

The Cognitive and Social Lives of Paradigms in Information Science

Abstract: Thomas Kuhn's ideas, particularly of paradigm, are used with some frequency in information science. The usages of paradigm and the problematic nature of Kuhn's thought are explored. Alternatives to Kuhn are suggested as a way out of the confusion his thought leads to.

Résumé : Les idées de Thomas Kuhn, plus particulièrement le paradigme, sont utilisées assez fréquemment en science de l'information. Les utilisations du paradigme et la nature problématique de la pensée de Kuhn sont explorées. Afin de contrer la confusion entourant la pensée de Kuhn, des alternatives sont proposées.

1. Introduction

Thomas Kuhn's *Structure of Scientific Revolutions* (referred to as *Structure* henceforth) was published in 1962, with a second, enlarged edition (with his Postscript) appearing in 1970. Very few works have had the impact that *Structure* has had. According to Web of Science®, *Structure* has been cited 9,268 times from 1990 to March 5, 2007. Its influence may be greatest in the social sciences, including the history and sociology of science. There are some features of *Structure* that have especially captured the attention of scholars—the ideas of normal science, revolutionary science, and paradigms. It is perhaps the notion of paradigm that is most essential to Kuhn's theory (if "theory" is the appropriate word for his thought). That notion has been a successful way to conceive of the force of what Kuhn calls tradition (the ways and means that scientists, collectively and individually, operate in a social and psychological dynamic). The realization of the social in the practice of scientists (that is, in the one particular form of human action) is very important. The field of information science is one that appears to have incorporated Kuhn's thought into its discourse, at least as citation to *Structure* suggests. The question is: *How* is Kuhn's thought appropriated; to what extent does it inform the work of information science? That question is explored here.

Before proceeding to examination of the information science literature, a somewhat hermeneutical examination of *Structure* is called for. At the heart of *Structure* is description of the practices of scientists; as was just mentioned, this is an important recognition. The practices of scientists and the norms of science are not necessarily the same, and they should not be confused with one another. An example of Kuhn's analysis of behavior is an explicit statement he makes early in *Structure*: "Normal science, for example, often suppresses fundamental novelties because they are necessarily subversive of its basic commitments" (p. 5). In explicating the concept of paradigm Kuhn writes, "When, in the development of a natural science, an individual or group first produces a synthesis able to attract most of the next generation's practitioners, the older schools gradually disappear. In part their disappearance is caused by their members' conversion to the new paradigm" (pp. 18-19). The behaviorist thrust of *Structure* is apparent in that

statement. In the Postscript, though, Kuhn seeks to clarify his definition of paradigm to introduce a normative element to the definition:

the term “paradigm” is used in two different senses. On the one hand, it stands for the entire constellation of beliefs, values, techniques, and so on shared by the members of a given community. On the other, it denotes one sort of element in that constellation, the concrete puzzle-solutions which, employed as models or examples, can replace explicit rules as a basis for the solution of the remaining puzzles of normal science (p. 175).

Structure, while decidedly problematic, did introduce some new ways of looking into the practices of scientists. Components of the social dynamics that typify some of the periods of “normal science” are essential to an understanding of scientific practice. Less explicit in *Structure*, but clearly described, are some psychological factors that attend to individuals’ and groups’ reluctance to abandon the paths they have taken.

Kuhn’s idea of paradigm has found many followers. Massimiano Bucchi (2004) writes, “the emergence of a paradigm signals that a research sector has consolidated itself into a scientific discipline” (p. 27). Science and paradigm are effectively the same; it is the notion of some collective or consensual definition that constitutes a science. Sometimes the interpreters of Kuhn are questioned; “Kuhn himself was simply seeking to show how science progresses through a combination of slow developments within particular puzzles and rapid, revolutionary bursts when one paradigm comes replace another” (David, 2005, p. 15). Peter Dear (2001) ascribes to Kuhn a particular philosophical attitude wherein the historian of science avoids preconceptions (p. 261). This unlikely belief would have Kuhn admit to following Edmund Husserl’s early ideas of phenomenology (that Husserl rejected later in his life). One of the most ardent admirers of Kuhn is Alexander Bird, who asserts that Kuhn’s thoughts constitute a genuine theory. Bird says that Kuhn’s impact on the social sciences, “had two aspects; the first was a change in the social science’s self-perception, the second was a suggestion of a new role and subject matter for the social sciences” (p. 267).

