THE UNIVERSITY of Hawai‘i in the late 1940s and 1950s was a small liberal arts college of some four to five thousand students. It could even have been called a “cow college,” given its establishment as a land grant institution. Indeed, cows and pigs raised by the College of Agriculture roamed freely just beyond the fence that marked one of the boundaries with the liberal arts buildings. Some of us, however, were fortunate to have encountered teachers who could have held their own anywhere in academia. Allan F. Saunders taught government and politics. He approached his topics by the Socratic method and drew from us the awareness that the non-institutional foundations of a democratic society, among other things, lay in encouraging the give and take of ideas, resisting the quick and easy solution to complex issues that bedevil society, and in not forgetting the indivisibility between means and ends. We took note of the appropriateness of government intervention to protect and guarantee individual rights and to promote the general welfare. These views may appear to be pedestrian and commonplace, but for the nisei (second-generation Americans of Japanese ancestry) students who constituted the majority of the student body and who were either from plantation or, to put it delicately, “impecunious” urban backgrounds, these ideas represented new ways to look at themselves and society.1

Saunders also stressed that in fulfilling the requirements for our minors, we take note of the reputation of instructors among “achieving” students. So it was that in my senior year, I took two courses in the Philosophy Department given by Harold McCarthy: Scientific Method and Logic, with emphasis on judging evidence and its uses, and on how to detect what he called “crooked thinking.”2

In the years 1950 to 1954 that overlapped with my senior and graduate student years, there occurred a defining event that put flesh on and gave meaning to all I had learned from Saunders and McCarthy. This was McCarthyism, distinguished by accusations based upon spurious, often nonexistent evidence, the big lie, and guilt by association. It is difficult to convey to others after half a century the intimidating miasma that McCarthyism cast on many sectors of American society and individuals, including those engaged in the study of Asia. It is no exaggeration to state that this experience has to this day made me extremely sensitive to and wary of anything that smacks of McCarthyism.
But what of my passion for documents? One course at Harvard was particularly crucial in awakening in me the excitement and enjoyment of working with primary sources. This was Topics in Chinese History, taught by Lien-shen Yang. He would take a topic such as the role and significance of water in Chinese civilization and spend several sessions developing the theme. He would invariably bring an armload of documentary sources to class to illustrate his points. One of the papers for the course involved selecting a Chinese primary source, translating it, and locating documentary support for every person, office, and historical or literary work mentioned in the source selected. This was an excruciatingly difficult task for me since I had only two years of elementary Chinese. Nonetheless, I had no choice but to search out the sources housed in Boylston Hall. Persistence had its rewards. I discovered with ever-increasing amazement and entrancement that written records stretching back for centuries existed to enable me to fully annotate the contents of the work I had chosen. The five-page typewritten paper, still in my possession, contained ninety-five footnotes. There is no question that this exposure to Professor Yang’s course was a turning point in my development as a scholar.

I took, however, a slight detour into unfamiliar territory before committing myself fully to positivism. I wrote an article for Edwin O. Reischauer’s festschrift “The Other Itô: A Political Failure.” I was at that time intrigued by what I thought was a “love-hate” relationship between Itô Hirobumi and Itô Miyoji, two of the four drafters of the Meiji Constitution. I happened to mention this to the late Hiroshi Wagatsuma, the social psychologist. He was at this time spending a year (1966–1967) as associate research psychologist at the Social Science Research Institute, University of Hawai‘i. He suggested that I use amae as an explanatory device. I agreed, but because I lacked the confidence to employ amae to tease out the relationship between the two Itôs, he kindly agreed to work closely with me. This he did in Hawai‘i and Japan. He also wrote a letter of introduction to Dr. Takeo Doi, a pioneer in research on amae, and I had two interviews with him. It should be pointed out that the section on amae did not constitute a large part of the article and that the rest was in straightforward narrative style based on written sources. A fellow Japan specialist, however, cast his critical eye on what obviously was to him not only a diletantish attempt, but simply wrong:

[Akita’s effort] is psychohistory at its most vulgar level . . . [and] he offers a second-rate view of what he considers a second-rate politician.

