Carroll Quigley on *The Matrix of Civilizations: A Dialectic*

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the conditions under which divorces could be granted. In spite of this clarification it is clear that even in Geneva the Protestant Divines were unable to completely control human sexuality. Compromises usually had to be found in cases of adultery, desertion or fraud; but they were generally tilted in favor of men. As strange as it may seem, there are many in the United States who would like to return to the "good old days" when divorce was exceptionally difficult to have. Little has been learned from the lessons in Genevan history.

Raymond J. Lewis

Carroll Quigley on The Matrix of Civilizations: A Dialectic

1. The Problem for this Review

Within the ISCSC, quite a few members regard Carroll Quigley's *The Evolution of Civilizations* (Macmillan, 1961) as a significant contribution to civilizational systems' theory, comparable with Spengler's, Toynbee's, Sorokin's, and Kroeber's. As an iconoclast and some sort of strange duck (a physical scientist-engineer — incidentally of a birth time comparable to Quigley's — who wandered in to the society's first organizing session at a AAAS meeting), I suppose, I was asked by Matthew Melko to review Quigley's Chapter 6, "The Matrix of Early Civilizations," with my idiosyncrasies in mind.

Out of respect for what I have gained from this society, in particular from that platinum-golden-brassy trio of Hord, Hewes, and Wilkinson, all led by the insatiable enthusiastic conductorial drive of Melko, I wanted to capture some heavenly mass for the occasion. I have thrown away two sketches for Missae Solemnes, and instead settled for an unending fugue with variations. It will be one of my Trumpeter Swan Songs. I intend to disagree with Quigley on almost every point, and to offer a dialectical alternative reading, with a full set of alternative references, from a physicalist view. So I start out by enunciating Quigley's themes.
II. Quigley in brief

1. Quigley offers analytic methodological tools, not the facts of history.

2. His tools do not include physical laws, since he believes that physical laws are idealizations, in fact very approximate.

3. His tools do not include the methods of the natural sciences, which he believes do not apply to the social sciences; rather, he believes that his techniques and social science perspective can guide natural scientists.

4. Quigley's methodology is elementary: gather evidence, make a hypothesis and a theory, test the theory. No fundamental principles are required.

5. The human organism's persona/personality depends largely on nurture. Nothing relevant to human behavioral activity can be learned from comparative biological study.

6. Human societal phenomena are affected by human thought processes.

7. The potential range of human development is so large, so variable, that its analytical breakdown into conceptual classes and phenomena is arbitrary and imaginary.

8. Quigley elects 6 levels for his analysis of human society (military, political, religious, economic, social, intellectual); but he judges that he could have selected 5, 50, or 600.

9. With humans we can state no laws comparable to those of physical science. Individual behavior is unpredictable, though there is some success in predicting the behavior of group aggregates.

10. Understanding the rules of social tendencies depends on having a special theory/view [perhaps Kroeber's superorganic?] of the social aggregate, arrived at through millennia of debate.

11. Quigley elects a "sufficient" consensus as a method of resolving the debate, and settles on a typology of social groups, societies, and civilizations.

12. First, there are parasitic societies and producing societies. Hunters-fishers-gatherers scrounge to survive. Later, by agricultural and pastoral activities, they increase the wealth of the world.

13. Humans were parasitic for one million years; only in the past 10,000 years did men become possible producers as well as largely parasitic.
14. There are simple and complex producing societies. The complex ones are civilizations. Tentatively defined, a civilization is a producing society with writing and cities. Quigley identifies 16 civilizations.

15. Civilizations have startups, pass through a long life experience, and go out of existence.

16. Each civilization has an instrument of expansion. Expansion involves incentive to invent, accumulate surplus, surplus used for new inventions: in toto, these constitute an instrument of expansion.

17. Civilizations experience seven stages of dynamic change, resulting from the fact that each civilization has an instrument of expansion that becomes an institution.

18. There are 5 dimensions, 4 space-time dimensions and a 5th dimension of abstraction.

19. The 7 stages of dynamic evolution in civilizations free-wheel through the 5 dimensions without laws of process and interaction.

20. The 3 space dimensions of a civilization comprise the geographic environment.

21. Quigley applies broadly the concept of "matrix," as he believes, in a mathematical sense. "Matrix," for Quigley, has to mean the ancillary sources, as in a womb-like workshop, that brings or holds a system (e.g., a civilization) together.

22. The matrix of otherwise unconstrained variables making up human personality emerges from mysterious drives; there is effectively an elan vital.

23. Society also has a matrix, a cultural matrix that is the collective totality of personalities.

24. Aside from the internal personalities, culture is networked relations, artifacts, and communicational symbols (language).

25. The social matrix also includes material, energy, and all the bondings; it is a complex mixture.

26. Quigley's abstraction of this he diagrams as society = humans + culture.

27. Civilization also has a matrix, a complex from which the early civilizations emerge.

28. Why do particular patterns emerge on the matrix of early
civilizations? Quigley says he needs an earlier, deeper explanation in a prehistory of peoples.

29. Every civilization begins with a mixture of cultures.

30. Quigley elects climatology for the drive in his dynamic chronology.

31. He first presents Neanderthals and states that they were stressed by glacial conditions, without sufficient mental flexibility to continue.

32. He then transfers his climatological storytelling to 3000-2000 BC, where he traces wave-like motions mixing peoples.

33. The events he selects lie in the Northwest Quadrant of the Old World, where, driven by a chronology of climate changes, they form the matrix of the earliest civilizations evolved. (Then he looks at civilizations in detail from the Mesopotamian onward.)

III. A Dialectic with Quigley's Arguments.

In his preface, Quigley states that he is not trying to write the facts of history. Rather, he wishes to outline the analytic tools that will assist in understanding history. In Chapter 1, he states that when he studied science, particularly the physical sciences, he found that its laws were idealizations, very approximate, rather than rigid, exact, and invariable (Boas, by the way, makes a similar claim). Since the 'laws' of natural science were only very approximately true, whereas social scientists are reluctant to accept any rule unless it had very few exceptions, the methods of the natural sciences were simply not applicable to the social sciences (which implies that social systems were/are not natural systems). The social sciences are different from the natural sciences; both areas can use scientific methods, but the laws must be considered to result in idealized theories, based on observations; and particular derivations have to be explained by other, unconsidered, outside factors. The laws he will mention apply quite well and are as worthy of being used as scientific laws for the formation of crystals and the crystalline solid state.

