# What Is the Proper Characterization of the Alphabet? I. Desiderata

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To a point, an alphabet can be viewed as a "language" and described by a "grammar"; however, since for any such language many different grammars are possible, to take the "linguistic" analogy seriously is to want to find criteria for judging which "correct" grammar is "best." If we grant that the alphabet's users have some systematic mental representation of the alphabet, then the basis for this judgment is clear: that grammar is best which best approximates to the system that people have in their heads. To show how psychological evidence bears on this question, two sophisticated "linguistic" analyses of the alphabet are examined; the conclusion is drawn that the evidence points toward another analysis.

*I have learned that a good question is greater than the most brilliant answer.* *Louis I. Kahn*

## 1. *Introduction*[1]

1.1. The proper characterization of the alphabet is the one that tells us what we want to know. What is that? Put simply, we want to know why it contains the symbols it does instead of some other symbols. Unless, of course, it is merely a congeries of arbitrary and unrelated characters. But if it were, any new arbitrary and unrelated character would be as suitable an addition to the alphabet

1. Part II of this series of papers will present a new characterization of the alphabet in terms of an iconic grammar employing distinctive-feature matrices and generative rules. The distinctive features—unlike Gibson et al.'s—are not chosen for their putative individual psychological reality; rather they are chosen to operate within an over-all system (or "grammar") whose generated *products* are designed to accommodate, and explain, a diversity of psychological findings. The products of this system are, more than letters, letters-cum-analyses: that is, entities structured at a variety of interconnected levels. Among those levels are distinctive ones having correlation with such familiar terms as "discrimination,"



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as any other; and this is patently false. The symbol" Ⅎ " is much less acceptable an addition than, say, " Λ ", as the Emperor Claudius discovered.[2] The alphabet cannot be a mere congeries because we, its users, clearly have an idea of "relative suitability," or "relative wellformedness," or in other words an idea of to what degree a proposed new letter has in common with the old ones the attributes they have in common with each other. So the alphabet must be a set of characters with rather marked homogeneities, irrespective of how conscious of those homogeneities we might be. Identifying these homogeneities is obviously a first step towards determining why the present letters, and apparently any new letters, must have those features. Given a correct statement of the common features and a correct statement of how far their differences may range, obviously we would be in a position to derive the full set of wellformed letters from the underlying "rules" which determine their eligibility as members of that set. In short, in the familiar sense we would be in a position to present a "generative grammar" for the full set of wellformed letters, where "generative grammar" is used in the sense laid down by N. Chomsky (1957, 1962). Since 1966 the discipline which aims at constructing grammars for pictorial "languages" has had a name, "iconics"; what we are proposing, then, is an iconic study of the alphabet. (On the risks of the linguistic analogy see [Hymes 1964], [Jakobson 1967], and [Minsky 1968].)

"(motor) production," and so on. To anticipate, these distinctive features are not strokes, but abstract properties of vectors.

Part III will present a number of remaining issues and will tie off some loose ends. In particular, Part III will take up the question of whether or not it is after all legitimate to postulate a single system underlying the several aspects of human performance (ranging from cognitive apprehension through slips of the pen) of the present-day Western alphabets. At this point evidence from iconics will be drawn upon to inform a discussion of the general issue of "competence" and "performance." A broader discussion of iconics will conclude.

2. That is, Claudius' "digamma inversum" is less acceptable, for /v/ or any other sound, than "V inversum" would have been. Cf. Diringer (1968, Vol. I, p. 421) and Jensen (1969, p. 524). Eventually, of course, "V" spawned two other letters, "U" and "W," to "mean" distinctive sounds formerly or variously meant by "V." Curiously, "V" itself is in turn a degenerate form of primitive "F," the digamma whose inversum failed so signally (Jeffrey 1961, p. 35).

So far, in the interests of simplicity, we have used the phrase "the alphabet" as if there were only one, and to a lesser degree the phrase "the grammar" in the same way. But there are many different alphabets, of course, just as there are many languages, and there is no more a priori reason to expect one grammar to specify two alphabets than there is to expect one grammar to specify two languages like, say, French and Spanish or French and Vulgar Latin. In appearance, the minuscule Roman alphabet is as different from the majuscule Roman as the Greek or Cyrillic majuscules are; what the grammatical relationship amongst these alphabets might be remains to be determined.

More seriously, though, we have not yet constrained our requirements on the grammar of "the alphabet" to the point where, for a given alphabet, just one grammar would be possible: or just one grammar picked as best. Accommodating noted homogeneities is an insufficient constraint because the rules of stating them could take innumerably different forms: as see the competing solutions examined below. How to choose among them? The answer can be inferred from the preceding paragraphs, and is in its broadest statement almost self-evident: if we are trying to describe alphabets in terms of homogeneities tacitly "known" to the alphabet's users, clearly the "best" representation of those homogeneities is the one that most closely corresponds to the representation that those users in some sense have in their heads. So, what the preceding two paragraphs add up to is this: there is no correct or adequate treatment of the alphabet that is typographic or epigraphic or paleographic or whatever unless that treatment takes into account what contribution our mind's apprehension of the alphabet makes to the alphabet's form; or what in some sense we "know" about the alphabet's characteristics. Knowing what conditioned ancient Greek alphabetic changes is obviously part and parcel of knowing what occasions our or the Greeks' acceptance of additions to the alphabet and what conditions variations in the forms of letters. This means that iconics is, among other things, a psychological study. Iconics must lead towards, and at some level confront, the cognitive and visual organization imposed on the alphabet by those who learn, know, and use it.[3] (This point will perhaps be obvious to psychologists or

[(Brown 1964), (Romney and D'Andrade 1964), (Colby 1975)] anthropologists; less so, until recently, perhaps, to students of graphics.)

Now, the notion that iconics is in part a cognitive study cannot have the same implication it would have had five years ago. Then, in linguistics and in the studies (like iconics) historically derived from linguistics, the basic implication was taken to be that a discipline could lay claim to pursuing "cognitive" researches if it achieved a high level of correspondence to fact while adhering to certain principles of economy which bore some indirect cognitive freight. The "facts" to be accounted for went well beyond the essentially adventitious data attested by usage, so that the languages that linguistics hoped to explain were infinite sets of sentences; and the sentences in turn were taken to be associated pairings of syntactic derivations and semantic readings. The principles of economy boiled down to a specific version of the scientific notion of parsimony: in the specific version, two grammars of equal power to account for a given language were to be graded by how many symbols each had (on a component-by-component

3. Although psychologically-oriented disciplines other than psychology itself tend to use "cognitive" to cover "psychological," this usage is permissible only because "cognitive anthropology" and latterly "cognitive linguistics" deal almost entirely with phenomena that psychologists themselves would largely term, as distinguished from "perceptual," "cognitive." I have probably over-used the term "cognitive" in the pages which follow; but I trust no harm has been done since when what is under discussion is clearly perceptual (in our case, "visual"), I believe the appropriate reminders are registered. Lest there be one or two cases of residual ambiguity, though, let me phrase informal definitions of "visual" vs. "cognitive" in terms of how one might "know" or "mentally apprehend" the letter "B": what one does is "visual" insofar as apprehension is atomic ("straight line"; "curved line"; "closed cusp"; perhaps "symmetrical") and/or traceable ultimately to cell-specific functions of the sort discussed by Hubel and Wiesel (1965); what one does is "cognitive" insofar as apprehension is structural or employs rules (e.g., "a 'B' has a staff flying two closed cusps, and is one of a class of similar letters . . ."). The purely visual association between "B" and "b" is about the same as that between "B" and "p"; that "B" is paired with "b" and not "p" is therefore, in a weak sense, "cognitive." The distinguishing factor is not whether or not there is a visual signal (certainly "p" is visually different from "b"), but whether or not the event in question (the pairing "B"/"b") is *predictable* from just that visual signal (predictable by, say, the proverbial Martian). As is plain, the "visual"/"cognitive"/"motor" distinctions being used here are rather informal terms; though they will be toughened up somewhat in Part III.

basis), with the sparer the winner. Naturally no one thought that pursuing a cognitive account, in the sense just sketched, meant that such an account would be necessarily achieved; but the consensus appears to have been that the product of such a pursuit would not be very wide of the mark, needing perhaps only last-minute tinkering to bring it into accord with refractory psychological findings. More recently, however, beginning about 1970, this view has fallen under a cloud. It has begun to appear that linguistics, hence iconics, might be well-advised to consider psychological evidence throughout its investigations and at every stage in the construction of its scientific accounts. Briefly, it seems that, as judged by the "number-of-symbols-per-grammar criterion," people do not always construct so "economical" a cognitive account of what they learn as would be possible in some idealized case (Watt 1974). (More correctly, their constructed account is partly "economical" by a criterion other than the one that minimizes the size of the grammar at the expense of increasing the complexity of the sentential derivations that constitute the language.) In sum, then, our earlier remark that iconics must clearly be a cognitive study has a rather more direct and more solid implication than it would have had a short time ago: that is, that we must take psychological data into account if we are to hope to achieve an adequate characterization of the alphabet. The "marked homogeneities" we took notice of earlier, the attributes that determine relative suitability for inclusion in our present alphabet—these, then, should be studied with respect to what psychological evidence we can gather, from whatever quarter.