While there are numerous apologists for Kuhn (and they frequently defend him by offering creative readings of *Structure*), many philosophers of science recognize some of the conceptual and argumentative shortcomings of the book. One criticism that crops up is expressed succinctly by Irwin Sperber (1990); Kuhn fails to recognize that government and corporate entities,

do control (1) the flow of funds for advancing or inhibiting the development of one paradigm rather than another as well as (2) the appointment of many of the most prominent scientists to direct powerful government agencies and great tax-exempt research foundations and therefore (3) the influential sectors of the audience to which a scientist presents his discoveries and paradigm proposals (p. 85).

In fact, in the most recent year for which data are available the top 100 universities received almost 81% of the total amount of funding awarded by the U.S. federal government (about \$23.5 billion out of \$29.2 billion). The amount and the proportion are noteworthy (Top institutions, 2007, p. A24). W. H. Newton-Smith (1981) points out a difficulty with Kuhn’s historical analysis. While Kuhn maintains that the shifts in the explanation of gravity from the seventeenth to the eighteenth centuries constitute an

alteration in the standards of explanation, “this undoubted transition is a transition in beliefs about what can be explained. There is no reason to think it represents a change in the very criterion of what counts as a good explanation” (Newton-Smith, 1981, p. 111). A damning observation is offered by the philosopher Stephen Toulmin (1970); in relating that Kuhn at first posited an absolute change from normal to revolutionary phases of science, then back-peddling from that stance, says that Kuhn destroyed the original distinction between normal science and revolutionary science (p. 41). Kuhn’s most vocal critic, Steve Fuller, observes, “To understand the overarching significance of *Structure*, . . . we need to start taking seriously that Kuhn’s book constituted. . . less a revolt against positivism than a continuation of positivism by other means” (2000, p. 287).

The Problems of Paradigm

The uses of “paradigm,” and of Kuhn’s work in general, are varied, but there are some commonalities that can be discerned. For the most part, Kuhn’s ideas tend to be received (that is, accepted) in the social sciences, but not analyzed in much depth. There is little questioning of his notions of normal science, revolutionary science, incommensurability, and paradigm. Kuhn’s own thought is simultaneously confused and attractive. While he has attempted to clarify the definition of paradigm in the Postscript, its meaning is still elusive. The description of a constellation of beliefs is useful, especially in that it directs inquirers towards definitions, questions, methods, and perhaps even values that those practicing in a discipline may share. What the description does not do is guide an outsider of that discipline towards the nature of the sharing—agreement, dispute, specific research projects as they fit into paradigmatic science, and external influences (including institutions and funding). Also, the kinds of examples and models of puzzle-solving are inadequately addressed. Kuhn emphasizes that the puzzle-solving element of paradigms can be taken as a response to anyone accusing him of relativism or flight from rationalism. A question that arises is related to the rationality of revolutionary science and Kuhn’s own claim that normal science, to some degree, stifles new or opposing ideas.

Perhaps the most telling statement in *Structure* is, “A paradigm governs, in the first instance, not subject matter but rather a group of practitioners. Any study of paradigm-directed or of paradigm-shattering research must begin by locating the responsible group or groups” (p. 180). If the reader focuses on the word “governs,” she or he can interpret the behavior based on paradigms as normative, rather than simply descriptive. The interpretation could hold that the paradigm is more than the shared beliefs and puzzle-solving techniques; it is the set of beliefs and puzzle-solving techniques that the community of scientists *should* agree upon. An even stronger interpretation would hold that the paradigm is *constitutive* of the community; it is the determining factor around which communities of scientists are formed. This recognition of implied normativity signals Kuhn’s confusion. He is clear that the scientist who looks at the world through the lens of a paradigm can see only what that paradigm points to. The inconsistency is the outcome of the limits to vision—does it enable the scientist to focus on the puzzles and the best ways to solve them, or does it distort the puzzle?