At the book's end (Conclusion), he asserts that the techniques he has developed for dealing with human social history, particularly civilizations, can be used to deal with the present or future, for example how some currently functioning country can be
depicted. Such (social science) problems of a real world cannot be solved by the use of the natural sciences alone. The direction of attack and coordination of scientific activities on world problems requires guidance and supervision by persons with a wider perspective than that provided by natural science specialization. Such perspective can best be found in the study of the past. "With such perspective the techniques ... can be used to guide natural scientists and other workers in dealing with the problems of the present or the future."

My position, which leads to my counterposition in a dialectic with Quigley, is the following. By 1960, I had 20 years of science-engineering experience in one of America's great technical institutions — the National Bureau of Standards. By that time, 1960-1965, I had begun to develop the full content of a general systems' science, one that meant to be fully applicable to what we called later on the study of "nature, life, humankind, mind, and society." That was on my philosophic agenda from a very tender adolescent age in 1935, and 'merely' awaited 20-25 years of growth and education to begin to understand its scope. My career, from college days and beyond, was as an applied physicist in instrumentation, and what became systems science and engineering for both government and industry in the form of doing and directing R and D. By 1960, I had already laid all sorts of foundations for systems problem solving in fluid mechanics, hydrodynamics, irreversible thermodynamics. My professor in graduate studies, G. Gamow, had already done the Hot Big Bang model for the evolutionary beginning phase of this universe. That most global problem, stellar processes relevant to precision time keeping processes, solar system processes, geophysical processes, had all been fairly captured in my understanding and problem practice by rather precise physical law. To toss that understanding casually by the wayside is/was simply beyond my possible beliefs. There did remain a domain of what I referred to as stormy weather systems that required further investigation. Those were problems associated with the mixing of meteorology and other Earth fields, turbulence, chaos, the problem of biophysical systems, and social systems. I and my colleagues tended to think of those problems as serious ones for us to study in or by a physics that we called homeokinetic physics, a physics of complex systems. We
regarded that as the challenging outcome of the problems we were confronted by during World War II, and that seemed to have emerged from the end of World War I and II. So, for example within the 40's, I began to work on turbulence, and on the biophysic of the human at high altitudes. Besides developing respiratory equipment and outlining the dynamics of respiration, investigating human body thermoregulation, showing that mammals obeyed the second law of thermodynamics with precision (required to deal, in the long run, with all the body exchanges with the environment, I also developed the foundation for space suits (in 1949). A summary of about 100 papers and studies that we did in applied biophysics is to be found in a 1972 report covering material back to 1940. Thus, if you average what we wrote in a first draft in about 1955, as a Philosophy for Mid-Twentieth Century Man (written as a counterthrust to a Marxian outline of science in the 30's by Levy), and the result of a first contract research study Introduction to the Contents of a General Systems Science (U.S. Army, 1968; to be referred to as Intro '68), you will grasp it was writing covering the epoch in which Quigley was writing.

I did not sell my first social systems contract until 1972. It followed upon the publication of my edited general systems study of 1968, as Toward a General Science of Viable Systems (McGraw-Hill, 1972; to be referred to as Toward a '72). For its acceptance by its publisher, that book had about 30 reviewers from almost all disciplines. It stretched the knowledge base of every such reader. Our 1955 effort (Philosophy '55) was premature. By mid-60's we were more ready. Our book captured the technical world market in systems science. It, hopefully, 'explains' why we felt ready to tackle a general model of social systems by the 1970's, and why I went cruising in AAAS for a general group attending to social processes rather than to take 20 years to 'do' history, social psychology, ethology, anthropology, sociology, economics, one at a time. Thus I found ISCSC. There I met Quigley and his material for the first time. My first impression, which I now find reinforced, was that Quigley's view was anti-physical science. In reexamining the book now, I carry that along still, as a very strong impression.

My problem: from the beginning, Toward A General Science
of Man-Systems, a Venture Into Social Physics, my 1973 contract study (Toward a '73) was meant to be a physically hard model of social systems good enough for running societies. Why for the Army, and in 1973? Because, among other subtleties, they are the largest employer of man-power, they are institutionalized social groups at many levels, and their interests reached up to geopolitical levels. Besides which, they were in process of extending their old psychological science-oriented organization (the Behavioral Science group that did or began I.Q. testing during World War I) toward a more anthropologically oriented organization (Army Research Institute) and I was one of their first contractors. So the only difference between Quigley and me was that I was a practicing physical scientist, trained and experienced. I proposed — in a fair test — to try to achieve what Quigley said physical science was not competent to do.

I will ask the question: did I find what Quigley did to have any influence on me? No. Did I find any significant ideas in Quigley when I got around to read him and to begin to interact with him in ISCSC in perhaps 1975 or so? Again, no. What I found in his book then, even more so now, is/was banality, a pedestrian quality, the sum total of almost all the positions that I have been persistently challenging.

So I will put out before you the subject themes, the persons, the views that I found necessary, in fact essential, to get to the point that Quigley says we must but are not competent to reach, but which we did reach — first to develop initial analytic tools, from 1972 to 1976 (see DOT, TSC report DOT-TSC-1157) — and then on to substance in 1993, when my colleagues and I published Iberall, Wilkinson, White, Foundations for Social and Biological Evolution (Foundations '93).

Let me tell you the persons whom the literature and ISCSC tossed and welled out to engulf us in my effort. I found L. White's energy-oriented cultural model and began to correspond with him. I was thrilled by Toynbee's modeling, particularly in Vol. 12, and the "second" ISCSC session devoted to his honor, and I began to correspond with him. I also found C. Arensberg's opening issue of Current Trends in Anthropology (Vol. 1, No. 1, 1972) and began to correspond with him. He put Chappie and Coon, 1941, Introduction to Anthropology in my hands (I had found
Harris' Rise of ... myself when it came out). That humbled me delightfully. It was and still is a marvelous introduction to the human problem. What Arensberg and I (Iberall, Soodak, Arensberg - a fourth author, H. Lasswell, died before we could finish the piece) published in 1980 in an invited chapter on Social Mechanics ("replete with formulas, etc., for graduate students in mechanics"), and what we have followed now in and with Arensberg's student Alexander Moore, is still an intermediate elaboration, in a physicalist's sense, of Chapelle. His book appeared a mere six years after my youthful — child-like — reaction to the problem in 1935. In any case those beginnings, plus the beautiful sounds I have always found Hord, Hewes, and Wilkinson making in a factual sense, kept me secure in my developments.

