

# Communications to the Editor

## TO THE EDITOR:

I was surprised to find Chad Hansen's "Chinese Ideographs and Western Ideas" in the May 1993 issue of *JAS* because its central claim, that Chinese characters are ideograms, has been held in disrepute by so many scholars for so long. For the same reason, I submit this response reluctantly, for merely doing so may suggest to some that I think the ideographic account of Chinese writing as an explanation of distinctive aspects of Chinese culture and thought is more deserving of space in a scholarly journal than, say, "scientific creationism" as an explanation of genetics and fossils. To my mind, both hypotheses belong to the same category and ought not be dignified. Nevertheless, the quality of Hansen's scholarship is so poor that I feel someone must apprise readers who are not specialists in linguistics of facts and sources Hansen fails to cite. There are equally wrongheaded writers, such as Donald (1991), who deal with the relevant literature (even if they sometimes misinterpret it), get most of the basic facts straight, write clearly, have something original to say, and may be excused for their missteps because they don't know an East Asian language. None of these qualifications applies to Hansen, however, and that is why I am writing.

Consider first Hansen's foray into science. Relying on a single secondary reference (Lesser 1989:186) and citing only unidentified "neurologists in Japan" and "[f]our different researchers in Japan" who have allegedly demonstrated that *kana* and *kanji* processing are associated with Broca's and Wernicke's areas, respectively, Hansen states, "these results show that mastery of Chinese characters is causally different from alphabetic mastery" (pp. 385–86). Evidently, Hansen's acquaintance with the literature is minimal.

Paradis, Hagiwara and Hildebrandt (1985), the definitive review of all published experimental results up to 1984 on the processing of *kanji* and *kana* in the brain as well as the entire clinical literature on acquired dyslexia in Japanese patients going back to 1901, pinpoints methodological flaws in every one of the experiments, shows how case histories contradict one another, and concludes that the data do not support hypotheses of localization of *kanji* and *kana* processing or radically different pathways for each type of processing. After an independent review, Flores (1992) reaches the same conclusion:

Although there is some evidence from Stroop-type experiments and some, less clear, from clinical data and from lateralization experiments, indicating some differences in the processing of Chinese characters as compared to alphabetically printed words, the conclusion that Chinese characters are processed more "like pictures" than like words can hardly be maintained.

(Flores 1992:50)

More recent experimental results by Horodeck (1987) for Japanese and Hayes (1988) and Cheng (1992) for Chinese cast further doubt on differential processing hypotheses; they show that native readers begin processing characters semantically only after assigning them readings. Eye-tracking experiments corroborating Horodeck's results for Japanese, confirming under even more stringent conditions that Japanese readers unconsciously subvocalize, have been carried out by Sachiko Matsunaga at

the University of Hawaii and will be presented in her forthcoming Ph.D. dissertation. (Of course, Hansen would not have known of this work in progress.)

Celebrated crosscultural studies of elementary education in Taiwan, Japan, and the United States by Harold Stevenson and his colleagues at the University of Michigan, widely known in this country for zeroing in on the social factors responsible for superior test scores among Asian students, show *en passant* that, in terms of reading comprehension, differences in language and writing systems are *not* determinative variables (see, for instance, Stevenson et al. 1987:175).

In the middle of page 384, Hansen quotes Sacks 1990:114 (actually 113) on common structural characteristics of different signed languages as if the mere existence of such universals proved that language "is genetically independent of sounds and speech." Yet, a few pages earlier, Sacks explains in detail how the pathbreaking work of Stokoe, which led to the recognition of American Sign as a genuine language, depended on an analysis of the inner structure of ASL lexical items in a manner comparable to the phonological analysis of speech (Sacks 1989:76–80; Unger 1990:392 n.4). Sacks's point is not that "sounds and speech" are unnecessary for language but that sight and gesture can be a sufficient substitute for them under appropriate circumstances. The evolutionary fact that speech is the default medium for language cannot be swept aside unless one is prepared to refute the evidence that language itself developed only after the adaptations in the human vocal tract that permitted phonation (Lieberman 1991).

Hansen has, in short, misunderstood what he has read and failed to read much that he should have in linguistics. His performance in other areas of scholarship is no better.

