13. NOTES ON BERGMANN'S NEW ONTOLOGY AND ACCOUNT OF RELATIONS

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ABSTRACT. Recent work of Gustav Bergmann develops an on-
tological framework within which an account of relations has
been sketched out. The approach is a kind of new logical
atomism which has some of the features of an Aristotelian
hylomorphism (of sorts). It recognizes a number of cate-
gories and groups of a hylomorphic kind, chiefly "determi-
nates" and "subdeterminates"--the latter only indirectly or
implicitly. Winsome though it is, the approach is flawed by
certain difficulties it gives rise to, among them inability to
speak of subdeterminates and failure of a relation to be had
by a referent towards a relatum. Instead of having a sense,
a relation is conceived of as a determinate which enters an
arrangement whose existence and nature are not properly
accounted for. Finally, Bergmann's Ideal Language is as-
sayed and shown not to be as useful philosophically in itself
as he takes it to be.

INTRODUCTORY REMARKS

This essay belongs to a series of papers whose aim is to show
that some differing accounts of relations in contemporary philosophy
(commencing with Frege) are flawed because they resort to what can be
labelled 'hylomorphism'. Some standard difficulties of Aristotelianism
reappear in these analytical approaches. All of them resort to "form" as
playing the role of "actualizing" a given "matter" (objects taken as ar-
guments, *relata*, or a relation along with the related terms--the form
then being, e.g., a *logical* form, as Russell thought when writing his
Theory of Knowledge) by making it into another entity. In these ac-
counts the actualizing or structuring form lacks the quality (of actuality
or objecthood or whatever) it bestows upon the matter it clings to. The
puzzle lies in those forms' baffling slipperiness; for they cannot be
meant or intended outside their role of actualizing or informing some
matter. But, when we point to (the process or result of) their playing
such a role, we cannot mean or intend the form itself, but only the in-
formed matter--or, if you please, the result of its being thus informed.

For some approaches, a problem also arises concerning matter it-
self, one closely resembling Aristotelian problems with prime matter--
problems which have prompted some interpreters to deny that Aristotle
posited any such entity[1]. What in these approaches plays the role of
Aristotelian matter, when taken "prior to" or outside of its being

"informed" by a form, lacks sufficient selfbeing and endowment with
qualities and ontological profile to be an entity directly meant as such.
Such a problem e.g. affects *Tractarian* "objects", which are both form
and content, but which can be meant as neither separately. It follows
that their ipseity always eludes us and evades being meant or signified.
As we are about to see, a similar problem affects Bergmann's new ontol-
ogy.

After going into Bergmann's ontology in general and especially his
account of relations, I'll touch on his most cherished tool, the Ideal
Language, and finally show the failure of this grand attempt. A short
appendix is devoted to examining Wilson's assay of Bergmann on ineffa-
bility.

SECTION 1. BERGMANN'S NEW ONTOLOGY

In a number of recent papers [Bergmann, 1978, Bergmann, 1981a,
Bergmann, 1981b] Gustav Bergmann has developed an ontological account
which deserves a careful examination, since in them he has some new
ideas about the nature of relational facts. In order to make my presen-
tation crisp, I shall attend only to the account Bergmann has put for-
ward in the three aforementioned papers, giving no attention to his ear-
lier views.

Bergmann's recent account is a new logical atomism, wherein cate-
gorial boundaries are absolutely rigid and stern (with just one exception
concerning identity statements, as I shall point out shortly). There are
two fundamental kinds of entities: (1) *determinates*, which are indepen-
dent and separable; and (2) *subdeterminates*, which are not separable
and cannot explicitly be objects (or "intentions", as Bergmann puts it)
of intentional acts. Subdeterminates can be intended only implicitly or
(so to speak) "slantwise", i.e., in union with other entities forming a
complex which is explicitly meant or intended by an intentional act.
Determinates are things (which are (non-absolutely) simple), complexes,
or classes. None of them is absolutely simple. Absolutely simple entities
are subdeterminates. They are of two types: *ultimate sorts* and *items*. US
(ultimate sorts) might be taken to be of just two types: universals and
particulars. Still, as Bergmann [1981b] makes quite clear, if the sort of
particulars is just one, there are infinitely many sorts of universals--
one for each Russellian type. To my mind this makes Bergmann's talk
about the sort of universals [in Bergmann 1978 and in Bergmann 1981a]
problematic. Each *item* is an individuating principle, a thisness in virtue
of which the thing composed by it along with one US is the peculiar
entity it happens to be and no other. But neither items nor US can be
spoken about or meant; they are too simple for that and so are not
separable. This is why they are not existents in their own right, since
they lack what I shall call 'selfbeing'. Bergmann compares the union of
an item and a US with an Aristotelico-Thomist union of matter and form
[Bergmann, 1978, p. 97]. His claim that Aquinas' *materia signata* is similar
to his own items seems to me less felicitous. As I understand his view,
items are much more like Scotistic *haecceitates*. In Bergmann 1981b [n.
20, p. 209], he credits his Thomist friend, J. Peterson, with prompting
him to ontologize the US. (It is, however, the Scotistic connections of
this ontology which seem to me most worth exploring.)