What I had to bring — as a physical scientist — to get started on my project were the following:

1. A clear understanding of physical network theory, which was developed by Kelvin in mid-19th Century to make the mid-Atlantic Ocean cable possible. This, followed by thermodynamics, and Navier-Stokes flow processes, via Maxwell and Boltzmann and Gibbs and Rayleigh in late 19th-early 20th Century, made irreversible thermodynamic and acoustic and mechanical and electrical networks all describable on a par.

2. The fact that the lowest levels of atomic-like constituents were not clarified by observation and by missing force components, and in fact were needed to understand the physical basis for chemistry, which was not clarified until the turn of this century with the discovery of radioactivity, had created a half century of confusion. The same physicist, Kelvin, who could clarify network and other problems, could not account for stellar energetics and its operational time scale until radioactivity was discovered, certainly due to the fact that the observational basis did not exist. Physics has always claimed that it is an experimentally-based science in which experiment and an extremely dense fabric of theory march hand in hand. Old theories are not casually thrown aside. They have almost all tended to be integrated with the new. The fact that the natural sciences of geology-geophysics, biology-biophysics, like astronomy-astrophysics long before, had no adequate basis for long term evolutionary processes was certainly a
missing piece of the business of physics. But within a few years of the discovery of radioactivity, the physical sciences could run through those problems, e.g., create a quantum theory, come up with a theory for the operation of stars — in fact all of the processes including developing a viable nuclear energy industry (see Eiseley's *Darwin's Century* for most of the beginning of that story). I think it more than interesting that Eiseley was at the ISCSC's organizing meeting, and it seems I am the only one who remembers that he was angling to try to become ISCSC's first president, an ambition which got swept away as soon as the power players appeared. To me, it would have been an interesting society under Eiseley). So with a clue to atomic-nuclear processes, within a few years the Bohr model of the atom began and the size, nature, and physical forces involved in the atom became known and accounted for. Physics simply jumped in and produced theories of even greater precision than ever before. For example, in my NBS section, it became possible to increase the timekeeping accuracy from the star and electromagnetic precision of thousandths of a second per day fluctuations of uncertainty and related small errors per year to very much greater accuracy of small fractions of a second per year which had become needed and used in quite a few Government and industrial processes.

3. At the same time I worked on such problems, I was also working on the thermodynamics of the living organism, showing in the 50's that such systems obeyed the second law of thermodynamics with precision, as well as the conservation of energetics of the first law (shown in the 1900's). This implied that both the biophysics and biochemistry had related closure. My colleague H. Soodak's mentor, F. London (who just died) was responsible with Heitler for the nature of the covalent chemical bond, the second basic bonding that makes biochemistry and its quantum mechanical explanation possible. I have been lightly attending aerospace medicine meetings since the 1940's, and have been making fundamental contributions to physiological dynamics on the basis of our biophysics all the time since. It is largely through so-called molecular biology, and its integration, from which much of the foundation of the applied so-called basic medical sciences has been developed (see our joint Engineering Regulation and Control conference meeting, with the international community of
physiologists in 1973. In that volume you will find that our homeokinetic modeling was being taken seriously as a basis for general organismic modeling). If you look further at our products of the 1960’s, you would find that we had, also, to join our modeling with an information theoretic. That subject had been developed out of Bell Laboratories interests in the communicational aspects of electrical networks, e.g., telephony, from the 1920’s on. (See our Army study 1966, on information science. That little handbook study shows how large power network theory, for electrical networks, was joined to small power communication networks to make the modern communicational industry and systems possible.)

McCulloch and I (he was one of the three fathers of cybernetics - the other two were Wiener and J. von Neumann) laid the foundations of a homeokinetic model for communication and behavioral dynamics in the mammal, including the human, in 1967. In 1991, I showed that such a foundation was the basis for accounting for sensory and motor action in the cognitive machinery of the mammals, including the human.

In writing these paragraphs, the triviality of Quigley's notion of the physical and engineering physics sciences becomes increasingly evident. So there is no point in detailing the misconceptions put forth in his Chapt. 1. It is hard not to fault the lack of understanding of "social" and other softer sciences that the physical sciences hold first rank of account in cosmology, galactic science, stellar science, planetary physics, even in the stormy weather systems of meteorology and hydrology and plasma processes, in geochemistry, and even in biochemistry, all through its genetic coding, and internal chemical organismic operation from the cell through the mammal.

Many philosophers and casual students say that life and society are not on the physical agenda. Nonsense. Since Oparin in the 30's, the physics-chemistry of life has been on the agenda. Current molecular biology, if you will, perhaps only up to the worm at the moment, is worked out in great detail as physical achievements and now engineering physical achievements (at an alarming rate). You may still — per Popper and Eccles, or per the wisecracking stuff in Yates on biological self-organization — sit on the sidelines and kibitz, but the study is a foregone conclusion.
It's gone physical.

Does social science then sit as a final isolated bastion? Nonsense. This is part of what McCulloch taught me is the overweening hubris of the human. As he put it to me, there are those great shocks (by the way these are real physical-chemical shocks) that occur when you learn that humans are not the central drama of natural creation, that the Earth is not the center of the universe, and that your parents were involved in sexual activities to produce you.

In social science, we started on the study subject in the 60's-70's. What did we have to learn past the subjects so far named? The real starting content of a social science was attempted in a *Philosophy for Mid-20th Century Man* (Philosophy '55). But an account of its roots actually leads back to such things as my last exposure, via Lewis Feuer, to his course (which I sat in on) on Dialectic Materialism at CCNY in about 1938-39, in which every splintered student politically oriented view was represented, as well as to many other courses I sat in on as a very early hippy. All this occurred before I started seriously on my scientific-technical working career. But its emergent themes can be found in *Toward a General Science*.. (1968 draft, 1972 publication). Besides having done a piece on information sciences for the Army, I also did a piece on technical forecasting in 1966, and on the problem of forecasting scientific-technological change, e.g., understanding of the atom as of 1900 (P. Duhem, W. Ostwald, others); the quantum theory and non-linear problems in physics from the 1910 to 1950 era. In 1967, I did a study for the Army on advanced technological planning as a companion piece to the Army Research Plan. These problems literally tied physical science, physical technology, human command-control, as exercises in the field of automatic control and in my specialty problem of directing R & D within government and in industry. I had no luxury of academic fuzziness. My ideas, as engineering physics ideas, had to work. We in the automatic control field, during and after World War II, had to develop automatic regulation and control and adaptive control schemes. A number of friends and mentors in the regulation and control field, literally developing the hard nosed engineering schemes of automatic control (examples: C.E.Mason, Philbrick, Owen Fairchild - see Temperature, its Measurement
and Control, 1941 for one of the most marvelous interdisciplinary conferences ever, Ziebolz, Buckingham, A. Sperry) assembling in the 20's to 40's, produced "us," the next new generation from about 1940 to 1955, with the Gibson Island, later Gordon Research Conferences, as a forum and meeting place. "We" were such names as N. Nichols, Kalman, Chestnut, myself, Beckmann, analogue control leaders moving later on to the digital control, business machinery computer revolution. Warren McCulloch forced me from the biophysics-thermodynamics of the biological organism across the gap to neurophysiological physical command-control of the organism from a startup 1962 biophysics to our first 1967 modeling of behavioral thermodynamics of a chemical-electrical nature. If you want to know what has developed from this, see — purely as an illustration — APS News (The P is Physical, not Physiological), June 1996, page 3, "Information Theory Provides Unified Framework for Neuroscience."

