The Reinvention of Grand Theories of the Scientific/Scholarly Process

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In the mid-twentieth century, the reigning understanding of the scientific process, logical positivism, disintegrated. Subsequently, there has been fragmentation in science studies (Hess 1997; Yearley 2005; Sismondo 2008; Restivo and Croissant 2008). Many have emphasized the postmodern theme that general theories or grand narratives are impossible. Despite a profusion of diversity, some sociologists and sociologically-minded philosophers of science continue to produce general theories of the scientific/scholarly process. Through textual analysis and interviews, we studied ten such theories empirically on eleven issues, assessing their compatibility or lack thereof with each other, aiming to determine whether a new general theory is emerging.

I. Data and Methods

This research was inspired by Werner Callebaut's (1993) classic in which he interviewed major contemporary philosophers of science (specifically of biology) at a time when the interdisciplinary label of "science studies" had hardly been invented. The "real" in his title, *Taking the Naturalistic Turn: How Real Philosophy of Science is Done,* was a playful reference to debates over realism in Philosophy—the title as a whole drawing attention to his intent to study science studies empirically. That, for Callebaut, was "real" philosophy.

In the research reported on here, the works of ten major theorists of the

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scientific/scholarly process were studied (but not exclusively of biology and chosen from a larger set which were considered). No formal sampling method was used in this selection. Theorists were chosen largely on three grounds—(i) that they be sociologists or sociologically-minded philosophers of science (i.e., who seek to understand science/scholarship as a human social activity); (ii) that they tend to believe that a general theory of such is achievable; and (iii) that their theories addressed a wide range of issues in understanding the scientific/scholarly process. Hence we did not include those whose intent was mainly normative or whose interest in science/scholarship was only incidental to an interest in culture and social organization more generally. Nor did we include those whose theories were primarily psychologically—rather than socially-oriented; whose work was almost exclusively an historical case study; or feminist science studies scholars, for example, who, while interesting in their own right, did not address an otherwise wide range of issues. The text of the major work relevant to the study of the ten chosen was studied (usually a book but in one case a long article although we sometimes also refer to other works by the same authors). For the list of names, affiliations, and major work examined, see Table 1. In addition, interviews were conducted with the majority (seven out of ten), usually at scholarly meetings between the summers of 2007 and 2009. In one case, the author, John Ziman, died after an interview was arranged but before it could be carried out. In two other cases (Bruno Latour and Donald MacKenzie) we could not arrange interviews for different reasons but included them in the study anyway based on written works alone. Quotes or references from the main texts, where it is obvious who the author is, are identified by page number only. Quotes or references to their other writings are additionally identified by year. In all cases quotations which are not referenced are from the interviews.

Each theorist is associated with one or two unique ideas about the scientific/scholarly process. These are emphasized in the summaries of their theories in Section II and listed in Table 3. In addition to clarifying the unique character of their respective theories, we explored their views about a series of issues. The issues and their views on them are discussed one at a time in Section III with the issues listed in Table 3 and their views on them summarized in Table 4. The issues considered arose in three roughly classifiable ways. The senior author had some preexisting interest in Darwinian-style theories of sociocultural change (Blute 2010 was in preparation during the same period) and some issues about history, origins, and change mechanisms (Table 2; see issues 3, 4, and 6) arose naturally from that. In other cases such as unique theoretical features, patterns of change, and compatibility (see issues 1, 5, and 11) emerged from our readings of the texts as obvious points of comparison and contrast. More

Table 1. Theorists, Affiliations and Major Work Analyzed

<u>Andrew Abbott</u>
Gustavus F. & Ann M. Swift Distinguished Professor, Dept. of Sociology, U. Of Chicago.
2001. *Chaos of Disciplines.* Chicago: University of Chicago Press.

<u>Mario Bunge</u>
Frothingham Professor of Logics & Metaphysics, Dept. of Philosophy, McGill University.
2003. *Emergence and Convergence: Qualitative Novelty and the Unity of Knowledge.* Toronto: University of Toronto Press.

<u>Randall Collins</u>
Dorothy Swaine Thomas Professor of Sociology, University of Pennsylvania.
1998. *The Sociology of Philosophies: A Global Theory of Intellectual Change.* Cambridge: Harvard University Press.

<u>Gili Drori</u>
Lecturer, International Relations Programme, Stanford University.
2003. With John Meyer, Francisco Ramirez and Even Schofer. *Science in the Modern World Polity: Institutionalization and Globalization.* Stanford: Stanford University Press.

<u>Scott Frickel</u>
Assistant Professor, Dept. of Sociology, Tulane University.
2005. With Neil Gross. "A general theory of scientific/intellectual movements." *American Sociological Review* 70:204–232.

<u>Steve Fuller</u>
Professor of Sociology, University of Warwick.
2006. *The Philosophy of Science and Technology Studies.* New York: Routledge.

<u>David Hull</u>
Professor Emeritus, Dept. Of Philosophy, Northwestern University.
1988. *Science as a Process: An Evolutionary Account of the Social and Conceptual Development of Science.* Chicago: University of Chicago Press.

<u>Bruno Latour</u>
Professor, Paris Institute of Political Studies (Sciences Po), Paris.
1979. With Steve Woolgar. *Laboratory Life: The Social Construction of Scientific Facts.* Sage Publications Inc. Reprinted with a new Postscript as *Laboratory Life: The Construction of Scientific Facts* in 1986 by Princeton University Press.

<u>Donald MacKenzie</u>
Professor, Dept. of Sociology, University of Edinburgh.
2006. *An Engine, Not a Camera: How Financial Models Shape Markets.* Cambridge MA: The MIT Press.

<u>John Ziman</u>
deceased, formerly Professor of Theoretical Physics, University of Bristol.
2000. *Real Science: What It Is and What It Means.* Cambridge: Cambridge University Press.

Table 2. Unique Ideas

Andrew Abbott	Fractal cycles
Mario Bunge	Origin of novelty in convergence (mergers)
Randall Collins	Interaction ritual chains & law of small numbers
Gili Drori	Globalization & cultural authority of science
Scott Frickel	Scientific/intellectual movements
Steve Fuller	Social responsibility of science, public involvement
David Hull	Individuals and interaction in evolution
Bruno Latour	Social construction
Donald MacKenzie	Performativity
John Ziman	PLACE has replaced CUDOS

than anything else however, they came from the issues discussed widely about the social nature of science/scholarship—internal versus external, progress, the role of competition and constructionism (see issues 7, 8, 9, and 10) for example. For each interview we drafted a list of questions along the lines of these issues (hence with much overlap between authors), but also extensively tailored to the work of each author. The interviews were open-ended; we allowed the theorists to discourse at their preferred length on a particular topic and in some cases, interviews became quite conversational. The interviews were taped and transcribed. In one case (Frickel) there was a mechanical failure and our comments on his views are from notes made immediately after the interview and do not include direct quotes. Our overall conclusion that a Darwinian sociocultural evolutionary theory can incorporate both the common and useful unique features of contemporary theories of the scientific/scholarly process is drawn in Section IV.

II. Unique Ideas

All of the theorists under consideration have one or two unique ideas which distinguish their theories from others including each other (see Table 2.)

Andrew Abbott, a Professor of Sociology, is most well known for his research on occupations. He has, somewhat unusually, combined an historical and a quantitative bent in his work. Abbott sees the pattern of change in science as a branching process similar to a segmentary kinship system. However, the additional twist is that he sees this taking place in a fractal pattern. Fractals are things which are self-similar on different scales. For example, in sociology among both functionalists (who emphasized cooperation) and neo-Marxists (who emphasized conflict) there are those who put somewhat more emphasis on one, and those who put somewhat more emphasis on the other. Similarly among those who emphasize quantitative versus qualitative methodologies. In short, distinctions on a larger scale are repeated on a smaller scale. In general he sees the process of change as competition causing a split whereby one side typically loses. The winners then spread out to occupy the niche space of the losers, a step which he describes as "ingestion"—the whole being a **"fractal cycle."** As a consequence, the old division is recreated so that in essence, there is nothing new under the sun. "There is no real progress, no fundamentally new concept. We simply keep recalling a good idea." (p. 17)