On the other hand, there are other nonseparable entities whose being consists in somehow either sticking to or seeping into separable entities. Among them are, first, the two modes of actuality and potentiality--each complex entity being pervaded by one and only one of these two modes. Second, there is exemplification, formerly viewed by Bergmann as a tie gathering a particular and a universal into a complex (a fact), but now regarded as a clinging entity (to be explained shortly). The remaining subdeterminates, such as those signified by connectives or quantifiers, are clinging entities too.

But let me now come to complexes. They are all determinates. First, *diads*. For any two determinates, x and y, there is a determinate complex which is the *diad* they both form, Bergmann's notation being either '$x \neq y$' or '(x,y)'. We must repress the temptation to say that *difference* or *otherness* is what makes the determinates different or ties them into the complex meant by '$x \neq y$'. Bergmann insists that nothing at all is needed for making them different or for gathering them into their being two rather than one, since they are different in virtue of themselves. Their diversity (i.e., the diad they form together) thus crops up, so to speak, into reality (once they are both admitted). Categorial boundaries between determinates are here (for once) levelled, since any two determinates of whatever category are allowed to form a diad. If g is an (0) (one-place) universal and a a particular, the fact that a exemplifies g is exemplification's clinging to the diad, $a \neq g$. Bergmann introduces ordered pairs Kuratowski-wise, using his diads, rather than classes, for the purpose. The ordered pair (or 2-tuple, as Bergmann is fond of saying) $\langle x,y \rangle$ is nothing but the diad $x \neq (x \neq y)$--remembering that diads are determinates themselves. Letting r be a two-place relation (i.e., an (0,0) universal), the fact that a bears r to b, where both a and b are particulars, is exemplification's clinging to the diad $(r, \langle a,b \rangle)$, i.e., to the diversity $r \neq (a \neq (a \neq b))$. In this way Bergmann has analyzed all atomic facts. Non-atomic facts are generated as follows. For every complex there is a non-atomic fact resulting from a unary operator (negation or a quantifier) clinging to that complex. Next, for any given diad of complexes there is a non-atomic fact resulting from a binary functor (conjunction, disjunction, and so on) clinging to that diad. Notice that diads, complex as they are, are not facts. They are circumstances. Other circumstances are: a particular's belonging to a class and a property's (of mental acts) meaning an "intention". Bergmann introduces classes as entities which are neither simple nor complex. They needn't concern us here (except for a brief remark I shall make shortly), though his class theory is doubtless worth considering.

Bergmann is satisfied that he has in this manner grounded order [Bergmann, 1981a, 146]. We should note the affinity which Bergmann is keen on acknowledging (in order to highlight the relevant divergence) between his way of constructing order and the much more usual set-theoretical move. He says: "I, for one, had I to make a class a constituent of an atomic fact, would rather give up. This is one reason why I think the shift from ordered classes to ordered diads is so important" [Bergmann, 1981a, 147-8]. The shift in question is important within Bergmann's ontology in that classes are complexes constituted out of diads, in a fairly mazy way. Thus, as Bergmann remarks, while diads are in a layer closer to the basis, ordered classes are only attained in "a layer at or close to the top" [*Ibid.*, 147].

The ontological framework is underpinned by three principles:

(1)   The exemplification principle: No universal exists unless it is exemplified;

(2)   The principle of realism: Whatever is thinkable exists;

(3)   The contrast principle: If a complex is sayable, so is the negation thereof, and conversely.

A fourth principle, adherence to which was earlier a badge of Bergmann's peculiar realism, viz., the complexity principle (according to which for two complexes to be different, they must differ in content-- i.e. in at least one of their components, a principle which is a version of a more general content principle) is now either hesitantly and reluctantly given up, or only waveringly and inconsequently hewn to. Finally, Bergmann sketches an Ideal Language (IL) in which there are no variables (since variables stand for nothing), in which quantifiers are dealt with in a way on which I shall have to comment presently, and in which no two signs ever signify one and the same entity (except when both signs signify classes, which are extensional). Another exception is necessitated by his treatment of quantifiers, as we shall see shortly. Moreover, the IL is so devised as to delinearize well-formed phrases, whenever wanted (on which a little more in Section 4 below).

In this world, all particulars are momentary [Bergmann, 1981b, p. 190 and nn. 6, 20, on pp. 208 and 281]. The rationale for ruling out continuants is that, if a particular, a, can exist at two different times, t, t', then a's being, say, green may be true at t and false at t'; thus, we should need to understand greenness as a two-place relation between a particular of any kind and another particular which must be a time. True, Bergmann acquiesces in talk about series of momentary particulars (e.g. minds are temporal series of states of consciousness), but he seems to suggest that such talk belongs to *ontological* discourse. Ontological discourse, as he conceives it, is an attempt to say what cannot (literally) be said and so cannot be translated into his IL, unless the series is conceived of as a class. This latter seems somewhat dubious, since Bergmann attempts to keep the resort to classes at a minimum.[2] *Momentary* entities are not to be mistaken for *instantaneous* ones; they are "of rather short duration" [Bergmann, 1981b, n.6, 208]. Whether such a view of moments can successfully cope with the problem Bergmann's account is designed to solve is another matter. At any rate, I shall have some comments to make later on Bergmann's ruling out continuants.