Ouch! I would have preferred a gentler rebuke, but I am nonetheless grateful that the reviewer opened my eyes to the dangers of uncomprehendingly using the theoretical “fad of the day” as an analytical tool. Doi
and Wagatsuma, both of whom I respect highly, certainly cannot be blamed for my inability to understand the subtleties of the role of *amae* in Japan.⁶

How then do I define positivism? What follows is not a philosophical, theoretical, or doctrinal definition. I admit to an inadequacy in handling subjects on that level of abstraction including, I might add, any comprehension of subjects such as postmodernism and deconstructionism.⁷ A recent work that does this well is Keith Windschuttle’s *The Killing of History: How a Discipline Is Being Murdered by Literary Critics and Social Critics* (Paddington, Australia: MacLeay Press, 1996). Windschuttle frontally confronts structuralists, poststructuralists, postmodernists, critical theorists, deconstructionists, and postcolonialists by juxtaposing subjects that narrative historians and the “new theorists” have both covered. I concede that the selection of Windschuttle is also a reflection of my bias against the “new theorists” and their writings.

Here I pause to note that as a positivist, I am persuaded that what is sometimes dismissed as “the lowly footnote” has an essential role. I was once described as a “shallow technician” who believed that writing history involved the mere “accumulation of primary sources and footnotes,” that in an article of mine, I had never once gone beyond the “level of footnote excavation” and had instead ignored “great historical themes.”⁸ I had always assumed that it is not possible to separate “great historical themes” from supporting evidence indicated in footnotes. And, of course, footnotes also help to lead scholars to sources that may prove useful for their studies. They also, sad to say, are needed to help us resist the all too human temptation to cut scholarly corners.⁹

The polymath Robert K. Merton was an avid believer in the importance of the footnote. His protégé, Thomas F. Gieryn, recalls:

> The now-normative style of citations was not well suited to what he wanted to accomplish with the anything-but-lowly footnote. In his book on the role of religion in stimulating seventeenth-century English science . . . Merton uses a footnote to explain how his exploration of the salutary connections between Puritanism and early modern science was inspired by yet another footnote, in Max Weber’s magisterial *Protestant Ethic and the Spirit of Capitalism*. . . . Imagine: two mere footnotes yielding perhaps the most enduring and debated theory in the history of science, eponymized as “the Merton Thesis.”¹⁰

My definition of positivism is based on the premise so well stated by George Orwell, who in 1932 declared that “facts” existed and were discoverable, and that there indeed existed a considerable body of fact agreed upon by almost everyone.¹¹ Harold Bolitho also subscribes to this proposition. While acknowledging that scholars may be known as conservatives
or progressives, or may be designated as pragmatists and radicals, and still yet as realists and idealists, what unites us all, he states, is that

we are linked by common presuppositions—what history is, what constitutes evidence, how documents can be used. If we disagree we tend to do so in mutually intelligible ways. We may not say the same thing, but we speak the same language.12

Positivism is a method or process by which these “facts” or “objects” are discovered, that is, by the inductive method based on observing and utilizing a vast array of data or evidence. The data, among other things, can be recorded interviews, telephonic conversations, speeches, questionnaires, railroad schedules, telephone books, and theater tickets.13 My preference is the written record: unpublished documents and published primary sources, including but not limited to diaries, memoirs, policy statements; and secondary sources such as histories, monographs, journal articles, essays, magazine and newspaper articles, and reviews.

It is a given that the meaning and significance of the data come filtered through the mind of the historian. Here an article on pain is instructive. Pain, for more than three hundred years, was believed to be a strictly physical phenomenon. A new understanding of pain was pioneered by Lt. Colonel Henry Beecher, who studied battlefield injuries suffered by World War II soldiers. Then came Canadian psychologist Ronald Melack’s “gate-control theory of pain” that led to studies of pain thresholds and tolerances. These studies show that innumerable factors influence the pain experience, including mood, prior pain experiences, the power of suggestion, gender (women are shown to be more susceptible to pain than are men), personality (introverts are more susceptible than extroverts), attitude (certain athletes are less susceptible than nonathletes), and culture (e.g., Asians appear to be more stoic than Hispanics, who tend to be more vocal).14 If understanding pain requires grappling with these intangible variables, how realistic is it to declare that the positivist historian carrying the baggage of his values and biases is able to discover the “facts” on which he bases his analysis and conclusions? In truth, it would be folly to state unconditionally that this is possible.