Those quarters are three:

(1) Experimental evidence, much of it gathered by perceptual or motor psychologists having no direct interest in iconics;
(2) Ontogenetic evidence (evidence from studies of how children master alphabets and other iconic systems);
(3) Historical evidence (the records of past and present evolutionary changes and "mistakes").

Rather than conduct the discussion in generalities, let us examine specific instances of the sort of evidence that iconics must confront. Under (1), then, there is experimental evidence that subjects trying to identify letters flashed at them more easily confuse "M" with "N" than "K" with "Y" (Gibson et al., 1963), and so clearly an iconic account attempting to jibe with this evidence must at some level describe the letters in question so that the first pair of letters shares proportionately more attributes than the second pair. Again under (1), there is evidence (Jonides and Gleitman 1972) that subjects can more easily pick a given letter from among numerals than from among other letters, and so an adequate account must tell us, presumably, why other letters form a less distinctive background for a particular letter than numerals do. Still again under (1), there is evidence (Huttenlocher 1967; Zusne 1970) that subjects (children) have greater difficulty in distinguishing left-right mirror reversals of letters than in distinguishing top-bottom mirror reversals, as long as presentation is horizontal as in a line of print; this finding must also be accommodated.

Under (2), there is developmental evidence (Watt and Jacobs 1975) that children typically master "N," the pair "S" and "Z," and "J" last, of the majuscules; and any adequate iconic account of the alphabet must explain this, clearly. Again under (2) it seems that many children get "S" backwards but "Z" correct or "Z" backwards but "S" correct, and this must be accommodated. Still again under (2), it seems that children after having mastered the majuscules have them all correct, but then after mastering the minuscules and/or the cursives start making "N" backwards on occasion, a habit that quite a few carry into adulthood; and this must be explained (and the explanation is not quite so simple as it might seem).

Passing to (3), there is historical evidence that "Γ" evolved into "C" (Watt 1973, based on Jeffery 1961), an evolution which must be accounted for by a correct notion of iconic evolution. Again under (3), history shows us that the lower-case or minuscule letters developed fairly simply from the upper-case or majuscule letters (Mallon 1952), in large part due to the interposition of "motor" (hand-movement) factors; and any account of the alphabet that could not square with this evolution should obviously, other things being equal, be disfavored. And lastly under (3), in boustrophedon Greek letters like "B", but not "S", once varied with whether the lines they occurred in were written (and read) from left to right or from right to left; and this "right-tending/left-tending" bias must obviously be accounted for.

It may still seem a little inconcinnous that evidence of the sort cited should be claimed to bear on the "simple" problem of characterizing the alphabets; but in each case a little reflection will show, I think, that the claim is, rather than far-fetched, if anything too modest. To nudge these reflections in the desired direction, perhaps I might remind the reader that, insofar as he concedes that the only characterization of the alphabets that holds genuine interest is the one that somehow matches the one we have in our heads, and insofar as he concedes inapplicability of any a priori criterion for selecting that characterization, to that extent he ought to welcome any evidence of the impress left on alphabets by the minds (and eyes and contrastively hands) of its users. On this reasoning children's reversing "J" and the Greeks' failure to reverse "S" and children's confusing "S" and "Z" and the effect (as in boustrophedon) on letter-orientation of line-orientation—surely all these sorts of evidence must seem, not only less arcane, but positively germane. I will not belabor the point here, where in any case citation of these kinds of evidence largely serves a negative purpose; but we will resume the discussion in Part III of this series.

Taking a somewhat different tack, we might informally judge a given grammar in terms of breadth (how much it accounts for) and depth (how deep it goes). The two are related, since if only a narrow scope of the language is taken into view the treatment, blind to the requirement that all possible items of the language be accounted for, will necessarily be relatively shallow. (We have to qualify this statement by noting that a grammar of narrow scope [say, the 26 letters of the present-day alphabet instead of the extended set of the wellformed *possible* letters] might nevertheless be judged less shallow if it were partly responsive to psychological criteria of the sort mentioned above. We will return to this point when discussing Gibson et al.)

Continuing this line of attack one more step, Chomsky once upon a time (1965) defined three levels of adequacy relative to which the achievement of a grammar for a language could be judged; these levels, deliberately intermixing "breadth" and "depth" of analysis, serve as part of the background of modern (generative) linguistics, and I would argue should serve the same function for iconics. The three levels of adequacy are (1) observational, (2) descriptive, and (3) explanatory. The lowest level, the observational, is both narrowest and (therefore) shallowest: it is achieved by an analyst who merely accounts for the data attested by some corpus or set of corpora, a compact representation of the data being the least adequate account. (For a finite list like the alphabetic letters, a mere list would therefore be the minimum adequate account.) A somewhat more revealing observational analysis might break down the elements of its corpus into "distinctive features," as seen below; at a deeper level, those features or the analysis in general might have some psychological warrant. The next level of adequacy defined by Chomsky, the descriptive, is attained by an account which characterizes the entire set of wellformed elements of the language in question, thus defining the notion, "wellformed element of language *L*." An analysis at this "broader" level could be "deepened" by squaring it with psychological evidence, thus becoming "cognitively-descriptive" (Watt 1974). (Perhaps the parallel term "perceptually-descriptive" might also prove useful, as see the discussions of Gibson et al.'s work below.) The highest level of adequacy, the "explanatory," is attained by a grammar constructed within a theory which selects that grammar over all others of descriptive adequacy by appeal to higher principles (e.g., of economy) which, in turn, might guarantee a priori psychological accuracy. This highest level, given the low probability of attaining it in the foreseeable future, corresponds in linguistics to the Grail.

Finally, we can relate these standards of adequacy to another set of distinctions, one split basically along the lines of "discrimination" vs. "production" (or "recognition" vs. "composition"). Clearly, to reprise, an analysis which aims only at discriminating among (say) the 26 letters of the majuscule alphabet, and so which will be "narrow" almost by definition, will be able to achieve no more than observational adequacy; how "deep" such an analysis will be will depend on considerations like those cited just above. The critical factor is the nature of the discrimination task as ordinarily conceived: that task is taken as being met if each element whose recognition is desired is so described that it is distinguished from all other such elements (to continue our example, if each majuscule is distinguishable from the other 25). There is no need to distinguish, though, each element from anything *outside* the set of elements whose recognition is sought (no need to distinguish "A" from "4" or from a map of downtown Pittsburgh, for example). The possibility of maintaining so narrow a compass is lost, of course, when we pass from discrimination to composition, since to compose (produce) an "A" one must know, not only how it differs from "B" and the rest, but also how it differs from everything else. Thus passage to a composition task automatically raises one's sights: to greater "breadth"; to "descriptive" adequacy; and possibly to greater "depth." As we will see below, it is an interesting question whether or not an analysis built as a discriminational account can be *extended to* (properly included in) a compositional account.