Mario Bunge, physicist turned prolific philosopher of science, is the author of more than 80 books and 400 papers. He is a traditional philosopher in the sense that he unabashedly embraces ontology and epistemology. "Philosophy without an ontology is spineless and without epistemology it's totally incapable of appreciating the true intellectual characteristics of modernity—namely (that) science is the engine of modernity." "All of these criticisms of ontology and epistemology come I think mainly from linguistic philosophers—Wittgensteiners—who have no philosophy of their own. They have nothing to establish their thesis." "I think philosophy should use science and the only way to justify a philosophical hypothesis is to see where it is (and) where it's not consistent with science . . . and nowadays technology too." Hence science has been a central preoccupation of his work. His ontology is "systemism": "everything is either a system or part of a system." According to his "CESM" systems model, all systems (including natural, social, technical, conceptual, and semiotic) (p. 33) have a composition, an environment, a structure, and importantly, a mechanism (p. 37) which is "a process in a system." He views his version of systems theory as the alternative to reductionism and wholism. His epistemology is "rational selection" by individuals. It would seem therefore that there would be little reason to include him in a study of sociologically-minded students of science except that the book under discussion has, as its central thesis, a proposition about the social history of science. His concern is with "emergence," the origin of qualitative novelty in science. While aware of extinction ("submergence") and of branching processes ("divergence") in the history of scientific disciplines for example, he thinks the most important way **novelty** emerges is via **"convergence."** He defines "convergence" in science in the preface: "the merger of initially independent lines of inquiry, such as developmental evolutionary biology, social cognitive neuroscience, socio-economics, and political sociology." Other examples include physical chemistry, biochemistry, psychophysics, and medical sociology (p. 5). These are "mongrel disciplines" (p. 124) created by a process he explicitly compares to biological hybridization (pp. 272, 280). He believes that real problems can only be solved by "border trespassing" (p. 5) (i.e., by inter, multi, trans, cross, etc. disciplinarity)—an example is why integration succeeds in social studies in Chapter 11. He believes that in this way the sciences are becoming more, not less unified—moving towards the state idealized originally by the logical positivists, the unity of knowledge.

Randall Collins is a well-known sociological theorist whose book under consideration is distinctive relative to the others not only in its length (more than 1,000 pages). It applies his theory of **ritual chains** (see also 2004) influenced by Durkheim and Goffman to the sociology of scholarship (specifically philosophy) as well as to science. His theory that intellectual change is based on face-to-face interactions in historically-situated networks is located in all of micro, meso, and macro realms. In intellectual life, cultural capital is constantly creating puzzles that need to be solved, both confirming and refuting past findings and coming up with new ones. Intellectual creativity at solving puzzles is the sacred object of intellectual rituals. With respect to the micro, face-to-face interactions at rituals such as seminars, lectures, etc. invest (or divest) actors with emotional energy. Emotional energy fluctuates depending on the nature of the interaction. Domination and inclusion in social experiences raise emotional energy while being dominated and rejection lower it. Rituals then are the key communicative tool among intellectuals.

In the meso realm, networks are the key. Ritual interactions take place within and among groups, social networks, and hence are chained in interaction ritual chains. Interaction rituals produce solidarity within networks and competition among them based on individuals' desires to gain emotional energy and cultural capital. Networks necessarily make their way "into the inner experience of the individual's mind" (p. 7), thus suggesting that the group or network is always present in the formulation of new ideas. The level of emotional energy attained also varies depending on the status position of a given network and can be affected as well by external pressures including institutional and organizational shocks. One of Collins' most noted ideas is a proposed **law of small numbers.** At any given time, a single argument may only support three to six "knots" or competing networks because of limited attention space. In the macro historical realm, chains across generations are important as well. New ideas are always based in part on past ideas and past sacred objects and emotional energy can be transferred to pupils. Hence his comparative world history of intellectual communities including those of ancient Greece, China, India, Japan, Judaism, the Arab world, and European Christendom is replete with genealogical diagrams of intellectual influences through generations, highlighting among other things, the law of small numbers.

Social life in "rapid discovery science" (fueled largely by technological tinkering and developments in mathematics) is both similar and different from that in philosophy. Science-in-the-making operates like philosophy and follows the law of small numbers as rival networks vie for attention space. However, science after the research front is characterized more by consensus and cumulation because it is easier to make a reputation by moving on rather than clinging to old controversies (pp. 532–543), a view Collins equates with Latour's (1987) two faces of science.

Gili Drori is a sociologist specializing in international relations. The book under consideration of which she is the first author is concerned with the **globalization** of the organizations and institution of modern, western science. From its origin in the west, science has multiplied and diffused around the world, and is recognizable in that form everywhere. The authors document its growth and spread in the form of science organizations (including the professional, socially, and developmentally-oriented) (Chpt. 3); national science policies (Chpt. 4) and Ministries of Science and Technology (Chpt. 5); the incorporation of science into elementary school curricula (Chpt. 6); and the inclusion of women (Chpt. 8). While recognizable everywhere, global science is not a monolithic practice. Elements of the spread can be "loosely coupled." For example, a nation which establishes sciences in universities may not subsequently develop research laboratories (Chpt. 7). Scientific "styles" are somewhat "glocalized." Thus, for example, "cutting edge" hard sciences are more likely to propagate in nations with high GDP's since they can purchase the equipment and fund the labs required to conduct them. Conversely, nations that were historically communist witnessed the emancipation of soft sciences whose practice was previously suppressed and discouraged by regimes that concentrated exclusively on the hard sciences (Chpt. 9).

This book emerged from the "neo-institutionalist" research program in the social sciences of her mentor and co-author, John W. Meyer. While true to that program in the sense of emphasizing the explanatory importance of institutionalized norms over rational choices for example, this book is somewhat different in emphasizing a distinction between organizations and institutions on the one hand and culture more generally. The central thesis of the book is that the main consequence of the diffusion of scientific organizations and institutions is best understood as a homogenizing cultural, cognitive phenomenon, a generalized belief in the orderliness of nature including our capacity to learn to control it. This rationalizing ethos (which Weber saw as emanating from capitalism but which is seen here as emanating from science), effectively replaces religion and pervades discourses not only of science itself, but also those of law, politics etc. The important effect of science is not to its practical usefulness or contribution to development or democracy. In fact, evidence they present suggests that expanded scientific research (as opposed to labor-force training), and particularly socially relevant research, actually slowed economic growth between 1970 and 1990 (Chpt. 10).

Scott Frickel is a sociologist specializing in science studies with a political emphasis who has studied the rise of genetic toxicology. The coauthored article under discussion (of which Frickel is the first author) presents a general theory of **scientific/intellectual movements (SIMs).** The theory proposes that change in science/scholarship takes place through social movements among practitioners, not unlike the social movements which occur in other realms of human activity. Social movements are a form of collective action which can include organizations as well as individuals. Although not formally centrally organized as a whole, they collectively attempt to bring about (or oppose) some social change—normally in the face of resistance, commonly from elites. Historically important social movements include abolitionism, the labor movement, and more recently the civil rights, women's, gay rights, and environmental movements for example. Political scientists note that social movements sometimes give rise to new political parties such as social democratic and green parties.

Frickel and Gross think that change in science/scholarship such as the emergence of new research programs takes place in a similar fashion. They emphasize the conscious, contentious, inherently political, collective, and historical nature of SIMs which vary in scope. They offer a series of propositions illustrated with many examples from the case study literature about what is required for success. These implicitly include the intellectual (grievances), the social structural (access to resources and contexts for recruitment), and the cultural (ideas framed in a way that resonate with the concerns of those in the field).

Although **Steve Fuller**'s PhD is in the history and philosophy of science, he is currently a Professor of Sociology. He relishes putting forth contrarian theses. Hence, for example, historians of science studies have it wrong in portraying Kuhn as the hero ("subjectivist, relativist and historicist") and Popper as the bad guy ("objectivist, realist and positivist"). Instead "Kuhn was . . . authoritarian and Popper libertarian in their attitudes to science" (2003, p. 13–14). Similarly, contemporary scientists view some fundamentalist Christians as waging a war on evolution in the name of intelligent design. To the contrary, according to Fuller, who wrote in *Dissent over Descent* on "evolution's 500 year war on intelligent design"

(2007b). His testimony on behalf of intelligent design in the Dover trial in 2005 in the U.S.A. evoked consternation in the science studies community. Michael Lynch (past chair of the Science, Knowledge and Technology section of the American Sociological Association and past president of the Society for Social Studies of Science) plaintively asked, "Did he not know?" (see Lynch 2009 and Fuller's response).