Incommensurability

The statements of Kuhn’s, just quoted, are not just a rhetorical aberration; it summarizes Kuhn’s thinking effectively. It is at the heart of the evidence for Steve Fuller’s conclusion, “Kuhn was indeed authoritarian and Popper libertarian in their attitudes to science” (p. 9). The reception and use of Kuhn’s program, as will be shown, bolsters Fuller’s observation. Fuller’s invocation of Popper is also instructive; Popper

(1994) articulated his vision of scientific work very simply; it can be summed up as “problems—theories—criticism” (p. 101). The third component—criticism—is the indicator of the biggest difference between Kuhn and Popper. The reception of the notions of paradigm, normal science, and incommensurability tends to take at face value Kuhn’s claim, “That scientists do not usually ask or debate what makes a particular problem or solution legitimate tempts us to suppose that, at least intuitively, they know the answer. But it may only indicate that neither the question nor the answer is felt to be relevant to their research” (p. 46). Can paradigms be both constellations of beliefs and the following puzzle-solving action *and* more important than the problems to be solved? On the other hand, if paradigms are so powerful and binding, then Kuhn’s concept of incommensurability can be more readily understood. It could be that even the scientists committed to conflicting paradigms are indeed practicing in different worlds, but the worlds are not epistemological or ontological. The “worlds” may mean something different; they may be linguistic. The worlds could be defined and differentiated by “success”—by some genuine explanatory (and possibly even predictive) advances, but also by prestige, notoriety, awards, reputations, and external funding. As Kuhn puts it,

That professionalism leads, on the one hand, to an immense restriction of the scientist’s vision and to a considerable resistance to paradigm change. The science has become increasingly rigid. On the other hand, within those areas to which the paradigm directs the attention of the group, normal science leads to a detail of information and to a precision of the observation-theory match that could be achieved in no other way (pp. 64-65).

Incommensurability, then, may not be linguistic, ontological, or epistemological; it may be social and psychological. In other words, it could be that the defenders of an existing paradigm, instead of saying, “I do not understand your position,” are saying, “I do not accept your position.” The difference between the two statements is enormous. If we assume that Kuhn’s Postscript expresses, upon reflection, a preferred concept of paradigm, then the use of the concept in information science can proceed. In her introductory essay to *Theories of Information Behavior*, Marcia Bates writes, “As Kuhn observed, in most natural sciences most of the time, there is a single predominant paradigm out of which researchers identify and test research questions. . . . In the social sciences, however, it is more common to have a general paradigm for the field, . . . but more than one metatheory” (p. 7). Many writers, as does Bates, use the word “paradigm” with approbation, as a sign of the maturity of a discipline or subdiscipline. The maturity is, ostensibly, warranted by virtue of agreement with regard to key concepts, questions, and problems, along with exemplars of method. In short, “paradigm” denotes the state of normal science. The foregoing constitutes what could be called the received view of disciplinary work, resulting in what Marc de Mey says is “the cognitive and social organization of science” (p. 89). The proposed paper will investigate the cognitive and social uses of “paradigm” in the literature of information science. The cognitive aspect is defined here as the intentional employment of the word and the idea represented by the word; that is, paradigm as a favorable or desired state because of the power that unity signifies. The social aspect is the use of the word to designate the state of collaboration, agreement, attention to a specific set of problems, and co-citation among those practicing information science.

2. The Present Study

This paper focuses on the prospect for epistemological growth in information science. De Mey, in an analysis of the meaning of “paradigm” recognizes four distinct components of the concept: symbolic generalizations, metaphysical beliefs, values, and exemplars (p. 96). The first two of these are of interest here. The symbolic generalizations are evident in the discourse of information science itself; even if the word “paradigm” is not used, some consistency of referents may inhere in the speech acts. For example, agreement (emergent or imposed) can be included in formal communication in ways that express perceptions of maturity, constancy, laws, etc. The metaphysical beliefs may be the most fundamental of the four components. These are less likely than symbols to be explicit in discourse, but the discursive practices may be interpretable in ways that allow identification of beliefs. Assumptions, methods, and prior work (in context) are indicators of the researchers’ beliefs regarding the objective nature of information science. In *Theories of Information Behavior*, Nicholas Belkin claims that the “anomalous states of knowledge” (ASK) hypothesis “is an explicitly cognitive explanation of the general phenomenon” (p. 45). There is a generalization underlying Belkin’s claim for (ASK), and there is also an expression about the “being” of information retrieval. These kinds of claims are examined in the present paper. The databases Library and Information Science Abstracts and Web of Science® (were searched for articles citing *Structure*. The search of Web of Science® was limited to the journals *JASIS&T* and *Scientometrics*. A total of seventy-six articles were examined to determine their uses of Kuhn’s book.