Before bringing this Section to a close, let me dwell a moment on quantifiers and negation. Bergmann's IL gets rid of variables by replacing an existential quantification '∃xf(x)' by a formula '∃⟨a,f(a)⟩', where '∃' has now become a one-place operator signifying a unary subdeterminate which clings to a diad. The resulting formula can be unpacked as '∃(a≠(a≠f(a)))' [Bergmann, 1981b, pp. 197-8]. Bergmann's policy of banning any two signs standing for the same entity is now challenged, since he has either to let '∃(b≠(b≠f(b)))' stand for the same complex entity as the just written formula (by relaxing his ban) or to take appropriate measures, e.g. laying down that only one of the formulas is well formed, thus restricting the field of application of the unary operator ∃ (the existential quantifier) to certain chosen diads. Bergmann [*Ibid.*, 198] chooses the former alternative (with the usual provisos

about 'f(a)' and 'f(b)', which needn't concern us here). The same can, of course, be said for the universal quantifier. One result of such treatment is that extensionality for quantifiers is lost: from p≡q you can no longer draw ∃xp≡∃xq (duly rewritten in the above manner). For one thing, when the variable 'x' has no free occurrence in 'p', the ensuing formula is banned by Bergmann as ill-formed, since, otherwise, what is supposed to be constructed will be one of the materials out of which it is to be constructed. For another, if 'p' contains free variables, then Bergmann seems to rule out p≡q as a premiss, since it is not a closed formula. Still, such restrictions are far from innocuous, as is well-known.

Let me now come to negation. The effect of negation's clinging to a complex is another complex that is potential iff the former is actual, and vice versa. Therefore, what is meant by 'p' is not the same as what is meant by 'not not p'. Potentiality as conceived by Bergmann has nothing to do with possibility, since impossible (contradictory) complexes are according to him potential. (Bergmann defines contradictory complexes as those whose potential mode is accessible to us.) That means that for any *two* entities, x,z, both the diad x≠z and its negation, the identity between x and z, exist, except that, while the former is actual, the latter is potential. Nevertheless Bergmann sorts out the meanings of certain sentences whose negation is unthinkable and so nonexistent (in virtue of his 'phenomenological' principle identifying *to be* and *to be thinkable*). Such unthinkables are, e.g., *greenness' being a (0)-universal*, or *this' being a particular*, i.e. those involving true categorial attributions. For a *Tractatus*-reader all this sounds pretty familiar. What for Wittgenstein is *sinnlos* represents for Bergmann a complex whose mode is accessible to us, whereas what for Wittgenstein is *unsinnig* is what for Bergmann is unsayable even if existent. (It remains fairly obscure whether such an existent--and there is bound to be one, since Bergmann [Bergmann, 1978, p. 98] explicitly says that it is thinkable although unsayable--is a thing or something else. We could take it that the meaning of 'greenness is a (0) universal' is nothing but greenness itself, the subject of our (pseudo) statement, viz., 'greenness' there meaning greenness' thisness or 'greenness as this peculiar universal it is' [as for Aquinas subjects signify something *ut suppositum* and for Aristotle they signify something qua its matter].) The absolute simples, items and US, are unspeakable-about. Bergmann also rejects the existence of complexes such as a complex's being pervaded by its mode [Bergmann, 1081b, 195], since, should the larger complex exist, its negation would also exist and would be permeated by the opposite mode and so on. All of this would boil down to a gratuitous ontologizing of assertion as actuality. (I hope I have faithfully represented Bergmann's merely hinted-at argument.)[3] Another argument in behalf of the same rejection of over-complexes is [*Ibid.*,] this one: "A complex's being pervaded by its mode being itself pervaded by one is unthinkable". I think these arguments are unconvincing. (An additional argument is suggested by Bergmann when he denies that *pervading* should be a tie. See below the fourth objection in Section 2).

The underlying tractarian arguments are more forceful. One of them is the following. If there is a complex involving another complex, the latter exists non-contingently. In virtue of excluded middle, the former complex is (to put it Bergmann-wise) either potential or actual--either alternative calling for the existence of the involved complex. Now, for Wittgenstein, no complex exists non-contingently. *Things* do, although