Steve Fuller is a prolific author whose research program of "social epistemology" straddles the border between the traditional philosophy and the sociology of science/of scientific knowledge. What are we to make of it, and how should we respond, once the normative character of traditional philosophy of science has been confronted with sociology—whether that of external influences or of internal processes of construction through negotiation and so on, not unlike those which affect and create any other institution? As he puts it, the central question is "how science is to be legitimated once social constructivist accounts are widely accepted" (p. 17). The traditional answer to such questions was provided by Max Weber long before the advent of modern science studies in his twin essays on "Science as a Vocation" and "Politics as a Vocation" (Gerth and Mills 1958). They cannot and should not be; science and politics are different "vocations." A scientist has no more authority by virtue of his or her expertise to pronounce on questions of value than does a layperson. Science can however be brought to bear on questions of value. At its best, given an end, science might be able to say how it can best be achieved; given a means, it might be able to say what its effect is likely to be; and given more than one end, it might be able to say whether they are compatible. Weber's view is sometimes stereotyped as describing science as "value free" but he was not making an empirical sociological claim, but was instead holding up an ethical ideal for both scientists and societal leaders not to overstep the bounds of their competencies.

Fuller has been consistent in his very different answer for science—science must become more **publicly accountable.** In order to achieve that, society needs to be "let in," science made into a radical "republic" (2007a, p. 109) which is democratized. Hence his support for creation scientists teaching intelligent design alongside of evolutionists teaching evolution in universities (2007b). More broadly, he believes that a "struggle for recognition" in humans, a struggle to "give," can replace that for survival in humans and proposes that social science should align itself with socialism (2006).

David Hull is a founder of the modern philosophy of biology and author of its first textbook. With others he has argued there is a class of general selection processes which include not only biological evolution, but also individual learning and the adaptive immune response (Hull et. al.

2001). All selection processes include variation, replication, interaction, and selection (in biology, according to Hull, genes vary and replicate, organisms interact and are selected, and lineages evolve). He is most known for two specific theses. The first, also put forward by Michael Ghiselin, is that biological species are "**individuals**" not in the sense of being organismic-like, but in the philosophical sense that they are historical entities with places and times implicitly included in their names rather than being the universally specified classes of traditional physics. The second is that (contra Dawkins) organisms, not genes, are selected in evolution and hence "**interaction**" is a necessary term in the basic description of an evolutionary process. In his *Science as a Process* (1988), he applies his theory to sociocultural evolution, specifically to conceptual and social change in science. Curiosity is necessary but not a sufficient condition for scientific change. It provides conceptual variation but science is importantly built on giving "credit" preferably with a citation to those who have influenced us (i.e., on descent) as well as on "checking" (which he equates with selection). He abandons the common philosophers' bent for thought experiments, and morphing into a sociologist, tests various inferences from his evolutionary theory of science on the social life of the biological systematics community using a variety of methods—historical comparative, archival (referees reports), survey, in depth interviews, and participant observation. (Like the anthropologist who becomes king of the tribe he is studying, Hull became President of the Society of Systematic Zoology for a time.) For example he finds that like biological species, the sociocultural *concept* of a species (and by inference all other concepts) has varied and changed historically. Like organisms, scientists act so as to maximize their (conceptual) inclusive fitness. In interaction, they both compete and cooperate but in a study of six years of editing and refereeing of the journal *Systematic Zoology,* he found that these processes were at least somewhat biased in favor of those with related, and against those with unrelated philosophical, theoretical, and methodological positions. He also considers the theory's ability to explain known empirical generalizations from the sociology of science such as the strength of self-policing in science relative to other professions and the harsher punishment of lying (which hurts all who have built on a fraudulent finding) than theft (which detracts only from the plagiarized). Ultimately he employs a "visible hand" explanation for the social and conceptual development of science. Science is so socially structured that what is good for scientists is also good for science.

Bruno Latour, a French sociologist, and his coauthor Steve Woolgar with their classic *Laboratory Life* (1979) initiated an important stream of research, laboratory studies of science. *Laboratory Life* was the first anthropological-style ethnographic study of the goings-on in a scientific research laboratory, in this case, the lab of neuroendocrinologist Roger Guillemin at the Salk Institute in San Diego. It helped the interest in this study that Guillemin was awarded 1/4 of the Nobel Prize in Physiology and Medicine for the development of radioimmune assays of peptide hormones in 1977, the year after Latour's book was published. The authors declared right in their first chapter that "our very specific interest in laboratory life concerns the way in which the daily activities of working scientists lead to the construction of facts" (p. 40). Some themes of *Laboratory Life* are that acquiring credibility (Chpt. 5) and investing it in the production of scientific papers (Chpt. 2) are the central activities of scientific laboratories. According to Latour and Woolgar, there are at least five degrees of "facticity" of statements from the speculative to the taken-for-granted, and that in the process of research and writing, statements move back and forth along this scale (Chpt. 2). An analysis of the literature on the molecule TRF(H) (Chpt. 3) shows that citations to the key papers eventually declined even as the term TRF(H) continued to increase in the titles of papers, illustrating the transformation of its existence into a taken-for-granted fact. In this process, resources matter. Eventually defining the problem as the sequence structure of the TRF(H) molecule required equipment that only a relatively few could afford. Facts only have meaning in particular relational or network contexts. For example, outside of particular kinds of laboratories, TRF(H) is only some kind of a white powder. Chapter 4 analyzes some laboratory conversations to show how facts are "stabilized" by micro processes of interaction which are not fundamentally different in science than they are elsewhere.

Much of this would be uncontroversial even to very traditional sociology. However the use of the term "**social construction**" in the subtitle and elsewhere helped spark a long lasting and sometimes bitter controversy, "the science wars," over subjectivity versus objectivity or relativism versus realism in science. It seemed to many to suggest that the subjectivity of scientists is everything and the objective properties of the natural world mean nothing in science. Of course extreme subjectivity or relativism is easy to ridicule: "show me a cultural relativist at thirty thousand feet and I'll show you a hypocrite" (Dawkins 1995, pp. 31–2) and "just try negotiating the AIDS virus into a benign commensal" (Hull 1994, p. 505).

Once Latour got everyone stampeding in one (the constructionist) direction, he put on the breaks. "Do you believe in reality?" "But of course!" (1999, p. 1) and perversely, "I have disputed for twenty-five years that science is socially constructed" (1999, p. 299)! But not quite. More broadly, in books subsequent to *Laboratory Life* (e.g. 1987; 1988; 1993; 1999) Latour has made himself somewhat more clear. Modernity posited a radical separation between the natural and the social and that was never the case; they are one and the same, hence the title *We Have Never Been Modern* (1993). The perspective that both man (including scientists) makes nature and nature makes man (including science) might be called interactionist, mutually constitutive, or what he sometimes calls in noun form "articulation" (1999, p. 142). Most often however he disputes even that degree of distinction. Instead he claims to have "done away with the subject-object dichotomy altogether" (1999, p. 294).

Most recently, he turned to an evolutionary metaphor. In 2008 he gleefully described an exhibit in the New York Natural History Museum titled "a textbook case revisited." The museum exhibit illustrated two views of the fossil history of horses—a nineteenth century linear progressive story of small to large bodies, many to fewer toes, and short to long teeth, and a twentieth century bushily branching and diverse story of multiple lineages each adapted to their local milieu of which one ultimately survived to the present. Latour views these stories through a science studies lens (as it seems do the curators) not as stories of objects, but of our knowledge of objects. He wonders why the messy, complicated modern view of the history of fossil objects is so calmly received, while the similarly messy, complicated modern ("romantic? postmodern? reflexive? constructivist?") view of the history of epistemic objects is greeted with such consternation. He thinks the curators have displayed a healthy *"relativism"* (emphasis in the original), defined as "establishing relations between frames of reference through the laying down of some instrumentation."

Donald MacKenzie is a sociologist, one of the most accomplished of the participants in the sociology of scientific knowledge movement whose work is also interesting because he studied a social science—financial economics. His overall view of science (which does not come until the last chapter of this prize-winning book) is fairly simple. Modern financial theory has the character of a "cascade." Some innovations are stillborn, but in other cases lineages of influence can be traced, achieved sometimes through reading and sometimes through personal contacts including students. In this way modern financial theory spread and was cumulatively modified through business schools aspiring to a more science-based professionalism beginning in the 1950s but accelerating greatly in the 1960s and 1970s largely in the U.S. This research program prized plausible, innovative, simple mathematical models which could be used as a foundation for further research. Models of efficient markets, capital asset and options prices, and of portfolio selection were proposed that incorporated neo-classical economic assumptions. To simplify, any instrument priced significantly above or below its asset value and the risk-free rate of return (which could be achieved by the purchase of government bonds) would be subject to arbitrage. The over-priced would be sold, the under-priced bought, and hence financial markets should be efficient, i.e., tend towards an equilibrium at which prices fully reflect all publicly available information. (Of course models based on rational expectations and efficient markets do not explain recurring booms and busts, episodes of "greed and fear" as they are sometimes called, including the recent ones in the housing and credit markets.)