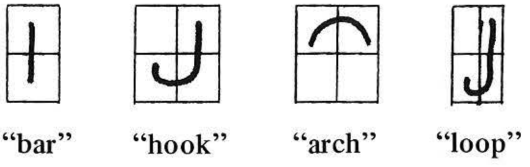
These three interdependent standards of measurement—"breadth" vs. "depth," Chomsky's three levels of adequacy, and "discrimination" vs. "composition"—will now be brought to earth in the context of a discussion, taken to some detail, of two existent iconic analyses of alphabetic characters; this discussion will occupy most of the remainder of the paper. Both analyses date from the 1960's; neither was undertaken with a view toward satisfying the three sets of criteria just proposed (though one aimed at some "depth"), and were it not for the fact that we will be using them purely as illustrations of expository points it would be a little unfair to charge them with the shortcomings to be cited shortly. Both are discriminational analyses. Sometimes we will be singling out their shortcomings relative to their own goal of accounting for (or enabling) discrimination; but at other times, and more to our basic point, we will be remarking where these analyses, even if accepted as discriminationally satisfactory, fail to account for compositional facts and/or fail to permit extensibility to a fuller characterization that *would* account for such facts.[4] The discussion will

4. We presuppose that extensibility is desirable, even though an inextensible recognition routine might be more economical than an extensible one. But after all, we are using Eden-Halle and Gibson et al. to illustrate expository points, not to charge them with dereliction of duty.

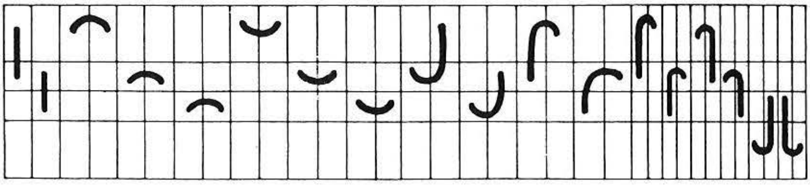
also throw into a lurid light the much-vexed question of whether or not what description of visual "languages" needs is "more rigor": unless further defined, this statement will be seen to be meaningless, since a successful discriminational analysis is obviously as "rigorous" as could be desired; yet from such an analysis much is lacking.

The two analyses to be considered are both well-known; and comparing them is made easier by their having Jakobson and Halle (1956) as a common ancestor, from which they diverge in complementary ways. They are the Eden-Halle analysis of cursives (Eden 1961, Eden and Halle 1961) and the Gibson et al. analysis of printed majuscules (Gibson et al. 1963, Gibson 1965, Gibson 1969).

Figure 1 (from Eden 1961). (a) A graphic portrayal of the four "distinctive features" of Eden and Halle.




(b) The 18 of Eden and Halle's "phonemes" that they retain to form letters ("morphemes").



1.2. *Eden-Halle.*

The analysis of English cursive letters by Eden and Halle was designed to produce a system capable in principle of reading handwriting, and it falls within a large class of "pattern-recognition" studies, among which Eden-Halle still stands as an early landmark. (A useful introduction to the so-called "linguistic" approach to pattern-recognition, exemplified by Eden-Halle, is Kirsch [1964]; surveys are Miller and Shaw [1968], Kanal and Chandrasekaran [1972], and Rosenfeld [1973]; a more recent full-length study, comprehensive if oddly uncritical, is Fu [1974]. For a broader view of pattern-recognition in relation to psychological findings as of the mid-1960's, see Neisser [1967], and cf. Shillman et al. [1974].) Eden and Halle characterize the modern English cursive letters in terms ultimately of four "line-segments" which they explicitly (p. 295, *n.*) identify as analogous to the "distinctive features" of a generative linguistic grammar (e.g., Chomsky and Halle 1968). These four line-segments ("bar," "hook," "arch," and "loop"—v. Figure 1, *a*) are then altered by rotation (their "reflection") on either horizontal or vertical axes to yield eleven visually different line-segments, of which only nine are retained at the next level. (The issue of visual ambiguity [cf. Baird and Kelly (1974)] is thus neatly and quite properly sidestepped). Those nine are at that level positioned in one of three partially-overlapping "fields" to constitute, finally, the "strokes" from which letters are to be formed. Eden identifies these "strokes" as analogous to "phonemes" (1961, p. 84, *n.* 6). (To minimize confusion, below I will spell the distinctive features as "Bar," "Hook," and so on, and the "phonemic" strokes containing or incorporating those features —along with others—as "bar," "hook," and so on.) Some of the "strokes" are also discarded before advancing to the next level; the eighteen that are retained are given in Figure 1, *b*. At the next and (more-or-less) last level these strokes are combined sequentially into proto-letters (e.g., those of Figure 2, *a*.), Eden's "morphemes" *(ibid.)*. Then by a subsequent "collation rule" (something like a "morpheme structure rule") the strokes of these proto-letters are adjusted into the recognizable letters of Figure 2, *b*; and by another "collation rule" (a little like a "sandhi rule") these letters, when placed into sequences to form words, suffer a final readjustment. An outcome of this process is shown in Figure 2, *c*.

Though designed to be used as a recognition or discrimination device, Eden-Halle is nevertheless a limited generative device, and it can be used to compose rough-and-ready approximations to handwriting, as demonstrated. Since it is compositional capacity that is of greater interest, we will take that aspect of Eden-Halle as our primary focus in considering its shortcomings. (I stress once more that these are for the most part shortcomings which in the "pre-cognitive" days of the early 1960's were quite invisible.)

The first shortcoming we might note is the most glaring—words composed in terms of the analysis and by use of its cursive letters and "collation rules" are often virtually illegible. Silent confirmation of this suspicion is provided by Figure 2, *c*: the "globe" there presented is so imperspicuous that in (Eden and Halle 1961) it emerged into print, presumably after several publisher's scrutinies, upside-down. But a more serious point engages still more directly the "observational" vs. "explanatory" issues raised just above: the letters formed by the Eden-Halle system do not, by and large, seem to be compatible with any of the three sorts of psychological evidence (experimental, ontogenetic, historical) cited above.

Historical evidence is slighted because the origins of the cursive letters are in some cases absolutely occluded by their Eden-Halle derivations; for example their minuscule cursive "s" (Figure 2, *d*) is composed of a "hook" plus a "cup," with the "s"-curvature of the letter, reflecting its derivation from printed "s," vanishing irrecoverably.[5] Such historical evidence is of more than pedantic interest, since passing to contemporary observation it is certainly possible that modern users, including children, are aware of the connection between printed and cursive "s," and that this tacit knowledge affects their performances. Nor is this merely a criticism of a particular solution within an Eden-Halle analysis of more general capability; obviously their analysis of "s" could not easily

5. This is the place to retract a statement I have made elsewhere (Watt 1973), based on a too-hasty reading of some old notes, to the effect that the "hook" of the Eden-Halle "s" is the *first* stroke rather than its second.

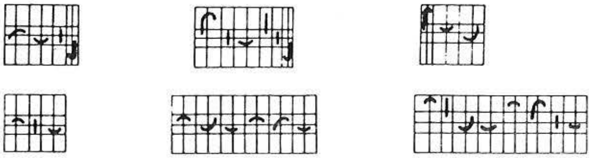
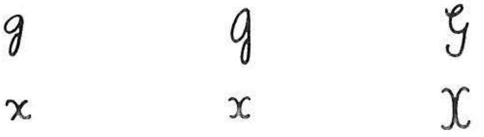
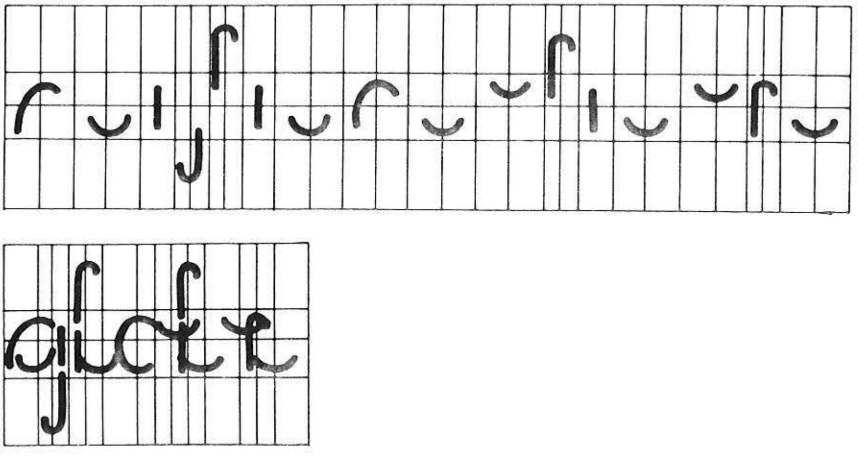


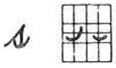
Figure 2 (from Eden 1961). (a) Above: Eden-Halle proto-letters ("morphemes").
*(b)* Below: the "collated" letters corresponding.