their existence is not necessary.[4] This argument as it stands obviously
cannot be accepted by Bergmann. We might try replacing it by one de-
pending upon the principle that whatever is involved in a complex is an
actual entity. But that will not do either; for only complexes are actual
(or potential), and, what is more, potential complexes, by being clung-to
by some subdeterminates, are constituents of other complexes. In spite
of that failure, I think that the source of his uneasiness about allowing
a complex's being pervaded by a mode to be something (over and above
the complex itself) can be pinpointed: If a complex's being pervaded by
its mode is something, it is a *larger* complex. But what are its compo-
nents? Just the complex and its mode? No, for then the mode would be
(exactly like) a clinging entity, since clinging subdeterminates are such
that, taken together with a determinate of some kind, they issue in a
complex of a peculiar sort. This would in this case mean that, were po-
tentiality and actuality *clinging* (rather than *pervading*) subdetermi-
nates, every complex would (separately) be both actual and potential.
Since that is impossible, we can safely conclude that, for there to be
the envisaged larger complexes, there would be bound to be a further
entity--a *pervading* tie. Yet, Bergmann's new ontology rules out ties
(and to my mind rightly so, since they are nothing else but bashfully
recognized relations contrived to escape honest toil, rather like the
"non-predicamental relations" of Medieval Aristotelians). Could Bergmann
change his new ontology on this issue by holding the modes to be
determinates rather than subdeterminates? No, because, in order for the
larger complex (that is, for the pervading of the smaller complex by a
mode) to differ from a mere diad, a new clinging subdeterminate,
pervading, would then be called for. And that would, in turn, entail that
any complex was pervaded by both modes--separately, to be sure--in
virtue of the underlying principle that all possible combinations are in
fact realized. This underlying principle can be deduced from the princi-
ple of realism: A possible combination is one which can be thought. (This
is precisely why Bergmann doesn't view a pervading as a combination.)
The difference between a complex's being the case and its failing to be
the case would then be nothing but actuality's pervading the complex to
be the case. And this would obviously trigger an infinite regress. (See
below, my 4th objection in Section 2.)

    A further--but weaker--reason for failing to allow a complex's be-
ing pervaded by a mode to be something over and above the complex it-
self is that, were it another entity, it would be a complex one such that
necessarily the given complex, p, would be pervaded by actuality iff its
being so pervaded were in turn pervaded by actuality--and so on;
which would be the case iff p's being pervaded by potentiality were
pervaded by potentiality--and so on. This would be the case iff p's fail-
ing to be pervaded by actuality (another, *different* complex) were per-
vaded by potentiality, and so on. All the mutual entailments expressed
by the biconditional formulae would be necessary.

    According to Bergmann, as put forward in [Bergmann, 1981b,
189ff], a believing that p is a mental act consisting of the conjunction
of two atomic complexes, one of them of the form *bel*(a), i.e. "a is
believing", the other of the form f(a), where f is a thought, i.e., a (0)
universal or property of mental particulars such that, analytically, fMp
(i.e. f means p or, put a bit more perspicuously, acts exemplifying
thought f mean or intend the complex p), while a is a mental particular.
Therefore the act is analytically targeted on its meaning or intention. If
it is necessarily the case that p iff q, then necessarily an act of believ-

ing that p is true iff an act of believing that q is true. I think a pro-
liferation of such mutual necessary entailments would not make Berg-
mann happy. Granted, Bergmann himself in fact acknowledges some mu-
tual entailments, as e.g. the one between (the truth of) thinking that p
and (that of) thinking that p and p, which is different. These cases,
though, involve pure logic.

Bergmann's conception of necessity is to my mind purely Leibnizi-
an: there is no other necessity but analyticity: for a sentence to be an-
alytic(ally true) is nothing else but for the complex it stands for to be
pervaded by actuality and such that its being so pervaded is accessible
to us. (But see below, n.10.) And I suspect that Bergmann is disinclined
to recognize nonlogical analytical truths--barring just a few exceptions
like his sentences of the form "fMp". Notice that the purported analytic-
ity of that kind of circumstances is not without a number of difficulties.
Herbert Hochberg, both in Hochberg, 1978, and in Hochberg, 1981, criti-
cizes Bergmann's contention on the analyticity of such circumstances.
Bergmann, 1981a, is in part a reply to Hochberg's criticisms in the
earlier paper. Hochberg criticized Bergmann's former use of "p'Mp'.
Bergmann now [1981a, 137] scraps the notation, acknowledging that what
is to be found at the left of 'M' is the name of a thought, not a struc-
tured expression which is implicitly a definite description. Hochberg in
[1981] retorts that, on relinquishing the old pseudostructural notation
for thoughts, Bergmann has forsaken the wished-for analyticity of his
M-sentences: "all he does is declare, in different words, that 'f₂Mf₁(a)'
is analytic" [Ibid., 163]. A related discussion is pursued by Wilson in
[Wilson, 1983, 452ff]. Most of all, Bergmann's new notation somewhat ob-
scures the notion of a thought's text. Bergmann has, however, endeav-
ored to clear it up in [1981b]. He there claims (p. 190) that usually an
act is conscious of its intention iff there is, in the conscious state (i.e.
collection of simultaneous mental acts) to which it belongs, a further act,
consisting of a string of words, which is the text of the former act. But,
of course, this is postulation. Bergmann emphasizes that the connection
between text and intention is *many-many*; so he prefers to regard "not-
p" and "p is potential"--if only the latter was sayable in so many
words--as two texts for the same intention. (This notion is made use of
below, in the Appendix.)

SECTION 2. OBJECTIONS TO BERGMANN'S ONTOLOGICAL VIEW

I turn now to criticism of Bergmann's account. Let me first ad-
vance a number of objections to the general framework just sketched,
and only then (in the next Section) focus on his way of dealing with
relations, which I find far from satisfactory.