MacKenzie's thesis about this science however is expressed in his title: *An Engine, Not a Camera: How Financial Models Shape Markets.* The mathematical models of modern financial theory are not simply a camera, i.e., they do not simply describe financial markets, but they are an engine, i.e., they have shaped those markets. This is a form of constructionism; science does not just describe and explain the world, it constructs it. However, MacKenzie does not like the term constructionism/constructivism and instead he prefers **"performativity"** from the philosopher J. L. Austin through Barry Barnes (1983), MacKenzie's mentor. The use of a theory can make the world resemble more (or resemble less—"counterperformativity") the way the world is depicted in the theory. In a detailed history including interviews with many of its creators (Paul Samuelson declares in a blurb that MacKenzie "knows this exciting story, and tells it well"), MacKenzie searches for performativity in modern financial theory and eventually finds his best case in the Black-Scholes-Merton options pricing model. Many traders even began to carry computer-derived sheets of theoretical prices generated by the model for all options traded on U.S. option exchanges on the floor with them after they became available (Chapter 6). "The fit between the Black-Scholes-Merton model and empirical patterns of option prices was originally only approximate, but it improved rapidly after the model was published and adopted by market practitioners" (p. 256). In short, the model was "an engine, not a camera."

John Ziman (1925–2005) was an English physicist turned philosopher and sociologist of science who became a prolific author on the latter subjects (e.g. Ziman 1968; 1976; 1978; 1984; 1987; 1994; 2000). He describes a gradual transformation from the "academic science" which prevailed from roughly the mid-nineteenth century when the term "scientist" was invented, until the 1960s during and after which academic science has been increasingly replaced by "post-academic science." The culture of academic science was characterized by Merton's (1942) norms of communalism, universalism, disinterestedness, originality, and skepticism which Ziman dubs **"CUDOS."** It was financed by generous donors and governments; carried out by individuals free to set their own research agendas

Table 3. Issues

1) What is the unique idea that distinguishes their theory?
2) Do they think a general theory of science/scholarship is possible?
3) Are they historically conscious?
4) Do they think new things come into existence in science/scholarship?
5) What is the nature and pattern of change?
6) What is the mechanism of change?
7) Is their emphasis internalist or externalist?
8) Do they believe in progress or cumulation in science/scholarship?
9) Do they emphasize competition, conflict or cooperation?
10) Are they constructionist?
11) Do they think their theory is compatible with that of any of the others?

and to publish; built on previous work by others; motivated by recognition in their scientific communities; and was largely carried out in universities institutionally organized there, as well as more broadly in scientific and scholarly societies in disciplines. In return "grateful" scientists, through peer review systems, provided their societies and governments in particular with credible, reliable, reproducible knowledge and the latter drew on them for expertise and advice. By contrast in "post-academic science," the norms of **"PLACE"** replace those of "CUDOS"—propriety, local, authority, commissioned, and expert. Knowledge may not be made public, work is done on local technical problems, governed by a managerial hierarchy, commissioned to solve specific problems, and the scientist is valued as a technical expert rather than for creativity. Post-academic science may be carried out in a privatized setting or in a university but in either case it is industrialized with a high division of labor. According to Ziman, there was a reason why skeptical views of science which scorned traditional claims of its "disinterestedness," for example, became popular in this period—because science was indeed becoming less so.

III. Views on Issues

The issues we explored are listed in Table 3. We summarized issue (1): the unique features of their theories in Section II above. At the beginning of the interviews, we confirmed their views on issue (2): that they thought that a general theory of the scientific/scholarly process was achievable. The only author about which a "yes" answer to this is questionable is Fuller, who turned out to not be very interested in that kind of question. The other nine issues we explored are discussed in order in this section with all of the results (except for the last "compatibility" issue) summarized in Table 4.

Table 4. Some Properties of General Theories of Science/Scholarship

	Abbot	Bunge	Collins	Drori	Frickel	Fuller	Hull	Latour	MacK.	Ziman
Unique Idea	fractal cycles	novelty convergence	ritual chains & small no's.	globalization	social/ intellectual movements	publicly accountable	individuals & interaction	social construction	performativity	cudos to place
General Theory	yes	yes	yes	yes	yes	?	yes	yes	yes	yes
History	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Novelty	no	yes	yes	yes	yes	yes	yes	yes	yes	yes
Change: Nature & Pattern	branching cyclical	emergence convergence	—	branching	displace-ment linear	linear but	evolution branching	implicit evolution	implicit evolution	evolution linear
Mechanism	none	rational selection	emotional energy	none	implicit selection	—	selection	rhetoric	implicit selection	selection
Internal or External	internal	internal	both	external	internal but	both	internal	internal but	both	now external
Progress Cumulate	no	yes	yes	no	—	yes but	yes but	yes but	yes but	—
Competition Conflict or Co-op.	comp. conflict	—	conflict co-op.	—	comp.	comp. co-op.	conflict co-op.	comp. co-op.	comp. co-op.	—
Constructionist	no	no	no	no	no	yes but	no but	yes but	yes but	no

(3) Are They Historically Conscious? (Table 4, line 3)

Virtually all of these theorists display a strong historical consciousness which might be simplified as the view that everything comes from something, nothing comes from nothing. Bunge explicitly states that "whatever emerges does so from some pre-existing thing" (p. 30). Whether we are talking about Abbott's repeated fractal splits, Collins' interaction ritual chains and genealogies of influence among philosophers, Frickel's recruitment into and spread of social movements, Hull's giving credit with citations yielding lineages, MacKenzie's cascades, or on grander historical scales, Ziman's story about the history of modern science in the west or Drori's about the diffusion of western science into a global phenomenon—for all of these theorists the story of science, whether in particular or in general, begins with history. Even in the work of Latour and Fuller where this is less explicit that tends to be the case. Latour's analysis of citations to TRF(H) displayed how as the term spread, the once novel became taken for granted. Fuller everywhere writes about the history of the philosophy and sociology of science/scientific knowledge as well as that of cultural and social theory generally. Moreover, to most of them the influence of the past on the present does not take place in some ethereal realm of free-floating ideas. Instead, it is a matter of people directly influencing other people—whether in person or in print. Most of what scientists know or think they know comes from textbooks and the rest from their graduate school mentors, colleagues, journals, etc. and a relatively modest amount of it from their own research. It is also worth noting here too that Frickel seems to have independently converged on the Hull-Ghiselin thesis about the particularistic nature of historical phenomena. "SIMs exist as historical entities for finite periods" (p. 208) and opportunity structures are "a function of contingent historical circumstances" (p. 214).

(4) Do They Think New Things Come into Existence in Science/Scholarship? (Table 4, line 4)

Abbott is the outlier here with his cyclical theory of history. "*Chaos of Disciplines* is a pretty original book, even though it's a book about how there isn't any originality. Which is kind of interesting." "Good ideas come back again and again . . . at the level of general thinking about social life there aren't that many different ideas and we've had most of them before and they're going to keep coming back . . . we use new names for them." "In New York, medical licenseship came and went basically a dozen times in the 19th century. There must have been something like a total of eighteen projects to either make licensing more strict or make it less strict over the course of the century. It's not as if . . . there was this battle passed and now we have licensing afterwards. In fact, things can be dismantled."

"If you happen to have one of the great ideas, you're only renting it. It's like a house you get to live in for a while, and then young people push you out . . . of course they move in with new names and they redecorate . . . but the ideas are still the same ideas we just don't get to live them forever."

By contrast, for all of the others while the present comes from the past, it is constantly being modified in the process—for Bunge, Hull, Frickel, and MacKenzie the emergence of new disciplines, concepts, and social relationships, SIMs and models respectively are the explanatory focus. Mackenzie, for example, shows not just cascades of influence in modern financial theory, but how its models were cumulatively modified as they were passed on, and on a larger scale, how the whole movement toward mathematical models was a transformation from the old institutional study of finance which emphasized what is sometimes called "plumbing" such as the banking system. To Collins, intellectual life is composed not just of ritualized chains of influence; *creativity* at solving puzzles is the sacred object of those rituals. To Latour, facts are constantly being constructed in social interaction. To Ziman, modern science has been transformed from CUDOS to PLACE, and to Drori, science spread from the west to be sure, but it became "glocalized" in the process.

