their life's work.

> As for what is harmful to the community, I cannot be explicit, but this I know, that I fear the "intellectuals," the sentimentalists, and the uplifters-- to me they are all one-- even as I do the politicians and the profiteers. If only, instead of heaping up evils, they could but neutralize each other, like alkali and acid![236]

Institutional Rationalization and the Managerial Revolution

According to Garland Allen "the metamorphosis of mechanistic into holistic materialism observed in biology from the 1920s onwards was paralleled by a similar metamorphosis in society at large" (Allen, 1975, p. 109). Between the 1880s and 1920s, a profound organizational and institutional shift took place in the field of American business. Starting with textiles and railroads, and extending into heavy industry, other manufacturing, and finance, the landscape of modestly sized entrepreneurial family companies increasingly became populated by large and multi-unit managerial corporations (Chandler, 1977). The proliferation of the large multi-unit corporation with an internally segmented labor force developed alongside what has come to be called “the managerial revolution” in the administrative practices of those institutions (Yates, 1989).

Firms grew larger partially by adopting the multidivisional form and by changing their work process (Edwards, 1979). At the same time, some of the physical sites of the labor process (i.e. factories and foundries) grew as well. Late Nineteenth-century steel plants like those at Lakawana

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[236] . Henderson, Lawrence J. "On the Social System." The University of Chicago Press.1970. p. 245 (originally in Aphorisms on the Advertising of Alkali's Harvard Business Review Autumn 1937. pp.17-23)

Iron and Steel employed 3,000 workers under one roof. By 1924, the Ford River Rouge plant employed 68,000, the largest single-site employer in the United States, and probably in the world at that time (Nelson, 1995).

Increased size, the multi-unit form, and decomposing the work process into sub-tasks were all strategies comprising a particular kind of institutional rationalization. Max Weber called it *zweckrationalität*: goal or ends-oriented rationality, a process of bringing means and ends into a tighter relationship. This rationality comprised practices such as increasing efficiency, getting more output from fewer or less inputs, and making things work more smoothly and predictably (Weber, 2009).[237] Rationalization was a set of approaches to solving problems, and not an inevitable outcome or a phenomenon affecting the whole of U.S. society at once. While some institutions underwent rationalization at this time, others did not (Fligstein, 1985).

As firms grew, they encountered diseconomies of scale that prevented further rationalization.. In particular, problems of communication and coordination between the different parts of a supply chain, and between the lower and upper levels of management became pressing problems (Beniger 1986, Fligstein 1990).[238] Litterer (1963) characterized one mode of solving these coordination problems within the firm as 'systematic management.'[239] Systematic management

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[237] See Jurgen Habermas's *Theory of Communicative Action* volume 1: *Reason and the Rationalization of Society* (Beacon Press, 1984) and Max Horkheimer's *The Eclipse of Reason* (Continuum, 1974) for a criticism of instrumental reason.

[238] But See Paul Edwards for a caution regarding the potential functionalism in this view. http://pne.people.si.umich.edu/PDF/infrastructure.pdf

[239] "Let us say that Systematic Management was that approach to management which attempted to build into the management structure certain operating processes which would assure coordinated effort in the achievement of organizational goals to previously established plans. As a result: (1) certain repetitive management activities were carried out through standardized managerial steps by special-manager personnel, thereby relieving line executives of this task; (2) the integration of perhaps widely scattered activities was brought about through formally designated interlocking responsibilities and the assured flow of precise information; (3) many repetitively occurring problems received pre-established solutions which simplified the range of the decision-making efforts lower-level managers had to carry out."

shares some features with scientific management (also known as Taylorism) in that both are rationalizing approaches to institutions within an organization. The main differences are found in the fact that while scientific management sought greater control of the work process by separating planning from execution and by decomposing labor practices into smaller and more routinized tasks, systematic management, on the other hand, was primarily aimed at rationalizing the *supervision* of the labor process, through developing standardized ways of performing managerial duties, but also by optimizing the recording, communication, storage, and retrieval of information within the firm (Braverman, 1977). The wider use of the vertical hanging file system, the typewriter, the mimeograph, and the internally circulated memorandum corresponds to big firms' solution to the problems of communication and control. Already existing methods of keeping financial records like cost accounting and financial accounting became more widely practiced. Firms such as DuPont and the Illinois Central Railroad adopted new forms of visual representation of organizational data like performance graphs and the Gantt Chart (Yates, 1989).

Gerson has characterized the style of rationalization occurring in the management of data and records under systematic management as "coordinative" rationalization (Gerson in Ackerman 2007, pp. 193-220). This kind of rationalization streamlines relationships between things by removing extraneous parts, or optimizes processes by selecting and supporting the best functioning parts of that process.

During the period of organizational and institutional rationalization of 1900-1920, the multi-unit corporate form spread to become the structurally dominant organizational form among U.S. business firms, and many other organizations outside of the business world adopted it as well. But why this should be is not intuitively obvious.

--Litterer, Joseph. *Systematic Management: Design for Organizational Recoupling in American Manufacturing Firms*. Business History Review 37 (Winter 1963: 369-391.)

During the early 20th century in the United States, organizations that had a tight means-ends linkage, like corporations that manufacture or transport commodities, or the banks that provide them with capital, provided an organizational template for other kinds of organizations that did not have such a tight linkage. Universities and philanthropic foundations are two examples of organizations that did not necessarily have a strong functional reason for adopting the form of the modern managerial corporation, but there is nevertheless good evidence that they did so regardless. By adopting the organizational forms of successful firms like those in the steel industry and the railroads, research universities and philanthropic foundations could stake a claim to the powerful normative appeals of efficiency and professionalism (DiMaggio and Powell, 1983). Mimicry of organizational form also implies the adoption of the techniques and ideas associated with those forms. In the case of the managerial revolution, American universities and philanthropic foundations took on the style of office work, record keeping, and communications that had come to characterize more "outcome-oriented" organizations.

Corporate Rationalization of Education and Philanthropy

In *Making America Corporat*e, Olivier Zunz points out that because large corporations grew to include so many areas of American life "[t]hus their influence entered American life through a variety of channels" (Zunz, 1992, p. 69). From 1900 to 1920, the managerial revolution in American corporations came to universities and foundations as well. Rockefeller's General Education Board and the Carnegie Foundation for the Advancement of Teaching pushed for the rationalization of the administration of colleges and universities, so that the use and management of foundation funds might be more effectively monitored. Without the accompanying new forms of financial accounting and management, foundation donors would have no way of evaluating the effectiveness (and efficiency) of their donations (Barrow, 1990).[240]

A substantial reorganization of the Rockefeller Foundations under the supervision of Raymond Fosdick began in 1928. The programs of the Laura Spelman Rockefeller Memorial Fund, the General Education Board, the International Education Board, and the Medical Education Board were transferred to newly created Divisions. In 1929 a Division of Social Sciences was created along with the Divisions of Natural Sciences, Medical Sciences, and Humanities. Edmund Ezra Day, former dean of the University of Michigan and future President of Cornell University, was made Director of the Division of Social Sciences. The Rockefeller reorganization reduced the power of the entrepreneurial "barons" who had hitherto had final control over their programs, and instituted a system of middle managers more compatible with contemporary managerial practices (Kohler, 1991).

Finally, University reforms increased the professionalization of academic training and specialization of academic research, which had the effect increasing the number of fields and sub-disciplines within each academic department. This in turn seemed to threaten the idea of the unity of knowledge upon which many administrators thought the institution of higher education depended. Harvard's president A. Lawrence Lowell expressed this concern in 1909: "We must construct a new solidarity to replace that which is gone. The task before us is to frame a system which, without sacrificing individual variation too much, or neglecting the pursuit of different scholarly interests, shall produce an intellectual and social cohesion." (Lowell, 1909. in Reuben,

[240] The reason philanthropic foundations moved into funding education in the first place was related to the reason that the foundations were formed in first place: There was a tax on income after 1913, philanthropic giving was tax-exempt for certain purposes, and education (but not political advocacy) was one of the categories of giving that were tax-exempt. See Olivier Zunz (2012) *Philanthropy in America: A History.* Princeton University Press.

1996. p 241). To this end Lowell introduced undergraduate concentration and distribution requirements, and it was on the basis of this hoped for “system” that Lowell recognized the value of the incipient Committee on Industrial Physiology.

The Formation of the Committee on Industrial Physiology

The CIP was formed out of the relationship between Elton Mayo and fellow Harvard Business School professor Lawrence J. Henderson. Mayo had been working at the Business School since 1926 and had acquired support from the Laura Spelman Rockefeller Memorial Fund directed towards the study of “Individual Industrial Efficiency.” Henderson had just moved from the Medical School to the Business School and was in the processes of re-orienting his research away from blood biochemistry and towards the study of human physiology under conditions of exertion or environmental stress. Mayo recalled the two of them becoming friends and allies soon after meeting, and having long talks at Henderson’s home about logic, the scientific method, and the irrationality of human behavior (Gillespie, 1993 p. 118).[241] Henderson and Mayo also discovered their common concern with the state of the modern world: they both believed that Western civilization was in danger of collapsing into chaos and despotism (Trahair, 1984 p.203).

The actual creation of the Committee was proposed in a grant application to the Rockefeller Foundation in 1929. Mayo and Henderson, along with Wallace Brett Donham, then the Dean of Harvard Business School, formed the core of the Committee. Donham would become an important organizational ally to the Committee and to the idea of the Committee being part of an endeavor called human biology. Also on the Committee at its founding were David Edsall, the

[241] Mayo to Donald K. David, Dean of Harvard Business School. August 1. 1962. Elton Mayo Papers. Carton 2, folder 2. Baker Library Historical Collections.

Dean of Harvard Medical School, and William Morton Wheeler, Professor of Biology and Director of Harvard's Bussey Institution. Wheeler was nominally chosen because of his expertise in biology, but he and Henderson were old friends, and Henderson credited Wheeler with introducing him to the sociological writings of Vilfredo Pareto.[242] David B. Dill, who ran the day-to-day functioning of the Fatigue Lab, often attended committee meetings as secretary. Although its membership changed over the ten years of its existence, these actors comprised its initial core.

Taking the Committee as a unit of analysis shows how the work of the Fatigue Laboratory and the work of the Hawthorne Studies were conceived of as components of the same project, a project that Mayo, Henderson, and also Wallace Donham sometimes described as human biology. The research work that constituted human biology was partially physiological and partly sociological. Most importantly, it was an attempt to formulate a new 'science of man' in the interwar period that could articulate diverse academic disciplines from anthropology, physiology, medicine, sociology, and business administration. The institutions of this research work -- the laboratory, the factory, and the classroom -- were successful because they were articulated according to certain kinds of rationalization that they shared with their important allies in university administrations, in philanthropic foundations, and in corporate management. This shared style of rationalization made these institutions legible to each other, and facilitated the formation of alliances between them.

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[242] Internal Rockefeller Foundation memorandum, April 7th, 1937. Folder 4069, box 342, series 200, R.G. 1.1, RAC>

1929: Applying Social Sciences to the Solution of Social Problems.

The grant proposal to the Rockefeller Foundation that made the CIP possible was an ambitious request for a almost $3 million of capitalization, and $94,500 a year, for ten years, for projects in the Medical School, Business School, and School of Public Health. Walter Cannon's work on physiology, Stanley Cobb's neurosurgery, and Charles Macfie Campbell's work at Boston Psychopathic Hospital were all mentioned as potential recipients of these monies.[243]

In an April 1929 letter to Edmund Day, Director of the Division of Social Science at Rockefeller, Dean of Harvard Medical School David Edsall described the growing complexities and interdependencies of the fields that the different schools and departments of Harvard study, as well as the increasing internal complexity of the schools and departments themselves. He stated that the work of the Business School was essential to the best development of the Medical School and the School of Public Health, and that the Engineering School and the School of Public Health were becoming more and more related to one another through their parallel work in the "psychological or psychiatric aspects" of problems of industry. In concluding his point Edsall suggested that

> I have looked upon the whole problem as one that cannot be put successfully under the Medical School, the School of Public Health, the Business School, the Engineering School, or any other part of the University alone, and, therefore, a matter that cannot well be operated under any on Faculty but could be much better handled by a separate committee with perhaps an individual acting as the coordinating factor, so as to ring them more closely into relation with the work that Dr. Henderson and his associates are doing is properly under an autonomous committee, reporting directly to the Corporation rather than to any Faculty. Were it that work alone, it would be more difficult to justify the continuance of such an autonomous committee, but even then it seems to

[243] Roughly 39 million and 1.25 million a year in 2012 dollars, respectively.

> be justified because it involves the Business School, the Medical School, and School of Public Health to have that work carried out effectively as it has been going on and will continue.[244]

Dean Edsall's part of the proposal for a third research group focusing on psychiatric and public health aspects of industrial problems, to be carried out at the Medical School and the School of Public Health, was not approved.[245] Raymond Fosdick's plan for a five-division structure within Rockefeller's programs had separated medicine out from the social and natural sciences (Kohler, 1993 p. 243). The physiology of fatigue, it seems, was sufficiently relevant to the social sciences for the Fatigue Lab to exist within that division, but Edsall's plans for medicine to be a part of this package was not.

The grant approval's project title page is *Harvard University-- Industrial Hazards. A Coordinated Program of Research in the Fields of Psychiatry, Physiology, and Industrial Hygiene*. While the Harvard group always called the project the Committee on Industrial *Physiology*, the grant from Rockefeller was for Research in Industrial *Hazards*, and referred to it as such in internal communications. Rockefeller was interested in translating the findings of the social sciences into solutions for social problems, in this case occupational health and safety, and industrial organization. The CIP, on the other hand (but especially Henderson) was always at pains to describe their work as scientific and basic research, which, while having important potential practical applications, was not applied research.

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[244] Edsall to Day, April 16, 1929. Folder 4069, box 342, series 200, R.G. 1.1, Rockefeller Foundation Archives, Rockefeller Archive Center, Sleepy Hollow, New York (hereafter designated RAC).