Hansen quotes, for example, "Du Ponceau 1835:8" (p. 373) and refers to the same work on page 382, claiming that Du Ponceau, in denying that Chinese characters are ideograms, "was expressing the 'educated' opinion" of his day. The truth is that, as the author makes clear in his (N.B.) 1838 work, Du Ponceau was opposing the established view, trying to correct an opinion the prevalence of which he abhorred. Indeed, judging from the comments of his contemporary, Rémusat, the equation of Chinese characters with ideograms had, by the early 1800s, "been carried to such an extreme that some writers seriously contended that characters antedated the very language they were (later) used to write" (Unger 1990:394 n.14). It is Hansen's, not Du Ponceau's, opinion that "persists only as an unexamined, a priori, traditional Western prejudice." As shown in Unger 1990, Chinese and Japanese scholars who wrote about language and script understood that Chinese characters stand for syllables of the Chinese language, not ideas or things that could be expressed or designated willy-nilly in any language at all. The myth that written symbols could stand for ideas or meanings began with Western writers, such as Plotinus, seeking an explanation for Egyptian hieroglyphs. The "Chinese theory of language" (p. 393 *et passim*), which, ironically, both Hansen and his nemesis Du Ponceau take for granted, seems to be an import from the West; the earliest text I have been able to locate in which a Japanese author refers to *kanji* as representing meanings (in contrast to sound-representing *kana*) dates from 1894, and I would be surprised if Hansen can cite a substantially earlier source in Chinese or Japanese.

Turning to errors of a more mechanical nature, Hansen's reference (p. 374) to the "one and a half" languages in the world that still use Chinese characters (besides sounding like a Sinocentric putdown of Japan) is wrong because it ignores Korean, not to mention the fact that the so-called Chinese dialects would probably be acknowledged as distinct languages if it weren't for politics and the overbearing social prestige of Mandarin (DeFrancis 1984). (Ironically, one prolific Japanese apologist

for the “semantic transparency” of *kanji* insists that Chinese characters are ideographic in Japanese but *not* Chinese writing, where they generally do not take multiple readings [Suzuki 1975]. How would Hansen refute that line of argument? On page 381, Hansen states that a particular Chinese character can be read “benkyoo” in Japanese. Wrong again. Not only do the rules for using Chinese characters in Japanese writing preclude a word such as *benkyō* from ever being the reading for a single character, it wasn’t until the Meiji period that the word *benkyō* acquired the sense of “study” necessary (given Hansen’s argument) for its use as a reading for the character in question.

Extending this list would be easy, but let me turn now to the inner logic and content of Hansen’s article.

At the outset, Hansen sets up a dichotomy between the Chinese and “Indo-European views of language” (p. 373) that is false on at least four levels.

First, to the extent that “Indo-European” is not just a malaprop for “Western,” Hansen evidently means to suggest that the “prohibitionist” view (that ideograms, like unicorns, are well-defined but fictitious) is nothing but a byproduct of work by certain linguists on the reconstruction of proto-Indo-European. The truth is, however, that Leonard Bloomfield, Edward Sapir, Franz Boas, Mary Haas, Li Fanggui (Fang-kuei), Bernard Bloch, Charles Hockett, and countless other “prohibitionist” scholars made the United States preeminent in linguistics mostly with their work on Native American languages, Thai, Japanese, Chinese, and other non-Indo-European languages.

Second, it is simply not true that the “prohibitionist” view has held sway since Aristotle (quoted on pp. 373–74). Comparative linguistics was not built on the premise that speech is primary (the Grimms still referred to *Buchstabe* rather than sounds); the primacy of speech was inferred from the successes of the comparative method. Just as Newton “stood on the shoulders of giants” and discerned the underlying principles that had escaped otherwise keenly observant predecessors like Kepler, so too did the Neogrammarians and, later, Bloomfield enunciate the “dependency principle” in a Kuhnian revolution that transformed philology into linguistic science.

Third, as already remarked, the burden of proof is on Hansen to show that a “Chinese theory” in which characters are explicitly identified as ideograms existed before the notion of ideogram was introduced from the West.