IV. Quigley Chapt. 1; and a counterdialectic

Quigley and I introduce methodological issues up front, he in chapter 1, 12 pages, I in an introduction (Toward a '72) with a summary and questions for the reader in 11 pages. Comparing those two introductions, one rejects physical science, the other offers a summary of its methodological philosophical foundations. I admit to adding four pages of metaphysical foundations at the end.

Quigley uses the metaphors crystal and crystalline to introduce a depiction of civilizations (he refers to it in Chapt. 1 pp. 1-2, Chapt. 3, pp. 35-36, Chapt. 4, p. 38, Chapt. 6, p. 94. Mercifully, it is not in the index). The metaphor is both bad and misleading. I would say instead that the person, as a least unit in the interactive collective of a human society, is the physical-chemical atom in the interactive collective of a human society. The physically defining metaphors here are person as "atom" (atomistic-like, atomism - in our language, used like organism to denote object and concept) and persons as "interactive" (possessing common "actions" where "actions" are the things that are done; in physics the energy-time measure products of a matter-energy unit's behavior). A better metaphor - in a physical defining sense - for the human "society" is that of the collective phys-
Quigley's crystal metaphor, like what he thinks about the physical sciences, or about "scientific method," is anyway irrelevant to the "structure" or form "framework" for his historical social science, which must in fact be extracted from his Chapts. 1-5. Quigley suggests a methodology for a theory of historical change (pages 2 on): (a) gather all relevant evidence; (b) make a simple, but all embracing hypothetical theory; (c) test the theory. Any such construct will remain tentative.

I learned the historian's philosophic point of view on this question from Hook (Philosophy and History), who indicated historians were split about the need for theory, with, e.g., Gershoyn on one side and Nevins (The Gateway to History) on the other side. We obviously opted for a need for hypothesis formation and listed mentally Spengler, Toynbee, Kroeber, Sorokin, Quigley, Melko, and Naroll as supporters. See our book, Bridges ..., p. 16. Accordingly, what we did (in our 1968 draft), to start off the social sciences toward a social physics, was to try to sweep through the contents of a social history in terms of its subordinate disciplinary components — of human activities in physiology, ethology, psychobiology, anthropology, sociology, economics, and look for their overlap with our physical sciences.

Our procedure in Towards A '72 was to abstract from high quality expert sources their ideas on the components of social science, and to develop an interwoven physics with those themes. Without detailing, I prefer to jump to a list of some of the integrative great expositions that I used as sources. Such big picture sources that we found useful (skipping the physics and engineering physics sources), largely of an interdisciplinary nature, that served as our "nourishment" to create a social physics (e.g., from perhaps 1955-1975), were about 70 sources that can be found listed in the book. In total, I had about 300 large scale references in my 1968 draft.

Our standard research tactic, all through my career, is/was to sweep through the field topic we were supposed to do a detailed system study of for some specific applied results, quickly find 10-20 large general sources which illuminated the great expert think-
ing in a subject and which had some physical relations. This concentration, within a week or so, gave us some overall sense of a field in a month, and permitted us afterward to examine specialized periodical literature sources, and produce - if necessary - a competent professional review of the field. Commonly, in our contract work, we would either work with our expert but troubled client, or go out in the field and make connections with outside experts if confidentiality was not at stake, and our clients encouraged it. Those initial 10-20 sources we discovered would be included in our book. For examples of our 'pro bono' public integrative giveaway work, see


Or for longer time summaries, see

Iberall, Cardon, Regulation and Control, Tokyo, 1969
Iberall, Bridges ... (covering 1960-1975), 1976
Iberall, Pulsatile and Steady Arterial Flow (loosely 1945-1975), 1975
Iberall, Guyton (eds.), Regulation and Control in Physiological Systems, 1973
Iberall, Toward a General Science of Viable Systems, 1972
Iberall, Wilkinson, White, Foundations ..., 1993

To return to the results of our standard tactic as applied to human social systems: we discovered the ethologists like Thorpe and others in animal, particularly mammalian, and then primate behavior. We could even examine a little the paleobiologists like Simpson and Dobzhansky, and others who could stretch to mammalian behavior, including the remarkable neuroanatomist H. Chandler Elliott (The Shape of Intelligence), who could stretch all the way; not far behind was J.Z. Young' textbook on physiology. From ethology, we could jump to psychology-psychiatry in Freud, Sullivan, Piaget, Gesell, and others in so-called psychological and physiological behavior. Jumping quickly, to name
other of the social science fields, we found history, starting from its theoretics, in Hook; a historical-anthropological overview in Huntington, Darlington, Linton, Childe, Mellaart, and the UNESCO series starting from Hawkes as a prehistorian; the anthropologists such as Meade, Benedict, White, Harris, Arensberg, Chapple, Polanyi; the sociologists like Marx, Weber, Durkheim, Murdock, impenetrable Parsons, Gouldner; the economists like Marx, Marshall, Schumpeter, Keynes.

I believe this gives a sense of the kind of material we found as sources for the formation of hypotheses about human society. In our standard fashion, it was not very difficult to find 20-30 such books that gave us a frame for these subdisciplines in a very brief time of weeks, and a reading list of a few hundred such books which we acquired within a few years, by about 1972. Thus in writing our 1968 draft form of a General System Science, about one-third of our three hundred references were from the social sciences.