[245] Staff Conference records. April 4th, 1930, folder 4069, box 342, Series 200 R.G. 1.1. RAC.

Wallace Donham's perspective on the importance of the work proposed by the Committee emphasized the cooperation and coordination of work between Mayo and Henderson, and presented them as being linked through the common guiding principle of equilibrium, whether biological or social.

> The situation at the Business School is something as follows: Henderson and Mayo have been collaborating for two years in an effort to begin careful investigation of the empirical facts with respect to the human situation in industry. Henderson's study of organic equilibrium is well known and needs no description here. Mayo's study of situations in industry has had to include studies of changes in production, studies of mental attitudes in workers, and studies of social institutions. Two things have occurred recently which no one concerned in the work expected to happen within so short a period as two years. The first is that the principle of organic equilibrium enunciated by Henderson have been found to possess high value as directive of industrial investigations of changed organic equilibrium in the worker. This must not be understood to mean that the laboratory methods of the Henderson group can be directly applied in industrial situations, for this cannot be done without considerable further elaboration of method.[246]

This "elaboration of method" became the clinical sociology that developed in the course of the Hawthorne Studies and its popularization in the Human Relations Movement.

The Committee on Industrial Hazards was approved to be funded for seven years at $875,000 a year. The monies were allocated to Mayo's research group, Henderson's research group at the Fatigue Lab, and a smaller "fluid" fund for supporting other work that the Committee had the final say on identifying and approving. The fluid fund was used to support a range of projects across disciplines and departments at Harvard, though not the ones listed in the grant application, including W. Lloyd Warner's 10-year "Yankee City" study in Newburyport, Massachusetts, operated out of the Anthropology Department.

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[246] RF grant application packet. Comment by Wallace Donham. Folder 4081, box 342, series 200, R.G. 1.1. RAC.

The Committee had secured a great deal of support and the independence to use these funds in an autonomous manner. But change was afoot at the Rockefeller Foundation that would make the Committee's position less secure in the 1930s. Max Mason, then President of Rockefeller, together with Raymond Fosdick, had restructured the divisions of the foundation with an eye towards a more rational organization. They also strengthened the role of the foundation's case officers compared to the division heads, supporting the layer of middle managers in place of the earlier model of personal fiefdoms. This led to a cycle of communication between Rockefeller and the CIP, characterized by continuous re-framing and re-justifying the work of the Committee.

The two biggest initial funding earmarks of the CIP were Lawrence Henderson's Fatigue Laboratory at Harvard Business School, and Elton Mayo's extensive study of workers at Western Electric's Hawthorn Works in Cicero, Illinois. Their principle investigators emerged from different disciplines (social anthropology and psychology in the case of Mayo, biochemistry and physiology in Henderson's) but overlapped heavily in their approaches to their work. Furthermore, the work of their two groups referred to one another's findings and concepts, especially the idea of social and physiological phenomena as complex systems in dynamic equilibrium.

The Hawthorne Studies

In 1951, C. Wright Mills observed that the 'human relations in industry' movement had transcended the limitations of the earlier 'Taylorist' forms of scientific management. The "managerial demiurge" had to move beyond analyzing the body of workers as if they were machines, and to take into account the psychological and social factors that defined the relationships between workers, and between workers and management.[247] The Hawthorne Studies under Elton Mayo were the methodological origin point for the Human Relations Movement that emerged from Harvard after the end of World War II (Gillespie 1991, ch. 8).

From 1923, Elton Mayo's work on industrial relations, health, and crime in factories in Philadelphia had been funded by the Laura Spelman Rockefeller Memorial Fund through the support of Beardsley Ruml, the Fund's director of fellowships. Ruml's support came with the explicit interest and approval of John D. Rockefeller himself (Cruikshank 1987, p. 163). Mayo settled at the University of Pennsylvania's Wharton School of Business for a few years but by 1926 he had moved to Harvard Business School's Department of Industrial Management and entered into an alliance with Henderson, which supported both Mayo's industrial research and Henderson's physiological work at the Fatigue Laboratory (Trahair, 1984 pp. 171-180).

Mayo's role in the design, execution, and interpretation of the Hawthorne Studies is contentious. He did not initiate the studies; they had already begun when he was invited to visit the plant in 1928 by the works' assistant manager, George Pennock (Smith, 1987). Nor did Mayo write the monograph, *Management and the Worker*, which was Hawthorne's first and most widely known publication. He preferred to leave that task to his collaborators William Dickinson and Fritz Roethlisberger (Roethlisberger 1977). What is certain is that Mayo played an important role in

[247] "The new (social) scientific management begins precisely where Taylor left off or was incomplete; students of 'human relations in industry' have studied not lighting and clean toilets, but social cliques and good morale. For in so far as human factors are involved in efficient and untroubled production, the managerial demiurge must bring them under control. So, in factory and in office, the world to be managed increasingly includes the social setting, the human affairs, and the personality of man as worker" C. Wright Mill *White Collar: The American Middle Classes.* 1951 p. 223.

interpreting and popularizing the results of the studies, and that his leadership role at the CIP was key in securing the funding support the project needed to continue.[248]

At the enormous Hawthorne Works in Cicero, Illinois, Mayo's assistants and Western Electric employees set up a specially equipped relay assembly testing room (the "T Room") and performed a battery of experiments on workers.[249] They observed small groups of cable-relay assembly workers and measured their productivity under an array of different conditions and organizational forms. The workers in the relay assembly testing room were all young unmarried women and either immigrants themselves or from immigrant families. They were interviewed regarding their attitudes including their level of job satisfaction and their relationships with their fellow test subjects (Gillespie 1991. pp. 130-131).

The best-remembered finding of these experiments was the eponymous "Hawthorne Effect." In a series of experiments measuring worker response to varying levels of environmental lighting, worker productivity seemingly increased when small changes were made to their working environment, but not due to the change in lighting in itself. In fact, productivity increased whenever workers believed themselves to be under surveillance, and it was concluded that worker performance and satisfaction were more effectively dealt with at the level of unconscious and unspoken desires on the part of employees, rather than explicit complaints or demands. However, the Hawthorne Effect was the result of only a subset of the broader investigative goal of the studies, which was to determine the forms of small-group organization that most supported and promoted the smooth functioning of the work process. Mayo's group theorized unionization, wildcat strikes, low productivity, and high turnover to be partly physiological and partly social in

[248] For more on the Hawthorne Studies see Roethlisberger and Dickson (1939), Landsberger (1961), Franke (1979).

[249] In 2918 there were 22,000 workers on the 200 acre campus. Gillespie, 1991 pp. 12,15

origin, and their overall aim was to optimize the organization of small groups of workers so that the things that worked well were promoted and the things that did not work well were avoided. More general, this also captures the concerns of coordinative rationalization. Indeed, it was the commitment to the coordination problems of the workplace that made the research style of Mayo's team intelligible to its funding audience, first Ruml at the Laura Spelman Memorial Fund, and later with the CIP.[250]

Elton Mayo's research, and the style of work of his research group in the Hawthorne Studies, shared important assumptions and commitments with the Fatigue Lab. But, as I discuss below, they differed somewhat in their style of work.

The Fatigue Lab's style of research-

The experimental practices of the Fatigue Lab have been examined in depth elsewhere (Horvath and Horvath, 1973; Scheffler forthcoming). But they bear a brief re-elaboration in order to emphasize what defined the Lab's style of work, and how that style of work made it legible to its allies. Technicians at the Fatigue Lab drew blood from experimental subjects under conditions of work or strenuous exercise. Blood samples were reacted with reagents of known strength, and the resulting products were then measured with a manometer, gravimetrically, or, occasionally, colorimetrically[251] Subjects' exhaled breath was captured in large rubber "Douglas" bags and its

[250] For a bibliography of Hawthorne-related published material see Gilliespie 1991

[251] *Field Manual of Laboratory Methods for Biochemical Assessment of Metabolic and Nutritional Condition.* Harvard Fatigue Laboratory, Morgan Hall, Soldiers Field. 1945., *Syllabus of Methods Employed in the Fatigue Laboratory of Harvard Business School and the Medical Research Laboratory of the Massachusetts General Hospital.* Unpublished protocols compiled by Dill, B. D., Consolazio, William V., and Horvath, Steven M. (1941?)

gas composition were measured and compared against known standards. Fatigue Lab experimenters participated in the studies themselves, acting as controls or a baseline against which the other experimental data were measured. They measured the metabolic rates and capacities of men and women engaged in a variety of different activities, but their focus was on the exertion of hard work or athletic performance, and on exertion at high altitudes or extreme temperatures, and to this end they often went to field sites. Fatigue Lab personnel carried out studies of workers at the site of Boulder Dam (Talbot and Michelson, 1933; Talbot *et. al.* 1933), a steel mill in Youngstown, Ohio (Kennedy, 1935. Talbot *et. al.* 1937), and white and Black sharecroppers in the Mississippi Delta (e.g. Robinson *et. al.,* 1941, Robinson *et. al.* 1941). Additionally, in 1935 the Fatigue Lab mounted the International High Altitude Expedition to the Chilean Andes to study the effects of low pressure and low atmospheric oxygen content on local miners and on themselves (e.g. Dill, 1938, Keys *et. al.,* 1938*,* McFarland and Dill, 1938)[252] .

The style of work at the Fatigue Laboratory in their own facilities and in the field can be characterized by a commitment to decomposing physiological phenomena into their component parts, and the extensive use of standards and standardized protocols for measuring, interpreting, and comparing physiological data. One example of this was their use of the nomogram as an instrument for standardizing interpretation (Hankins, 1999). Further, the Fatigue Lab style explained human physiology as a process, conceptualized and modeled as a dynamic system in search of an equilibrium state.[253] The Fatigue Lab's commitment to decomposing complex

[252] For a bibliography of publications coming out of the Fatigue Lab see Horvath and Horvath, 1973.

[253] "Biological and social situations are better conceived as a balanced relation between a great number of variables, such that at no time can the total balanced relation be entirely disregarded."

Harvard University — Industrial Hazards, Brief Report by Elton Mayo. May 27th, 1936. Folder 4082, folder 343, series 200, R.G. 1.1, RAC.

systems into smaller sub-systems to be dealt with one at a time (but also in relation to the whole) was highly compatible with the way that many institutions of management, administration, and the work process were being rationalized at the same time in the United States. This shared set of commitments made the process of building viable alliances with other rationalizing institutions more likely to succeed. At the same time, the emphasis that the Fatigue Lab placed on standards, the proper calibration of instruments, norms, and protocols tracks one of the two important elements of systematic management. The standardization of management duties, and the development of definite regulations concerning health and safety follow the same kind of rationalization (i.e. standardizing) that the Fatigue Lab did in its work. This made the Lab's work legible as an institution to its allies, who saw in it a reflection of their own growing concern with the problem of standards and norms.[254]

The Fatigue Lab's approach to physiological problems meant that its ideas and its way of going about work were recognizable to Wallace Donham and Elton Mayo in the Business School. Donham had been instrumental in introducing a case-based method of education to the Business School, importing it from the Law School. Case-based pedagogy and case-based reasoning presuppose agreed-upon cases to use as standards of comparison (Isaac, 2012).[255]

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[254] "An analogous argument may explain why labs and lab science came to have such a prominent place in modern industrial corporations: the analytic categories and practices of lab science were congruent with the new managerial hierarchies and procedures of large-scale industrial capitalism, whereas those of the older shop culture were not. As well, scientists proved useful allies for modernizers striving to transform the traditional business firm into the modern managerial leviathan."

Kohler, Robert E. "*Lab History: Reflections"* Issis Vol. 99, No. 4 (December 2008) pp. 761-768.

[255] See Issacs (2012) for more on the use of the case system at Harvard and its importance for the development of Thomas Kuhn's concept of a paradigm.

Mayo's work offered an intelligible common grounding to its audiences at Harvard and Rockefeller on the basis of one kind of institutional rationalization within its audiences practices, namely the coordinative rationalization of the flow of information. Henderson's appealed to another: the standardization of the supervisory duties of administrators under systematic management. Both Henderson's and Mayo's groups' attention to social and physiological processes as equilibria-seeking systems is not obviously compatible with any particular kind of institutional rationalization, but there are significant overlaps and borrowings between the figure of equilibria in human biology and the equilibria of neo-classical economic models (Weintraub, 1991).

1933-1936: Education, Synthesizing Knowledge, and Coordinating Organizations.

Why should there have been a laboratory of experimental physiology in the basement of Harvard Business School? What did blood-gas manometers and treadmills have to do with training business leaders and managers?

Lawrence Henderson gave his answer with characteristic bluntness: young men who planned on being leaders in the field of business should not study law or theology, because they were artificial systems of thought that had no scientific basis. By comparison, medicine and engineering at least attempted to ground themselves in the natural sciences, and Henderson thought that there was an important connection between business and science that would benefit both. Henderson then positioned himself as the crucial point of contact between the world of business and the world of science.

> ...There is one development of science as yet but slight, yet a promising development which the student of business may, it seems to me, properly take interest in and promote, and that is the study of human biology.
> The industrial revolution or, if you please, putting it more generally, the economic and social changes of the last century and a half, have produced a new environment in which men live. They live a different life. The activities of their muscles, their postures and their mental processes, all are more or less modified, and nothing or next to nothing is known about these things....There, it seems to me, is one of the great possibilities for advance in understanding the problems of labor. There is an opportunity which it seems to me the business schools should seize...[256]

Wallace Donham echoed Henderson's sentiments in an article for the *Journal of Educational Sociology* in 1935. "In any period of rapid progress vast maladjustments are inevitable" and those maladjustments demanded government intervention in business affairs in order to restore equilibrium. Referring to the program at Harvard Business School, Donham (1935) claimed that:

> A man with such training should be able to make his contribution, either in industry or in government, to the intelligent solution of the many bewildering problems facing us today. In time, more stable industrial and governmental relationships should evolve if these men are trained in adequate numbers.