(c) The word "globe" as generated by Eden-Halle, first as a sequence of proto-letters and then as a twice-collated "word."

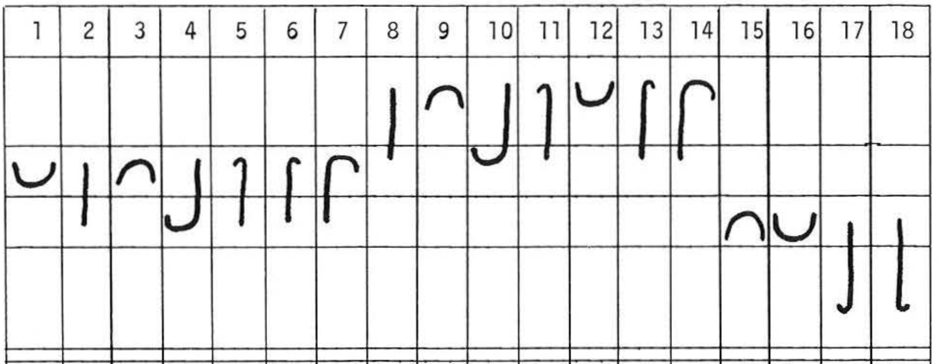


(d) The minuscule letter "s" in terms of its "distinctive features."



be rectified in the direction indicated (a new "Serpent" feature might do the trick), just as their system could not easily dot its "i's" or cross its "t's." (Eden [1961, p. 84, *n.* 5] observes that "t's" cross and "i's" dot are redundant: a remark applicable to discrimination, but scarcely to composition.) On the issue of whether or not the Eden-Halle analysis is compatible with experimental or ontogenetic evidence, we ask whether or not their distinctive features have, or bid fair to have, the psychological implications that such features have been taken to have elsewhere. To begin to answer this question, however, it will be useful first to recast the Eden-Halle analysis into a more accessible form. They say that their solution is in terms of "distinctive features"; but as the reader must have observed himself, this notion fits into Eden-Halle in a way unfamiliar to those used to the linguistic systems from which it was borrowed. Their distinctive features "Bar," "Hook," "Arch," and "Loop," do eventually occur in the complexes, or bundles, which define the "strokes" ("phonemes") from which the letters will be formed. But these bundles of "distinctive features" are unusual in that they include elements other than distinctive features: since the features themselves, before they emerge into bundles, have been radically altered by rotation and placement "operations" (not given a linguistic correlate). Since it is not clear why a given stroke is more distinctive for being "Hook" than for having been rotated 180° or for having been visibly lowered, Eden and Halle's segregating their "distinctive features" from their "operations" does not seem motivated. Thus, since in any case we will want a way of accounting for expected responses to rotation as a minimum discriminator between strokes or letters —hence as a potential "distinctive feature" in the accepted sense— we will adapt Eden-Halle accordingly, introducing four new distinctive features: "Raised," "Lowered," "Rotated," and "Mirrored," as shown in Figure 3.

We turn now to consider how "deep" Eden-Halle strikes *as a discriminational analysis.* The sorts of psychological evidence we might expect for a mentally internalized "distinctive feature" system like that of Figure 3 have often been discussed (v. Gibson 1969). Two are: (1) if strokes are mentally distinguished from each other by these distinctive features (and by nothing elese), it should



| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | 12 | 13 | 14 | 15 | 16 | 17 | 18 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| BAR | | + | | | | | | + | | | | | | | | | | |
| HOOK | | | | + | | | + | | | + | | | | + | | | | |
| ARCH | + | | + | | | | | | + | | | + | | | + | + | | |
| LOOP | | | | | + | + | | | | | + | | + | | | | + | + |
| ROTATED | + | | | | + | + | + | | | | + | + | + | + | | + | | |
| MIRROR-ED | | | | | + | | | | | | + | | | | | | | + |
| RAISED | | | | | | | | + | + | + | + | + | + | + | | | | |
| LOWERED | | | | | | | | | | | | | | | + | + | + | + |

Figure 3. Chart of distinctive features for "Strokes" forming letters of the alphabet (adaptation of Eden-Halle).

follow that under difficult viewing conditions their confusability should increase as the proportion of shared features increases (for phonological features, see Brown and Hildum [1956]). And (2), if children are indeed internalizing a Figure 3 matrix, they should first discriminate between strokes having the greatest number of distinctive-feature differences (for phonological features, see Scvachkin, as cited in Tikofsky and McInish 1968, Gibson 1969). But when we ask if the Eden-Halle solution suggests confirmability in terms of such psychological attributes, the answer is no. For example, referring to the chart of Figure 3, the 2nd and 3rd strokes are distinguished by disagreement on two features, Bar and Arch; but the 2nd and 4th strokes are also distinguished on two features, Bar and Hook; yet obviously the 2nd stroke ("bar") is much closer to, hence expectably more easily confused with, the 4th stroke ("hook") than the 3rd ("arch"). Yet again, notice that the 13th and 14th strokes also differ by two features (Loop and Hook); yet surely the 13th and 14th strokes are much more easily confused. So far we have dealt only with Eden and Halle's original four features, which have the curious characteristic that no two strokes can differ on just one feature (e.g., the "hook" and "arch" must differ on features "Hook" and "Arch" *both*); but our failure to find expectable psychological concomitants extends also into the eight-feature system, for notice that the 1st and 16th strokes differ by one feature (Lowered) and the 1st and 3rd do also (Rotated); yet surely the confusability of these two pairs should not be identical. Turning now to the opposite effect—maximum difference *cum* earliest discrimination—we find the same lack of correlation. To take just one example, the 11th and 7th strokes differ by four features (Hook, Loop, Mirrored, and Raised), while the 2nd and 15th differ by only three (Bar, Arch, and Lowered); yet presumably it is the latter pair that are more easily distinguished, certainly by children; the literature on children's reversals of letters is too univocal for us to think otherwise (v. Watt and Jacobs 1975).[6]

6. Of course "Mirrored" and "Rotated" could be struck from the list of distinctive features, which would aid things; but it is not at all clear what role these two attributes play in Eden-Halle unless they *are* distinctive features.

We might also note again, that Eden-Halle was a pioneering study and that doubtless its authors would change much in it if they redid it. In particular, one might speculate that they might change their original four distinctive features "Bar," "Hook," "Arch," and "Loop" so as to permit a more economical definition of their strokes. For instance, their "Hook" is obviously resolvable into a "Bar" plus a "Rotated Arch," and if the "Arch" could be appropriately reduced, the "Loop" could be similarly constituted. Then "Hook" and "Loop" would be eliminated entirely from the list of distinctive features, being replaced in each case by a sequence of two distinctive-feature complexes "Bar" plus "Arch" or "Bar" plus "Reduced Arch," with or without rotations and/or mirrorings. Other improvements in Eden-Halle could also be envisaged; but *passons*.

Of course, it may not be correct to suppose that "confusions" should uniformly correlate with "distinctive features" of the sort in question—see below for some problems with any such supposition—but on the other hand, the failure of Eden-Halle to correlate is so thoroughgoing as to discourage further attempts, at least from this angle, to invest it with psychological significance. More generally, it can be said that the failure of Eden-Halle to rise above observational adequacy was, rather than an accident, a direct product of their aim of constructing an *analytic* device for recognizing handwritten characters (even though the device takes the form of a generative device). Such an aim is, if realized, destined to produce a paradigm case of Chomsky's "observational adequacy." Eden and Halle emphasize this aspect of their work in a number of passages: for instance, in one they straightforwardly admit that letters (like "z") that have distinctive variants ("ʒ" and "ƶ") are simply entered in their lexicon as two separate and unconnected forms (Eden and Halle 1961, p. 290). Such decisions are no hindrance to the achievement of a purely analytic device: but that letter variants are not related to each other is fatal for any theory aspiring to a higher level of adequacy, since at those higher levels contact is made with interconnections: with what the users of the alphabet tacitly "know" about it, insofar as that knowledge determines the alphabet's forms. Equally telling, on the same score, is the fact that in Eden-Halle there is no systematic connection between majuscule and minuscule counterpart.