On macrohistory in particular, past a youth spent with van Loon, and H.G. Wells, Hook was a major philosophical source who provided us with insight into the historian's mind. It took some time until about mid-60's before we found E. Huntington and McNeill, and de Coulanges, the National Geographic Society's *Everyday Life in Ancient Times*, later Barraclough and Braudel. But it was the first ISCSC meeting, with Toynbee's special paper and my beginning to correspond with Toynbee, that led to my contributing to a proceedings issue of *Mainsprings* ("Towards a Thermodynamic Theory of History"). After a year of nominal acceptance, my paper was rejected. I sent it off to *General Systems* where a much friendlier editor (Axelrod) and a highly respected physicist-philosopher (Margenau) reviewed it and published it belatedly in 1974. The very key question was raised as to what I meant by determinism and causality. It forced me to offer the concept of circular causality in which A causes B and B causes A in a circular path, but not along the same connections between A and B. This was essential to establish a fundamental theory of causality, e.g., a macro-micro link in systems, why an atomistic theory and a collective theory are required to form operational systems.
V. Quigley Chapt. 2, and further dialectic

Quigley's notion of social lawfulness is that in a science for human society, its phenomena — implying its laws — are affected by the human thought processes (pp. 11-12). In our opinion, the logical reduction involved in that thought of Quigley's is that neither predictive nor retrodictive theoretical accounts can be developed in social science. At best it might have its own variable "mental" laws which "subjectively" influence outcomes (both in the future as well as attempting to be reflected back into the past). Most physical scientists might take some such view, and thus regard a physics of society to be an impossibility. That would also follow, for us, if we took such a view; but we did not and do not. We presumed, and presume, the absolute opposite, namely that human social phenomena could be deterministically cast, albeit as or by a statistical physics, statistical mechanics, statistical thermodynamics. And that is why we have always had a complete disconnection from Quigley.

Our work on this line is the following. We went in the 50's and 60's from engineering physics regulation and control to adaptive control (see Kelley, Manual and Automatic Control, and the meetings we were running in the 50's and 60's). We challenged the control engineers with the statement that they had no theory for the factory, even if they did for the quickening responses of control devices, and turned to the biological organism as an object system to learn about a theory of the autonomous factory, working on power and communicational thermodynamics up to about the 1960's. My 1962 review paper on biological regulation and control also starts from our 1950's thermodynamic work and from such data sources as Pavlides. In our work with McCulloch (by 1967), we concluded that the logic of the biological organism was not an electrical network logic, but a chemical "network" — a chain of processes — within the organism.

My reports, pursued with physiologists, pharmacologists, neuro types in those two fields, biochemists, all from a physical-chemical engineering thermodynamics (such as developed in time by the Katchalsky school) all laid forth the view that a complex of largely chemical thermodynamic engines with interactive behavior regulated the internal environment of the organism, so that, as per Cannon's homeostasis (and C. Bernard's hundred
years earlier statement in the 1850's), the fluctuations or "vicissitudes" in the external environment — in which the close up part of that environment was also a part of that system — were regulated out in the interior. Bernard and Cannon had not supplied the internal mechanisms; we showed them to be thermodynamic engine cycles in the interior. We called this theoretic homeokinetics and its processes homeokinesis, the dynamic regulation by a large number of loosely coupled thermodynamic engines.

By 1967, McCulloch and I were ready to extend the schema to the brain and its mind, and when he died I carried the process forward with Maturana's and Eccles' neurophysiology student, R. Llinas. As per our 1930's understanding, we had to recognize that such homeokinetic systems were stormy weather complex systems and we had to define and develop a rather full science for such homeokinetic complex systems. That is what we have done and are continuing to do in an ever increasingly precise fashion. We have extended homeokinetics into one scientific field after another — cosmology, galactic processes, stellar processes, planetary (Earth) processes, biological, and — since the late 1960's — into social physical processes. It is on that basis, of a well-developed alternative physical model, that we challenge the Quigley model.

VI. Quigley, Chapt. 2 continued, with additional dialectic

We continue to expound Quigley's model. The development of the human system is long time delayed — its persona-personality depends largely on nurture (p. 14). The patterns and objects in the social environment form its culture. Nothing relevant to human behavioral activity can be learned from comparative biological study (ethology - p. 15). The potential range of human development is large, inhomogeneous, variable, so that its breakdown into classes and phenomena is arbitrary and imaginary (p. 16).

We entirely disagree. There is (only) a chemical potential, physically defined, carried aboard the new human person in its genetic and coded developmental-evolutionary material. What emerges is operationally a "product," (not a "sum") of that chemical culture and the physical-chemical environment of exposure, which represent different hierarchical levels of chemistry. (Thus
we unify C. Bernard's homeostatic regulation, Pasteur cell theory of specific chemistry, the epigenetic view of biology and psychology, chemical dynamics and its higher ordered hierarchical nature, within an irreversible thermodynamic foundation to create a general homeokinetic model, first of the chemical chain, then of the cellular and organismic chain, and then of its higher ordered social collective, all within a precisely coordinated chemical-morphological field. What can constitute its apparent "personality" may or may not be so relevant to social operation.

Quigley decides that his historical analysis (a principle upon which selection of facts is based) would elect six levels (military, political, economic, social, intellectual, religious) among what could be otherwise 5, 7, 60, or even 600 levels (page 17). So be it. I want to know and see if I can accept what was the principle of Quigley's choice. The only answer I get is Quigley's fancies, which perhaps are supplied by his teachers or other experts. I am willing to believe that it may have something to do with conventional political history, conventional and political economics, etc., but also with Spengler, Toynbee, and others as historians.

Quigley then puts forth a theme that the "matrix" (in a technical mathematical sense) of otherwise unconstrained variables making up the "personality" potential function emerges from mysterious drives. This is meant in a teleological, purposeful sense (p. 17), e.g., Bergson's elan vital.

There is also a cultural "matrix" (in Quigley's sense), not only occupied by the collective total of its personality potential functions, but also includes all materials and energies, as well as all of the bonding issues. An individual, he says, is a nexus, its totality of relations form a status. Culture is thus subtle and complex with its complex mixture of short melodic forms. It is the chief subject of study in all the social sciences. It is adaptive and persistent (p. 20); it can be diagramed in various ways, e.g.,

\[
\text{society} = \text{humans} + \text{culture} \quad \text{(p. 23)}.
\]

Such a description tells me very little. But we should note, p. 24, that Quigley states that in addition to internalized personality, culture lies outside of human beings in their network of relationships, artifacts, and communicational symbols. While we could
accept some such sentence, we believe that it would take many
pages of physical exposition to reduce it to operational content.
Arensberg once said to us (Soodak and I), that he admired our
understanding and devotion to our atomisms in physical study. We
immediately responded with the nonrhetorical question "Do you
mean that social scientists are not aware that their atomisms
are persons?" He replied in the negative, stating that they mixed
up ideas and artifacts, and movements, and the like as all part of
the atomism-containing cultural solvent. We were and are still
shocked by that lack of discrimination. It reduces the descriptive
problem to metaphor and arbitrariness of choice that we continue
to complain about.