Having secured for itself a significant amount of money and the operational independence to do with that money what it pleased, the Committee almost immediately ran into trouble. A series of personnel changes at Harvard and Rockefeller at the height of the Great Depression threw the CIP into a fit of self-searching and self-promotion.

In 1933 President Lowell retired and was replaced by James Conant, formerly a professor of organic chemistry. The change of leadership precipitated a round of letters between Donham, Mayo, the Division of Social Sciences at Rockefeller, and President Conant as the Committee tried to catch the new president up on what the CIP was, what exactly it did, and why it had so

[256] Lawrence J. Henderson. *Business Education as Envisaged by the Scientist.* The Ronald Forum, November 1927. Lawrence Henderson Papers, Baker Library, Harvard Business School

much institutional independence. In 1936 Max Mason left his position as President of the Rockefeller Foundation and was replaced by Raymond Fosdick. Again the security and independence of the Committee seemed threatened and again the letters flew back and forth between Cambridge and New York explaining the scope of work of the Committee, defending the investments already made by the Foundation, and usually asking for the time period and amount of the grant to be increased.

A 1936 report of the work of the Committee sent to the Rockefeller Foundation included a copy the March issue of the Harvard Alumni Bulletin, which gave an overview of the whole scope of the Harvard Fatigue Lab's activities. The report repeatedly emphasized the coordinative role of the CIP. This may be because it was partially written for the audience of James Conant, the new President of Harvard.

> Since 1926 when the work here began, Harvard University has shown itself to be a unique institution in respect of the spontaneity and quality of the collaboration offered us. This collaboration has been thorough when opportunity offered and has ranged widely through the University. Since L. J. Henderson crossed the river to join us seven years ago... we have worked at various times in close cooperation with the Medical School, the School of Public Health, several hospitals, the Department of Anthropology, the division of Government and the School of Business Administration...
>
> The Rockefeller Foundation on its side had given us universal intellectual freedom. The President of Harvard has lately conceived the idea of "roving" professorships and research in areas where the conventional departments overlap or miss. We have been the first group of workers set free to do exactly this. If we had been in the position of competing for endowment with established divisions of the University, we should not have been able to show our present achievement.[257]

But what was sold to administrative and financial patrons under the label of increased and better organized co-ordination of diverse units in a big and multi-centered organization like Harvard

[257] Harvard Alumni Bulletin, March 10, 1936.

University then had to deliver, and this turned out to be very difficult to do. The Committee was saddled with the problem of having to try to tie the work of anthropologists, physicians, biologists, and social scientists together. In practice this was mostly unsuccessful, and it meant that little by little the CIP restricted its support to the groups working under Mayo and Henderson, and used the discretionary funds for supporting other groups less and less (Trahair, 2005, pp. 296-298).

Saving Democracy From Disequilibrium

The events of Great Depression and the New Deal running up to WWII threw basic assumptions about the proper relationship between workers and employers, and between government and citizens, into question. The stability of the whole world seemed to be in peril. The sociologist George Homans, who had been a member of the Harvard Pareto seminar and a Harvard Junior Fellow, remembered the ambient sense of crisis of this time as follows:

> These were the 1930s, the years of the Depression and the New Deal, of “hating Roosevelt” at home, of the Spanish Civil War and the rise of Stalin and Hitler abroad… Mayo was inclined to view all our troubles as manifestations of an underlying social disorganization. (Homans, 1984)

Despite being such staunch anti-New Dealers, the members of the CIP presented themselves as willing and eager to take on government contracts, and the Fatigue Lab sustained itself after Henderson's death with war work (Horvath and Horvath, 1973).

This was the final frame that the members of the Committee used to communicate the value of their work in the late 1930s: they positioned themselves as available experts for the United States government to solve pressing social problems (O'Connor, 1999). Henderson got the CIP to serve as part of a National Research Council study on labor and working conditions in industry, and

Elton Mayo reframed his goals for expert managers and administrators. Mayo tailored his message to his audience as he presented the role of the expert manager or administrator as the guarantor or protector of democracy itself, instead of, as he had been previously described it, as the protector of society from an excess of democracy.

In the 1920s, when Mayo was teaching at the University of Queensland in Australia, he had promoted himself as an expert in the control of industrial unrest. In his 1919 book *Democracy and Freedom* (Mayo 1919) and in a series of article for an Australian mining newspaper he offered psychology as a solution to what he called "a damaged industrial morale" in the workplace. By the 1940s Mayo characterized American democratic institutions as laudable. But in the 1920s Mayo was sharply skeptical of popular representative democracy, associating it with the rise of Labour parties in the United Kingdom and Australia. Referring to the tendency of party and machine politics to put mediocre men in positions of power, he wrote:

> These considerations show how far popular democracy, in its ideals and methods, has wandered from the path of progress….The decisions of "collective mediocrity" enthrone sentimental opportunism and passing opinion; they do not begin to express the social will.[258]

In 1924 Mayo brought his criticism of democracy to America at an informal conference for social scientists organized by Rockefeller's Beardsley Ruml in a paper titled *A New Way of Statecraft* (Trahair, 1984 p.190). By 1937, although Mayo was still promoting the idea of the expert as the intelligent manager of industry and administration, now the expert was an industrial sociologist instead of a psychologist, who could interpret and manage groups rather than individuals. But also significant here is that Mayo stopped offering expertise as an *alternative* to democracy and

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[258] Elton Mayo. "Civilization and Morale," *Industrial Australian and Mining Standard*, Jan-Feb 1922:67, pp. 16, 63, 111, 159-163, 253. Elton Mayo Papers, Baker Library, Harvard Business School.

its problems, and now argued that expertise could *save* democracy from its tendencies towards disequilibrium. In a January 1937 report from Harvard Business School's Industrial Research Department, Mayo wrote

> ...It is clear that at least in some respects the problems of psychiatry, of industrial unrest, of the birth of political "pressure" groups have common roots. We hope in the near future to be able to uncover certain of the conditions that determine a people to fascist control and exaggerated nationalism.[259]

And in a letter to Wallace Donham in April of 1937 he wrote

> Owing to the increased tempo of modern change, the control historically exercised by traditional routines, folkways, social sentiments is weakening. The individual, however educated and able, tends to suffer a personal or family isolation, tends to develop a neurotic sense of fear and insecurity. The social group, both internationally and within the nation, is also relatively isolated; it responds by developing a similar sense of fear, insecurity, and social hatred towards other groups. The democratic governments of the world, those who survive, have never based their understanding of government functions upon studies such as the those I have described... the exponents of government at present understand social change so little that their efforts to revive a sense of kinship are often attended by intensified hatred between groups -- and chaos. By this road Italy and Germany ran into Fascist dictatorship.[260]

1937-1939: Funding Drawdown

The expanded grant to the Committee expired in 1937, and despite a coordinated campaign by Mayo, Donham, and Henderson's allies and associates, the Rockefeller Foundation's Division of

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[259] Report of Industrial Research Department (in Mayo-Donham correspondences folder) Graduate School of Business Administration. Harvard University. January 19th, 1937. Elton May Papers, Baker Library, Harvard Business School

[260] April 1937. Mayo to Donham. Folder on the Committee on Industrial Physiology. Baker Library. HBS.

Social Sciences decided to begin drawing down their support for the Committee's work.[261] In any case the attention of CIP members also seems to have wandered. Mayo began to spend several months of every year in England with his wife and daughters giving lectures and attending conferences, and Henderson had withdrawn from the daily activities of the Fatigue Lab and focused on his role in the Society of Fellows, the Pareto Seminar, and teaching his course Sociology 23. Joel Isaac (2012) calls this inter-and-extra-departmental tissue of committees, groups, and clubs 'the interstitial academy,' and he argues that it, and Henderson within it, played a substantial role in the development of the human sciences at Harvard.

Lawrence Henderson died in 1942, and the Fatigue Lab disbanded shortly after the end of World War II.[262] Elton Mayo has been remembered by his disciples in the historiography of Harvard Business School as the founder of the Human Relations Movement. This speaks to Mayo's capacity for continually re-working his ideas as the situation seemed to call for, but it leaves out the vision of human biology as a shared project of social scientists and biologists. Of the original members of the Committee, at least one remained faithful to the project that the Committee had initiated. In 1945 Wallace Donham, retired since 1942, still framed his arguments about

[261] "It is recommended that further support on a diminishing scale be given to a novel set of investigations upon important aspects of social relations, industrial management, and individual behavior. Internal Rockefeller Foundation memorandum, April 7th, 1937. Folder 4069, box 342, series 200, R.G. 1.1, RAC>

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-- December 23rd, 1937 Meeting of the Committee on Industrial Physiology. Burwell, Donham, Hernderson, Mayo, Bock, and Dill (Secretary).

Internal Rockefeller Foundation memorandum, April 7th, 1937. Folder 4069, box 342, series 200, R.G. 1.1, RAC>

[262] The Fatigue Lab carried out temperature and altitude related physiological studies during the war, and its Director, David B. Dill, went to to work for the Army Chemical Research and Development Laboratory in Maryland, taking the Fatigue Labs package of instruments, tools, and concepts with him. (Fairish, Matthew "Creating Cold War Climates: The Laboratories of American Globalism" in *Environmental Histories of the Cold War*, McNeil, J.R. and Unger, Corina, *eds.* Cambridge, 2010. p.67)

management training at business schools in terms of human biology. In a letter to Paul H. Buck, then Harvard's Dean of Arts and Sciences he wrote:

> Some dozen years ago, Professor Mayo's Department of Industrial Research and the Fatigue Laboratory were established in the Business School. These two undertakings are correlated. They collaborated in the study of men in everyday life, thus contributing to what may be described as human biology. The guiding idea of this enterprise is that certain aspects of human biology bear the same relation to the work of the Business School that other departments of human biology bear to the work of the Medical School. The importance of this view has been appreciated by the Rockefeller Foundation, which has provided generous support for both departments, and the University has recognized the wide bearing of the subject by establishing a Committee on Industrial Physiology, under which both departments operate.
>
> The work of the Fatigue Laboratory is closely related to that of the Department of Clinical Medicine in the Medical School in that it attempts to determine for normal men some of the innumerable physiological and chemical factors, and the interactions between them, that make possible a clear conception of the individual human being as an integrated physiological mechanism.[263]

Elites and Equilibrium

The concerns of Henderson, Donham, and Mayo in the projects of the CIP were conditioned by their common need to find and cultivate allies who would enable them to carry out their work. But their ambitions for the CIP were larger and more far-reaching than just to exist. They wanted to have an effect on the trajectories of the commanding institutions of the United States through their influence on the training of America's future elites. Donham's institutional reforms at the Business School, Mayo's interpretation of sociology in industry, and Henderson's physiological-philosophical commitments approached the problem of producing the right kind of leaders in business and government as the key to solving the problems confronting the U.S.

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[263] February 14th, 1945. Wallace Donham to Paul H. Buck. Elton Mayo Papers. Baker Library Historical Collections Department. Carton 2, folder 2.

Hendersons' interpretation of Vilfredo Pareto's theory of the circulation of elites is important for understanding the CIP. Or, as Mayo characterized Pareto's views in 1933,

> It is in dealing with administration that civilizations have usually, though not always broken down Pareto… discusses the importance of high quality in the administrative group in relation to the maintenance of social equilibrium …leadership in any society vests in two types of *élites* -- the governmental and the nongovernmental, the latter including the direction of all industrial and economic activities. (Mayo 1933, p.166)

The form governing elites took varied depending on the time and place, but since it was the nature of established elites to become decadent and weak, they must be continually renewed and reinforced with new members from outside their ranks to keep the body of elites energetic and strong. "If anything occurs to interrupt this social mobility... then the failure to maintain a 'circulation of the elite' will find reflection in disturbances of social equilibrium" (Mayo, 1933 p. 167). And to bring the question back to the subject of labor relations, Mayo concluded, "better methods for the discovery of an administrative *élite*, better methods of maintaining working morale" (Mayo, 1933 p. 171). Or, as Lawrence Henderson put it in 1939: "When the physiology of conditioned reflexes shall have been applied to these problems, physiology may perhaps achieve a new kind of social utility."[264]

[264] Lawrence Henderson, 1939. Introduction to the manuscript entitled "What is Social Progress?" Henderson Papers Series III Box 4 Folder 34. Baker Library, Harvard Business School.

CHAPTER 5: THE LIFE OF EPISTEMIC THINGS: THE FACT OF FATIGUE AS DISEQUILIBRIUM IN THE HARVARD NEXUS

This chapter is about how the scientific object of fatigue was produced in the Fatigue Laboratory, how it interacted with a particular notion of what the role of 'fact' was at Harvard in the 1930s, and how this transformation of fatigue from a scientific object to a fact in the case study system came about. First I will explain what a fact was for the network Joel Isaac called the Harvard Paretans: a group of ambitious social scientists centered around Lawrence Henderson who worked to base social science at Harvard on a firm scientific and epistemological grounding. I will show how the role of the fact for the Paretans came to sociologist Talcott Parsons though the influence of Lawrence Henderson. Then I will explore the process by which the Harvard Fatigue Laboratory took the physiological phenomenon of fatigue and turned it into a fact. Finally, I will discuss the role that the fact of fatigue (understood as disequilibrium) played in the training of elite judgment at Harvard Business School.

The question of the role of different varieties of rationality will be examined throughout. The adoption of the case method across disciplines and departments at Harvard, Lawrence Henderson's course sociology 23, and the experimental phenomenotechnique of the Fatigue Lab and the Hawthorne experiments share certain "elective affinities" in their practical commitment, not to rationality as such, *but to the dynamic between rationality and irrationality*.[265] Instead of a purely instrumental rationality which produces irrational ends through rational means, the

265 See Weber (1922) on elective affinities, which was a term imported from chemistry, interestingly.

Harvard complex of scientific elitists worked with a notion of biological and social rationality that was rational at root but prone to irrational action without the judgment and expertise of human biologists.[266] This is what Dean of Harvard Business School Wallace Donham meant when he said that "...human biology in its largest aspect comes far nearer being a foundation for the study of business administration than is applied economics."[267] By this light human biology was the set of techniques and practices and reflections on problems that restores equilibrium to the human system. This was as true in the case of social discord in the factory, neighborhood, or polity as it was in the case of fatigue in the physiological organism.