1st Objection

The contrast principle wreaks havoc with Bergmann's account, since ac-
ceptance of it compels him, on the one hand, to accept tractarian inef-
fability conclusions and, on the other hand, to countenance as existent
such utterly queer "entities" as, for any two determinates, a and b, the
identity (or nondiversity, as Bergmann would rather say) between a and
b (a=b, or not a≠b), as well as, for any determinate, a, the diversity
between a and a itself, i.e. a's selfdiversity, a≠a (see [Bergmann, 1981,
140]).[5]

2nd Objection

The difference between actual and potential complexes is in no way explained, much less grounded or analyzed. We are called upon intuitively to grasp the difference in question. At any rate, potential entities are as real or existent as actual ones; thus, to exist is not the same as to exist actually. False sentences are as corresponding to reality as true ones, except that, while the former denote potential complexes, the latter denote actual ones. All of this runs counter to Russell's reasonable claim that, while there are facts meant by true sentences, false sentences mean nothing. So Bergmann falls back on Aristotle's unanalyzable modes but he does not thereby identify existence *tout court* with actual existence--an identification which doubtless gave rise to serious difficulties (*actual* entailing *non-potential*), but which proved serviceable all the same. For it blocked the inference from stating an entity's potential existence to stating its existence, period. (Thus Bergmann deems wholly existent or real Locke's being emperor of Japan and even a marsupial for that matter. See below, my criticism of Bergmann's analysis of nonrelational atomic facts.)[6]

3rd Objection

Bergmann's potentiality has little or nothing to do with what is either in Aristotelian doctrine or usually taken to be potential, i.e., something that is not but can be. Potential complexes cannot and could not, according to Bergmann, be actual, nor could actual ones be potential.[7] All that debars us from resorting to usual notions when trying to understand the Bergmannian dichotomy. All Bergmann tells us is that analytic complexes can be known to be actual and that contradictory ones can be known to be potential. As for the remainder, we don't know and shall never know. Yet, what does knowing that a complex is actual (or potential) amount to? I'll have something to say below about the pervading of a complex by its mode. What at present I wish to insist upon is that I see no content at all that I am acquainted with which could be expressed, signified, or what have you, by 'potential' and 'actual' as Bergmann uses them. Not surprisingly, Bergmann himself bans those two words from his own IL--for the reasons I outlined at the end of the foregoing section of this paper. It is only in ontological discourse, a kind of discourse which (unsuccessfully?) tries to speak about what is un-speakable about, that we use those words. But then my uneasiness is increased. Those words, as used by Bergmann, are technical, not commonsensical--a distinction Bergmann's methodology recognizes. But they are banished from the IL. The ground they occupy is most precarious and problematic. What alone seems to me to correspond to our purported grasping a complex's actuality (or potentiality) is that we grasp the complex (or the negation thereof). Could not Bergmann then dispense with modes altogether? Yes, after a fashion. Still, he would then face this result: Not only would true sentences and false ones alike denote existent complexes, but those complexes would be on equal footing, no general feature serving to sort out the former from the latter. What difference would then remain between true and false beliefs? None. Still, whatever difference is acknowledged by Bergmann plays a minor, subdued role in his account. As for analytic or contradictory statements, what alone characterizes them is that their mode is accessible to us. This could be replaced by making a distinction between complexes accessible to us and complexes whose negations are accessible to us. A complex would be accessible to us iff it can be believed with evidence, and

we could surely lay down that this happens iff its negation is disbelieved with evidence (see [Bergmann, 1981b, 195]).

As for all other complexes, since their mode remains forever inaccessible to us, who cares? After all we'll never be able to establish that one of those complexes is actual rather than potential. Negation could be defined as the subdeterminate that on clinging to a complex yields another complex whose conjunction with the given one is contradictory and whose disjunction with the given one is analytic. Although such a "solution" is far from satisfactory, it would cope with the same problems Bergmann's account has been devised to deal with and would have the great merit of getting rid of those dark notions of potentiality and actuality. (When trying to meet the coherentist half-way, Bergmann says [Bergmann, 1981b, 205] that, for such believings as are neither analytic nor contradictory, the best we can do is assign and reassign modes to them, blindly as it were, yet systematically, so as to achieve maximum coherence. Let us pass over what assigning a mode to a complex may be. I take that way of speaking to be a mere lapse. At any rate just as much could be secured by a similar quasicoherentist approach which, while on other matters holding on to Bergmann's views, would waive his modes in the manner just sketched--except that the coherence ideal would then be grounded in nothing but our own mind's drifts and trends.)