Finally, having set up his dichotomy, Hansen contradicts himself by claiming that the majority of linguists today “reject the dependency principle even for alphabetic languages” (p. 382). If that’s true, then why call it the “Indo-European” view?

Hansen’s next move is to claim that *both* the “Indo-European” and Chinese views are wrong. “Characters and ideas, I will argue, both fail to explain meaning and do so for the same reasons” (p. 375). Thus, the focus of attention shifts from what we might call the Boodberg-Creel debate to the quite different problem of linguistic skepticism. (If, as Locke argued, our knowledge of the meaning of words is essentially private, then what does it mean to say we communicate with each other when we use language? The agnostic skeptic answers that we can never be sure we really communicate; the radical skeptic says we never really communicate at all.) Hansen’s treatment of this problem is unoriginal, shallow, and, once again, out of touch with the literature.

One would think from reading Hansen, for example, that only Wittgenstein has had something worthwhile to say on the subject. In reality, attempts to deal with skepticism, including efforts by major figures such as de Saussure and Frege, are plentiful, richly varied, and packed with subtleties one would never guess existed

from Hansen's discussion; for a detailed (and, in my opinion, brilliant) survey, see Taylor 1992. By contrast, Hansen, with his handwaving appeal to Chomsky 1965 (has he heard about the changes that have come to MIT?) and completely unsupported presumption that logic and mathematics are languages (p. 384), offers little more than an East Asian version of an early (and inadequate) response to the skeptical challenge classified by Taylor as "phylogenetic naturalism." It is epitomized by Condillac's declaration that

languages are the products of Nature. They are constructed, we may say, independently of us. And in using them we have only obeyed like slaves our methods of sensation and perception.

(Taylor 1992:65)

How is this any different from Hansen's conclusion?

Learning to distinguish the written graph for mountain from the graph for cloud and to distinguish mountains from clouds are analogous, unexplained, but natural human abilities.

(Hansen 1993:396)

Hansen's discussion of the problem of skepticism is faulty in another way too: it is inconsistent with his attack on the "dependency principle" in the first half of the article. Having insisted that Chinese characters are actually ideograms, he suddenly concedes that

Chinese characters must be learned. They are not innately or intuitively readable. Written Chinese has a conventional grammar. Characters are not self-interpreting, universal, or inherently meaningful symbols. Many characters are phonetic compounds consisting of an ideographic radical [*sic*]. Some are rebus characters—characters borrowed from another use simply to stand for a word with the same sound. Characters do not look much like pictures and synonyms are not identical.

(p. 387)

If statements such as these are true, then one must wonder what property an ideogram could possibly have that distinguishes it from any other written symbol—and Hansen never explicitly defines 'ideogram'. Hansen freely grants them because he is convinced that "none of these facts is denied in the Chinese theory of their language" (p. 387). But since he never cites texts that enunciate the "Chinese theory of their language," why should we believe him?

Furthermore, Hansen is just telling us what we would *not* find in the uncited texts, not what we would. What sort of texts should we consult? On page 395, Hansen states that "in China we never find any debate, doubt, or revision of any explicit belief or idea theory in Chinese texts," which amounts to saying that the missing texts are about characters and language, not about "ideas." I have looked for pre-nineteenth-century Chinese and Japanese texts of this very kind; not only was I unable to find any in which the author argues that characters convey meaning without reference to speech, but, in the process of hunting, I came across several in which the author criticizes such beliefs as crude (Unger 1990:396–97). Is that the sort of evidence Hansen had in mind? If so, then his "Chinese folk theory" is truly a theory "of the folk" (*vulgaris*) for it certainly isn't what eminent writers such as Shen Kuo (or Gua), Zheng Qiao, or Wang Yinzhi thought. It may have been what illiterates and "mediocre scholars," to use Xu Shen's phrase, believed, but,

in that case, one can hardly accept the inferences about Chinese ways of thought that Hansen draws from it.