3. Uses of Kuhn and Paradigm

Of the seventy-six citing articles, fifty-two simply accepted Kuhn’s thoughts on paradigm, normal science, etc. There was no critical analysis of the ideas in *Structure* in these articles. Many of the remaining articles also demonstrated uncritical acceptance of Kuhn, but with some interesting appropriations. One author likened Kuhn’s notion of paradigm to organizational dynamics, for example. One author spoke of the “service paradigm: that should define libraries. One writer said that Kuhn’s analysis clearly demonstrates that science is not cumulative, while another claims that Kuhn clearly demonstrates that science *is* cumulative. Some citing authors misread—or selectively read—Kuhn. Bob Pym (2006) wrote, “While Kuhn was mainly concerned with the physical sciences he also applied the same arguments to social or, as he termed them, quasi-sciences, with a direct relevance to the paradigms favoured by our profession” (p. 65). Kuhn repeatedly averred that he did not intend his examination to refer to anything but the physical sciences. Greg Newby (2001) stated, “Because information science is a relatively young field, it may be that what we perceive as paradigm shifts—that is, evolution from one major approach to research in [information retrieval] to another—are really just a transition to what Kuhn would call ‘normal science’” (p. 1028). Since Kuhn refers to normal science as that period dominated by one paradigm, Newby’s statement makes little sense. Two sets of authors applied co-citation analysis as a means of discovering the paradigms that would define periods in information science. Marcia Bates (1999), in exploring the nature of work in information science, said that the field operates according to a paradigm—gathering, organizing, storing, retrieving, and disseminating information. The list comprises unique areas for inquiry though, which could have unique theoretical bases.

Some citing authors take issue with, or point out difficulties in, Kuhn’s work. Birger Hjørland (2005) noted that Kuhn’s entire program demonstrates a historicist bent

(that is, belief that there are historical laws that are analyzable and that allow prediction). Another author observed that Kuhn's use of "paradigm" was so vague as to render it useless as an analytical tool. Chen, et al. (2002) pointed out, "Kuhn's theory has been well received on the one hand by, for example, sociologists of science. On the other hand, philosophers have launched various criticisms, especially on Kuhn's earlier views on incommensurability" (p. 679). In a more extended critique, Fonseca and Martin (2005) argued, "The incommensurability of paradigms meant that the standards against which paradigms could be legitimately measured were paradigm-specific. . . . Accordingly, Kuhn's accounts are particularly destructive of a particular kind of ontologically oriented global theorizing" (p. 51).

Two citing authors engaged in substantive examinations of *Structure*, but with different results. Henry Small (2003) admitted that when he was a doctoral student he was influenced by Kuhn's book. Small also admitted that Kuhn did not provide a methodological tool to discover how to find precisely what paradigm is shared within a community. Small did, though, explore the potential of citation mapping to achieve the goals Kuhn implied. Small maintained, "Citations then have the character of hypotheses or conjectures, recursively embedded in other hypotheses" (p. 396). Citation analysis presents a conceptual challenge in the context of Kuhn's idea of paradigm, though. If Kuhn's suggestions are correct, citations to those scientists articulating the foundations of science should diminish, since (according to Kuhn) the foundational concepts would pass from the current literature into the textbooks. It would be, then, that it is not hypotheses that would be imbedded; the core beliefs that form the community's belief structure would be imbedded in textbooks. Small concluded, "Despite the pervasive influence of Kuhn's theory across the sciences and social sciences, it remains untested and controversial. There is a need to get beyond the rhetoric and translate this theory into testable form" (p. 399). Rather than Small's second sentence following from his first, it could be said that since the forty-five years (at the time of this writing) since Kuhn first published his ideas no effective test has been developed, his theory is in actuality *not* a theory in any meaningful sense.

Archie Dick (1995) took a different stance with regard to *Structure*. He wrote, "In spite of Kuhn's clear statement that the acquisition of a paradigm marks the sign of maturity in the development of any given scientific field, library and information science theorists follow the example of most social scientists to use the term to examine and develop what they believe to be its scientific aspects" (p. 223). In generous terms, library and information science, as do many of the social sciences, put the cart before the horse; acquiring a paradigm will lead to becoming scientific. Dick, drawing from the information science literature, noted that there is a tendency in the field to have a desire to adopt a paradigm and, so, be recognized as a science. The desire creates a tension, and the tension is extended beyond theory development to the foundations of self-perception. Dick (1995) expressed the tension: "It is possible that the essential qualities of universality and broadness in the professional discipline [which Dick saw as positive] precludes it from paradigmatic status since theoretical consensus is achieved only by sacrificing other competing paradigms" (p. 225).