I will counterpose to it the methodology of the physical sci-
ences and the way I connected them with the social sciences. The
physics of systems tells us we have to discover the atomistic
structure of the system we wish to study. The atomisms and their
motions we study by kinetics, how forces — physical forces, and
we have only four forces to draw upon — act on those particulate
units. Newton provided us with the first way to deal with the
motion of each particle and the motion of the particles around
their "center" of motion. He did this for the simplest of cases, the
so-called rigid body collective. After him, others did spring-like
collectives, fluid-like collectives, systems governed by their inter-

nal electrical forces, gases, liquid, solids, plastic solids, solids
with memory storage, and the like. We pioneered in the physics
of complex constellations of atomisms, e.g. those found in or as
nature, life, humankind, mind, and society. But the methodology
of analysis remains the same. We spent 25 years creating such a
foundation for biophysics, bioengineering, mostly in the compa-
ny of the specialist disciplines of biology. When we came to
social science we did the same thing. We chose our favorite tar-
get persons in anthropology, sociology, ethology, psychology, his-
tory. We read their literature and talked to them, attended their
meetings until we were comfortable with their ideas (I am the
only physical scientist who has attended ISCSC meetings for the
past 25 years).

It was clear to us that we needed first to take on the detailed
study of humans. This, we quickly concluded, lay in anthropolo-
gy. We do not do the dynamic history of a system until we first
know its atomisms and how they link up to a collective. We
quickly learned e.g., from Harris, *Rise of...*, about the bridging
view of synchronic and diachronic descriptions of human soci-
eties. That was the bridge, for us, to the spatial and temporal
spectrum of processes that one might find in humans. We found
nothing to reject in Tylor and (AAAS president) Morgan. We
found a book like Linton, 1957, *Tree of Culture* and Pigott, *Dawn
of Civilizations*, 1961 to be useful kindergarten reads.

Once we understood that living societies e.g., human but not
exclusively, were all collections of interacting memory-possess-
ing organism units engaged in repetitive performance cycles at
generation time scales again and again, our physics was obligato-
ry. Namely, regardless of the kinetics of the individual (but,
notice, I said I had spent 25 years on the operation of the individ-
ual biological organism, so that I knew a great deal about its lower
system’s dynamics and kinetics), I could then examine what might
be the dynamics of the collective, ultimately to reach the histori-
cal dynamic trajectory of its diachronic modes. I did not have to
invent a dissection of levels of description, I simply would begin
to see how the simplest systems flushed out their fundamental
intrinsic flow variables. That is what we spent the time doing —
flushing out the simple system "conservational" flows. (We use
the term conservations because our physics is built on the process
of pair by pair interactions in a collective. That comes from the
kinetics, and all we have to do is identify those physical variables
that are conserved during collisions).

We have developed the scheme that explains the most gener-
al form of all interactions in both simple and complex collectives.
So our physics is almost completely obligatory. And we have
worked very hard to introduce the concepts among anthropolo-
gists, and psychologists, and systems engineers, finally with some
success. We have had almost negligible success, however hard
we tried, in ISCSC. We try again.

The conservational flows start from matter, energy, and
momentum. This follows from Newton’s laws of mechanics, and
to that we also have to add the conservation of electric charge in
electrically nonneutral exchange processes (That flow is funda-
mentally involved in matter-energy chemistry at all levels). In
internally complex atomisms, ones that show a great deal of inter-
nal memory function and long time delay in their internal atomistic processes, we showed that the transport (the flow from within the complex atomisms and external to themselves between the atomisms) takes place by a higher ordered complex of internalized action (activity is the common language term; the energy-time product is the physical term) as an integrative form of momentum (the actions emerge by adding up the detailed momentum movements). Chemistry is involved in the making, breaking, and exchanging of force bonds between fragments of those atomisms, and in the biochemistry of living systems, that chemistry is encoded in the genetic molecular components in cells at a lower level. This leads to the reproductive, procreative, demographic process among living systems as a fourth flow conservation. As a higher ordered chemistry, it is expressed by Malthus’ law, the rate of change of population is proportional to the population. What that law means, and it thereby becomes a thermodynamic law, is that, although all those who live are born and die, there is a conservation process, homogeneous above a certain scale, whereby autonomy of an organismic atomism can emerge to form a breeding collective.

VII. Quigley Chapt. 3; and dialectic continued

In Chapt. 3, Quigley points out that he is concerned with collectives of individual persons, whose behavior [as atomisms] is unpredictable. If that were true, as asserted, there would be almost no science possible. As per our remarks to Arensberg, to know your atomism in at least a deterministic statistical physics is fundamental.

Quigley avers that some success may be achieved with group aggregates. Statistical physics, mechanics, perhaps? But this statement, as is common, misunderstands the basis for success in the physical sciences, which proceed in a very "law-like" way from the atomism to the collective, a process begun by Newton.

In any case, says Quigley, with person aggregates, we can state no law comparable to physical science. The rules of social tendencies, p. 26, depend upon either a collective, an organism, or an extra [Kroeber's superorganic?] view of the social aggregate. He states that this question has involved millennia of debate. From that debate, he elects a "sufficient" consensus, and settles.
for some particular special characteristics, as did Marx, Weber, and innumerable occupants of the nooks and crannies of the social sciences. To the contrary: the unique character of physical science makes such typological search unnecessary, because whatever is not physically excluded will take place.

Returning to Quigley's typology: he settles on social groups, societies, and civilizations, p.27. I do not feel compelled to deal with his details pp.27-32; suffice it to jump to his remarks about civilization. On p.31, he states that he will have to distinguish between producing societies and civilizations. In a paragraph before he states that there are parasitic societies and producing societies. By their 'gleaning' [may we say hunting-fishing-scavenging-gathering?], they survive. A second type, led by pastoral activities, are producing societies. "They seek to increase the amount of wealth in the world." [I do not know what wealth, or wealth of the world means]. The distinction, he says, is of most fundamental importance. Man was parasitic for a million years earlier [I dispute this, adducing the fact that life forms have drastically transformed the surface physics-chemistry of Earth]. Only with agriculture and domestication, less than ten thousand years ago, did Man become a possible producer, though remaining mostly parasitic.