Finally, there is a defect in Hansen's article that is both a failure of elementary scholarship and a breakdown of logic: demanding that linguists use 'word', 'language', and 'ideograph' in the vague senses they have in everyday English (p. 378). This is like forbidding physicists to use 'force', 'charge', or 'mass' in the precise and useful ways they do. Furthermore, the implication that linguists don't acknowledge colloquial usage is just not true. Take 'word', for example. We all know that native speakers of English typically define 'word' in terms of orthography. This is why linguists have introduced the concept of 'morpheme' and why DeFrancis (1989:55) takes care to distinguish 'words' from 'frames'. Indeed, much confusion in the case of Chinese is created by the common but incorrect assumption that every syllable of Chinese represented by a distinct character must be *ipso facto* a morpheme (i.e., a meaningful unit) of the Chinese language. Hansen seems to think that, by denying the use of well-defined terms to the opposition, he causes their ideas to vanish into thin air.

In sum, even those who agree with Hansen that Chinese characters are ideograms should be unhappy with his defense of their position. It shows all the earmarks of pseudoscience: dismissal of genuine experts as fastidious nitpickers blind to the "obvious"; refusal to acknowledge common definitions and their *raison d'être*; hasty, broad-brush characterizations of cultures and concepts; unwarranted generalizations; failure to address disconfirming evidence; tortuous reasoning to a worn-out conclusion as if it were a new discovery. It saddens me to think that the reviewers who gave Hansen's article their imprimatur did not notice or did not care about the points I remarked on above—and I have omitted many others for the sake of brevity. In some ways, I think the reviewers are more culpable than Hansen. He merely gave *JAS* the opportunity to add another item to the long list of books and essays that mystify, obscure, and exoticize China; they advised *JAS* to go ahead.

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#### CHAD HANSEN REPLIES:

I want to thank Dr. Unger, a fine historical scholar, for overcoming his reluctance and expressing his reaction to my article. I recommend his article to anyone interested in pursuing the prohibitionist case. I will take this opportunity to address only two issues in reply. First, I want to reflect on how we should deal with radically different perspectives within an interdisciplinary scholarly community such as Asian Studies. Second, I will expand on how my philosophical and conceptual arguments bear on various historical and experimental studies.

Dr. Unger's article and his reply vividly illustrate how different the perspectives, assumptions and methods of our respective disciplines are. My philosophical colleagues are incredulous that any theorist today would defend the dependency principle. Dr. Unger's fellow prohibitionists find it equally hard to respect anyone who rejects it. We clearly face the problem Zhuangzi formulated so well two millennia ago. "If someone of your persuasion decides, already being of your persuasion, how can he decide?"

Radically different conceptions of scholarship and scholarly integrity accompany these gaps in substantive belief. Philosophical norms of scholarship focus on distinguishing clearly between alternative positions and carefully assessing the objections to each. We strive to prove a thesis, not to cite others who believe it. Good scholarship, from this perspective, is careful, charitable, and yet critical reading of arguments and clear presentation of counter-arguments. We may include the work of others. Then, we cite them as a matter of intellectual honesty—to avoid taking credit for their distinctions, hypotheses, and reasoning. Still, we choose the arguments; we include them and take responsibility for whether they are sound and pertinent. We do not simply appeal to authorities.

Our differences may even extend to the meanings of words. I had to look twice at Dr. Unger's statement that "Sacks's point is not that 'sounds and speech' are unnecessary for language but that sight and gesture can be a sufficient substitute for them under appropriate circumstances." When philosophers use the word 'necessary', it means no substitute is possible. If a sufficient substitute exists, then the item is not necessary. Perhaps Dr. Unger and I technically agree. We both think that sound is a typical, but not the *necessary* (my sense) form of language.

An interdisciplinary field, such as Asian Studies, should have outlets to explore and to bridge such deep gaps in assumptions and methods. I personally applaud *JAS* editors for venturing to air this issue in the face of the predictable abuse from prohibitionists. I prefer the *Journal's* providing a forum for challenges to conventional wisdom to its being an instrument in repressing them. Still, I could not simply advocate that the editors publish any lucid, unorthodox view that arrives in the mail. How are we to avoid Dr. Unger's worry—the practical equivalent of creationism.