The task is somewhat less burdensome if the positivist is open and explicit about his prepossessions, and, as Henry Commager suggests, “strives manfully to avoid bias.”15 To those who scoff that it is ever possible to “manfully avoid” something as subjective and intangible as bias, I state two crucial characteristics of the positivist method that serve as checks against bias. One is that questions asked of the data must be framed in an open-ended manner that “will allow a free and honest choice, with minimal bias and maximal flexibility.” In other words, the historian does not approach his or her sources with a preconceived, fixed position.16
other is openness to alternative interpretations of the analysis and conclusions, that is, a willingness to refine and revise one's views and conclusions in the face of compelling evidence.

What of the role of theory to guide research and analysis and the formulation of conclusions? I define theory as an expression of a general principle whose validity can be tested and replicated anywhere. Given this, I harbor grave doubts about the use of theory by those of us in the humanities in coming to grips with problems involving the human condition and behavior. Perhaps we best eschew theory as a fig leaf to cover the limitations inherent in our field. The social scientists, of course, are more inclined to use theory as a starting point to guide their research. Still, I am seeing a distinct and welcome trend among some in Japan studies to assess the limits and even the utility of theory's place in the social sciences.

David L. Howell portrays James B. Lewis as “a positivist who knows he cannot be positive about much. . . . He walks the reader through both the primary sources . . . and the secondary literature . . . before settling down on his own, often highly qualified conclusions.” I would be pleased to be described in this manner. I would also fit comfortably in the methodological mold constructed by Reinhard Zöllner, whose work “exemplifies the possibilities of delineating historically and thematically complex matter through an analysis of primary sources . . . [who] can hardly be reproached for being averse to historiographical theories . . . [and who also] clearly prefers not to give theory priority over historical-philological results.”

There is good reason for the “highly qualified conclusions” favored by positivists. This is to avoid postulating results that appear to follow reasonably, neatly, plausibly, and even logically from the evidence laid out by a scholar, but, upon close and skeptical (but not cynical) examination of the evidence, leave room for alternative conclusions. One such example is John Walter de Gruchy’s study of Arthur Waley, in which he closely examines Waley’s published and unpublished correspondence and the memoirs of his period, as well as the “homosexual tendencies of many members of his circle,” including King’s College, Cambridge, and the Bloomsbury group. Midorikawa Machiko asserts that there is “little direct evidence” about Waley’s sexual orientation and that de Gruchy “falls back on assumptions, suggestions, and syllogistic argumentation” to make his case. She also lays out linguistic arguments—which Antoni has called “historical-philological results”—to show that for her, what de Gruchy has concluded is speculative.

The danger of drawing the wrong conclusion, even though the facts may be correct, is well-illustrated in C. A. Tripp’s recently published work, The Intimate World of Abraham Lincoln (New York: The Free Press, 2005), as noted by Joshua Wolf Shenk in “The True Lincoln.” Tripp relates that
Lincoln, in his late twenties and early thirties, shared a bed with a young man and as president may have on occasion done the same in Washington with an officer of his guard unit. But, as Shenk points out, it was a common practice for men in mid-nineteenth-century America to have done so, and this practice, as well as men “openly,” “physically and verbally” showing mutual affection, did not raise any eyebrows.22

Penelope Francks is an economic historian who does not use the word “theory,” but instead uses “model,” in her case the “growth linkages” and “proto-industrialization” models. For her, the model of “growth linkages” between agricultural and nonagricultural activity explains the “successful functioning” of a “virtuous circle” in Japan that resulted in expanding employment opportunities, rising and often quite equally distributed rural incomes, and improving standards of living. Moreover, the success explainable by this model had two more consequential results, that is, the weakening of the trends toward proletarization and the rise of inequality that followed in the wake of industrialization elsewhere. In short, “proto-industrialization” in Japan and East Asia differed from the European experience, which is explained by the generally accepted “proto-industrialization” model.23