The scientific object of fatigue became subsumed within a key notion for the Harvard Paretans: the fact. The fact was a re-purposable piece of knowledge about the world that could be presented as part of a case study for students and faculty at Harvard Business School and other parts of Harvard's interstitial academy. The notion of fact allowed the scientific object of fatigue-as-disequilibrium to articulate with the institution of the case study, leave the laboratory, and become mobile in the seminar rooms and study halls of Harvard. Thus the work of the Fatigue Lab becomes an integral, though subsidiary, piece of the larger institutional project that Isaac identified in the Harvard interstitial academy: to establish the social sciences at Harvard on a sound scientific footing.

The conclusions the Harvard Paretans drew from the Fatigue Lab's results were idiosyncratic. Fatigue was not caused by physiological exhaustion but rather the onset of a disequilibrium

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266 Horkheimer, Max *The Eclipse of Reason* (1947)

267 Donham, Wallace. December 27, 1937. "Memorandum to Dr. Henderson." Fatigue Laboratory reports 19xx-19xx. Baker Library Special Collections, Harvard Business School.

caused by non-physiological factors.[268] The role that fatigue-as-disequilibrium played in the Harvard interstitial academy was not so much its positive content, but rather the part it played in the proposition that fatigue could be explained on the basis of irrational human sentiment. Although social and economic forces were indeed based on the same physical and mathematical laws that underpinned the rest of the universe, social scientists should not expect society to follow regular law-like rules. Social action was not governed by rational principles, and therefore the science of society would not be the study of a rational system. The larger implications of this distinction were profound. In contrast to, for example, John Dewey's notion that democracy and science were based on nearly-identical institutions of rational deliberation, experimentation, and consensus, the Harvard Paretans emphasized their essential dissimilarity. Science and democracy were not the same thing, did not follow the same rules, and should not be confused with one another. Institutions in the form of culture and tradition could help regulate social action, but a rapidly changing society abandoned its traditions and thus was in danger of systemic disequilibrium and crisis.

Facts

Henderson's promotion of Pareto's work is often noted as (or accused of!) being a significant influence on that of Harvard Sociologist Talcott Parsons, but Parsons was by his own account lukewarm on the content of Henderson's thought. Parson's sociological synthesis did indeed included Pareto, as well as Durkheim, but his theory of social action was most heavily indebted to that of Max Weber.[269],[270] Henderson's actual influence on Parsons was in the form of two very

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268 (Scheffler, forthcoming; Ravinbach, Ansome. *The Human Motor*; Dill, David B. *(Industrial Fatigue*)

269. Haberman, Jurgen. *The Structure of Communicative Action*.

narrow methodological or meta-theoretical notions and the relationship between them: the *fact* and the *conceptual scheme*. Parsons noted this approvingly in the introduction to his reputation-making monograph *The Structure of Social Action* (1937): "Adapting Professor Henderson's definition, in this study a fact is to be understood to be an 'empirically verifiable statement about phenomena in terms of a conceptual scheme.'"[271] Though seemingly common-sense categories, the fact and the conceptual scheme play an important role in Parson's sociology. Additionally, the roles played by the notions of 'fact' and 'conceptual scheme' for Parsons provide insight into the articulation of fatigue as a scientific object in the Fatigue Lab, and fatigue as a fact that could move out of the lab and into the broader institutional networks of the Harvard nexus. Parsons' sociology is not my main interest here. I dwell on his first monograph at such length because his theory of social action relies on a specific notion of the relationship between facts and conceptual schemes, and because this relationship will make it clear how fatigue as a scientific object became a fact that could move out of the laboratory and into Harvard Business School.

Parsons began *Structure* with the famous phrase "Who now reads Spencer?" following it up with the proposition that the aforementioned Herbert Spencer might now be safely considered "dead", along with the whole "positivistic-utilitarian tradition." According to Parsons, beliefs about the social and natural world were changing: bigger was no longer considered to be always better, the future was not guaranteed to be better than the past, history was not necessarily progressive, and

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[270] Of course, Parson's version of Weber's thought is uniquely his own, owing partially to his role as Weber's greatest early popularizer and translator in the U.S. Parson's translation of excerpts from Weber' *Wirtschaft und Gesselschaft,* published as *The Theory of Social and Economic Organization* is still standard.

[271] Parsons, Talcott. *The Structure of Social Action*, 1937. Harvard University Press. p. 41

the preferences of individuals each seeking their own personal benefit would not always aggregate into the greatest good for the greatest number of people. Consequently, notions of "[l]inear evolution has been slipping, and cyclical theories have been appearing on the horizon." He concluded by declaring that the collapse of positivism and evolutionism was "[a] revolution of such magnitude in the prevailing empirical interpretations of human society… hardly to be found unless one goes back to about the sixteenth century."[272] But why might that be? "What is to account for it?" Parsons considered and then cautiously bracketed the explanation that evolutionary, progressive, and utilitarian ideas might be on the decline because of "an ideological reflection of certain basic social changes." Instead he proposed that the decline of the notion of progress were to be more completely explained by his "working hypothesis" that "…a considerable part has been played by an 'immanent' development within the body of social theory and knowledge of empirical *fact* itself'."

Having posed the decline of utilitarianism and evolutionism in theories of social action as "The Problem," Parsons laid out what he saw as the solution: a theory of "voluntaristic" social action grounded in a commitment to the functional irrationality of people in groups. The body of *Structure* was devoted to exploring first positivist and then idealist theories of social action. Durkheim, Weber, and Pareto received the most in-depth treatments, with a large cast of supporting characters from Alfred Marshal to Karl Marx. Marshall was important for Parsons, because

…economic theory and the question of its status involve a crucial set of problems in relation to the theory of action in general and to the positivistic system, especially its utilitarian variant… The question is as will be seen, the most important single link between utilitarian positivism and the later phase of the theory of action[273].

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272 Parsons, Talcott. *The Structure of Social Action*, 1937. Harvard University Press. p. 6

273 Parsons, Talcott. *The Structure of Social Action*, 1937. Harvard University Press. p. 13

The proper field for studying strictly rational social action was neoclassical economics, which was at that time displacing American institutionalism at Harvard. Sociology, by contrast, was best suited to studying the effect that values and beliefs had on group action. Values were not strictly rational in Weber's sense of *zweckrationalität*, the rationality that brings means and ends into harmony in the interest of efficiency. However, they did obey a deeper structural rationality: they could be evaluated on the basis of whether they were functional or not; that is, on the basis of whether or not *they worked.*

The key to understanding Parson's reconciliation between Durkheim's positivism and Weber's anti-positivism hinges on his reading of Pareto's theory as being scientific (rational) but sensitive to the irrationalities of human society. Pareto was the bridge between Durkheim and Weber that allowed Parsons to climb over Marx.[274] And the methodological relationship between conceptual scheme and fact, or as Parsons put it, between "[t]heory and empirical fact[275]" prepared Pareto to be that bridge between the positivistic and idealistic theories of action.

There were several consequences of this methodological relationship that have bearing on the life of the fact in the Harvard Nexus. First, naive empiricism was ruled out; facts did not just pile up and combine into generalizations that became theory. "It goes without saying that a theory to be sound must fit the facts but it does not follow that the facts alone… determine what a theory is to

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[274] And over George Herbert Mead's interactionist sociology as well— see Jurgen Habermas *The Theory of Communicative Action, volume 2*.

275 Parsons, Talcott. *The Structure of Social Action*, 1937. Harvard University Press. p. 6

be."[276] Instead, theory had its own power to shape what would constitute acceptable facts, and the theories of each particular field would have to have some kind of interrelated coherence, and "constitutes to a greater or lesser degree an integrated 'system."[277]

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The historiography of science is rich with examples of the production of scientific facts through the use of instruments in laboratories. The controversy between Robert Boyle and Thomas Hobbes on the existence of vacuum and the role of experiment in natural philosophy is one of the most famous.[278] But there was an important difference between Boyle's workshop and the Fatigue Laboratory, between the Royal Society and Harvard Business School.

One of the features of the production of facts in the Fatigue lab as compared to the demonstrations of 'matter of fact' in Royal Society was the relatively automatic and seamless way in which facts sprang from the basement of Morgan Hall. To see how, we need to make an excursion into the history of the modern fact. The concept of the matter of fact developed out the context of the English legal system in the late seventeenth and early eighteenth centuries and spread from the law into other fields. In the English system of common law, the determination of matters of fact were left in the hands of lay juries, while matters of law were handled by legal professionals. The upshot of this structure was that, in the case of a crime suspected to have occurred, juries were (and still are, in the Court of Common Please of the County of Philadelphia)

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276 *Ibid.*

277 *Parsons, Talcott. The Structure of Social Action, 1937. Harvard University Press. p. 6Ibid.*

278 Shapin and Schaffer. Leviathan and the Air Pump. Schaffer, Simon.

entrusted with determining whether a certain fact had or had not occurred: "The act, the fact, thus required proof[279]."

The life of the fact as a thing goes even deeper than that, and is coupled with the development of many of the key institutions of mercantile capitalism like double-entry book keeping. Mercantile writing, according to Mary Poovey, was attractive to early modern natural philosophers like Robert Boyle because it was accurate, and thus reliable, but also because it was *transparent* to the observer. This quality of transparency coincided with the Baconian virtue of the transparency of induction[280]. Both the method of induction and the notion of the modern fact privileged "things in themselves" as the basis of theory and the basis of knowledge. The question then is: what is the difference between the fact for early modern English political economy and natural philosophy and the fact as it lived, moved, and functioned in the Harvard Nexus in the 1930s? As Simon Schaffer put it, about facts in the time of Boyle and Hobbes, "The acceptance of a matter of fact on the basis of an experimental report involves conceding authority to the reporter and to the instruments used in the experiment[281]." He continued: "It is misleading to treat the authority of such experiments as self-evident…When experiments are interpreted as conveying unarguable lessons about the contents of Nature, this indicates that a controversy has already reached a stage of provisional closure[282]." Two hundred fifty years later, facts for the Fatigue Lab were seemingly less dependent on the good reputation of the individual observer as a witness and more

279 Shapiro, Barbara. 2003. *A Culture of Fact: England 1550-1720.* Cornell University Press. p. 10

280 Poovey, Mary. *A History of the Modern Fact: Problems of Knowledge in the Sciences of Wealth and Society*. 1998. University of Chicago Press.

281 Schaffer, Simon. 1989. "Glassworks: Newton's Prisms and the uses of experiment" in *The Uses of Experiment: Studies in the Natural Sciences.* p 67.

282 Ibid. p 68.

on the good institutional reputation of Harvard and its Business School. One of the features of the production of facts in the Fatigue lab as compared to the demonstrations of 'matter of fact' in the Royal Society was the relatively automatic and seamless way in which facts sprang from the basement of Morgan Hall. And as we shall see, in the Harvard Nexus, facts did not require proof. Facts, including the fact of fatigue-as-disequilibrium, *were proof*. But how did facts become proof? As we will see below, facts became proof through their role in the case system, but first they had to become facts in the first place, though the Fatigue Labs instruments and practices and concepts.

Phenomenotechnique

The Fatigue Lab instantiated a specific set of artificial conditions as part of an experimental apparatus, or phenomenotechnique. The Fatigue Lab phenomenotechnique produced and measured fatigue as a scientific object (also called an epistemic thing) as the end of a long ensemble that linked together environment, experimental subjects, practices, instruments, and representations of measurements. The findings of the Fatigue Lab indicate that fatigue as a scientific object should be viewed as disequilibirum: the result of a combination of factors which were complex, a-causal, and not reducible to the physiology of material exhaustion in the sense of the expenditure of total caloric or energetic reserve. The idea of equilibrium was not a preexisting concept. It developed along with the tools and techniques and notions that were used to bring it into clearer view. As Rheinberger put it in his essay on Bachelard,

> …application is built into the very meaning of concepts and into the rules of concept formation, because the technical is built into the experimental phenomena, and because, just the other way around and in a symmetrical fashion,

> the noumena are built into the instruments and take on an instrumental form that further serves to develop the whole phenomenotechnical machinery."[283]

Objects of scientific research (in this case fatigue) cannot be characterized apart from a consideration of the instrumentation and other technical apparatus that made them. However, Rheinberger also wants to preserve some analytic distinction between the phenomenon under investigation and the experimental practices that call them forth. He calls the first term "the research object, the scientific object, or the "epistemic thing."[284] This represents the phenomenon under investigation, and thus the desideratum of the research, the problem to be solved. Rheinberger calls the package of practices and instruments and ideas used to create the phenomenon, and through creation investigate it, the "experimental conditions" or "technical objects."

> It is through these technical conditions that the institutional context passes down to the bench work in terms of laical measuring facilities, supply of materials, laboratory animals, research traditions, and accumulated skills carried on by long-term technical personnel. In contrast to epistemic objects, these experimental conditions tend to be characteristically determined within the given standards of purity and precision. The experimental conditions "contain" the scientific objects in the double sense of this expression: they embed them, and through that very embracement they restrict and contain them.[285]

Through repeated use, a series of instruments and institutions come to entail one another more and more closely, and thus the instruments become imbued with the theoretical content of their institutions. The instruments and their practices become regularized, habitually linked, mutually

283 . Rheinberger, Hans-Jörg. Perspectives on Science, Volume 13, Number 3, Fall 2005. MIT Press p. 324
284 . He goes on "They are material entities or processes--physical structures, chemical reactions, biological functions-- that constitute the objects of inquiry." Rheinberger, Hans-Jörg. *Towards a History of Epistemic Things: Synthesizing Proteins in the Test Tube* p. 29
285 Rheinberger, Hans-Jörg. Towards a History of Epistemic Things: Synthesizing Proteins in the Test Tube p. 29

entrenched. The 'Fatigue Lab package' of treadmills, blood draws, manometers, and graphical representations of data, developed in tandem with ideas about fatigue as a physiological thing.