4th Objection

On Bergmann's account a complex is pervaded by the mode of potentiality iff negation's clinging to that complex is pervaded by actuality. It is pervaded by actuality iff negation's clinging thereto is pervaded by potentiality. We have already noticed, however, that no entity's existence grounds a sentence's truth or its negation's falsity; the world contains the same entities whether the sentence is true or false. For a complex to be pervaded by a mode is not a new more complex complex. This is why Bergmann chooses the verb 'pervade', meaning that the pervading of a complex by a mode is not a *tertium quid*, but is the same as the pervaded complex thus pervaded. Otherwise, for the new kind of complex to arise, a tie or bond would be called for, namely pervading. Clearly, Bergmann now dislikes any tying entity, but couldn't he accept a clinging subdeterminate cleaving to a "complex" formed by another complex and a mode? No. For one thing, a mode is a subdeterminate which, if it is glued on a complex, needs no intermediary at all. It would, otherwise, be a determinate, but this is impossible--as we saw at the end of Section 1. For another, were *pervading* a clinging subdeterminate, every complex would be--separately--pervaded by both modes, since clinging subdeterminates cling to whatever they can. (Nevertheless see below, Section 3, how Bergmann seems in the end bound to regard exemplification as such a tie between a complex and a mode.) On the other hand, according to Bergmann, once a complex is there, negation's clinging to it is there too, automatically and without intermediaries. If p is a complex entity, so are not-p, not-not-p, and so on. All of them exist and differ from one another, but, according as to whether the number of "clingings" by negation is even (including zero) or odd, those complexes, as regards the pervading mode, enter two disjoint equivalence classes (so to speak, for Bergmann would of course have nothing of the sort, since classes gather only particulars or perhaps other classes as well). This account of negation seems to me utterly mistaken, especially with the infinity of already existent complexes which are logically

equivalent among themselves, and with the incomprehensible pervading
of a complex by a mode. If, as I've guessed, that pervading is nothing
else but the complex itself "as so pervaded", we get saddled with Aris-
totelian reduplications or "qua" expressions-- of which there is no
available clear logical account and whose historical role has been to
serve as a smoke screen to conceal a theory's inferential virtualities or
profile.[8]

5th Objection

I find it very hard to understand what an entity like $a{\neq}a$ may
really be, and, still harder, for blackness to be the same as Somalia's
being impoverished by drought. True, any actual diad is necessarily ac-
tual, as any potential diad is bound to be potential. (For the only ne-
cessity concept Bergmann allows for in his account is analyticity and he
reckons all identity or diversity formulae as analytic, since--barring the
two aforementioned exceptions--in his IL no two expressions ever stand
for the same entity.) Still, necessarily potential as they are, the differ-
ence between a thing and itself, as well as its identity to anything else
are all the same real entities, which, Bergmann contends, exist and can
be thought. Now, Bergmann's argument against the existence of words
standing for categories or US is that, were there such words, sentences
containing them as well as their negations could be said and conse-
quently also thought, whereas a thing of a certain category's not being
of that category is unthinkable. But is not a's being the same as each
and every entity as unthinkable as greenness' failing to be a univer-
sal?[9]

6th Objection

Within Bergmann's framework, facts have no properties (they are
bare facts) and bear no relations[10]. We cannot say anything about a
fact or any other complex--except of course that it is the same as, or
other than, any determinate. Still less can we speak about subdetermi-
nates, such as exemplification, negation, conjunction or existential and
universal quantification. They cannot even be sensibly said to be either
identical to or different from any entity. (As for such subdeterminates
as US and items as well as the two modes of potentiality and actuality,
their plight becomes hopelessly bleak. They cannot be either named or
meant or otherwise pointed at, as other subdeterminates are, by syncat-
egorematic signs. And, of course, they enter no facts or circumstances.
Their failing to do so is, still, nothing at all and accordingly both un-
thinkable and unsayable. The proposal's main theses are, thus, literally
unsayable--as Bergmann himself outspokenly acknowledges. True, Berg-
mann *Tractatus*-wise says that a US and likewise every form is rep-
resented by a shape of a sign denoting a thing with that US. Likewise
every form is represented by a shape. As regards, e.g., potentiality
there surely is some geometrically describable shape shared by all false
sentences. We do not know, however, and will never know what it is. In
a similar vein, a "Sellarsian realist" could take universals to be un-
speakable-about *entities* which can--or must--be represented only by
shapes or the like.) Thus, either we face Frege's own troubles, or, still
worse (with nothing like correlate or surrogate objects being available
within Bergmann's framework) we condemn as unsayable all what we are
saying or purporting to say (as Bergmann himself does). The worst fate
that can befall a theory is entailing its own ineffability. (Oddly enough,
Bergmann's account rules out classes' exemplifying properties and bear-

ing relations, since within his ontological framework classes are not par-
ticulars. As I understand Bergmann's doctrine, classes may belong to
classes of classes and be the same as themselves as well as different
from anything else. Besides that, nothing else can be said about them.)

7th Objection

According to Bergmann, difference is literally nothing, as we saw
above. This is why a difference between two things is not a fact, but a
mere circumstance, with no constituent other than the two different de-
terminates by themselves, whose existence entails *eo ipso* the circum-
stance's own existence. Now Bergmann claims as much about another cir-
cumstance, namely a particular's belonging to a class--the membership
sign, he says, standing for nothing. I wonder then what difference, if
any, there is between a particular, a's, being a member of a certain
class b and the circumstance that $a \neq b$. (Bergmann says that for mem-
bership order matters, whereas it does not for diversity. I'm not quite
sure he is right on the first score, if we espouse his way of "con-
structing" classes, which incorporates a 'Fundierung' principle not just
for truth but apparently also for sense. Be that as it may, what seems
to me most clear is that, membership being literally nothing (since an
element's belonging or failing to belong to a certain class requires no
tie whatsoever), no order can be achieved through "it".) Nevertheless,
a's belonging to b may be potential while $a \neq b$ is actual. (And, of course,
the converse, if, when 'b' stands for a particular, '$a \in b$' is allowed to
be well-formed. This is not clearly settled by Bergmann, but, in its be-
half, telling considerations can be put forward which would match Berg-
mann's own case for allowing '$c \neq d$' to be well-formed whenever 'c' and
'd' stand for determinates). Yet, one and the same complex cannot be
pervaded by both potentiality and actuality. Therefore a's belonging to
b and a's being other than b are different circumstances, but there is
nothing at all, either inside or outside them, which makes them differ-
ent.