(5) What Is the Nature and Pattern of Change? (Table 4, line 5)

Evolutionists usefully distinguish between two kinds of similarity. Similarity can be homologous (due to a recent common history) or analogous (homoplastic, i.e., convergent) in which organisms without a recent common history nevertheless converge on a similar solution to a problem. For example the similarity in bone structure of the forelimbs in vertebrate groups as different as amphibians, birds, and mammals, despite the very different uses to which they are put, are homologous, while the similarity of body shape of fish and marine mammals representing independent adaptations for efficient motion in an aqueous medium are analogous or homoplastic. In science studies, historians are often impatient with philosophers who confound these. For example a philosopher might describe two theorists as "idealists" even though there is no historical connection between them. The philosophers may not necessarily be wrong about the similarity, it is just that what historians want to know is the actual historical pattern of connections. It was not surprising then that when we asked Hull with a background in philosophy whether he considered himself a philosopher or a social scientist these days that he replied: "In general I tried to be put in (with) scientists, systematics, actually doing science . . . history has been the hardest part. You just have to spend too much time reading carefully and checking references and what have you because if you're going to do work in a particular area you have to do it according to the standards of that area. Now philosophy, you can make up crazy examples . . . you do that in history, you're in real trouble, or sociology, you're in real trouble."

A variety of patterns of change are also possible. Change can be linear in which one kind of thing is gradually or rapidly transformed into a different kind (phyletic evolution or anagenesis). Ziman views change in modern science in this linear way, it has gone from CUDOS to PLACE. Similarly with Frickel, a new research program arises and "displaces" (or fails to displace) an existing one although he briefly notes that not all aim for dominance, they may instead simply carve out a niche for themselves. Alternatively, change can be branching, the generation of different clades, different lines of descent (cladogenesis). Abbott sees change in science in this way, repeated branching (on the same dimension) yielding a fractal pattern. Drori too emphasizes branching. "Every time an institution spreads around the world, depending where it lands, it already starts shaping itself a little bit differently becoming not a global or a local but rather a glocal institution. So in replication you replicate something but because it is situated in a somewhat different institutional environment it is no longer the same institution because it interacts, its boundaries are porous to different demands, to different relations, therefore it changes its own core." "We surely are imitating, but we don't replicate."

A variety of kinds of mergers are also possible in evolution. Bunge thinks the most important kind of change which takes place in science is emergence by mergers among different research programs, disciplines, etc. (what he calls convergence and compares to hybridization). The usage of the terms inter, multi, trans, and cross disciplinarity are quite varied in the modern science studies literature as too are the psychological and sociological mechanisms by which they can be brought about. For example an individual might apply facts, theories, or methods from one discipline to a problem in another; dual or multi-person teams of individuals with different disciplinary backgrounds can work together on a problem; or whole disciplines or segments of them can merge creating a new discipline. These are not unlike horizontal gene transfer, symbiosis, and hybridization respectively in biological evolution.

Stability as well as change is possible. Collins thinks that three to six alternative research programs can coexist in competition for a time. It is interesting to note that biological ecologists are even more stringent. They argue that if species are in "perfect" competition, then no more than one will persist, a principle called "competitive exclusion." If more than one species is occupying exactly the same niche, then their properties are unlikely to be exactly equally adapted to it. Of course multiple species do persist as do multiple research programs, topic, fields and disciplines in science—in part because they overlap in the niche only partly or not at all. To put it in Frickel's social movement terms, they are not attempting to recruit the same people. Indeed according to some evolutionary theorists, it can sometimes be precisely because it is adaptive to avoid competition that they have diverged in the first place.

Hull's evolutionary theory can in principle admit of all of these patterns. The distinction he is preoccupied with establishing between philosophical individuals and classes, however, makes hybrids difficult to cope with because they are neither—so he prefers to call them chimeras (p. 505). We have classified MacKenzie and Latour as implicitly evolutionary. In MacKenzie's case, that is not because he is particularly conscious of these distinctions, but because he tells many stories of "cascades," and also of individuals independently coming up with similar ideas, of individuals from different backgrounds getting together to collaborate, and so on. In Latour's case, it is not because of *Laboratory Life,* but because of the conclusions he seems to have drawn more recently from his museum experience. The theorists who do emphasize some particular nature and pattern of change, when specifically asked about others in our interviews, mostly readily conceded that this variety is possible. For example Frickel (whose own major research after all has been on the development of a new interdiscipline—genetic toxicology) conceded that patterns other than displacement are possible and Bunge conceded that patterns other than mergers are possible. Bunge: "one (pattern) is specialization and the other convergence. They have been going in parallel for centuries although reconvergence is relatively new—it appears only let's say about two hundred years ago with the emergence of biochemistry and biophysics and psychophysics and so on. The two processes, specialization and synthesis or convergence are necessary. I think they are—they should go on. One of the latest ones and most important is the emergence of cognitive neuroscience." When discussing the history of science studies Fuller tends to describe a linear picture. When questioned about this, however, he claimed that from Kuhn "you've got this very fragmented, diffuse, differentiated kind of history of science which nobody in their right mind can deny." The interesting thing is that he actually wants it to be linear, i.e., towards "improving the human condition."

(6) What Is the Mechanism of Change? (Table 4, line 6)

Neither Abbott nor Drori appear to provide a mechanism of change. When asked about this Abbott responded: "I'm confused by this stuff, it just kind of happens. I don't know, that's natural. I'll go back to Park and Burgess on this, competition is a natural state of things." And later he added: "I'm doing the same thing that Newton did, change is the primitive and that means you have to explain stability." Of course competition in and of itself does not provide a mechanism of change. Why, for example, do not alternatives always just go on competing indefinitely? Similarly Drori does not provide a mechanism. When asked why this particular religion, i.e., science, spread rather than just Christianity itself she did profer that progress is a theme from Christianity and that science "billed itself" as practically useful even though traditionally most technological innovation came from practitioners not scientists. Collins on the other hand does provide a mechanism, gain and loss of "emotional energy," but one which is difficult to accept. It is implicit in this that when we observe others gain or lose emotional energy from holding a particular position, we would tend to be swayed for or against that position. But why go to competition for emotional energy rather than to the Weberian-Mertonian tradition that science is about status, for admiration and emulation by our peers which Fuller does, for example, when he spoke to us of a "struggle for recognition"? There are other alternatives as well, including language and reason. For Latour, it is largely rhetorical devices in interaction that move statements back and forth along the scale of "facticity." For Bunge, it is "rational selection" by individuals. But that cannot explain the spread of something socioculturally. A person may learn to individually/rationally choose something but be unable to persuade others of it. That the goal is receipt of emotional energy however is particularly problematic given that it is long established in psychology that intermediate rather than high or low levels of arousal is most effective in performance (a principle so well established that it reached the status of a law, the "Yerkes-Dodson law" early in the twentieth century). Indeed extremely high and extremely low levels of arousal are the very definition of emotional pathology—mania and depression respectively. Collins has interestingly commented elsewhere that he went there because he needed a universal goal, what he calls "a common denominator of choice" (Collins 2005, p. 9).

Evolutionists like Hull do provide a common denominator, i.e., a universal mechanism of change—selection. Apart from rates of innovation and sampling error in finite populations, selection is meant to incorporate everything that causes something to spread. Ziman too eventually explicitly embraces a Darwinian-style sociocultural evolutionary theory of the process change in science (2000, pp. 276–288). Because selection depends upon interaction between properties of the social movement, say which is spreading, and properties of the environment, the reasons for success or the lack thereof can vary enormously. Some view these as ultimately historically specific; others hold out hope for more specific but universal laws.

We therefore view the kind of specific stories that MacKenzie offers for "cascades" of models and the general intellectual, social structural, and cultural reasons Frickel offers for why a SIM spreads as implicitly selectionist theories of the mechanism of change in science.

There are mechanisms of particular patterns of change as well. Consider for example branching. Drori sees this as taking place for essentially geographic reasons—the social milieu in different countries is different, thus glocalizing science, and she is probably right. In other cases however, branching may take place "sympatrically," as evolutionists say, for purely ecological reasons, or divergence can result from adapting to different niches in the same geographical area, or even because it is adaptive to be different, to avoid competition. Similarly, consider Bunge's pattern of convergences, i.e., mergers and the three different ways mentioned in (5) above about how they could be achieved. An individual might apply facts, theories, or methods from one discipline to a problem in another; dual or multi-person teams of individuals of different disciplinary backgrounds can work together on a problem; or whole disciplines or segments of them can merge creating a new discipline—similar to horizontal gene transfer, symbiosis, and hybridization respectively in biological evolution. In our questions and conversation about these Bunge kept mentioning new disciplines, e.g.,"biochemistry and biophysics and psychophysics and so on," but still wanted to maintain that "convergence should occur mainly in individual brains." Then paradoxically he went right on to discuss the importance of outsiders in the formation of new disciplines—e.g., like the expertise Crick (a physicist, specifically a crystallographer) brought to Watson (a biologist) and before that the father and son Bragg team bringing together traditional crystallography with a new tool, x-rays, and a mathematical tool—Fourier analysis!