The historical trajectory of the Fatigue Lab's experimental ensemble developed fatigue as an epistemic thing in two different *epistemic streams* that ran in parallel. The first stream took the epistemic object of fatigue and uses it as its object of study. Exercise physiology and aviation physiology took the performance of the body under extremes of temperature, pressure, and exertion to be the ground on which further exploration would be built. This was further refined and took off after WWII in the Fatigue Lab's successor laboratories in the US and around the world.[286] The second stream took form in Harvard's interstitial academy among the human sciences. Rather than taking fatigue as an object whose attributes deserved further study, this stream took fatigue as a physiochemical process to be profoundly disconnected from the social and environmental contexts that produced it. (See Fatigue as Disequilibrium, below.) Fatigue was not posed as an object of study but as a *problem*. It asked not "what is fatigue?"-- what are its various chemical and biodynamic characteristics that are amenable to rational analysis. Instead this epistemic stream asked "why do people get fatigued?" and set out to determine the essentially emotional and sentimental and organizational factors that are the irrational causes of this essentially rational process.

The experimental ensemble of the Fatigue Lab was yoked to the package of practices and theories of the case system. Fatigue as a fact became a non-physiochemical phenomenon, defined as a disruption of bodily psycho-physiological equilibrium. The case system took fatigue-as-disrupted-equilibrium and ported it into the pedagogical reforms that used the case system at Harvard Business School, Harvard College, Sociology 23, and the Harvard Society of Fellows.

286. Johnson, Andrea. PhD dissertation, and forthcoming.

But then there is also the question of rationality. The point is that the physiological body is rational. It is a system that can be modeled at least in principle using Willard Gibbs' equations for describing the equilibria of heterogeneous systems. But the social system, and the human animal as an individual, was not rational. Their equilibria were easily and habitually out of balance. They needed an outside force in the form of an expert administrator or manager to reestablish their equilibrium. In traditional societies this function was fulfilled by tradition, culture, and custom: "residue," in Pareto's terms. But modern industrializing society had dissolved the old bonds of tradition and become based almost entirely on what Pareto called "derivations": superficial and changeable notions with no solidity. In the absence of traditional sentiments, leadership in the form of managers was indicated.[287]

After his retirement as dean of the Harvard Business School, Wallace Donham commented that the project of human biology, of which he considered the Fatigue Lab to be the central part, was "... to determine for normal men some of the innumerable physiological and chemical factors, and the interactions between them, that make possible a clear conception of the individual human being as an integrated physiological mechanism."[288] The project of human biology was distinct from medicine, social hygiene, or eugenics, because it conceived of its own work as being the study of the normal, and not the abnormal or pathological.[289] But a research program based on the study of normal men would have to first find some normal men, and this species did not exist in the wild. It had to be domesticated, trained, and kept. Generating experimental data on human subjects that could be credibly compared with one another was one part of this work, but the other

287. See the Chapter 4.
288. February 14th, 1945. Wallace Donham to Paul H. Buck. Elton Mayo Papers. Baker Library Historical Collections Department. Carton 2, folder 2.
289 Foucault, Michel. Anormaux. Canguilhem, Georges. *The Normal and the Pathological.*

piece was to generate data about normal humans that could credibly stand in for humans in general.

To be able to make the production of generalizable human subject data an accomplishable task, the staff of the Fatigue Lab had to do three things. First, they needed to trust that their instruments would give reproducible results which could be compared to other experimental runs, in the Fatigue Lab and in other laboratories of labor physiology. With this in mind they were always vigilant about calibrating their tools, and checking the calibration against available standards. Second, they had to be able to rely on standards of measurement from other fields and disciplines, or else bend those standards to their own use. Third, the Fatigue Lab had to arrive at a satisfactory way to standardize their subjects (normal men) or else at least develop a method for comparing humans in a "standardizable" way. But the work of standardization at the Fatigue Lab was always partial, conditional, and in constant need of maintenance, improvement, and re-working. The complexities and site-specificity of the human physiological phenomena under investigation resisted the efforts of Fatigue Lab practitioners to corral them. Messy biological life was always running over its banks and exceeding its own standards.[290]

But the practices of calibration, comparison, and setting standards did travel. They were the real “immutable mobiles,” much more than the inscriptions produced in the lab. They moved from the lab to the field, to army field camps, and back to the lab in the form of written protocols, word-of-mouth and hands-on instruction, and in the form of the instruments themselves. . The practices of the Fatigue Lab were standardizing, and they were also being standardized at the same time. And it was the successful standardization of practices, problems, perspectives, and commitments that mattered, not the successful standardization of subjects or results. These technologies provided

290. Canguilhem, Georges. Knowledge of Life.

the basis for the Fatigue Lab to be simultaneously the progenitor of exercise physiology, high-altitude physiology and aviation medicine, and environmental stress physiology after the end of the Second World War.[291] But the purpose that they served, and the problematic that they addressed in the interwar period was bound to none of those nascent fields. Instead, the technologies of subject standardization, and the technological phenomena they were a part of, became "facts" for the Harvard Paretans. As facts, they articulated with case system of pedagogy in the Business School, the administrative coordination project of the Committee on Industrial Physiology, and the efforts of the Harvard Paretans to ground their social sciences on a sound scientific basis.[292]

Standardizing Subjects

In order to establish an epistemic thing the Fatigue Lab workers had to measure against known standards, and when there was no previous standard is to have to base the first standard on something that was definitionally not a standard when they began. For example, the standard for plotting the dissociation curve oxygenated hemoglobin was based ton he blood of a single individual, the Fatigue Lab research associate Arlie Bock.[293] Collection of blood and urine required a docile subject population. In other Fatigue Laboratory studies, subjects were industrial or construction workers, students, amateur athletes, and sharecroppers. The field manual was compiled using experiments performed on conscripted soldiers, giving the practitioners a lot of

291. Andrea Johnson, PhD Dissertation. Andrea Johnson, forthcoming; Sarah Tracey date?
292. Isaac (2013)
293. This single-source standard turns out to have had consequences for that particular dissociation curve. Bock was a heavy smoker, and the standard dissociation curve plotted on the basis of his hemoglobin was found to be inconsistent with other measurements. For more on this see Astrup and Severinghaus. pp.189-190

control over the conduct of a disciplined population. Instructions were given on how to get the best results from urine collection.

> "The Subjects are roused at 4:30 A.M. And are lined up at a latrine just before 5:00 A.M. At command they empty their bladders into the latrine. They are then ordered to drink at least one-half pint of water to ensure diuresis [urination]. They are also ordered to urinate in the next hour and a half only at command. ... Just before 6:30 A.M. each subject is issued a paper cup with his identification on it. At the command all subjects urinate into their cups and are instructed to empty their bladders completely using more than one cup if necessary... subjects unable to urinate at command must be detained until they can urinate and the time for such men have to be noted particularly carefully"[294] [brackets mine]

Fatigue Lab subjects performed their work in diverse environments. Their performance as part of the emerging epistemic thing depended on the interaction between their physiology and the conditions of their environments. This combination gives us our first ensemble. Let us call it Experimental Ensemble I:

The Experimental Ensemble I: Environment--Subject

Instruments, or, a blood-gas manometer is not a series of tubes...

At the Fatigue Lab's main facilities in the basement of Morgan Hall, students, patients, researchers, and volunteers were put on treadmills and stationary bicycles and made to breath into carbon monoxide analyzers.[295] Horvath and Horvath's (1973) practitioner history of the Fatigue

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294. Ibid. p. 10

295. Folder 53, July 30, 1936. Fatigue Laboratory sends "1 Bicycle Ergometer" to Boston City Hospital. "For Dr. Wilkins" (Donation?) General Electric sent an undated letter informing Dr. Dill that some materials would be two to three weeks late in coming from GE's factory in Fort Wayne:
"1 motor generator set- two unit four bearing- 500 watts - 1725 rpm - 125 volt d.c. Type KC - 73 frame - 1 HP - 1725 rpm 110/220 volt - single phase, 60 cycle motor for $121.70"

"CR-8000 front of board, wall type field rheostat for $6.00"

Lab gives many examples of the environments that subjects of the Fatigue Lab were linked into.[296] Fatigue Lab researchers traveled to the extreme heat of Boulder Dam and an Ohio steel mill, the humid heat of a plantation on the Mississippi River Delta, and the thin high altitude atmosphere of the Chilean Andeas.[297]

The hallmark piece of instrumentation for the Fatigue Laboratory's work was the blood gas manometer, in Morgan Hall as well as in the field. The manometer was a delicate series of glass tubes, stopcocks, and collection vessels whose calibration, upkeep, and repair took up a considerable amount of the Fatigue Lab's work. The manometer also occupied a large portion of the Lab's commercial correspondence with suppliers, instrument manufacturers, and other labs. In the basement of Harvard Business School's Morgan Hall the Fatigue Lab employed a Warburg blood gas manometer, a modification of the earlier Haldane-Barcroft manometer.[298] While in the field they took along a portable version operating on the same principles. This smaller manometer was called the Van Slyke apparatus, after its originator Donald D. Van Slyke of the Rockefeller Institute in New York.[299]

January 14 1936, an order to the Mine Safety Appliance Co., of Pittsburgh Pennsylvania, for "1 M-S-A Carbon Monoxide Analyzer, precise type of non-recording, non-continuous style"

296 Horvath and Horvath. *The Harvard Fatigue Lab: Its History and Contributions.* 1973. Prentice Hall.

297 See Horvath and Horvath, but also, D.B. Dill's *Life Heat and Altitude* (1938), Sarah Tracey on the International High Altitude Expedition, and a paper on the physiology of Black sharecroppers in the FL in the Delta:
Thompson, J. W. "The Clinical Status of a Group of Negro Sharecroppers" The Journal of the American Medical Association. July 5, 1941, pp. 6-8

298. Named after John Scott Haldane (1860-1936) the British physiologist, not his son John Burdon Sanerson Haldane (1892-1964), the population geneticist. See Robert Bud's 1998 Instruments of Science: An Historical Encyclopedia for more on the blood gas manometer. Also Frederic L. Holmes of Yale for Warburg manometer.

299. The 1945 Field Manual (see below) refers to the portable blood gas manometer used in the field to measure the gas content of respiration collected by the Douglas bags as a "Haldane

Fatigue Laboratory personnel developed a literature of protocols, lists of necessary materials that were compiled and bound twice. The first manual was put together primarily by William Consolazio shortly after the Fatigue Lab set up shop in Morgan Hall in 1927, and it was published by Harvard University Press in 1940. The second manual was produced in 1945 on the basis of the Fatigue Lab's wartime contract work with the Office of Scientific Research and Development, though the Committee on Medical Research.[300] The Fatigue Lab's interwar experience with measuring human response to extreme conditions of temperature and pressure had made it a source of knowledge that was applied towards testing cold weather and high-altitude clothing, as well as determining the nutritional needs of soldiers in the field.[301] Both manuals contain protocols for measuring the levels of a variety of gasses dissolved in human venous and arterial blood, as well as urine. The Morgan Hall protocols, and those detailed in the appendix of L.J. Henderson's Siliman Lectures, relied on the methods outlined by Donald Van Slyke in 1924-1927. Van Slyke's method was to agitate blood samples under a partial vacuum and to measure the partial pressure of the dissolved gasses manometrically, by measuring the displacement of a mercury column in a graduated glass capillary.[302]

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apparatus," or a "Haldane gas analysis apparatus," which seems inaccurate. It is possible that "Haldane" was the working name for all gas-manometric equipment, no matter what variant: Haldane, Barcroft, Warburg, or Van Slyke.

300. As "The Laboratory Manual of Field Methods for Biochemical Assessment of Metabolic and Nutritional Condition" (1945)

301. See Folk, Edgar G. "The Harvard Fatigue Laboratory: contributions to World War II" Advances in Physiology Education. (Historical Perspective) September 2010 vol. 34 no 3. 119-127. See also Sarah Tracy on the Development of the K Ration

302. Van Slyke, D. D., and James M. Neil. "The Determination of Gases in Blood and Other Solutions by Vacuum Extraction and Manometric Measurement. I The Journal of Biological Chemistry. 1924. 61:523-573.
Van Slyke, D. D. "Note on a Portable Form of the Manometric Gas Apparatus, and on Certain Points in the Technique of its Use." The Journal of Biological Chemistry. 1924. 73:121-126.

The *Field Manual* of 1945 contained an extended set of protocols for assaying carbon dioxide, carbon monoxide, oxygen, and nitrogen levels by reacting samples of heparinized blood with reagents. The blood-reagent reaction produced a new chemical compound that was gaseous at that partial pressure. This compound would boil off into the gas phase and increase the pressure in the measuring chamber. For example, ferrocyanide reacted with dissolved blood oxygen to produce carbon dioxide bubbles, the partial pressure of which could be measured by the manometer.[303] Dissolved nitrogen reacted with acid phosphate and bicarbonate.[304] The manual of 1945 called for different reagents and combinations of reagents to measure different dissolved chemicals of interest. The assays could measure venous or arterial blood, and there were recommendations for 'arterializing' venous blood by drawing from the veins of hands that had been submerged in hot water.[305] The gas mixture in respiration (breath) could also be measured.[306,307]

The 1945 *Field Manual* described protocols for the fluorometric measurement of riboflavin, thiamin, and N-methylnicotinamide levels in urine. These vitamins and by-products glow under ultraviolet light, and in a dark room such as a tent, the fluorescence of the compounds in urine samples could be compared against that of known standard concentrations of the compound in

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Van Slyke, Donald D., and Julius Sendroy, Jr. "Carbon Dioxide Factors for the Manometric Blood Gas Apparatus." The Journal of Biological Chemistry. 1927. 73:127-144.