8th Objection

Bergmann's ruling out all but momentary particulars is not, of
course, gratuitous. What he thereby achieves is important. However, the
price he pays seems to be unbearable. (Bergmann in fact joins P. Un-
ger[11] in upholding that I (or he) do(es) not exist. Or else I am a non-
particular, which is what he says. But talk about *those* nonparticulars
seems to belong to ontological discourse, which is not, and cannot be,
either literally true or subject to an adequate paraphrase.) Are there
other alternatives? One of them would be to regard some verbs or pred-
icates as being of variable adicity. Bergmann would retort rightly that,
on the ontological side, nothing seems to correspond to any such device.
Another alternative would dispense altogether with categorial barriers.[12]

9th Objection

In this ontology, existents do exist, but there is nothing by hav-
ing of which existents exist, since existence is nothing at all. There are
two reasons for Bergmann to refrain from positing a property of exist-
ing. First, it would straddle categorial boundaries. (But Bergmann   is
not deterred by similar qualms from taking identity and diversity to
straddle categorical borders--within the range of determinate entities.
Were Bergmann to acknowledge a separation principle by dint of which,

for any formula 'p' and variable 'x' with free occurrences in 'p', and for any determinate, z, z exemplifies ᚢp inasmuch as it is true that p[x/z], then, of course, he would hit a snag. For then all determinates would have to belong to the same category, that of being either identical to or different from any given determinate. This is an excellent reason for Bergmann to stay clear of any such separation principle, either in that strong and unqualified formulation or in any other form.) Second, there cannot even be different properties of existence which are categorially bounded, since, if any such property exists, it is exemplified by every determinate within its range, and thus, for any such determinate, x, that x exemplifies existence (of the relevant category) will be a fact. But for a number of reasons, this cannot be the case. Such atomic facts would be analytic (contrary to a basic tenet of Bergmann's ontology and theory of knowledge). And any such fact's negation might be unthinkable, and therefore nonexistent, since it may seem doubtful whether it makes sense to think of--or even entertain of--an entity that it does not exist. (It may seem that we think that it could have failed to exist; but surely Bergmann will reply that, on second thought, we should notice that no such possibility can obtain, since, if for p to be the case is contradictory, so also for p to be able to be the case. And for an entity not to exist is a contradictory state of affairs. That argument, however, might be countered by pointing out that contradictory complexes do exist according to Bergmann. Even so, an existent contradictory complex is, according to him, nonatomic or else a diad.) Furthermore, only simples exemplify properties--all other determinates being property-less.

Existence is not even a subdeterminate, although the reasons for that are surely more intricate and less plain. At any rate, what I wish to underline is that, should we put up with existents' existing without existence, why not endure black particulars being black without blackness and so on? Admittedly, there are special reasons against accepting existence, but that is beside the point. The point is that, if one turns nominalist as regards existence, he can in exactly the same way turn nominalist as regards properties and relations in general, thus embracing outright Sellarsianism. We can always feign to understand 'is black' as a diacritical mark or squiggle in virtue of which 'a is black' differs from 'a is white', the former statement committing the speaker to (recognizing) black things, the latter to white things--appropriate measures of paraphrase being taken for the cases of subject-position property-terms, in virtue of a reasonable principle of supervenience, which may be accepted by a realist, viz. that a universal cannot have a property without at least some of its "inferiors" (entities that exemplify it) being in some way or other modified or affected thereby. The nominalist thus can conclude that, even though in some cases we don't know what the proper paraphrase is, we do know that there is some such paraphrase. Sharing Bergmann's realist convictions, I reject such a nominalist gambit, but all arguments I have been able to develop for backing up realism would, upon our allowing for a commitment to existents without existence, either crumble altogether or at least lose their strength and become dependent upon contrast-principles which seem to be highly questionable.[13]

10th Objection.

My final general objection again concerns modes. Since Bergmann's ontology is clearly a form of neoaristotelian hylomorphism, let me compare his doctrine about modes with Aristotle's. For Aristotle, there is in

every actual thing, barring a Pure Actuality like God, a principle of its
actuality together with a potentiality principle which receives and con-
tracts the former. So Aristotle can--in a way which, however, bristles
with difficulties--explain how two things can share one same actuality or
form (form always being the actuality principle). That Bergmann cannot
do. Both his modes are more like Aristotle's forms (even though, to be
sure, Bergmann himself carefully and watchfully remarks that his modes
are not Bergmannian forms--a Bergmannian form being nothing over and
above the entity that has it). A mode is shared by several complexes.
Now, there is no such entity as the mode's pervading the complex. All
this we know. Neither is there any compound made up by both the com-
plex and the mode. How, then, can two complexes share their mode? For,
there is nothing in them which can be the mode and which also can be
in other complexes.