(7) Is Their Emphasis Internalist or Externalist? (Table 4, line 7)

There do exist extremes in their relative emphasis on whether causal factors are mainly internal or external to science. Abbott commented: "*Chaos of Disciplines* is actually an internalist theory . . . The sociology of science community contrived to completely ignore this book . . . the key reason not to like it is that it's internalist." When Bunge was asked directly whether he attributes a lot of significance to external factors in science he was brief: "No. Except in cases where a dictatorship makes the research impossible or distorts research in the cases of Stalinism and Nazism—those cases of course." Yet at one point he offered an externalist explanation for several cases, e.g., he cited admiringly the work of an Argentinian who early in the twentieth century wrote on the *Sociology of Philosophy* and he explained away Ziman's emphasis on societal influences on science that way (see 12 below). On the other hand, when we suggested to Drori that since she does not view science as functional, that would be consistent with Merton's view that it is mostly about seeking status from peers, she disagreed. "Science depends so much on external resources, then why would he assume that all the satisfaction you get is from each other." Also after describing the particularly strong growth of the social sciences since the Second World War she said, "Clearly it was the era of policy, of public policy. And science has really been instrumental in the world of the fifth branch of government. Science is the science of policy making these days." Embracing both extremes, Ziman's theory of the transition from CUDOS to PLACE implies that modern science has shifted from largely responding to the internal to largely responding to the external. However, the majority includes both—we have put Collins, Fuller, and MacKenzie in that category. While Frickel's emphasis is clearly internal he makes a point of noting: "Although the fields in which SIMS develop are relatively autonomous from wider cultural and political-economic contexts, we take it as axiomatic that SIMs are influenced by direct or indirect pressures emanating from the broader cultural and political environments" (p. 209). Similarly, MacKenzie, while mostly preoccupied internally, often refers to external conditions that affected various developments, e.g., institutional changes in the environment that made trading in financial futures respectable, specifically that the opening of the Chicago Board Options Exchange (Chpt 6) was assisted by the Nixon administration. "Attention to performativity does not render redundant economic sociology's focus on the embedding of economic actions in cultures, in political systems, and in networks of interpersonal connections" (p. 263). Hull shifted ground. In his book he implies that science is largely an internal matter by typically equating selection with "checking," i.e., evidence. But in our interview he admitted that his completely ignoring that science exists in society there was a bit dense. He claims to "always pay attention to my critics" and that seems to be the case. He amusedly explained: "I was getting financial aid from the National Science Foundations' little subset for history and philosophy of science. So they (the university) didn't have to pay me a salary for (he calculates). For twenty years they never had to pay my salary. And I got money for the university . . . Did it ever occur to me that money was that important? No, I see it in retrospect. At the time I was doing my stuff I was internalist, idealist, all those things!" Latour too is fascinating in this respect. In *Laboratory Life,* the emphasis is largely internal although even there he saw the role of funding as important. In *The Pasteurization of France* (1988) on the other hand, the emphasis is almost the opposite. There he is much concerned with explaining the broader context of just how and why Pasteur's work was received both favorably and unfavorably in various quarters in France, albeit in true Latourian fashion, he wants us to understand how Pasteur made France as much as France made Pasteur.

Not surprisingly, those who emphasize the external are more interested in the relationship between science and society. Obviously Fuller has the strongest interest in this with his emphasis on the social responsibility of science and the need for more public participation in decision-making in science. There is a great difference among theorists in their attitudes towards the relationship. Basically traditionalists like Bunge think science needs to be defended from society while radicals like Fuller think society needs to be defended from science.

(8) Do They Believe in Progress/Cumulation in Science/ Scholarship? (Table 4, line 8)

Frickel's view on this tends to be the norm in science studies in adhering to Bloor's (1976) strong program in the sociology of scientific knowledge which purports to be impartial with respect to the truth or falsity of a belief, i.e., to be symmetrical in its explanation of both. Hence Frickel avoids making these kinds of judgments: "SIMs may be progressive, pushing the field forward in new directions, or reactionary, urging a revival of past ideas to counter what are perceived as pernicious current tendencies" (p. 208). Ziman's voice in his book too is one of studied, objective neutrality towards the changes that he sees as having taken place in modern science. But despite this neutral voice influenced by modern science studies, from his previous work we in fact doubt that he liked these changes very much and regretted that because of his untimely death, we did not have the opportunity to ask him about that.

A minority of the theorists gives a pretty unqualified "no" or "yes" to this question but more complex positions are more common. Abbott responded: "I don't believe in that much." "We're all under the sign of this progressivist ideology of the nineteenth century that . . . we're going somewhere. And maybe we aren't going anywhere. Just going." We infer that Drori too tends towards the negative on this point because of her view that the spread of a scientific ethos as the new religion (as opposed to labor-force training) has not been particularly practically useful nor contributed to development or democracy around the world. Collins on the other hand responded: "He (Abbott) says you never accumulate. Well I don't think that's true. If you take sort of a pragmatic standpoint, there are quite a lot of areas where we feel like we can subsume this stuff that was done 100 years ago. I think that political sociologists are better than Max Weber now. Max Weber was very good but the really good ones, Michael Mann is one that I like, I think we know more about certain things about the state than we used to. And I don't doubt that's true in many of the physical sciences." Bunge too gives an unequivocal "yes" answer to this question. He thinks that decision making in science is largely a matter of rational choice, "rational selection" by individuals, but also emphasizes that mergers of research programs are normally a good thing, particularly in the social sciences, and particularly for solving practical problems.

Some of the more interesting "yes but" views explicitly or implicitly separate cumulation from progress. Latour generally sees cumulation to a degree but does not view that as progress particularly. MacKenzie's "performativity" is a good example of that. The models he studied were definitely cumulative but does that imply progressive in the sense of coming to approximate reality more? At times yes, where performativity is present, but that is because they create their own reality. Moreover models built on rational expectations and efficient markets in his view could not explain the crash of 86 let alone, he would surely think, the most recent one. Fuller falls into this camp in his own way. "I'm not very attracted to this idea of truth as the end of knowledge—it doesn't tell you very much . . . You can get reliable knowledge about all kinds of things . . . Nazis can get reliable knowledge . . . Why (do) we want knowledge in the first place . . . we're trying to do something to improve the human condition." Elsewhere he makes it clear that it is not progress unless it is progress towards socialism (2006b).

There are four elements in Hull's view on this. First, by equating selection with "checking," i.e., with logic and evidence, he tends to create the impression that he believes in cumulation and progress. Secondly however, when directly addressing the question he emphasizes that like biological evolution, while it may be locally progressive, "it can (only) be made to look globally maximizing by careful editing" (p. 474). This is the view that Latour arrived at through his museum visit. Thirdly, in a sociologically sophisticated "however to the however," Hull also believes that the institution of science is (happens to be?) socially-structured in such a way that what is good for scientists is also good for science. He agrees with Merton that scientists seek status among their peers ("credit" he calls it). But in the scientific process, two conventions are important. The first is that the first published gets the credit which militates against secrecy in science, i.e., it forces scientists to make their work public so that others can use and build on it. But of course mistakes, publishing non-repeatable results, bring discredit, so a balance is created between ambition and recklessness. The second convention of citations—showing that work flows from other well-established work—increases its acceptance, but also detracts from claims to originality so a balance is created between continuity and innovation. These conventions of the institution of science, Hull believes, typically make self-policing in science more effective than elsewhere in society and tends to create the impression again that he believes in cumulation and progress. Fourthly, in his research Hull described many examples of overt conflict in science such as priority disputes. That should not be good for science as a whole any more than a biologist would claim that what is good for one particular species is necessarily good for the eco-system. Subsequently therefore he came to admit, that while still defending his visible hand explanation, that "this discord can hold back the progress of science" (1977). Actually, one could argue over whether, as implied by the subtitle of his book "an evolutionary account of the social and conceptual development of science," he did or did not successfully turn an evolutionary into a developmental theory of science.