As a general conclusion, we might say again that present-day analysts of visible languages, or of pictorial systems, do not in demanding "more rigor" of their analyses demand nearly enough. Eden-Halle is "rigorous" enough to pass into a computer recognition program; but by any sort of explanatory or cognitive criterion it must be judged a fascinating failure.

### 1.3. *Gibson et al.*

Often cited, the work of E. J. Gibson and her collaborators (Gibson et al. 1963, Gibson 1965, Gibson 1969) was aimed directly at the achievement of "psychological adequacy" in the characterization of an alphabet, in this case the simple Roman majuscules. Their distinctive features are attributes, not of strokes as with

Eden-Halle, but of the letters themselves (so that Eden-Halle's "morphemes" are Gibson et al.'s "phonemes"). In intent, their features were not chosen by whether they *can be* used to distinguish letters, but by whether they *are*; thus Gibson et al., though they were not aiming at an account of compositional power, were aiming at a discriminational account having merit as a "perceptually-adequate" account. Their systems of hypothesized distinctive features were tested with subjects to try to ascertain whether or not informants seem actually to use those distinctive features in their visual letter-discriminations. The actual "confusion" test of the hypothesis was, like many another, rather simple, since what it set out to show was only that as letters are more similar when judged by degree of shared hypothesized distinctive features, they are concomitantly more often confused (mistaken for each other) by four-year-olds.[7] However, the distinctive features themselves were not chosen blindly, or to provide the right test correlations: rather each was meant to have the warrant of independent psychological (neurophysiological) evidence.

Let's have a look at this evidence. One distinction between kinds of lines is that between straight and curved; Gibson et al. accept this as a basic distinction on evidence from neurophysiological research with frogs (Maturana, Lettvin, McCulloch, and Pitts 1960). Another set of basic differences, among straight line-segments, distinguishes verticals, horizontals, and various diagonals; these they accept on evidence from work with cats (Hubel and Wiesel 1962). [To interrupt, we see that Gibson et al.'s reasoning ran somewhat as follows: "If animals can be shown to make elementary distinctions between 'X' and 'not-X,' then 'X,' with values plus or minus, is a good candidate for inclusion in our set of distinctive features, if it can be used to discriminate letters." In the cases at hand, one asks of a given letter: "Does it have a straight line?" (i.e., "Is it plus or minus 'Straight-line'?"), and so on, accumulating a description of letters in terms of the distinctive

7. Even if the correspondence between "number of shared features (duly weighted)" and "degree of confusion" had been very close, such a series of experiments could at most have proven that the hypothesized set of features was *compatible with* the evidence.

features hypothesized.[8] NB that with Gibson et al. the "distinctive features" of letters are characteristics which typically apply only to *part* of the letters; thus, "B" is assigned the feature "PLUS Straight-line" because it *contains* a straight line. We return to this point below.] Reference may be made to Figure 4 from this point

8. Granting the validity of the individual features, one might suppose that their effects as discriminators, hence their appearance in discrimination matrices, were free from any further uncertainty; but not so. Batchelder and Narens have recently pointed out (1975) that performing quite straightforward Boolean transformations on a feature matrix—constructing a new matrix whose features consist of pairs of the old matrix's features joined by logical connectives—can produce a radically different characterization of the items being discriminated; they also point out that the result of such a transformation, however bizarre, is not automatically discountable on objective grounds of "unnaturalness" or whatever. An example will illustrate. Let us for convenience take a small three-feature matrix in which five letters are distinguished—(i), below—and transform it into a new (ii) in which each possible pairing of (i)'s old features appears, connected by the Sheffer stroke (' | '), in a new feature, so that for two old features ' $\alpha$ ' and ' $\beta$ ', new feature ' $\alpha | \beta$ ' is to be assigned its plus and minus values in (ii) in this way: "MINUS in (ii) if features ' $\alpha$ ' and ' $\beta$ ' are both PLUS in (i), under the same letter; otherwise PLUS"; as follows:

(i)

| FEATURES | C | H | R | A | P |
|---|---|---|---|---|---|
| Curved | + | | + | | + |
| Symmetric | + | + | | + | |
| Diagonal | | | + | + | |

(ii)

| FEATURES | C | H | R | A | P |
|---|---|---|---|---|---|
| Curv \| Sym | | + | + | + | + |
| Sym \| Diag | + | + | + | | + |
| Diag \| Curv | + | + | | + | + |

As we see, the (ii) feature "Curved | Symmetric" is assigned MINUS (blank space) under the letter "C", because in (i) both "Curved" and "Symmetric" were PLUS under "C"; whereas the same (ii) feature is assigned PLUS under "H" because in (i), under "H," "Curved" and "Symmetric" were *not* both PLUS.

The transformation (i) into (ii) radically changes the relations among the letters of the matrix, as predicted: for instance, in (i) "R" is more similar to "A" than to "H," whereas in (ii) this relation is exactly reversed. And notice that, since we did not guard against it, (ii) has lost information, since in (ii) "H" and "P" can no longer be discriminated at all. (And note that (i) is not *recoverable* from (ii).)

The point just made would be less serious did not the "features" derived from rummaging through such sources as Hubel and Wiesel (1965) often appear to be neurophysiologically joined by logical connectives, in that for example a "hypercomplex cell" able to distinguish one "feature" does so only while also distinguishing another.

on (with qualifications to be noted). As to curved lines, Gibson et al. identify as distinctive features "Open" (like "C") vs. "Closed" (like "B"), basing this decision on data from research on the early development of form discrimination (Gibson et al. 1963). They include a feature of "Intersection" on similar grounds, citing Piaget. Two additional features of "redundancy"—"Cyclic change" and "Symmetry"—are added to the list; the second of these is present in letters which are symmetrical on the horizontal axis ("B"), the vertical axis ("A"), or both ("X"); while the first, a somewhat more arcane attribute, is said to be present in letters showing parallelism, though it seems to be found in "E" and "H" but not in "Z." In any case, "Cyclic change" is claimed to be based on some stabilized retinal image research in which parallels act as units in appearing and disappearing (Pritchard, Heron, and Hebb 1960). Lastly, "Vertical terminates" (as in "H" but not "E") and "Horizontal terminates (as in "F" but not "H") are added as features, on the ground that termination ("discontinuity" in their terminology) has been shown to fire certain cortical cells (Hubel and Wiesel 1965). Note that all of these features—hence presumably any system based on them—are "visual" rather than "cognitive."