4. Discussion

There is neither space nor need here for a detailed excursion into the vast literature on Kuhn, but some commentary is required. There is an imperative question that one is left with after reading *Structure* carefully: What is the point? The question is

not a facile dismissal of Kuhn; it is the foundation of a critique of *Structure*. The question *must* be addressed. The answer that Kuhn provides, albeit in a borderline self-contradictory way, is that the point of science is social ascendancy. A social group dominates primarily by satisfying pragmatic (in a fairly narrow sense) set of criteria. The foremost criterion is the attraction of disciples. Normal science does not emerge until there is a critical mass of adherents of a particular paradigm. In fact, it is this social phenomenon of adherence to the paradigms by a majority within the community that defines normal science. The rather univocalism of the community is another criterion; the majority does not simply dominate, it represses. In neo-pragmatist terms, there are two additional criteria: (1) dominance of the funding agenda of federal agencies, corporations, and private sources (as Sperber mentions), and (2) dominance in departmental faculties. These criteria are ineluctably social, even though there are certain to be epistemological, ontological, and practical elements of normal science. The social aspect is important, and that is the contribution of Kuhn to the study of the practices of scientists.

Kuhn's notions of normal science, revolutionary science, incommensurability, and paradigms are all problematic. The popularity of *Structure* is itself a curiosity, but it may be explained in part by the imagination it prompts in its readers. Less-than-careful readings of it allow people of different social, political, and metaphysical perspectives to find something to seize upon. In information science the appropriation of Kuhn could be described as a particular "received view." Throughout the social sciences a common reading of *Structure* is as a recipe for scientific professionalism. Fuller says, "In the last twenty years, however, a new generation has come to dominate the history, philosophy and sociology of science. They take *Structure* as the unproblematic foundation for its inquiries—as if the original criticisms had never been made" (p. 24). It seems inevitable from Kuhn's text and its theses and antitheses, both presented side-by-side and without apparent acknowledgement of conflict, that misreadings will flourish. Jan Golinski (2005), for example, praises Kuhn's historical acumen, even though Kuhn extrapolates wildly to the entirety of scientific development from one period in the history of physics (pp. 26-27). Golinski also mistakenly reads *Structure* as depicting a nonlinear and discontinuous flow in science, but Kuhn himself says that there is progress (in the sense that the present is an improvement over the past) and that the progress is by replacement of paradigms. Similarly, Barbara Herrnstein Smith (2005) responds to claims that Kuhn's account of theory choice by scientists is in no way irrational, ignoring the reality that the text of *Structure* is fraught with assertions of *arational* reasons for choosing one theory over another.

For Kuhn, truth is not a consideration for his theory of scientific practice. He states in *Structure*, "the proponents of incommensurable theories cannot communicate with each other at all; as a result, in a debate over theory-choice there can be no recourse to *good* reasons; instead theory must be chosen for reasons that are ultimately personal and subjective" [emphasis in original] (pp. 198-99). The claim is absurd; if there can be no communication, the difficulty of any individual abandoning one theory in favor of another would be almost insurmountable. According to Kuhn's thinking, it would be extremely difficult for any scientist to make a cognitive leap to *propose* an alternate theory. Further, if there is still some sharing of the fundamental questions to be addressed there would have to be some common communicative action. In this notion, though, he has numerous contemporary adherents, including Richard Rorty, Nelson Goodman, Steven Shapin, and others. There is another camp, however, for which truth is central to the goals of science. Karl Popper (1989) is foremost in the camp: "At the heart of this new optimistic view of the possibility of knowledge lies the doctrine that *truth is*

manifest. Truth may be veiled. But it may reveal itself. And if it does not reveal itself, it may be revealed by us [emphasis in original]” (p. 5).