One suggestion is an appeal to "informed opinion"—especially where that opinion is nearly unanimous. This is an adequate response in, say, a newspaper editorial. However, in a mixed scholarly community facing a Zhuangzi situation, relying on that kind of response amounts to browbeating and intimidation. It stifles intellectual exploration and development.

Dealing with such divides requires that we openly test the reasoning itself rather than criticize, revile, and seek to ostracize or discredit the person who presents it. We should try to present the underlying reasons for what strikes us as obvious. Presenting a reasoned elaboration of our own initial commitments and analyzing the reasons given for the opposing commitments open the way to intellectual advance or accommodation. Either the defense of the suspect views will become incoherent or irrational under extended analysis *or* the rest of us *should* take them more seriously.

In this case, of course, we lack the unanimity Dr. Unger's response requires. Besides the disagreement among linguists about Bloomfield's principle that I documented in the article, I have found the sinological community itself still almost as divided as it was when Creel and Boodberg first clashed. I had assumed, as Dr. Unger does, that the prohibitionist strategy of personal abuse had worked.

Despite my relief at its failure, I still regret that prohibitionists continue to rely so heavily on personal attack—accepting the seriousness, competence, and worth only of those who agree with them. Since the main thrust of his critique is that I am unworthy, it is hard to extract clear objections to my reasoning. It makes any reply look like self-adulation. The tactic, however, undermines confidence in the critic's judgment. If their position were as strong as they pretend, why focus on preventing opponents from having a hearing? Why spew venom on the reviewers and editors and attack an author's scholarly credentials instead of simply showing what was wrong with the reasoning? If the alleged error is not a simple one, then it is all the more important to concentrate on explaining it rather than calling its opponents "wrongheaded."

That brings me to my second point. Scholars in other disciplines sometimes complain that philosophers ignore the "real world." Philosophers, in turn, worry that researchers may unconsciously project their conceptual assumptions into their formulation of problems and into their reports of observations. Scientists propose hypotheses to explain observable phenomena. The plausibility and coherence of any hypothesis used to explain those observations is a logical and semantic matter, not an empirical one. Philosophers, of course, should be ashamed when they get empirical data wrong. Empirical researchers, on the other hand, should be aware of when and how conceptual or reasoning errors may vitiate their conclusions.

My point was a conceptual-philosophical one. I criticized the widespread conceptual arguments for the prohibition of 'ideograph' and for the dependency principle. I did not otherwise discuss, either to endorse or reject, the various scholarly enterprises dealing with the history of the Chinese language. The conceptual argument shows that we can treat historical results as accounts of how one ideographic language developed. Historical data, absent a conceptual grounding, can neither prove nor disprove the claim that Chinese is ideographic.

My discussion otherwise bears on that historical scholarship only insofar as those researchers take the dependency principle as given. I do find frequent appeal to the principle in the writings of some scholars. They could, however, defend their conclusions in other ways. My analysis does present a challenge to scholars in these fields either to produce better arguments or to treat the traditional Chinese view as a live hypothesis. It might still be rejected as failing to explain the data, but we may not rule it out a priori.

A slightly different point applies to empirical research in psychological linguistics. In that field, however, cautioning researchers against taking the dependency principle for granted is unnecessary. As Dr. Unger's citations show, the dependency principle is at the core of an ongoing debate about how we process written and spoken language. Science does not work like religion. No pope makes "definitive" pronouncements on matters of theory. The conclusions of a literature review could never close an issue so tightly that it would justify barring any further discussion in professional journals. Can you imagine a serious biologist getting tenure for undertaking and publishing a series of experiments designed to disprove the hypothesis that a divine being created the world in six days?

I do worry about the sensitivity to the empirical/conceptual divide in Dr. Unger's presentation of scientific results. He misstates the Wernicke hypothesis and its relation to the Japanese aphasia studies. Lesser, Geschwind and Schnitzer were not specifically concerned about Japanese. They cited the Japanese result as *further confirmation* of the Wernicke hypothesis about separate localization of linguistic functions in the brain. The Wernicke hypothesis, which contradicts the dependency principle, explains a wide range of other phenomena as well.