We must note in passing that the system just described (and presented in Figure 4) is not precisely Gibson et al.'s system. They do not enter "Straight" or "Curved" as distinctive features in their list, perhaps because they are redundant given the fact that in their account all "Straight" letters, and only these, are "Horizontal," "Vertical," or "Diagonal." Again, Gibson et al. enter two distinctive features to cover the "Open" value of the "Open/Closed" discrimination for "Curved" letters; one of these features is used just to distinguish "U" from "C," and since identifying the presence in "U" of "Straight" lines will do this just as well, I have jettisoned this part of their analysis. Lastly, the letters in Figure 4 are not in any case exactly as Gibson presents them (1969, p. 88), even where the distinctive features are the same, for that presentation appears to contain a number of typographical errors, which I have silently corrected (while doubtless adding some new ones).[9]

9. Perhaps I should also mention that I have retained a couple of feature-attributions because, while they might not seem to fit the letter concerned as a

| | A | E | F | H | I | L | T | K | M | N | V | W | X | Y | Z | B | C | D | G | J | O | P | R | Q | S | U |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| CURVED | | | | | | | | | | | | | | | | + | + | + | + | + | + | + | + | + | + | + |
| STRAIGHT | + | + | + | + | + | + | + | + | + | + | + | + | + | + | + | + | | + | + | + | | + | + | | | + |
| HORIZONTAL | + | + | + | + | | + | + | | | | | | | | + | | | | + | | | | | | | |
| VERTICAL | | + | + | + | + | + | + | + | + | + | | | | + | | + | | + | | + | | + | + | | | + |
| NE DIAGONAL | + | | | | | | | + | + | | + | + | + | + | + | | | | | | | | | | | |
| SE DIAGONAL | + | | | | | | | + | + | + | + | + | + | + | | | | | | | | | + | + | | |
| CLOSED/OPEN | | | | | | | | | | | | | | | | + | | + | | | + | + | + | + | | |
| INTER-SECTION | + | + | + | + | | | + | + | | | | | + | | | + | | | | | | + | + | + | | |
| CYCLIC | | + | | | | | | | + | | | + | | | | + | | | | | | | | | + | |
| SYMMETRY | + | + | | + | + | | + | + | + | | + | + | + | + | | + | + | + | | | + | | | | | + |
| VERT. TERMINATES | + | | + | + | + | + | + | + | + | + | + | + | + | + | | | | | | + | | + | + | | | + |
| HORIZ. TERMINATES | | + | + | | | + | + | | | | | | | | + | | + | | + | | | | | | + | |

Figure 4. Chart of distinctive features for letters of the alphabet (after Gibson 1969).

The set of features is a little altered from Gibson's, though not much (see discussion in Section 1.3, above). I have basically used Gibson's characterization of the letters even where I found it dubious. I have added "Horizontal Terminates" for "C," "G," and "S"; and as mentioned above, I have redone "U" altogether: v. *n.* 8. The description ignores serifs.

| | A | ∀ | F | ꟻ | H | rh | T | ⊞ | K | \|X | | D | ↀ | DX | 8 | 8 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| CURVED | | | | | | | | | | | | + | + | + | + | + |
| STRAIGHT | + | + | + | + | + | + | + | + | + | + | | + | + | + | + | + |
| HORIZONTAL | + | + | + | + | + | + | + | + | | | | | | | | |
| VERTICAL | | | + | + | + | + | + | + | + | + | | + | + | + | + | + |
| NE DIAG. | + | + | | | | | | | + | + | | | | | | |
| SE DIAG. | + | + | | | | | | | + | + | | | | | | |
| CLOSED/OPEN | | | | | | | | | | | | + | + | + | + | + |
| INTER-SECTION | + | + | + | + | + | + | + | + | + | + | | | | | | |
| CYCLIC | | | | | | | | | | | | | | | | |
| SYMMETRY | + | + | | | + | + | + | + | + | + | | + | + | + | + | + |
| VERT. TERM. | + | + | + | + | + | + | + | + | + | + | | | | | | |
| HORIZ.TERM. | | | + | + | | | + | + | | | | | | | | |

Figure 5. Letters and anti-letters characterized after Gibson.

Most of the anti-letters—each indistinguishable by the Gibsonian characterization from the next true letter to its left—would occur to anyone looking critically at Gibson et al. The first did occur to Restle [1975, p. 196], with several others whose claimed indistinguishability, however, is false. E.g., Restle's Ŧ, claimed to be Gibson-equivalent to F, is symmetrical where F is not; Restle's Կ, claimed to be Gibson-equivalent to H, is not symmetrical (by the Gibsonian criterion) where H is; Restle's ≠, also claimed (p. 197) to be Gibson-equivalent to H, contains a diagonal where H does not; and so on.

I should note that perhaps some of my anti-letters—with their genuine counterparts—should be +Cyclic. The last 5 figures are all of them Gibson-equivalent.

In any event, the intended predictive thrust of Figure 4's chart of distinctive features is obvious enough: namely, that for any two letters the higher the percentage of their features they have in common (hence putatively the more similar they are), the more likely those letters are to be confused.[10] To test this, Gibson et al. ran a "confusion experiment" and derived correlations between the resulting confusion matrix and their distinctive-feature matrix. (It bears mention that they were therefore testing "perceptual" features by an experiment in which "cognitive" judgments might skew the results; but we must defer further discussion of this point until Part III, where the requisite range of phenomena and hypotheses can be brought to play. See, in the meantime, Restle's discussion (1975, pp. 197-200) of "cognitive" processors like "EPAM" and "Pandemonium," which can overlie perceptual features in such a way as to make feature/confusion-matrix prediction a less simple matter.) Since the features chosen and their letter-descriptions do have a measure of intuitive appeal, it is no surprise that these correlations proved promising; at least the con-

general rule, they do fit the form of the letter used by Gibson et al. For instance, their "K" is correctly ascribed the "Intersection" feature because the form they used in their experiments was: K (Gibson et al. 1963). For another instance, their use of their "Vertical Terminates" and "Horizontal Terminates" features does not apply to standard fonts because of the presence in the latter of serifs, which generally (though not always [Hochuli 1973]) replace a "Vertical Terminates" with a "Horizontal Terminates" and vice versa.

However, in other cases I have corrected what I took to be simple errors. As one example, surely if both "F" and "T" are assigned the feature "Vertical Terminates" then "L" ought to be assigned that feature too? Yet in Gibson (1965) it is not (a mistake lovingly preserved in Gibson [1969]). Several other dubious entries also concern "Vertical Terminates" and/or "Horizontal Terminates"—for instance, why should "A" be assigned "Vertical Terminates" when "X" is not?—but there are errors elsewhere as well, besides decisional disparities which it is hard to account for. For instance, how can "A" be assigned "Vertical Terminates" in the first place, when it has not been assigned "Vertical"?

10. It must, of course, be percentage of shared features $(\frac{\text{intersection}}{\text{union}})$ that is decisive, rather than *number* of shared features; for if the latter measure were used, then complex letters sharing a few features could be judged as similar to each other as simple letters sharing almost all of their features. (The reader is not invited to test how well this statement holds up in the Gibson et al. matrix of Figure 4, however; the statement applies only to the *ideal* case.)

fusion matrix correlated better with the feature matrix than chance, and better than the results of a Ramo-Wooldridge letter-overlap technique. However, there were still many cases where something was obviously amiss, as Gibson et al. concede; and clearly their line of attack would have to undergo rather extensive improvements to justify thinking that its constituent distinctive features have genuine warrant of psychological reality. We now take up these and other shortcomings and limitations.

One limitation of the Gibson et al. approach is simply a limitation of any orthodox discriminational analysis (however "deep," and whether "visual" or "cognitive"). It can be encapsulated as follows: the Gibsonian distinctive features that describe P and L are the same ones that describe ꟼ and ⅃. The reader can easily ascertain that this is so from a glance at Figure 5, where several letters and non-letters have been assigned their obvious Gibsonian analyses. For Gibson et al. the addition of features to distinguish ꟼ from P would have been superfluous and unallowable, since ꟼ is not a letter and so need not be discriminated. (A similar point is made by Restle [1975, pp. 175f.].) This means that when the reader discriminates between P and ꟼ, as may one presume he just did, he is performing a feat beyond the ability of the Gibsonian description to describe. Since this discrimination must be made by children learning the alphabet (at first many children are indifferent to whether "P" faces right or left), the issue is apposite; and it is certainly critical with regard to the facts of *composition.* Of course the new feature "Right-Facing" could be added to the complement of features (with "MINUS Right-Facing" meaning "left-facing"), and indeed such an addition might begin to remove some of the anomalies which, in the Gibson et al. confusion matrix, follow in the trail of "J," the majuscule alphabet's one left-facing letter. However, this addition cures but one problem ("T" would still be indistinguishable from "L," for example): the indicated panacea lies elsewhere, as we will see shortly.[11]

11. These limitations apply whether "distinctive features" are extracted as in Gibson et al., or whether, as in Rumelhart and Siple (unpublished) the "Features" are line-segments used essentially in template-matching. Incidentally, the success of the latter analysis in explaining away the Reicher-Wheeler Paradox,

is, if generalizable to reading discrimination at large, puzzling, since any template-matching analysis will be hard put to account for the apparent fact that on first exposure people instructed to "read backwards" can more easily read (a) than (b):


Line (a) is printed and read retrograde and each letter is printed and read retrograde; line (b) is printed and read retrograde but each letter is printed and read oriented normally. Thus, line (a) is "Mirrored" in Kolers' terminology (Kolers 1968, 1969; Kolers and Perkins 1969, 1975), while counter-scan line (b) is "reversed Mirrored."