Popper is far less influential in information science thought (and in the social sciences generally) than Kuhn (although he may have been misread as often as Kuhn). As opposed to the seventy-six citations to *Structure*, only eleven citations to Popper’s work in information science can be located. Smith (2005), examining Kuhn from a broad social sciences/humanities point of view, represents a typical treatment of Kuhn; she repeatedly lumps Popper and the logical empiricists of the Vienna Circle together, despite the fact that Popper’s entire program was opposed to positivism and verificationism. Popper’s work is not without problems, but he does avoid some of the pitfalls that Kuhn falls prey to. By avoiding some of the historical-analytical problems that befall Kuhn, Popper represents one (among several) alternative to the philosophy of science. It is, perhaps, one of his (1989) statements that most clearly distinguishes him from Kuhn: “I believe that we simply cannot do without something like this idea of a better or worse approximation to truth” (p. 232). He also points out—indirectly—another problem with Kuhn’s suggestion of paradigm shifts. Popper (1989) writes, “Newton’s dynamics, for example, even though we may regard it as refuted, has of course maintained its superiority over Kepler’s and Galileo’s theories” (p. 236). Newton’s dynamics remains useful for many purposes; it has not been absolutely abandoned. Moreover, a successor theory is unlikely to be absolutely different from its predecessor; some elements will be held in common. Given this, a gestalt shift from acceptance of one paradigm to acceptance of another is difficult to explain (although Kuhn says it is so).

Paul Hoyningen-Huene writes,

for Popper, a theory’s task is to capture and represent purely object-sided reality. Though we may never hope to accomplish this task conclusively, we may still continually improve our attempts. A theory must be improved or discarded as soon as its differences with (empirically captured) reality reveal themselves. For Kuhn, by contrast, a theory’s task can’t consist in the representation of purely object-sided reality, as this task is unfulfillable. Instead, theories participate in the constitution of what the given community takes to be, and experiences as, reality (p. 239).

This is quite a simplification (or oversimplification). The differences between Kuhn and Popper can in no way be so easily described. As Fuller points out, “Textbook caricatures of Kuhn and Popper tend to resort to facile binaries like ‘relativist/realist’ to capture the two sides of the argument” (p. 32). Popper (1994) provides the most effective refutation of Hoyningen-Huene: “It should be obvious that the objectivity and the rationality of progress in science is not due to the personal objectivity and rationality of the scientist. Great science and great scientists, like great poets, are often inspired by non-rational intuitions” (p. 13). It is Popper, not Kuhn, who is more open to the human side of scientific practice; his account is more welcoming of the power and place of inspiration and imagination. To return to Fuller’s observation that Kuhn is libertarian and Popper is libertarian, it is appropriate to ask how these two influential thinkers should be read. Kuhn, in attending to description of the practice of scientists, promotes—intentionally or not (and the difficulty of discerning his intention is perhaps the most serious difficulty of his work)—the existence of conformist environment wherein an orthodoxy is held and guarded: “The new paradigm implies a new and more rigid definition of the field. Those unwilling or unable to accommodate their work to it must proceed in isolation or attach

themselves to some other group” (Kuhn, 1970, p. 19). A telling statement by Popper (1989) illustrates his differences with Kuhn and argues once again for criticism being the primary distinguishing characteristic: “My thesis is that what we call ‘science’ is differentiated from the older myths not by being something distinct from a myth, but by being accompanied by a second-order tradition—that of critically discussing the myth” (p. 127).

The literature of information science indicates that the field is as confused as Kuhn himself. The social and cognitive elements of any discipline’s (or sub-discipline’s) development are real, but different. One means of analysis—historical, metaphysical, logical—can be used to examine both elements simultaneously only by remembering that the two elements are discrete, even while interrelated. One way for information science to overcome the confusion is to forget about Kuhn and his notions. Forget about paradigms, since they are illusions, chimera that have been reshaped by commentators following Kuhn; no theory could be at the same time proposed by a small minority of scientists in opposition to one held by the majority of the community *and* emerge with a wealth of empirical results and an overwhelming record of puzzle-solving successes. Instead, the field should approach progress as Popper does and monitor the successes and failures of a theory, constantly revising theories to account for the failures and to be responsible for more successes. The descriptions offered Kuhn are certainly not without value, perhaps more as cautions than anything else. What Popper suggests as an alternative are not descriptions, but norms. In short, the process is dialectical; the contradictions that inevitably arise must be addressed directly. There are social aspects to the process, but there are also normative ones; description of one should not be conflated with adherence to the other.

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