Dr. Unger seems to want to leave the impression that his "definitive" 1984 literature review rebutted that hypothesis. Since he did not quote any relevant passage from that review, I am not sure if he thought it denied that the reported observations actually took place (e.g., the researchers were lying) or that the Wernicke hypothesis could not explain those observations. The former is implausibly bold (to say the least) and the latter would require quite an impressive argument. In this field, it is the Wernicke hypothesis, not the dependency principle, that is the counterpart of the Darwinian hypothesis. Dr. Unger instead quotes Flores's "similar" conclusion without noting that Flores grudgingly *endorses* my point. Evidence (other than aphasia research) *does* exist of real differences in how we process characters and alphabetic writing. Flores expresses doubts (as I do) only that Chinese characters work like pictures.

Even if Dr. Unger's interpretation of the scientific literature had been correct, it would merely have demonstrated a real, substantive scientific dispute. Experiments bearing on both sides of the dependency principle can hardly show that the very idea of visual language is "wrongheaded."

Obviously, the rhetoric of prohibitionists helped shape my argumentative position. I did not set out to prove that the traditional Chinese view was true *simpliciter*. I tried to show that the Chinese analysis was conceptually *respectable*—that the traditional Chinese view deserves further study. Prohibitionists are, thus, not entitled to browbeat

or intimidate those who seek to develop, explore, and clarify it. The traditional Chinese view of their own language, I argued, is conceptually clear and fully consistent with current science. Further, I reasoned, it is useful in a host of interesting scholarly projects—one of which is understanding and appreciating ancient Chinese linguistic philosophy. We also can gain some reflective distance and insight into traditional Western linguistic thought by treating the Chinese view as a viable alternative.

Since Dr. Unger's full article cited my *Language and Logic in Ancient China*, I assume he knows that I presented a detailed theory of classical Chinese linguistic philosophy. I certainly recognize the right of rivals to challenge my interpretive conclusions, but Dr. Unger simply objected *on principle* to my analysis. It was that form of objection that I addressed in this article. I also shamelessly footnoted my more recent book as a source of still more argument about classical philosophy of language. I appreciate that a busy academic with no respect for an author may not take the time to follow up such references, but it does make the accusation that my account has no textual basis less than charitable.

From his opening statement to his conclusion, Dr. Unger's points presuppose that I defend a crude caricature of the Chinese view. I explicitly rejected that caricature on the third page of my article. I challenged his definition of 'ideograph' as a category mistake and argued at some length that it was incoherent. Still, Unger writes as if I defend that definition. Prohibitionists, I openly allowed, *rightly* object to the notion of a "universal language" and to the "pictographic resemblance" explanation of a character's meaning. Dr. Unger's criticisms presume that to reject those, one must embrace the dependency principle. I argued, on the contrary, that the traditional Chinese conception makes the rejection of all three fully coherent. Characters refer in virtue of their specific history and the cultural conventions of use, not in virtue of pictographic resemblance.

For the record, I also said that my view differed from either the traditional Western or Chinese views. Any adequate account needs to add syntactical compositionality. The differences between the position I defend and the position he wants to attack underwrites Dr. Unger's characterization of the nuances of my analysis as puzzling and inconsistent. He says nothing to show it is *internally* inconsistent.

I will submit the remainder of Dr. Unger's barrage of points to the reader. I will not reply to open-ended negatives (accusations that there are things I did not say, authors I did not cite, or unspecified developments at MIT) or the use of rhetorical questions as conclusions (How would Hansen . . . ?). Dr. Unger assumes that his ability to ask such questions proves they cannot be answered. They are easily answered but not in the limited space it takes to ask them. I do agree that extending the list of such "objections" would be easy. If he would detail the missing proof of the dependency principle or why he thought answers impossible, I would be happy to reply in equal detail.

Speaking from my internal author's perspective, he carries his misstatement of my "central claim" over into every comment he extracts from my article. He never notes when it is the dependency principle under discussion rather than the simple claim that Chinese is ideographic. I appreciate that one may not easily glean an author's intentions from even a charitable reading of his essay. If that is the case, I am surely at fault. Even with the gap in assumptions and method, however, I find it hard to understand how he could miss my explicit rejection (with diagrams, no less) of the construction he puts on my argument.

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