I introduce this terminology for a purpose, since in the work of Kolers and his collaborators it does not seem that "M" lines *are* more easily read than "rM" lines, by the naive (unpracticed) person: though after about three to five hours' practice in reading these and other distortions those same people apparently do find "M" lines easier (Kolers 1968). (In a subsequent experiment of admirable ingenuity Kolers and Perkins [1975] found that training subjects in one sort of distortion differentially affected their performances in reading others; roughly, training them in a distortion in which [as in "M"] letters share the orientation of their lines [facing rightwards if the line scans rightwards] or in which [again as in "M"] letters are retrograde [backwards], makes reading "M" easier than reading "rM" *unless* the training distortion can be read "normally" by mentally standing it [or oneself] upside-down, or unless it trains one in reading text in which the letters are oriented "counter-scan" and/or in reading text in which its backwards letters are also upside-down and so [probably] fail to ease the reading of the backwards letters of "M." [Thus, in Kolers' terminology, "rN" and "I" raise "M" over "rM"—as of course does "M" itself—while "R," "rR," and "rI," with "rM" itself, raise or leave "rM" over "M"].)

The critical factor that prevents Kolers et al.'s researches from impinging directly on the " 'M' easier than 'rM' " claim made above is this: those researches deal with lower-case letters, while our claim applies to upper-case letters only, as only the latter bear on Rumelhart's results. The difference is not trivial, since the upper-case letters have as a set the property that no one of them is distinguished from another only by a difference in orientation (one facing leftwards, the other rightwards), whereas of course amongst the lower-case letters "p" and "q" differ only in orientation ("Mind your *p*'s and *q*'s"), as do "b" and "d." This means that with upper-case letters, but not with lower-case, orientation-invariant properties can be relied on for identification; which in turn means that consistency of letter-orientation and line-scan, as in normal text and in "M" texts, can be more important than whether or not the individual letters, as in "M," are backwards.

Harcum and Filion (1963) did use majuscules, but it is very hard to interpret their results. Briefly, they report two experiments in which subjects were required to reproduce, from tachistoscopically-presented displays of eight-letter words or "nonsense words," which letters they had seen and in which orientation (normal or retrograde); in the first experiment it seems that "M" sequences are less error-provoking in the right half of these eight-letter sequences than are the "rM"

Other limitations are more the product of Gibson et al.'s particular analysis; and these militate against the analysis *on purely discriminational grounds*. For instance, a glance at Figure 4 will show that "C," "O," and "S" have three features each, and "G," four; and that "C" shares with "G" two of the five features they have jointly; with "O," two of the four they have jointly; and with "S," again two of four. Since the basis for comparison is percentage of shared features, clearly the prediction is that "C" should be confused with "O" and "S" more often than with "G." But this is counter-intuitive (as Gibson et al.'s own confusion matrix II bears out: "C" was confused with "G" as often as "M" was with "N"; while "C" was [implausibly] no more confused with "O" or "S" than it was with "H"). How can this particular prediction have gone so far awry? The answer is that in terms of Gibsonian features "C" must be described as "Symmetric," which "G" of course is not, and "G" must consume two additional features, "Horizontal" and "Straight," just to describe "G's" little tail. What is wanted, obviously, is a description of "G" *that will include that of* "C." But this brings us hard up against a cardinal facet of the Gibson et al. analysis: description of parts of letters is impossible. This statement stands despite an extremely curious circumstance

sequences (Harcum and Filion's "DRW" and "SRW" respectively), but more so in the left half, where the "fixation point" was in the middle. However a closely similar second experiment seems to unseat this result; and, since misreproducing *either* a letter *or* its orientation was counted as an "error" it would be hard to interpret these results in any case.

That "M" is easier than "rM" is an informal finding based on asking some forty people of every description which they thought was easier to read, and getting a unanimous response; the experiments necessary to test this conjecture are shortly to be run at the University of California, Irvine. Meanwhile perhaps Kolers' results might actually be taken to be corroboratory, in the following sense: if after about three hours' practice with reversed letters subjects find "M" easier than "rM," "M" must be intrinsically easier than "rM" after all, except for the unfamiliarity of the retrograde minuscules. What did Kolers' subjects learn? If they just learned 26 new retrograde letter-templates or just learned how to reverse their original 26 templates, his result is something of an anomaly, since if template-matching were literally all that was involved the original (normal) 26 letter-templates should serve just as well, always, backwards and forwards. Thus, one is apparently constrained, at the very least, to adopt the view that "templates" are easier to match if their letters match the direction of scan. But what sort of templates are those?

of their description, remarked above: namely, that their distinctive features seldom apply to all of the letter they are ascribed to, applying rather to some fragment. Thus, for instance, "B" is assigned the feature "Straight" and the feature "Curved," but of course "B" is not as a whole simultaneously straight and curved, nor is any segment of "B." This circumstance is in fact a serious shortcoming, rather than an avenue along which the troubles with "C" and "G" could be resolved, because though the Gibsonian features mainly apply only to parts of letters, Gibson et al. provide, and are able to provide, no way of specifying *where are located* the parts that give the letters these distinctive features. Which (again) is why "Γ" and "L" would have to be given identical descriptions in the Gibson analysis.

What this means is that Gibson et al.'s expressed hope of improving their system by weighting some features (Gibson 1969, p. 90) will be unavailing, since in this instance what needs fixing is not the features' weights but their domains. Thus, suppose in order to bring "C" and "G" closer together we assigned a lower weight to the "Symmetric" feature, found in "C." This would be disastrous elsewhere. For instance, "F" and "T" disagree only on this one feature "Symmetric," otherwise sharing six of their seven features; while comparatively "F" and "E" are already too far apart, sharing but five of eight. Reducing the weight of "Symmetric" would only draw "F" and "T" still closer together, so that the problem with "C" and "G" would simply have been sloughed off onto two other letters. Of course the system could acknowledge the diminutiveness of "G's" tail by adding a "Halved" feature to "G's" description,[12] which new feature could have the attribute of halving the values of all features ascribed to any segment to which "Halved" were assigned, thus reducing "G's" distance from "C" from the other direction—except that, again, the Gibson et al. analysis *cannot recognize* contained segments in order to attribute features to them. No half-way measure will suffice (for a still more modest measure see Geyer and DeWald

12. Such a "Halved" feature would have the sort of psychological warrant sought by Gibson et al., as see Hubel and Wiesel (1962).

[1973] and, from a slightly different perspective, Figure 6, below); the kind of rectification suggested by Gibson is foredoomed.

Nor is this all. So far we have been examining shortcomings discernible just in comparing the Gibson feature matrix with their own confusion matrix or with useful addenda like that suggested by the reader's being able to distinguish q from P . That is, to this point we have stuck to the discriminational evidence. When we turn to the compositional (and more "cognitive") evidence, the points made above become even stronger. Some of this evidence has been cited above (and see Watt 1975, Watt and Jacobs 1975) and so I will only summarize it here. Briefly, then, the Gibson feature-matrix seems unable to account for the clear fact that children learning to print the alphabet often pair "S" and "Z," making one backwards so as to be closer to the other (the Gibson et al. confusion matrix does not predict this fact either); or for the

Figure 6. Chart of distinctive features for several letters of the alphabet (combining the least desirable aspects of Eden-Halle and Gibson et al.).