(9) Do They Emphasize Competition, Conflict or Cooperation? (Table 4, line 9)

We certainly confirmed in our interviews that those who study science at least compete for status. When asked about how satisfied they were with how their theories had been received, only one expressed unreserved satisfaction. Drori replied "very nicely," which made us wonder if this is a gender thing. Bunge, the author of over 80 books, complained that he has always had a problem getting his books published! Abbott noticed that sociologists of science had not reviewed his book. Latour complains in print that he does not recognize himself in others' accounts of his work: "the science warriors too often waste their time attacking someone who has *the same name* as mine" (emphasis in original 1999, p. 299). Hull, who is about as prominent a philosopher of science or at least of Biology as one can be, complained "[I am] not as satisfied as I wish I were. It just takes so long to get the news out." But he also added "that's a characteristic of science in general, not anything particular to me." Even when the tone of the response tended towards the positive, there usually were significant "buts" attached. Frickel thought pretty well, but complained that social movement people have not really paid much attention. Collins told us about the various translations that had been done of his book including to Chinese, Russian, and Spanish, but explained that "since I regard the intellectual world as conflict between different factions, I wouldn't expect to get more than the support of a particular faction." Fuller expressed satisfaction that the journal he founded has lasted for 20 years so far and mentioned a couple of students he had produced. But he also fretted, "if there's a problem, it (his research program) hasn't really been embraced by any particular discipline, it's really kind of more free-floating inter-disciplinary program at this point so I think it's institutional future is pretty uncertain."

Bunge, Drori, and Ziman do not seem particularly interested in this question. When Drori was asked directly about the role of conflict and cooperation in science she responded, "They both conceive of the process as a functionalist one and here is where I have a problem with it . . . the role (of science in society) is not as a functionalist institution that reaps particular rewards for us." Generally there is more of an emphasis on negative than on positive social relationships in this literature. Abbott: "Competition is a natural state of things." Collins: "I regard the intellectual world as conflict between different factions." Frickel: "Intellectual life is constituted around oppositionality." Fuller spoke of scientists "battling over common turf." Most however admit (or at least by the time of our interviews admitted) of positive relationships as well. Despite the fact that Abbott's book emphasizes competition in initial splits and implicit conflict in the subsequent ingestion, he added to us that "cooperation is something that just happens like competition . . . People are always in interaction . . . sometimes it's conflict, sometimes it isn't." Collins noted that there are "multiple conflictual groups (in science but) the groups themselves have Durkheimian solidarity to them." He then referred to his more recent book on violence in which one theme is that violence is actually rather difficult for people to engage in to make the point that recently "I've found the Durkheimian space kind of more available." "I kind of come back much closer to Talcott Parsons' position, or certainly Durkheim's position." When we challenged Frickel in our interview that perhaps competition and conflict relative to cooperation may have been overemphasized in his work and in science studies more generally, he admitted that may be the case. In his book, Hull described social relationships in science as a mixture of conflict and cooperation, cooperation obtaining particularly among those with historically related philosophical, theoretical, or methodological views. While reviewers tended to think that he overemphasized the nasty side of science, he claimed in our interview that "I think I may have emphasized the cooperation more than the competition," i.e., the visible, like the invisible hand, implies overall efficiency. Both Latour and MacKenzie describe many examples of competition for status by publishing but also describe much collaboration in research as well. Fuller is interesting in this respect. While emphasizing competition, what he seeks is cooperation. His ideal of "republican science" is the combination of "individual freedom and collective responsibility" (2007a, p. 109) and the latter is why Collins called him a utopian in our interview.

There is a lot of confusion in the science studies literature as in the social science literature more broadly surrounding the concepts of competition, conflict, and cooperation. Historically sociologists, for example, tend to distinguish between conflict (as in Marxism or conflict theory more generally) and cooperation (as in functionalism). Traditionally, neoclassical economists recognized competition and cooperation but defined conflict as outside their subject matter! Biologists usefully draw distinctions among all three. Competition exists when utilizing the same resource(s). If resources are plentiful, the relationship can be 0 0, if scarce it can be − −, i.e., what one gets another does not so each is limited by the presence of others. In either case however there need be no actual social interaction. For example in science, one can pursue one's own research program while simply ignoring rather than attacking or supporting that of others. Cooperation and overt conflict on the other hand do involve actual social interaction which can be mutually beneficial + +, antagonistic + −, altruistic − +, or even (rarely) − − spiteful. All of these kinds of social interactions can be favored under similar conditions (e.g. crowding) but also under different ones (e.g. degree of relatedness as Hull emphasized). Social scientists however tend not to make these distinctions, hence my use of "positive" and "negative" social relations above, i.e., terms like competition, conflict, opposition, etc. mean pretty much the same thing there. However, in this science studies literature and our interviews there are hints of recognition of the important distinction between competition and conflict. For example Frickel distinguishes between competitor and countermovements (p. 216) and writes: "intellectual life is constituted around oppositionality . . . but can vary with respect to how they go about formulating these characterizations and critiques. This can be done polemically . . . or in a sophisticated fashion" (p. 224). "SIMs are rarely contentious in the same way that political activism is contentious" (p. 226). In our interview Abbott stated, "Competition is a very general mechanism for me. It's not like this focused cut-throat disorderly thing that we have sometimes (assumed)."

(10) Are They Constructionist? (Table 4, line 10)

Some of the difficulty in answering this question stems from the fact that to be constructionist (or constructivist, used interchangeably) has taken on more variety of meanings than the terms commonly used to define most issues in social and cultural theory. Virtually no one today uses "construction" in the original Berger and Luckmann (1967) sense of an emphasis on the micro over the macro. Probably the most common meaning it has taken on in social and cultural theory more broadly implies just an emphasis on sociocultural over biophysical causation, i.e., to do social science. That is the sense in which Collins declared to us that: "Social constructionism is so deep in everything I'm doing." That is the only sense in which Frickel too could be considered constructionist: "We . . . take it as axiomatic that SIMs are sui generis social phenomena." Similarly for Drori "it's not because anything has changed in our physiology!" and Abbott as well: "Well, I'm pretty constructionist in the sense that I think that I assume the pragmatist theoretical tradition. I think social life is made. We make everything. We make our ideas . . . so I'm a pretty thoroughgoing constructivist in that sense."

But that is not the sense of scientists "constructing" facts, for example, with which Latour originally stirred up such a controversy in science studies. It was rather implying or being interpreted as implying an emphasis on subjectivity over objectivity or relativism over realism which did. Bunge most definitely is not a constructionist in that sense; indeed he subsequently published a book defending realism (2006). Nor is Ziman for the most part, nor Abbott. "What happened to the sociology of science is the generation came out in the 1970s and decided to just, you know, kick everything out in the teeth and so on, and then they get on this ludicrous constructionist stuff." "I really gave up reading the sociology of science because the sociology of science was just so caught up with these ridiculous debates about social construction and it just seemed incredibly silly to me." We think it is fair to classify Latour, MacKenzie, and Fuller as "yes, but" on this question. The "but" in the first two cases is because Latour later either backed off that position or claimed to have been misunderstood, and MacKenzie prefers his own term. Fuller declares "social constructivist accounts of science are largely correct" (p. 11) but claims elsewhere that the difference is that realism holds propositions to be true irrespective of place and time and constructivism does not," i.e., he equates it with historical specificity. Our "no, but" interpretation of Hull is derived from the following. While not a constructivist proper, he does concern himself with his own subjectivity, "reflexivity." When he says scientists think this or do that, he typically stops to check his conclusions by reflecting on his own thoughts and actions. "If I am engaged in the same activity as my subjects, then anything that is true of them had better be true of me" (p. 6).

(11) Do They Think Their Theory Is Compatible with that of any of the Others?

One of the things that most surprised us in the interviews was how unfamiliar most of the authors were with the work of most of the others. We were prepared for some of this and so, as much not to embarrass them as anything else, we had potted summaries of the work of others available if they wished to comment on them anyway. In providing these, we could not always be certain of the extent of their previous knowledge, but most of the time they were frank about that, Abbott perhaps most of all: "I don't actually read other people's theories at all." And later, "I read lots of things, I read, but what I read is data. So I read ethnographies, I read novels, I read case histories, I read stuff, I read history, I read *about* things, but I don't read other people's theory. Ever. Haven't done that since the 80's." In any event, the most informed author seemed familiar with about half of the others. This clearly showed that in choosing general theorists of the scientific/scholarly process, we had not chosen what evolutionists would call a "natural" or "homologous" group, i.e., one the similarity among the members of which is due to a recent common history. Rather, the group was "analogous" or "homoplastic," i.e., one whose members had converged on a broadly similar topic from different historical backgrounds. Given the different disciplinary backgrounds, it was not that we expected to see homology. It was just that given the high level of generality of all of their theories, we expected they would have been scanning the intellectual horizon so to speak, and would have picked up on others aiming to occupy a similar niche, whether pro or con.