Whatever I may think about “models” and “theories,” I find much to appreciate in Francks’s article. It is, in the first instance, time-, circumstance-, and location-specific. Second, while she uses a wide range of sources and data, she is modest, tentative, and circumspect in her statements and conclusions. She writes, for example, that “it appears from the evidence [presented] that at least some elements of the virtuous circle of rural agricultural linkages were in place in nineteenth-century Japan” and there is “circumstantial evidence that such a linkage was in operation, even if the data is imperfect.”24

The thrust of her article is that Japan did make a successful transition from Tokugawa to Meiji Japan, with a generally stable, prosperous rural area and an essentially equitable distribution of income. I also share her position that economic development in Japan “inflicted costs,” but that these costs were “not borne in vain” since the goal of most developmental strategy, sustained industrial growth, was eventually achieved, characterized by “wider and more even spread of income-earning opportunities.”25

Jennifer Robertson, a well-regarded anthropologist who “works in and on Japan,” naturally enough believes that theory has its place.26 Yet for her, there are theories and there are theories. She makes the distinction by pointing out that among some “recent [anthropological] monographs in Japan” there is an inclination toward using theory to hide the lack of Japanese archival and empirical material, while giving the reader the impression that this represents a “totalizing explanation.” Theory, for her, should serve as a “reasonable conjecture” reflecting the history-spe-
specific circumstances in Japan and as a road sign pointing to further research. She concludes with a statement that is difficult to rebut:

Obviously, the mere invocation of a theory of practices is not a viable substitute for exploring and recording actual, everyday practices and collective activities in particular places and times.\(^{27}\)

Her cautionary words should be heeded by all Japan specialists, though I suspect that some fields need to pay closer heed than others.

The political scientist Aurelia George Mulgan comes down strongest on the irrelevancy of theory. She declares that political scientists working on Japanese politics do not learn about politics. They are, she declares, more concerned about absorbing the “latest artificial constructs” that are devised to aid them in understanding and explicating Japanese politics, that is, “the latest theories, models, [and] frameworks in the discipline.” The consequence is that they drift away from the “documented realities” of Japanese politics. This sometimes makes them appear to live and move in “an artificial world of spurious scientism, grand theories that exhibit selective blindness to contradictory facts.” The further result is the “perpetuation of vacuous theories” based on logical deduction, not on empirical research. George Mulgan’s unequivocal skepticism about the utility of theories in political studies rests, I believe, on two grounds: one, theories narrow the range of questions that can and should be asked and limit the scope for alternative explanations; and two, theorists must deal with data that do not conform to their theories.\(^{28}\) Specifically, she is critical of the rational choice theorists, a criticism shared by Gerald Curtis, who questions them for embracing “theoretical assumptions that make no sense at all in Japan.”\(^{29}\) Moreover, as Harold Bolitho also points out, on major themes in his field, the waters have been “muddied by the discovery of evidence resistant to tidy analysis.” And yet, he continues, there are those who “after wading through the morass of evidence to the contrary [manage] to find what they want, if only in attenuated form.”\(^{30}\)

Recently, Joan Mellen, a noted scholar of Japanese films, has added her voice to the kind of skepticism articulated by George Mulgan, Curtis, and Bolitho. She focuses on the critical neglect of *Seven Samurai*, what Donald Richie, the doyen of critics of Japanese films, calls the “finest” of Akira Kurosawa’s films. The reasons for the “least written about” and “most misunderstood” masterpiece, she notes, are the younger foreign critics’ enamourment with “theory,” their distaste for history, linked as it is with their “skepticism regarding objective truth,” and their political agenda. She asserts that these positions have led them “to pretend that Kurosawa’s masterpiece is in fact one of his lesser works.”\(^{31}\)

There may be a moral, however, to the phenomenon of the bewildering
array of theories that each have their day and then seemingly lose their attraction. This is reminiscent of the rise and fall of Herbert Spencer’s reputation. Spencer was regarded as Britain’s most “penetrating and significant” social philosopher, but the trajectory of his influence was “straight up and then straight down.” This shows that “even the most magisterial of sociological systems builders” can have his day, only to be “heard of no more.”32