Instead of merely recording PLUS and MINUS values, numerical values have been registered, indicating the number of strokes or segments which the feature in question describes. For the Roman majuscules, the range of such numbers is from 1 to 4.

| | A | B | C | E | K | L | N | U | X | Z | Q | P | R |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| STAFF | 3 | 1 | | 4 | 3 | 2 | 3 | 2 | 2 | 3 | | 1 | 1 |
| APERTURE | | | | | | | | | | | 1 | | |
| TAIL | | | | | | | | | | | 1 | | 1 |
| BREAST | | 2 | 1 | | | | | 1 | | | | 1 | 1 |
| HALVED | 1 | 2 | | 3 | 2 | 1 | | 3 | | 2 | | 1 | 1 |
| ROTATED 45° | 1 | | | | 1 | | | | 1 | 1 | | | |
| ROTATED 90° | | | | 3 | | 1 | | 1 | | 2 | | | |
| ROTATED 135° | 1 | | | | 1 | | 1 | | 1 | | | | |
| ROTATED 180° | | | 1 | | | | | | | | | | |

fact that many children continue to reverse "J" after they have ceased to reverse all the other letters; or for the fact that when writing the alphabet children often make "S" fifty per cent taller than all the letters preceding. Historically, the Gibson analysis can account for very little of the abundant epigraphic and paleographic evidence of the evolution of the majuscules: for instance, it seems to have no way at all of predicting or even accommodating the composition-derived facts that " I " evolved into " Z ," or that " Γ " evolved into " < ," or that " ξ " evolved into "S," or that in boustrophedon (in which odd-numbered lines were written from right to left) all right-left asymmetric letters were always reversed except that "S" was often exempted and "N" was often gotten backwards (Watt 1975). And as to casual empirical (contemporary historical) knowledge, the Gibson analysis has no way of predicting or accommodating the plain fact that many adults, in printing their majuscules in "OPEN" signs for gas-stations and the like, reverse "N." (That they commonly reverse *only* "N" is not predictable from the printed majuscules alone [Watt 1975].)

The Gibson et al. analysis, based on "visual" (mostly perceptual) features and aiming at an account of "visual" discriminations, cannot handle the demands we have outlined; those demands, however, add to "visual" factors "cognitive" and even "motor" factors. Taken thus, our criticism—though perhaps elucidative—is in a way unfair. It is fairer, though, if we raise our sights a little and ask if, whatever its faults as it stands, the Gibsonian analysis can be *incorporated* (properly included) within a broader and deeper account which, containing cognitive explanations and the necessary references to "motor" aspects, will make up the deficiencies noted. If so, then Gibson et al.'s analysis, after some tinkering, would suffice; if not, not. Alas, the answer appears to be in the negative. The inability to attribute "visual" features correctly to internal letter-components becomes at any cognitive level an inability to attribute "cognitive" features correctly to internal letter-components; it must to a strict Gibsonian analysis remain forever a mystery why children typically get "N's" backwards well after they have gotten "E's" made correctly: for nothing in a "feature" anaylsis of whole letters even hints at such events. In fine, no strict Gibsonian analysis—even under the

Geyer-DeWald recension (1973)—is properly-includible in a correct broader account; the most permanent accomplishment of the work of Gibson et al. is likely to be its enduring status as the pioneering study in its field.

But it is not our purpose to arraign Gibson et al. They chose a limited goal, that of representing the minimal discriminations necessary for four-year-olds to distinguish the existent majuscules. Theirs was therefore a goal within "observational" adequacy, despite their reliance on psychological warrant in choosing their distinctive features, and certainly that level of adequacy was achieved. Owing to the psychological warrant of their features, they moved a step towards descriptive adequacy, even towards "*perceptually*-descriptive" adequacy; but as we have seen in their inability to distinguish ꟼ from P , full descriptive adequacy was assuredly not achieved.

1.4. *Comparison of Eden-Halle and Gibson et al.; Summary.*
Eden and Halle used distinctive features to describe strokes (their "phonemes") which were in turn combined into letters (their "morphemes"). Gibson et al. used distinctive features to describe letters directly. As we have seen, Gibson et al.'s decision to ignore the internal segment- or stroke-constituency of letters seems to have carried with it a heavy penalty; they should not have ascribed their distinctive features to such high-level units. In short, they mistook "morphemes" for "phonemes."

On the other hand, Eden and Halle's own distinctive features (aside from the four we added)—"Arch," "Loop," "Hook," and "Bar"—are effectively strokes themselves; they cannot be combined, and so the small set of strokes each defines—internally differentiated by "Raised," "Mirrored," etc.—is with respect to these original four features, atomistic. We suggested that "Hook" and "Loop" did not strike us as atomistic primitives; but more generally "Arch" and "Bar" are surely not primitives either, since both can easily be broken down in terms of Gibsonian distinctive features like "Vertical," "Curved," and so on. Since we have already suggested that Gibsonian features may be valid, it seems that Eden-Halle have made a mistake at a lower level comparable to Gibson et al.'s at the higher: that is, they mistook "phonemes" for "distinctive features."

As we see, then, the two illustrative analyses we have been examining have similar but almost complementary shortcomings, and a first step towards improving both analyses might be importing into each the best attributes of the other. Roughly, Eden-Halle attacked the right domain (their phoneme-analogs were strokes, not letters), while Gibson et al. came closer to employing the right kind of distinctive features (theirs were much more freely combinable, and had some psychological warrant). Thus the obvious next move should run something like this: the printed majuscules should be dissected into their proper strokes—as Eden and Halle did for the cursives—and those strokes should then be analyzed in terms of Gibson-like distinctive features ("Vertical," "Curved," and so on).[13] And in fact this is a necessary, though scarcely sufficient, step.

The alert reader will have seen that any such step, however, can also be viewed as carrying a penalty, for if letters are to be broken down into their strokes before being characterized (stroke by stroke) in terms of distinctive features, then they can no longer be characterized by "Symmetric," a feature which necessarily applied to the letter as a whole. We might call "Symmetric" a "global" distinctive feature, one which in our proposed reanalysis would have to apply over more than one segment rather like an Harrisian phonemic long component (Harris 1951); other "global" features in this sense would be "Cyclic," "Intersection" (which of course must deal with two line-segments), and perhaps "Vertical (and Horizontal) Terminates" (which deal with the absence of two line-segments where two could have been present). However, the loss of these "global" features is only temporary, since they are all features that can be calculated, or derived, from sequences of line-segments expressed without them. (A system can recognize "Symmetry" by inspecting underlying abstract representations, just as we can; several examples of such calculations will be exhibited in Part II.)

13. Lest anyone think that, on the contrary, the proper next step should be to combine Eden-Halle-like features with the Gibson et al. choice of letter as "phoneme," I have constructed the chart of Figure 6 as a counterargument. As is plain from Figure 6, where several imaginative ploys have been put to use, the case is a dead-end, because without being able to pinpoint where in the letter the distinctive features fall, the description is hamstrung.

2. *Conclusion.*

We now return to our starting point: the desiderata that we should ask a serious characterization of "the alphabet" to realize. Our point of departure was the observation that iconic analyses in search of something more than "more rigor" must be judged by criteria of adequacy; this led us to consider what sorts of psychological evidence might be brought to bear on the achievement of "cognitively descriptive" or even "explanatory" adequacy. We used two accomplished and well-known iconic analyses, having a common ancestor, as illustrations. The first, Eden and Halle, was apparently designed with no psychological relevance in mind at all; and, not surprisingly, it fails to have much. The second, Gibson et al., was erected on the basis of several direct psychological importations; but even so it failed to achieve much more than the sort of "observational" adequacy that Eden and Halle achieved, because if the psychological evidence is all drawn from discrimination studies, and if the analysis itself is aimed only at explaining discrimination (among attestable members of the object set), then discrimination—or, in other words, human pattern-recognition—is all that can be achieved, howbeit via features having some psychological warrant or other. We found fault with Gibson et al. regarding their system's discriminations; but we found graver fault with their system's apparent *inextensibility* to accommodate additional (and fairly obvious) psychological information.

Building grammars without success criteria is, to paraphrase the national bard, like playing tennis without a net. It is too easy to propose a new characterization of the alphabet, generally of an alphabet in some new and untoward font, in terms of which some theory or other can be upheld. These exercises are in most cases interesting mainly for whatever immediate utility they may have (as, in a computer system). We will return to this topic in Part III of this series of papers, "Further Complexities" (that is, "Further Perplexities").

But this brings us to a convenient breaking-point.

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