In that period, he points to simple producing societies and complex ones that he calls "civilizations". As a temporary definition he says that a "civilization is a producing society that has writing and city life". He then proceeds to identify sixteen civilizations.

Most of this seems very pedestrian. My alternative approach: Jane Jacobs and I have been arguing out these issues in very friendly fashion from a starting period not too long after her books The Death and Life of Great American Cities and The Economy of Cities books came out in the 1960's. A latest round of our discourse, on which came first, agriculture or urbanization, is to be found in Foundations '93.

VIII. Quigley Chapt. 5, and dialectic

I will skip Quigley's Chapt. 4 on Historical Analysis [one presumes a major purpose in writing the book] and its pursuit of the sixfold levels of culture, and pass quickly to his Chapt.5 on
Historical Change in Civilization.

To state my dialectical position first, for a change: just as in physical systems, after we have defined the atomistic and the collective form and processes with flows and potentials, we have to write equations of state (whether for collectives that are gas-like, liquid-like, solid-like, glass-like, biochemical-like, or social-like). Beyond that, we need equations of condition and of change that permit us to get at diachronic processes in terms of their historical trajectories. At that point, we are prepared to begin "historical" process study — of development, and of evolution.

Thus we fully grasp Quigley's opening sentence in Chapt.5: "It is clear that every civilization comes into existence, passes through a long experience, and eventually goes out of existence". In Yates (ed.). Soodak and I highlight some such statement as one of the fundamental physical laws of all matter-energy systems in the universe. But we do not do it by analogies or metaphors (e.g., p.66-69), whether as a biological analogy, or Darwinian evolution, or Spenglerian idea, or Toynbeeean. That set of analogies and metaphors I have always opposed, as one or another sort of typology that I always found empowering thought in the ISCSC, and that I have tried to combat from the day I got there 25 years ago.

Quigley reaches a point in Chapt. 5 where he states that every civilization begins with a mixture of two cultures. That is a remarkable sentence. It took me and Wilkinson about twenty years from 1972 to when we finally used such an idea to define a civilization. (Actually, in deference to Wilkinson, we refer to startup as a pre-civilizational culture-civilization.) This I arrived at after twenty years of listening to argument in ISCSC about what a civilization is. But characteristic differences with Quigley remain: for us, as expressed in our metaphor definition, a culture is a real physical-chemical solvent (a true state of matter, a fluid-like system involving real physical binding sources) for a societal collective and its complex internal interactions, so that the continued coexistence of such two or more cultures involved fitting their individual driving value potential into a common acceptable chemical thermodynamic 'cultural' fit.

This is the place to recur to Quigley's crystal metaphor, and point out, to the contrary, that there is nothing idealized crystalline about such collectives. Real solids exhibit process changes
like dislocations, described in Zwikker. One can find there "emergent" properties beginning to appear in the liquid phase or liquid state — when one crosses the liquid-solid transition — in X-ray diffraction characteristics which 'foreshadow' their institutional forms in a plastic-elastic solid (this puts it within a physical metaphor).

On p.69, Quigley states that the pattern of change in civilizations [these would be dynamic processes of change if this were true physics] consist of seven stages which result from the fact that "each civilization has an instrument of expansion that becomes an institution" (involving incentive to invent, accumulation of surplus, and the surplus accumulated serving the use of the new inventions). Taken together they represent an instrument of expansion.

From our point of view, all complex systems in nature are instruments of expansion with their institutionalized forms. That is what our ten physical propositions in Yates are about in general. (My professor) Gamow offered the operational definition of the cosmos as such an instrument of expansion (1950). Such definitions have been sought and written for the hydrodynamic origin of galaxies. Similarly, from the discovery of radioactive elements in 1903, the empowering of stars as instruments of expansion has emerged by the 1950's within the scope of Gamow's and Einstein's theory of cosmology and general relativity.

IX. Quigley, Chapt. 6, and dialectic

I have to dive deeply to find whether I finally do or do not agree with the title of Chapt 6, the matrix of early civilizations. Does it have the same meaning for both of us? I say yes. The dictionary definition identifies matrix with or as it emerges from the womb, thereby acting as an ancillary source system which holds the system together. In my introduction, I define viable systems as an emergence of their operational unity as from a womb, laboratory, factory, environment in which the system has its startup. Clearly, both Quigley and I have to put forth a startup, a beginning.

Once again I will give our side of the case first. We promised a startup picture in Toward a '68, in which, in fact, a startup, a life phase, and a deterioration phase was promised for systems in gen-
eral, and I said that the book dealt first with the long life phase of systems. It took a long time, 25 years, woven in with ISCSC history (e.g., Melko asking for comments on the deterioration phase of civilizations in 1980), to get to its present state of development. I have been involved in many Star War battles in this society and many other groups. This included the opposition of Nelson, and Quigley, and Prigogine, and a variety of prominent physicists, urban planners, and the like. I really thank them all for the proper goading; and we have done our job.

When we began our work on civilizational startups, in our usual working style, the most telling picture to us was Mellaart, *Earliest Civilizations of the Near East*, 1965 (later it was Sherratt's *Cambridge Encyclopedia of Archaeology*, 1980). Mellaart — plus the Life-Time picture book on — stamped the picture of those earliest trading (and warring) civilizational collectives in my mind. Thus we knew that the startup for civilizations lay in the Mesolithic. That was likely clear from Childe's Neolithic decomposition on. So we started our theory, as a dynamic stability transition from then.

That startup story has an earlier phase to it. There was a presentation by Sahlins, prior to his 1972 *Stone Age Economics*, in a NY Academy of Science meeting, in which he offered a Marxian argument for the transition from hunter-gatherer to agriculturist. His presentation, as a stability theory, did not satisfy me. All of those emergent forms I named before are physical transition problems. So that transition problem too went into my agenda, and cropped up as soon as we began to work on a theory for civilization. A physical theory for such collectives we verified in a *Collective Phenomena* article in 1978 by Iberall and Soodak, and in an Iberall-Wilkinson article in *GeoJournal* 1985 - see also Yates and *Foundations*.