303. *Field Manuel of Laboratory Methods* p. 68
304. p. 76
305. *Field Manuel of Laboratory Methods* p. 66
306
. *Ibid.* p. 98

307. “Energy expenditures can be determined by calculation from respiratory data. The subject's expired air is collected by means of a system of valves and airtight bags. It is measured by a suitable gas-meter. Samples are analyzed for oxygen and carbon dioxide by means of the Haldane apparatus. From these various data the oxygen consumption per minute is calculated and from this figure, the energy expenditure.” *Ibid*. p. 93

question.[308] Blood glucose, non-protein nitrogen, inorganic phosphorus, and calcium levels in serum and urine were all assayed by colorimetry.[309] The chemical of interest was reacted with a reagent to produce a precipitate of a known color, which was then measured against a standard color sample and mapped to a line on a reference chart or nomogram (about which see more below.)

Along with the detailed and methodical explanations of experimental practice, the field manual also provided a set of tables, charts, and lists of standards. These were as valuable and necessary to the practical success of the assays described above as any other piece of instrumentation or protocol. Just like its experimental protocols, the manual also had standardizing protocols. For example, this one for making reagents:

> Standardization: If the quality of the chemical is good and weights are accurate, the "strong AgNO3" will be 0.1400 N and the "working standard" 0.0140 N. The strong KCNS will be 0.2000 N and the "working standard 0.02000 N. This ideal situation rarely occurs and the solutions must be standardized as follows:[310]

Without reliable standards, or reliably calibrated instruments, the data produced through experiment would be cut off from meaningful comparison with other experiments, other researchers, and other bodies of experimental knowledge. Calibration and the recourse to standards did not make the data from different sites or experiments the same, but they did render them equivalent, that is to say, comparable. Standardization and the use of reference tables also streamlined work by allowing a practitioner to 'cheat' slightly in assuming that a given sample

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308. *Ibid.* p. 40
309. *The blue 2,6-dichlorophenolindophenol in acid solution is decolorized by ascorbic acid. An aqueous solution of the dye standardized against known amounts of ascorbic acid can therefore be used to titrate samples of serum and urine.*-- Field Manual of Laboratory Methods p. 48

310. *Ibid.* p. 28

would behave in the expected way without having to check every detail themselves. As the field manual put it

> Routine calculations when reagents and pipettes are always the same are much simplified by the use of line charts, examples of which are given on the following page. The line chart for serum is constructed by marking off the vertical axis with a straight line drawn accurately between. These two points are obtained by subtracting all of the figures which are constant in the equation above and then calculating the serum value for titration figures of 0.200 and also 0.500 ml.[311]

Conversion factors for temperature, known rates of reaction of reagents, and previously compiled data on the metabolism of human subjects allowed for the construction of a metric of already accreted knowledge against which to compare the experimentally derived data obtained with the assays listed in the field manual. Without known standards, practitioners would have no reliably way of knowing how to contextualize their findings. What counted as normal, or as exceptional, had to be determined by comparison with already compiled standards. In this way, the field manual provided the instructions on how to produce data, but also instructions on how to interpret it correctly within the body of already achieved results.

The example of the Specific Gravity Method described below makes clear the precision of explanation of the assays listed in the 1945 Field Manual. It was a true 'handbook,' in the sense that Ludwig Fleck would have used the word; an accretion of practices, shared common sense, tacit knowledge, and 'knacks.'[312] In other words, the tricks of the trade, communicated in a manner fairly distinctive from that of either a paper published in a scholarly journal or a talk given in front of lay people, funders, or patrons. With this cautious and detailed step-by-step guide to work, the field manual actually told its reader how to carry out an experiment. The assay

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311. p. 29

312. See Fleck's The Genesis and Development of a Scientific Fact. 1979. University of Chicago Press.

protocols were direct, detailed, and came with cautionary warnings and time-saving tricks gleaned from trial-and-error and experience. Stock solutions of copper sulfate were prepared so that, at a given temperature listed on a prepared temperature chart, a stock solution of specific gravity 1.100 was obtained. From this stock, a range of solutions with specific gravities from 1.000 to 1.100 was diluted. The protein content of serum, and combined protein and hemoglobin content of drawn while blood was assayed by the assay.

> The concentration of protein in serum and hemoglobin in cells determines a large part of their specific gravities. The specific gravity of whole blood is accounted for by that of serum and cells. The present method estimates specific gravity by the behavior of drops of serum and whole blood in solutions of copper sulfate of various specific gravity. The serum protein and whole blood hemoglobin are then calculated, or more simply read off from a line chart.[313]

With instruments used in standardized and routinized ways on subjects in particular environments we arrive at out next ensemble, Experimental Ensemble II:

The Experimental Ensemble II: Environment--Subject—Instrumentation

But we are not done with instrumentation, because the instrumentation does not speak, it just indicates how much the chamber's pressure has changed from its baseline, or how close the color indicator compound resembles the referent standard. Trained skilled Fatigue Lab personnel had to consult the reading on the manometer's graduated mercury column, or hold a vial of fluid up to a colorimetric reference card. Then these Data Readings had to be entered into some medium, a notebook, a piece of scratch paper, a standard form with spaces for the expected information. This further elaboration brings us to Experimental Ensemble III.

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313. p. 18 (Field Manual?)

The Experimental Ensemble III: Environment--Subject--Instrumentation--Data Readings--

1st Halfway Hybrid: Fatigue as a System

Rheinberger's typology of "epistemic things" and the "technical apparatus" that produces them are idealizations of a more fine-grained set of links and mediations. There is another category between epistemic things and technical objects: the half-way hybrid. A hybrid is the preliminary results; the data before it has been fully contextualized; the graphs and charts that represent the outcomes of batteries of calibrations, observations, and experiments.

> Whether an object functions as an epistemic or a technical entity depends on the place or "node" it occupies in the experimental context… It organizes the laboratory space with its messy benches and specialized local precision services as well as the standard scientific text with its specialized sections on "materials and methods" (technical things), "results" (halfway-hybrids) and "discussion" (epistemic things.)[314]

There are two salient half-way hybrids in the journey of fatigue from phenomenon to fact. The first is the conceptualization of fatigue as an a-causal system of interrelated factors. The second is the graphic representation of that system in the form of the nomogram.

The publication of the book version of Henderson's 1928 Siliman Lectures, *Blood, A Study in General Physiology*, stands as a good mid-point for Lawrence Henderson's physiology and philosophy. He had largely abandoned his earlier interest in the fitness and adaptiveness of physical and chemical properties in favor of a nascent notion of physiology as system. During the 1930s Henderson came to regard his earlier philosophical speculations on the fitness of the

314. Rheinberger, Hans-Jörg. Towards a History of Epistemic Things: Synthesizing Proteins in the Test Tube p. 31 (references Greg Myers "Writing Biology:Texts in the Social Construction of Scientific Knowledge." Madison: University of Wisconsin Press.

environment as a mistake. His notions about teleology, he concluded, had been hopelessly muddled, and the truth-- if there was any such thing in physiology-- would be found in Gibbs's mathematical theory of equilibrium in heterogeneous systems. In the 1930s, Henderson became interested in sociology through the work of Vilfredo Pareto, the Italian engineer turned economist and sociologist. Henderson saw in social systems the same tendency to equilibrium that he saw in physiological systems, and while he was unable to formulate a social equivalent to Gibb's mathematical theory, he recognized a strong analogy between the methods of Gibbs and those of Pareto. He claimed that in, heterogeneous systems, whether physiochemical, social, or even historical, the search for cause-and-effect relationships was at best misleading and at worst meaningless, because every component of the system was affected by every other component and even by itself in a feedback loop.[315] He had developed a sophisticated method of visual representation of multivariate physiological data, but they fell short of the further integration of data of his later nomograms. *Blood* also contained an appendix on standard laboratory methods written by Fatigue Lab manager David B. Dill.

Henderson's plan for revitalizing Claude Bernard's general physiology (actually de-vitalizing it!) was to really make it general: that is, to make it abstract on the basis of generalizable standards. Willard Gibbs' equilibrium of heterogeneous solids provided the tools and model for him to subject Bernard's physiology to mathematical analysis.[316] Armed with Gibbs' statistical

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315. Hankins, Thomas L. Blood, Dirt, and Nomograms: A Particular History of Graphs. Isis, 1999, 90:50-80. p.80

316. But the twentieth century had begun before physiologists were ready to make use of the theories of Bernard and Gibbs. Even today, in spite of many scattered investigations and of the influence of the important treatise of Bayliss, the science is still in its infancy. Its significance is now widely recognized, but Claude Bernard's program has already grown old and has not been revised.--Henderson, L.J. Blood: A Study in General Physiology. 1928. Yale University Press. New Haven. p. 4

thermodynamic equilibria Henderson asked "How far and in what respects may the activities of all living things be regarded as identical?"[317] In order to answer this question Henderson posed the question of organismal physiology in terms of a system.[318] For physiology to be a system for Henderson meant that it was an ensemble of interacting elements whose patterns of interaction were generalizable to other systems or to systems in general. "Therefore" Henderson wrote in 1928 "we may begin with the following statement: Protoplasm is a physico-chemical system among the components of which are water and carbon dioxide, hydrochloric and phosphoric acids...and other substances."[319] For example, describing one set of dynamic interactions of mutual dependence that occur between salts, acids, and bases in aqueous solution have the effect of maintaining a constant acidity in that solution, he elaborated:

> Indeed the study of such system, to which the name buffers is commonly applied, has considerably extended physiological knowledge. It was such investigations which first made possible the quantifiable description of a physico-chemical equilibrium in protoplasm...

Physiology as a specifically physico-chemical system links its micro-level phenomena like buffer chemistry to the more complicated meso-scale action of physiology proper (locomotion, digestion, nervous excitation, respiration.) Henderson ascribed comparable behavior to many

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317. Henderson, L.J. Blood: A Study in General Physiology. 1928. Yale University Press. New Haven.   p. 5

318. While the term "system" has been around along time in philosophy, its entry into the natural sciences is probably of more recent vintage. See Foucault, Michel. The Order of Things. (System vs. Method) See also Hegel's System der Wissenschaft. But see Susan Buck-Morss Hegel,Haiti, and Universal History for the proposition that Hegel based his understanding of a world-system on his reading of Adam Smith's division of labor, specifically his description of the pin factory. The concept of system as a bounded set of objects with relationships between one another comes out of thermodynamics. Sadi Carnot used a notion of system to describe the movement of heat throughout an engine in the 19th century

319. Ibid. p. 6

systems, including protoplasm, and argued that it could be attributed to "...the high degree of connection (or low degree of independence) between its components."[320] Tightly coordinated systems share many characteristics and do so at many levels. Equilibria at the molecular level in buffer resemble equilibria at higher physiological levels, and this "does but emphasize the importance and the generality of the principle, which describes one of the striking aspects of all organisms."[321]But the conceptualization of physiology as system also allowed Henderson to join his project to other fields and disciplines concerned with problems that could be similarly formalized: economics and sociology.

Philip Mirowski and E. Roy Weintraub have both demonstrated intellectual and institutional links between the physicist Willard Gibbs, the mathematician Edwin Bidwell Wilson, and the larger field of mathematized economics in the early 20th century.[322] Weintraub even suggests that Henderson exerted some influence on the thinking of a young Paul Samuelson through his leadership role in Harvard's Society of Fellows, but it is also quite clear that Henderson modeled his own investigations in physiology on neoclassical economics, particularly his conceptualization of systems and equilibria.[323]

Tightly coordinated systems with many parts were challenging to describe mathematically, and so Henderson looked to others who had studied similarly complicated systems. He was especially

320. Henderson, L.J. Blood: A Study in General Physiology. 1928. Yale University Press. New Haven. p. 11
321. Ibid.
322. Gibbs taught at Yale; Wilson was his closest student there, and was an ally of Henderson at Harvard.
Mirowski, Philip. More Heat Than Light:Economics as Social Physics, Physics as Nature's Economics. Historical Perspectives on Modern Economics. Cambridge University Press 1989.

Weintraub, E. Roy. Stabilizing Dynamics: Constructing Economic Knowledge. Cambridge University Press. 1991.

323. See below for more on the Harvard Society of Fellows.

enthusiastic about Leon Walras' mathematical economics, and that of Walras' colleague at the University of Lausanne, Vilfredo Pareto. The Lausanne economists, contemporary with Williams Stanley Jevons in England, and Carl Menger in Austria, conceived of the economy as a system tending towards equilibrium, and they described market behavior through of a series of simultaneous differential equations. As discussed above, Henderson set upon the theory of general equilibrium in economics as a promising avenue through which to characterize the complex relationalities of animal physiology. Henderson know the method had limits, of course

> The large number of components of protoplasm is a condition of the first importance in determining the nature of the system. In physical chemistry and other physical sciences it is customary, as above suggested, to deal with but a small number of variables…The mathematical treatment of a large number of variables usually presents very serious difficulties, as the history of the dynamical problem of three bodies implies.[324]

Henderson concluded that causality in the sense of two inelastic bodies colliding into one another was impossible to ascribe to the more multi-causal and dynamic systems in physiology, economics, or sociology. He approvingly cited his colleague Alfred North Whitehead's *Science and the Modern World* to draw into question the use of the notion of causality at all, given recent developments in physics and philosophy.[325] In the place of mechanical causality on the one hand and the invocation of some kind of *telos* or entelechy on the other, Henderson offered up his recommendation: "What is needed is mathematical analysis of the facts, unencumbered by the prejudices of either vitalists or mechanists."[326] The means to overcoming mechanism without falling into vitalism or teleology was the notion of system, grounded in abstract mathematical

324. Henderson, L.J. Blood: A Study in General Physiology. 1928. Yale University Press. New Haven. p. 10

325. Whitehead Alfred North. Science and the Modern World. New York, 1925. Whitehead's philosophy was apparently capacious enough to include Henderson, Conrad Waddington, Donna Haraway , and Isabelle Stengers without apparent discomfort.

326. Henderson, L.J. Blood: A Study in General Physiology. 1928. Yale University Press. New Haven. p. 18

models and representations. System connected Henderson's physiology to the high prestige of the physical sciences, and gave him the right to speak on social and economic affairs as well. In the conclusion to *Blood*, Henderson proposed that life as a system was the only useful answer to physiology's oldest question: "what is life?"