Hull, sociological convert that he is, was most socially astute in attempting to provide as big a tent as possible. He had not read Collins but after we told him about it, his reaction was that perhaps he should. On Collin's emphasis on the importance of face to face interaction, his reaction was "yeah, yeah" and when told about the law of small numbers his reaction was "sounds right to me." When the emphasis of other authors' differed from his, he incorporated it and then went on to extend it. For example on Bunge's emphasis on mergers he noted: "splitting is difficult; merger is even more difficult. The question is why. Splitting is not as common as we thought it was, merger turns out to be more common than we thought it was . . . Now how different and how common is merger in all these areas whether it's biology or science studies? We have to do empirical research because there's no a priori answer to that question." In a review of Ziman's book in *Nature* he had previously called Ziman's view of science "detailed," "realistic," and "well-rounded." In our interview he stated: "I made my stuff sound as Mertonian as possible because I was aware of his school . . . plus I thought there was plenty to say. Ziman approached me and wanted to start developing a working relationship which was fine with me. But then he died. Science evolves. He (Ziman) looked back, (and saw that) science has changed through time. And I think he's right. Do we now have finally the right notion of science? No, it keeps changing." He sometimes used the opportunity of any congruence to drive home a theme important in his own work. With respect to Drori, he talked about the importance of variation in sociocultural evolution (using the example of variation in tenure requirements in universities), and, while unaware of Abbott's work nor apparently of what fractals are, he discussed mechanisms of speciation including the sympatric (geographical) and allopatric (ecological). Given that we, the interviewers, were sociologists: "Of all social scientists, I like sociologists the best, like being the tallest midget in the freak show (laughing)."

Collins was among the more knowledgeable about the others and was generally conciliatory or at least polite towards them as well, but often with a "but" attached. He had reviewed Abbott's theory favorably in print as compatible with his own as long as the law of small numbers was added. In our interview however he also made a point of disagreeing with Collins' view that there is no cumulation in science. On Hull: "Well I don't know this in detail . . . there is one point that I think has not been well picked up by people. Biological evolution looks like a candelabra with branches and branches and some branches end . . . Continuous branching with a certain amount of dying off. The intellectual world has that in common but the biological one doesn't have a law of small numbers as far as I can see. You can have thousands of species whereas the attention space that humans can deal with seems to be much more limited than that." He claimed to agree to some extent with Latour, Ziman, and Fuller. "I actually like Bruno Latour quite a lot and sort of incorporated some of his stuff . . . I particularly like the idea about networks of laboratory equipment." "I like Ziman too actually . . . he's more narrowly a philosopher of science per se." On Fuller's view of the need for broader participation in decision making in science he agreed but opined: "I'm not as utopian or optimistic as Steve Fuller is."

At the opposite extreme, Bunge was the least accommodating of others although his criticisms were focused and to the point. On Abbott's fractals "it clearly seems to me a wild fantasy . . . it's a very old hermetic tradition that the microcosmos and the macrocosmos are similar." On Drori, "science was global in the 17th century: a network of about 200 scientists who participated and collaborated." As for the cultural authority of science, "I wish it had." "There has been a (postmodern) reaction against science in the States and in western Europe . . . that you won't notice in Asia or Latin America." "About 17% of students are enrolled in science and technology courses in the States and roughly the same in Europe nowadays. In China and India it's twice as much, 34%. In India, China, Japan, and Latin America science still enjoys great prestige. And they are increasingly producing science." Despite having claimed in his text that his "present work is partly a belated spinoff of evolutionary biology," when asked about Hull's evolutionary theory of curiosity, credit, and checking in science his response was: "Of course selection—one selects, one replaces a hypothesis with another one—one eliminates what one regards as falsity. But the analogy is completely superficial . . . Popper says the same thing," and he went on to contrast "lawful" natural selection in biology with "rational" selection in science. He explained away Ziman's theory of PLACE on the basis of the kind of physics Ziman originally did. "Ziman was a specialist in solid state physics and solid state physics is applied science and very close to engineering. Solid state physicists are asked to study special materials—for instance semi-conductors that have direct applications to computer science, the computer industry. And so of course that kind of work is closely supervised by managers and it is subsidized by big companies and so on." He went on to argue that is not the case with astrophysics, psychology, pure mathematics, particle physics, or paleontology, for example. He also added his view that the current pressure exerted by governments including the Canadian one on academics to work with the private sector is "pragmatism in action" and "destructive of science—basic science."

Drori too was not shy about disagreeing with some others but not exclusively. On Hull, "we (she and her coauthors) give a lot more credit and power to the interaction between science and other institutions," i.e., they are more externalist relative to Hull's internalism. On Ziman, "I disagree with him . . . I do not think that science became more applied." "I come from Stanford, which is mythologized as the hub of silicone valley and where undergraduate students struck it rich creating Google, so it's not that scientists are blocked from making money, it's just that they will not describe it as such. They talk about self satisfaction and contributing to community, it's more relevant than applied." She was not familiar with Abbott's work and went on to talk about glocalization, but she chose to interpret Fuller as "a call for maintaining scientific freedom" and so agreed with him.

Frickel is pretty balanced in his reaction to the theories of others, sometimes agreeing, sometimes disagreeing. He worked Collin's competing for "intellectual attention space" (p. 205) and his "law of small numbers" (p. 216) into his theory but he also thinks Collins places too much emphasis on competition for status which "unnecessarily flattens out variation in the motives of intellectual actors" (p. 211). In his view, intellectual dissatisfaction is a prerequisite. He cites Latour (pp. 208, 221, 227) and Fuller (p. 204) in a more formulaic way than anything else, but when asked whether he agreed with Fuller that science should become more responsive to the public interest his answer was definitely positive, referring to he and Fuller as being on the same wave length even though there was no mention of that issue in the target article. Fuller agreed with Drori as he put it that "the greatest impact that science has on people, not just scientists but people in general, is at the ideological cultural level." But he also thinks that "if you were to ask a scientist, give them enough time to come up with their own sociology, it would kind of look like Randall Collins . . . it's purely internal . . . larger political and economic changes and so forth are pretty much (a) backdrop." Latour's book was published before any of the others so we would not have expected him to take their work into account, but while published later than most, MacKenzie is also light on reference to the others. He only cites some of Collins' earlier work and Latour on the constructed nature of the social world (p. 26).

IV. Conclusion

We conclude that it is a powerful argument in its favor that a Darwinian sociocultural evolutionary theory can incorporate both all of the common and all of the useful unique features of contemporary grand theories of the scientific/scholarly process. This general view of science was pioneered by Stephen Toulmin (1972) and David Hull (1988) which is not necessarily to defend every aspect of their theories. However, we conclude this for nine reasons which correspond to the discussions of issues in (3) to (11) in Section III. Evolutionary theory's twin pillars of sociocultural descent with modification incorporate both the traditional and the innovative nature of science and scholarship. We truly do both build on the shoulders of others but do so creatively as well. While sometimes change can be cyclical as we revert to older ideas, very often novelty instead emerges. Evolutionary theory recognizes all of the patterns of change that have been observed in science by the theorists discussed—linear, branching, converging, and merging. It offers a universal mechanism, selection, which (along with innovation rates and sampling error in finite populations), can be understood to include all of the historically particular and generalizable reasons why social movements embracing different concepts, theories, methods, research programs, etc. spread or do not spread successfully in science. It recognizes that many factors both internal and external to the culture and institution of science are important in its evolution. It accepts that cumulation and progress towards improved understanding are common in science but not universal. No credible theory of science could ignore the follies of phrenology, Lysenkoism, Nazi race science, or cold fusion, for example. It includes all of competition, cooperation, and conflict. Competition is ubiquitous in science as elsewhere, but common too are both cooperation and overt conflict. The latter are both means by which competition takes place under some circumstances. Less well known is that evolutionary theory also incorporates the subjective or constructed as well as the objective or structured nature of scientific knowledge. (Biologists, for example, have recently come to appreciate more so than they did in the past that organisms construct their environment as much as they are structured by it; see Lewontin 1983; Odling-Smee et. al. 1996; 2003.) In science, new environments both internal and external to the institution itself can restructure old ideas, new ideas can reconstruct old environments, and sometimes both can even occur simultaneously so that they structure and construct each other—are mutually constitutive more or less as Latour has it. In conclusion, historical social science recognizes diversity and change, postmodernism celebrates them, but only evolution can explain them.

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