We started our transition from a 1,000-12,000 year (ybp) transition toward settlement. I really wanted and tried to get Hord, Hewes, and finally my colleague Wilkinson to do the experimental test paper for such a transition into a more stable settled, urbanized form. They all found it convenient to suggest I do it. So I had no alternative. I did. That was our 500 year process paper. Melko needled me and asked how come I didn't do it with Wilkinson. Because he always encouraged me to keep
going by myself. So, (a) I got help from our one of our anthropologist colleagues, D. White (who had been involved with Murdock in the large scale ethnographic files studies and is a large system network person) to help me add additional details. And of course, the inimitable Hord, whom I asked to look at our piece, immediately added the thesis of another A. Moore and dropped it in my lap. That "merely" extended the Mesolithic startup from 12,000 ybp (years before present) to 18,000-20,000 ybp. Is/was this a big deal? Yes and no. I didn't do or know the facts. I am, as often, indebted to Hord. But that 6,000 year or so earlier contribution clarifies the physics problem for me.

When did a transition to civilizations take place? Our physical theory did not give that sort of absolute space-time answer. What it did was indicate what the changing space and time scale could be from a before process, hunter-gather, to an after process, urbanized settled process. So, the problem of a transition to settlement, e.g., by settlement with plants via horticulture and agriculture, or nomadic pastoralism via transhumance, say, with animals, was on my agenda. The pictures and time scales that prove (test) out theory are found in The First Cities and in Mellaart.

Let us contrast this with Quigley's approach to startup. Quigley says the startup problem involves 5 dimensions, 4 space-time dimensions [in agreement with physics as independent variables, but these are not the dependent system variables that have to be derived] and a 5th dimension of abstraction (and there are also the culture variables). He thus identifies his seven stages of dynamic evolution in civilization among those 5 dimensions.

For Quigley, the three space dimensions comprise the geographic environment. [They really do not. Whether humans were there or not, the environment is a higher ordered process that has to be defined by equations of condition to give its specific character. That is what we have to do to specify every one of the systems we mentioned before examining any 'historical' dynamic problem of change that we wish to study. If we don't do that, as clearly emerges in Quigley's presentation, you begin to assume and put in, no longer as an analytic modeling study, what you want to prove]. Pages 94-97 are meant to introduce that geography.

Quigley infers a sandwich pattern of language, physical type
person, and type of social customs, pp. 98-100, as processes determined by the geography, actually the ecology. [This, good, bad or indifferent, is trying to build up an ecological determinism which really has nothing to do with the 4 space and time variables although they may be the resultant of such hidden variables on this Earth or some other laboratory. No connection has been made.]

Why do those patterns appear within the matrix of early civilizations? To get to an explanation for that, he has to find a deeper explanation for an earlier prehistory of the people, p. 102. He needs a 'dynamic' chronology (or base) and he elects climate. He is thus searching for an ecological model, e.g., of climate very minimally, rather than a value-in-exchange model like Marx or Sahlins was trying to offer to get things started, or an energy model of the Odums', or a demographic model, or an engineering model of technological process and change, or a material flow model of the chemist. Climate becomes in Quigley ecology that includes now the meteorological system as well as the hydrological system as well as most other Earth science systems, all as variables of space and time. This is a very inadequate way to introduce stormy weather systems which Charney and von Neumann introduced and which requires the use of the world's largest supercomputers to handle. I have to refer you back to Foundations, Chapt. 11 which, while it may frighten you, at least shows you how Earth modeling has to be dealt with.]

Quigley's mode of theorizing by choice is again found on pp. 102-108, where he discusses Neanderthal versus 'us'. On p. 107, he opts for a belief that Neanderthal, overstressed by glacial conditions, did not have sufficient mental flexibility to continue. [Obviously from Neanderthal's success, all it did was to limit their range].

Quigley continues his climatological modeling on pp. 110ff, tying these issues down to perhaps 2,000-3,000 B.C. as wave-like motions of peoples. He then traces further movements in a climatically oriented story up to p. 123, where he states that the events described in the chapter, performed on the three-zoned Northwest Quadrant within (and driven by) a chronology based on climate changes, form the matrix in which the earliest civilizations evolved. After that, in successive chapters, he details
various civilizations starting with the Mesopotamian; but we shall go no farther. We stop with the startup of civilization.

I find Quigley's chapter 6 to be the typology I abhor. It is simplistic, it is pedestrian, it is a just-so story, it is no scientific model. It is a picture sketch, nothing like the story of Darlington, or McNeill, or Braudel, or Barraclough. I would respond similarly, perhaps more analytically to a much more complete economic, or energy, or matter, or technology driven model. Actually, I would consider all of these simplistic, because all of the relevant variables, as in all irreversible thermodynamic flow fields, with their clearly identified driving potentials have to be identified. We put the outline of such a model in under 2,000 words in PNAS in 1985, and in Iberall, Soodak, Arensberg at greater length. These other one variable models do not want to acknowledge that they are all matter-energy systems, with internally complex actions covering a long time scale, that have memory functions, and are language-using by virtue of a real 'abstract' internal potential function, and involving a physics-chemistry of a very restricted range with regard to the upper and lower hierarchical arrangement of systems that drive this space-time sandwiched system. Outside of a particular restricted level of chemistry, there ain't no more. The physical-chemical environment of potentials is quite circumscribed. Raise the temperature above, say, 200 C or so, all gone. Lower the temperature below to -50 to -100 C, again all gone. Reduce the oxygen content above a 12,000 foot equivalent, gone. Multiply the oxygen content of the surround tenfold, gone. Turn off the solar flux, gone. Turn off the internal heat from the Earth, gone. (see Foundations for that story). The restrictions on condition are very severe.

What emerges, by development, evolutionarily is a certain class of chemical processes in organisms that develop hierarchically each with limited species life spans. Within those domains, in a physical - time environment, perhaps 2-1/2 dimensions high and in a broad Earth time, with basically a distribution of physical-chemical potentials, then atomistic organisms can emerge. You can read that story in Foundations and correlate it with Elliott. Much or most of that story is geochemically determined. They all operate as social collectives at their atomistic levels. It isn't until you have a physical-chemical story of all those processes in time
and place (see our NASA study on Long Space Voyages in about 1980), that you can begin to tell any of the local and parochial history of more detailed events.

One has to compare Quigley's notion of startup in Chapt. 6, and ours e.g., in Foundations, back in Bridges to the Thermodynamics of History and another companion article in Bridges. Quigley's book does not lay out any usable methodological foundation. It leaves the social scientists with the narrow horizon which is their substitute for science: typology, and sometimes beguiling grandmother/grandfather tales.

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