> In the beginning we were confronted with Claude Bernard's question: What is "the elementary conditions of the phenomenon of life?" At the end of this long study a provisional answer may be given... The elementary condition of the phenomenon of life is a particular kind of physico-chemical system.[327]

2nd Halfway Hybrid: The Nomogram

But in adopting the tools of Gibbs, Henderson encountered the same problems in physiology that the Lausanne School wrestled with in economics. First, a long series of simultaneous differential equations was nearly impossible to solve, and thus a "general equilibrium" was more useful as a concept to guide thought and research than as a notion with practical applications for the system as a whole. Second, the picture that emerges of a complex phenomena modeled in this way is actually a static one, a series of snapshots or tableaux of different relationships. Different aggregate states could be characterized, but change over time had to be assumed to be an instantaneous transitions from one equilibrium state to another.[328] Henderson was acutely aware of this as a methodological problem, and he cautioned that the notion of system in his work should be understood as a tentative working solution to a difficult problem.

> Meanwhile it should be noted that we have encountered one more characteristic of protoplasm: As a physico-chemical system it is never in equilibrium, but only, at best, in a stationary state like the candle flame or whirlpool. This is, however,

327. Henderson, L.J. Blood: A Study in General Physiology. 1928. Yale University Press. New Haven. p. 373
328. Mirowski, Philip. More Heat Than Light. Donzelli, Franco RISEC Volume 53 (2006) Number 2: 491-530.

> not a fact of the first importance in many investigations, for in some respects, as an approximation, it is both convenient and sufficient to assume the existence of a state of equilibrium, which does indeed exist in some of the intrinsic processes.[329]

Faced with these difficulties, Henderson moved from a series of empirically-derived graphs representing the relationship between different physiological variables (oxygen and exertion, carbon dioxide and oxygen, nitrogen, Oxygen and Ph) to a system of representation which captured the multiple relationships between many different variables simultaneously: the nomogram.[330] Reflecting on his journey to the notion of physiology as a system, his engagement with Gibbs, and his growing disenchantment with the notion of simple causality in phenomena with many interacting variables Henderson praised the nomogram.

> In systems where the number of variables is large and the degree of their mutual dependence high, the application of mathematics, though difficult, is of peculiar interest. In fact, as mathematical economists like Walras, Fisher, and Pareto have seen, it is is even more important in such circumstances than in the simple phenomena studied by physical science... Under these conditions the nomographic method, particularly in the form developed by D'Ocagne, has proved itself an indispensable aid to the physiologist.[331] [underline mine]

An early kind of comparative graph that would influence the development of the nomogram was developed by the French revolutionary government as a way to quickly convert local weights and

329. Henderson, L.J. Blood: A Study in General Physiology. 1928. Yale University Press. New Haven. p. 13

330. "L.J. Henderson, a Harvard physiologist and the first president of the History of Science Society, attempted to analyze mammalian blood solely as a physical-chemical substance. He found that the only way he could describe a chemical system as complicated as blood was by a diagram called a "nomogram." This lecture tells the history of Henderson's nomogram and of nomograms in general. It describes the origins of the graphs in the eighteenth century, their development in nineteenth-century engineering practice, and their importance in the twentieth for describing physical and chemical systems". --Hankins, Thomas L. Blood, Dirt, and Nomograms: A Particular History of Graphs. Isis, 1999, 90:50-80. p.50

331. Henderson, L.J. Blood: A Study in General Physiology. 1928. Yale University Press. New Haven. p. 12

measures to the new general Parisian standards.[332] 19th century French civil engineers extended the use of nomograms for estimating the relationships between the grade and curve of railroad excavations and the volume of dirt necessary to excavate to achieve those angles. But Hankins (1999) has it that Henderson hit on the practice of superimposing his graphs one on top of the other in the course of the research which resulted in *Blood*. By this account, Henderson developed the technique independently, and showed it to E.B. Wilson at MIT, who told him it was called a nomogram and had been developed by the french civil engineer Marurice d'Ocagne. Henderson met with d'Ocagne in 1921-1922 and there learned how to use the serial superimposition of his graphs as a quick reference tool for calculation, similar to a slide rule.[333]

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332. "In 1795 the National Convention in Paris decreed that in order to speed the introduction of the metric system, the Committee on Public Instruction should prepare graphical scales for conversion from the old units to the new ones. These scales would allow the citizens of France to estimate the ratios between the old and new units without making calculations." -- Hankins, Thomas L. Blood, Dirt, and Nomograms: A Particular History of Graphs. Isis, 1999, 90:50-80. p.56

333 "Henderson's diagram in his Blood lectures was a new use of nomograms. He adopted it because he could find no other way to represent all the interactions between the components of a system so complex... Henderson found that blood had at least seven major components and many minor ones, all interacting with one another, and he could find no way of describing all these interactions in a single system... Henderson approached this problem by graphing each of his seven variables against two others in what he called "contour line charts"-- that is, he placed one variable on each of the x and y axes and described the third variable by a family of curves filling the space of the graph... Describing all possible relations required 105 such charts, all calculated from his data and printed in his monograph on blood. But it was, of course, impossible to keep all 105 charts in one's head at the same time. It then occurred to him that if he superimposed several of these charts on the same graph he could express the interdependence of more than three variables.. The result was a mess, but an interesting mess. Henderson had seen many graphs, but none like this one, so he took it to E. B. Wilson at MIT and asked him what kind of graph it was. Wilson, who had been a student of Gibbs and had published his lectures on vector analysis, knew about graphs. He said it was a "nomogram," a kind of graph that had been invented in 1891 by the French civil engineer Maurice d'Ocagne. In 1921-1922 Henderson was in Europe and went to see d'Ocagne at the École des Ponts et Chausseés in Paris. D'Ocagne was especially excited by Henderson's work because it opened up a vast new area for the use of nomograms and because it showed that nomograms could be used to find and express laws governing extremely complex systems. D'Ocagne showed Henderson how to convert his messy overlapping contour charts into a point alignment chart; it is this chart that appeared in Blood as the famous figure" 41.-- Hankins, Thomas L. Blood, Dirt, and Nomograms: A Particular History of Graphs. Isis, 1999, 90:50-80. pp. 74-75

One needed only to have the value of one variable in a system of interrelated phenomena in order to estimate the values for all the others. This allowed for rapid calculation of many values for a physiological system that would otherwise take weeks of experiments to derive:

> The nomogram has several great advantages. First, it allows for great economy of expression: the diagram is much less cluttered than a graph with Cartesian coordinates. Second, the same nomogram can express all the parameters of a formula and can handle many more variables...Whenever speed is more important than precision nomograms have an advantage.[334]

Data readings entered into notebooks end up at points on a graph or other visual representation of quantitative data. This is Experimental Ensemble IV. Overlaying graphs one on top of the other yields a further refinement: the nomogram, which is the terminus of Experimental Ensemble V.

The Experimental Ensemble IV: Environment--Subject--Instrumentation--Data Readings--Halfway Hybrid (graph)

The Experimental/Representational Ensemble V: Environment-- Subject--Instrumentation--Data Readings-Graph-Superimposition of Halfway Hybrids (Nomogram)

Fatigue as Disequilibrium

334. Hankins, Thomas L. Blood, Dirt, and Nomograms: A Particular History of Graphs. Isis, 1999, 90:50-80. p.71

The Experimental Ensemble VI: Environment--Subject--Instrumentation--Data Readings--superimposition of Halfway Hybrids (Nomogram)--Scientific Object (Fatigue)

Although Henderson was the Director of the Fatigue Lab, the man who really managed its day-to-day research work was David B. Dill. He was the principle technical and administrative presence at the Fatigue Lab from its move to Morgan Hall in 1927 until 1941, when he was commissioned into the Army.[335] Dill was much closer to the experimental apparatus of the Lab, and far less inclined than Henderson to philosophizing about the relationship between science, system, and society. So it is instructive that Dill's extensive practical experience with fatigue through phenomenotechnique led him to conclude that, as physiological concept, *fatigue did not exist*. Or, rather, it did not exist in the form of tiredness, boredom, or slowdown in the workplace.

> The physiologist does not deny the applicability of the word fatigue to monotonous industrial work although modern usage seems to favor the word boredom. The question has been discussed by Mayo (3) who points out that the words are used interchangeably. At any rate some investigators see in a decreasing output an indication of fatigue (4) while others use the same yardstick for measuring boredom. It is safe to deny, however, that boredom describes the breakdown of a finger as it is exercised with an ergograph. The one phenomenon is a problem for the physiologist, since it involves well-known physical and chemical changes in muscle. The two phenomena are similar only in superficial respects; fundamentally they are very different[336]

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335. Dill and went to work for the Aeromedical Laboratory at Wright Field in Dayton Ohio in 1941, the Army Quartermaster Corps in 1943, and then the US Army Chemical Research and Development Laboratory from 1947-1961. He retired in 1961, conducted research in environmental physiology at the University of Nevada, and died in 1986. (See Falk, G. Edgar 2010)
336. Dill, D. B., A. V. Bock, H. T. Edwards and P. H. Kennedy. "Industrial Fatigue" Journal of Industrial Hygiene and Toxicology. 18:417 (1936) p. 417

Instead Dill proposed that workplace fatigue be considered "as a failure to maintain equilibrium or, to use Cannon's terminology, a breakdown of homeostatic forces."[337] Not only was fatigue not reducible to the total expenditure of a working person's muscular energy or respiratory capacity, it was not reducible to anything in particular. Dill's findings at the Fatigue Lab convinced him that an adequate explanation for fatigue would have to be *a-causal*, the result of the alignment and coincidence of several independent factors.

> The attempt to relate fatigue to simple and specific causes such as accumulation of fatigue substances, depletion of fuel or inadequacy of oxygen supply have been only partially successful... <u>We must now accept the less concrete theory proposed by Henderson that a large number of interrelated factors are concerned in the control of respiration.</u> Under some conditions one factor may be predominant, under different conditions others come forward.[338] (underline mine)

Dill's a-causal and sentimental fatigue corresponded to Henderson's a-causal notion of system in physiology and sociology. The half-way hybrid of the nomogram was admirably suited to depicting this a-causal system in all its irreducible complexity. This notion of fatigue also relied on there being a disjuncture between the rational operations of the physiology of the body, and the irrational workings of sentimental human beings in groups.

The phenomenotechnique of the Fatigue Lab produced physiological fatigue as a scientific object. By this point the Experimental Ensemble I have sketched in the last section is no longer solely experimental, but are also representational, referential, and now discursive. Results were reviewed, polished, written up, and organized for submission to a journal, conference, or committee report. The discussion section of a journal article contains the

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337. Dill, D. B., A. V. Bock, H. T. Edwards and P. H. Kennedy. "Industrial Fatigue" Journal of Industrial Hygiene and Toxicology. 18:417 (1936) p. 417
338. Dill, D. B., A. V. Bock, H. T. Edwards and P. H. Kennedy. "Industrial Fatigue" Journal of Industrial Hygiene and Toxicology. 18:417 (1936) p. 425

next link on the chain of the Experimental Ensemble: the scientific object, *fatigue.* In the process fatigue was defined as an a-causal disequilibruum of the physiological system, the result of many interdependent factors that could not be analyzed in isolation from each other and from the whole, rather than as absolute muscular or respiratory exhaustion.

The Case of the Epistemic Thing

So, then, what were facts for? They were for putting into cases. The case method was developed for use in Harvard's Law School in the last two decades of the ninetieth century by the then Dean, Christopher Columbus Langell.[339] Harvard's Medical School adopted the case method of teaching "around 1900... where its use was designed to professionalize clinical training."[340] Dean Donham's adaptation of the case method to the conditions of business education took the form of the "problem book," a series of written hypothetical descriptions of situations in a specific area of business administration.[341]

Donham hoped that the case method at HBS would accomplish two things, one in the content of its curriculum, and the other in the composition of its teachers. First, it was meant to seperate of facts from judgment, in contradistinction to the legal use of the case system. Donham hoped that

339. The case method of teaching was introduced into professional education in the Law School of (*sic*.) Charles William Elliot. It's champion was Eliot's handpicked dean, Christopher Columbus Langdell, who found in the case method a solution to a set of challenges surrounding the professionalization of the study of law. Some of these challenges centered on academic standards in American law schools... Langdell's turn to the case method was part of a wider movement after the Civil War to rationalize the canons of legal knowledge and the training of lawyers. As would later hold for Henderson's scientific methodology, matters of pedagogy were not side issues for Langdell but central to the problem of knowledge.
340. Isaacs. pp. 75-79
341. Such as "Marketing, Factory Management, Employement Management, etc." Roethlisberger, Fritz. *A Delicate Experiment* p. 135

this would in turn develop student's respect for facts in making decisions. He anticipated that developing student's ability to make judgment-based decisions would become the basis of policy in business and government. Counter-intuitively, the case system emphasized a respect for facts as part of a decision-making method rather than for their particular factual *content* in a given case. It was the method of using facts to make decisions that Donham worked to enshrine.

Second, Donham's also used the case system to push for the interchangeability of instructors, a goal which was thought to be "wholly impossible under a lecture system."[342] Interchangeability was of course resisted by the faculty, but Donham was able to hire new faculty during the rapid (1919-1923) expansion of Harvard Business School, and by 1923 "two-thirds of the School's courses were taught by the case system."[343] The case method therefore constituted a common commitment to standards of evaluation of evidence and styles of reasoning across the schools of Business, Law, and Medicine. Lawrence Henderson's training in the Medical School and Donham's at the Law School provided them with a pre-developed tacit level of understanding of how problems should be attacked, and what counted as an acceptable answer to them. Donham considered facts, articulated through cases, to be the link between knowledge and policy.

> It is my judgment based on experience that any specialized social science used as the basis for policy determination to the exclusion of facts which lie outside the abstraction of the particular sciences are dangerous and unsound as the basis of policy determination and action. It is my judgment, based on experience, that the social sciences can be coordinated effectively at the point of action and when so coordinated are effective aids to policy determination.[344]

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342. Ibid. p. 139 See also either Roethlisberger or Homans on interchangeability

343. Ibid. p. 140

344. Donham, Wallace. December 27, 1937. "Memorandum to Dr. Henderson." Fatigue Laboratory reports 19xx-19xx. Baker Library Special Collections, Harvard Business School.

Henderson mobilized his other key epistemological concept, that of the *conceptual scheme*, to relate the facts as they were packaged in cases to the notion of system as the proper conceptual scheme to interpret those cases.

> It was in this context that Henderson came to embrace the notion of the case as the key to his epistemological concerns. For Henderson, the handling of cases encapsulated the techniques of the skilled research scientist and provided a model for training historians and neophytes in an understanding of the research program. This perspective combined elements of scientific philosophy with the pedagogical vision outlined by [Christopher Columbus] Langdell. But it also drew on classical understandings of the case dating back to the casebooks of Hyppocrates. In his history of science course, Henderson celebrated the insights provided by the Hippocractic case, and his other offerings in the subject involved selected examples of good and bad scientific reasoning from the ancients down to the writings of Mach. Henderson's unification of inquiry, epistemology, and pedagogy proved especially potent in the interstitial academy.[345]

Here for the sake of space the ideogram of my ensemble will be shortened. Phenomenotechnique and halfway-hybrids are now black-boxed. Fatigue as an epistemic thing will soon be black-boxed as well. At the end of long and highly mediated chain of techniques and representations, fatigue is about to be ported into another strange link on the chain, that of *fact.* Facts, in turn, become the building blocks for an institution for pedagogy, the case study. Finally, we have achieved the link between laboratory, physiology, organizational reform, and reactionary anti-rationalist politics. Joel Isaac's picture of the 'Harvard Nexus' is again indispensable for making this connection: "Henderson's social systems theory offered an interpretive framework in which Harvard's marginal human scientists could fashion cases and thereby attempt to gain an institutional recognition for their research projects.[346]"

345. Working Knowledge p. 85
346. Working Knowledge p. 87-88

Trained Judgement

Foucault's studies of the human sciences marked a distinction between two kinds of knowledge/power in those domains: the disciplines of the body (anatomico-politics) and the regulation of the population (biopolitics.) In *Society Must be Defended* he wrote that the techniques of discipline (drilling, training, barracking, imprisoning, surveillance) were quite old, dating back to the 17th century in France, and that the practices of regulation of the population were introduced at the end of the 18th. But he also noted that both disciplinary and regulatory techniques had to be continually adjusted as conditions demanded. "...It is as though power...found itself unable to govern the economic and political body of a society that was undergoing both a demographic explosion and industrialization."[347] He mentioned "medical institutions, welfare funds, [and] insurance..."[348] as sites of adjustment for these series during the 19th century. Since the two series, technologies of the body and technologies of the population, operate on different levels, they can be made to work with one another and articulated so that they mutually reinforce each others' effects. I have shown how the human sciences in United States in the early 20th Century faced new versions or variations on those conditions, and found new adjustments of regulatory and disciplinary technologies in reply. George Homans related Henderson's shift away from physiology and towards social theory to these turbulent times, and to the anxiety they provoked when he the Director of the Fatigue Laboratory's state of mind at this time:

> Another large element in his [Henderson's] transfer of interest [to social theory] was doubtless the impact of the social malaise of these unsettled years. "There is

347. Foucault, Michel. *Society Must be Defended* p. 249
348. Ibid. p. 250

> not one of us today," asserted the two protégées who were virtually Henderson's spokesmen, "who look on our human society without bewilderment.[349]

The shocks and challenges to the liberal order in the interwar period in the US appeared as demographic problems and labor problems. I have already written about Raymond Pearl's human biology as an answer to demographic changes. This chapter demonstrates that human biology at Harvard was being oriented towards addressing the challenges constituted by the changing labor force and labor process.

One aspect of the institutional changes introduced by the New Deal and World War II was a move towards increased impersonal and objective rationalization based on calculability, efficiency, and optima.[350] This strand of rationalization has been a powerful one. But it has been accompanied, twinned and on occasion countered, by a strand of elite managerialism which refused to relinquish its hold on institutional power on the basis of the argument that objectivity was no substitute for judgment. In his discussion of objectivity, standards, and statistics, Ted Porter wrote that

> The resistance of elites to having their judgment forced into the procrustean bed of calculation is at the heart of the problem… It is not by accident that the authority of numbers is linked to a particular form of government, representative democracy. Calculation is one of the most convincing ways by which a democracy can reach on effective decision in cases of potential controversy, while simultaneously avoiding coercion and minimizing the disorderly effects of vigorous public involvement.[351]

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349. Cynthia Eagle Russert "The Concept of Equilibrium in American Social Thought." 1966 Yale University Press. p. 114. (references Homans and Curtis, Introduction to Pareto. p. 3)

350. *How Reason Almost Lost It's Mind* (2013) Erickson *et al*. University of Chicago Press.

351. Porter, Theodore M. Objectivity as Standardization: The Rhetoric of Impersonality in Measurement, Statistics, and Cost-Benefit Analysis. Annals of Scholarship. 1992 p. 19-59

I have already noted above that early 20th century American science was reconsidering how-causes and why-causes (see “Scientific Anti-Democrats”). At the same time that questions of morphology gave way to ones of system there was a simultaneous shift in the epistemic virtues associated with scientific practice. Daston and Galison (2007) have shown that in the first decades of the twentieth century astronomers, physiologists, and crystallographers began to mistrust the mechanically produced images of natural phenomena made possible by advances in instrumentation. In the place of a “mechanical objectivity” produced by mechanical means, they developed a notion of an interpretive objectivity of the images and data produced by those same machines and instruments. “...they were after an interpreted image that became, at the very least, a necessary addition to the perceived inadequacy of the mechanical one[352]”

> The real emerged from the exercise of trained judgment… Manipulated to build on the natural, but structured to bring out specific features by means of expert understanding, the twentieth-century image embodies professional experience; it is pictorial presentation by (and for) the trained eye[353].

This is important for an understanding of the role that the fact played in the case system, and in turn for the role that the case system played in the training of students at Harvard Business School. Joel Isaac is right to argue that the Harvard Paretans were more concerned with establishing themselves as a viable social scientific community based on sound and scientific epistemology than on any abstract political issue, but he underemphasizes the fact that, by providing a basis for the rational scientific study of irrational social phenomena, the interstitial

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352.*Objectivity* (2007) p. 311

353. *Objectivity* (2007) p. 355

academy contributed to the institutional legitimacy of what C. Wright Mills would call "the Power Elite" in the post-WWII America.[354]

Erikson *et.al*. (2013) have sketched the outlines of a distinctive style of Cold War rationality characterized by rule-following above judgment and which defined as rational that which was produced by a rational process.[355] The distinctive combination of stripped-down formalism, economic calculation, optimization, analogical reasoning from experimental microcosms, and towering ambitions that characterized Cold War rationality bore the stamp of an equally distinctive moment in the history of the American sciences.[356] S.M. Amadae has shown that the technology of rational choice theory was an important part of the stabilization and maintenance of liberalism as a political and economic settlement during the Cold War. Rational choice took the utility-maximizing rational actor as its subject, political and economic choice as its problematic, and a self-regulating society as its desideratum.[357] But Henderson represents another, overlooked mutation of rationality in the post-War era.

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354. "During the New Deal the corporate chieftains joined the political directorate; as of World War II they have come to dominate it... In part at least this has resulted from one simple historical fact, pivotal for the years since 1939: the focus of elite attention has been shifted from domestic problems, centered in the 'thirties around slump, to international problems, centered in the 'forties and 'fifties around war." -- Mills, C. Wright. *The Power Elite*. (1956) Oxford University Press. p. 275

355. Erikson, Paul, Judy L. Klein, Lorraine Daston, Rebecca Lemov, Thomas Sturm, Michael D. Gordin. How Reason Almost Lost It's Mind: The Strange Career of Cold War Rationality. (2013) University of Chicago Press.

356. Erikson, Paul, Judy L. Klein, Lorraine Daston, Rebecca Lemov, Thomas Sturm, Michael D. Gordin. How Reason Almost Lost It's Mind: The Strange Career of Cold War Rationality. (2013) University of Chicago Press. p.5

357. Amadae, S.M. (2003) Rationalizing Capitalist Democracy: The Cold War Origins of Rational Choice Liberalism. The University of Chicago Press.

Henderson's sociological writings were not influential in disciplinary sociology compared to the influence they had on other American social sciences. His book manuscript for *On the Social System* was turned down by academic and commercial publishers until 1970, when it came out accompanied by an introduction by its editor Bernard Barber (1918-2006). Barber attended Henderson's Sociology 23 lectures as an undergraduate at Harvard. He was Professor of Sociology at Barnard College from 1953. His books include *Science and the Social Order* (1953). Barnard observed of his sociological training at Harvard that "Henderson's ideas were especially influential because they often confirmed or supplemented basic ideas I was learning, at the time I took his course, from some of my other undergraduate sociology teachers-- particularly Parsons, but also Pitirim A. Sorokin and Robert K. Merton."[358] Barber observed that by second half of the 20th century, irrationality, emotions, and the importance of values had been acknowledged to be importance factors in human behavior and social structure.

> But Henderson was the child of a positivistic, rationalistic age, when both the social sciences and much common sense held that reason was predominant in human affairs and that the emotions and sentiments were mere relics of an earlier stage of human history. This positivism and excessive rationality were especially strong during the early and middle years of Henderson's life... Henderson passionately opposed the positivistic view of human behavior. He fought against this "error" endlessly in both his teaching and his writing.[359]

This criticism of positivism and “excessive” rationality further reinforces Henderson's affinity for the writings of Pareto.[360] The Italian's economic writings were strictly and mechanically rational,

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358. Henderson, Lawrence J. "On The Social System: Selected Writings, edited and with an introduction by Bernard Barber. The University of Chicgao Press. 1970. p. vii
359. Henderson *On The Social System* p. 25
360. Note that Henderson’s adaptation of Pareto was usually described by his contemporaries as anti-positivist. Confusingly, though, Henderson was influenced by Ernst Mach’s phenomenalism, and his approach to epistemology was seemingly compatible with the empiricism of the logical positivists in the Vienna Circle. The confusion actually lies in the term positivism, rather than in Henderson’s philosophy. For Henderson really did oppose positivism— the utilitarian positivism of Herbert Spencer, while at the same time upholding the phenomenological positivism of Mach and Karl Pearson.

but his social theories explicitly ruled out rationality as a factor in politics, society, and culture. Pareto's residues (deeply and unconsciously held values and "sentiments") and derivations ("beliefs functioning as ideologies",[361] rationalizations and justifications of behavior) provided Henderson with the terminology he needed to develop a rational system of irrational social action.

The conclusions Henderson drew from Pareto's writings were distinct from those that would become Cold War rationality as outlined by Erickson *et. al.*: first, anti-utilitarianism, both as an explanation of social action, and as a norm of evaluation of social action. Second, elitism. The great majority of people labored under useful misconceptions, which, though false, should not be challenged. Rulers should know the truth, but should not try to convince the ruled. Rather, they should rule in the interest of stability. For the governed, disorder is more dangerous than superstition or ignorance. Indeed, too much education can itself be harmful for the governed. Third, social change is almost always harmful because it is disruptive. Social systems can adapt to change by gradually changing their sentiments to accommodate new circumstances, but too much change or too fast a change causes anomie, dysfunction, and social ill health.

Here is one of the key differences between Henderson's approach to questions of complexity and equilibrium in biological and social systems. His contemporary biologist Raymond Pearl regarded individuals as actors having characteristics (age, mortality rate, fecundity rate) that interacted with one another on the basis of density-dependent effects. No higher organizing power was necessary for populations or economies to come to a steady state. For Henderson, by contrast, the social system consisted of more or less well-functioning interpretive schemas that were non-logical (and non-mechanical) in their operations. Rather than operating best when operating

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[361]361. Henderson *On The Social System* p. 25

automatically in the sense suggested by Pearl, Henderson's social systems could be, and should be, fine-tuned and manipulated in order to achieve the desired outcomes. A social system could benefit from an expert's knowledge, so long as the expert did not expect the system to do something it was not capable of. For example, an expert could use knowledge of the function of sentiments on the social system to encourage stability, smooth functioning, and lessen the bad effects of rapid change.

George Homans' is as good a voice to end with here as any, not just for having begun with it. His memoirs recall the conclusions that Lawrence Henderson and Elton Mayo both came to regarding what was wrong with the country in the 1930s, and what would be necessary to fix it. It was not a deepening of democratic institutions and values. Rather,

> These were the 1930s, the years of the Depression and the New Deal, of "hating Roosevelt" at home, of the Spanish Civil War and the rise of Stalin and Hitler abroad... [Elton] Mayo was inclined to view all our troubles as manifestations of an underlying social disorganization, as "the acquisitiveness of a sick society"-- his transposition of the title of Tawney's book. He once said of Hitler "He is restoring by drill the cohesion of a broken people." No doubt Hitler was doing just that: it is still an inadequate view of what he ultimately intended. To be fair to Mayo, I must recognize that he offered the remark before Hitler had made it clear he was bent on war and holocaust. Mayo made it before we had begun to take *Mein Kampf* seriously.[362]

The key point to re-emphasize here is that Henderson, Mayo, Donham, and their fellow members of the 'Harvard nexus' were not forerunners of "Cold War rationality." They believed that the natural universe of physics and chemistry was amenable to rational laws but that people and societies were not. This is what separates them from the scientific democrats, and this is what makes them seem strange to post-war calculative rationality.

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362. Homans, George. *Coming to My Senses* 1984 p. 149

The scientific elitists believed that their pedagogy would train experts who could substitute their judgment for the unreliable sentiments of employees and citizens, and the blindness of impersonal processes.

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