SECTION 3.   OBJECTIONS TO BERGMANN'S ACCOUNT OF RELATIONS

I shall now consider objections addressed to Bergmann's treatment
of relations proper (i.e., difference and identity as well as junctions be-
tween complexes and membership which is for now being sidestepped).
What strikes me as particularly implausible about this account of rela-
tions is that it amounts to saying that a's having r to b is nothing but
the 2-tuple's ⟨a,b⟩ exemplifying r, provided that we grant (what is
doubtful, as I shall shortly underline) that Bergmann's account of a
nonrelational fact is an adequate way of construing a particular's exem-
plifying a property. For, if a's exemplifying property g is nothing else
but the diad's (a,g) being clung to by exemplification (the clinging in
question being nothing at all, not even a tie or a subdeterminate--
which, by the way, requires that all our discourse about it is nonsense),
then the diad's (⟨a,b⟩,r) being clung to by exemplification is bound to
be nothing else but ⟨a,b⟩'s exemplifying r. Now ⟨a,b⟩ is nothing else but
(a,(a,b)), i.e., a≠(a≠b). The true subject of the exemplification of r is, on
that analysis, the difference between a and a's being different from b.
Whatever else can be said for or against this approach, that fact alone
seems to me a most unpalatable result. My loving my wife is certainly
not the same as love's being exemplified by my being other than my be-
ing different from my wife. It is, instead, I who exemplifies love.

Notice that the usual set-theoretical analysis of relations is much
less unbelievable. We can, albeit somewhat artificially, construe love as a
property of ordered pairs, if ordered pairs are sets of sets. The set
whose only two members are Wittgenstein's singleton and the class com-
prising just Wittgenstein and Weininger can be said to belong to admira-
tion. (Though unnatural, this at least *looks* bearable.) After all, that set
(to put it in Bergmann's words) exhausts its nature gathering into a
single determinate, viz., itself, all and only those determinates which
satisfy its enumeration clause [Bergmann, 1981a, p. 142], all of which
behave the same way. But nothing of the sort can be said about diads
as Bergmann analyzes them. The "circumstance" (we'd ordinarily say
"fact") that a is different from something else, b (i.e., a's being other
than b), is surely something which, at least in a sense, separates a from
b or interposes itself between them--rather than yoking or gathering
them.

(Should Bergmann reply that I am mistaking his diads for diversi-
ty-facts, for which there is no room in his ontology, I would reply that,

although they are not facts in Bergmann's technical sense, they are cir-
cumstances which are actual or potential according as their two consti-
tuents are different or the same. A sign meaning a diad is true iff the
diad is actual, otherwise false. A diad's negation (i.e., negation's clinging
to the diad) is the circumstance of both constituents being one and the
same. As ordinarily understood by set-theorists a class cannot be as-
serted or denied. If it could be denied, the negation might mean the
class's nonexistence, i.e., the lack of a link uniting or fastening the
class's members together into a collection. A Bergmannian diad's negation
is, instead, an identity between the two involved terms, something which
joins or couples them more closely than the diad. [See Bergmann, 1981b,
n.3, p. 207, and n.16, p. 209.])

My second point against Bergmann's analysis of relations is that I
do not think his account of nonrelational atomic facts can be subscribed
to for a reason similar to that noted above. What Descartes' pride con-
sists in is not exemplification's "clinging to" the difference between
Descartes and pride. If that clinging can be something, it surely con-
sists in that for Descartes to be other than pride (or conversely) exem-
plifies--period. (According to the accounted hinted at in note 12, that
statement can be truthfully made. The difference in question exemplifies,
since it exemplifies many properties, e.g., existence, being thought
about, and many others. But nothing of the sort can be granted Berg-
mann. Above all, it must be insisted that this would have but little to do
with Descartes' being proud.)

Let me now bring up a more important point concerning Berg-
mann's analysis of both nonrelational and relational atomic facts: What
we have is something like Aristotelian hylomorphism. An entity which
lacks selfbeing (ontic separableness and, of course, actuality), i.e.,
exemplification, by "clinging" to a diad (which, unlike Aristotle's prime
matter can be actual by itself) makes the diad into a new complex entity
(which, unlike Aristotle's sunolon, can be merely potential). The "cling-
ing tie" is nothing. Once the diad and exemplification are there, the
latter's clinging to the former is there too. (That, of course, raises an
issue dealt with at the end of objection 2 in Section 2.) Like Aristotle's
substantial form, exemplification needs no intermediary or go-between
tying it to its receiving substratum. But then the difference between
the diad (Fox, whigness) and the fact of exemplification's clinging to
that diad is that the one can be potential and the other actual.

In all facts allowed for within Bergmann's categorial account of
facts, the diad clung-to is bound to be actual, while that diad's being
clung to by exemplification may be either potential or actual. When it is
actual, I see no difference between the diad and the fact. Exemplification
fails to play even the Aristotelian role of actualizing its receiving
substratum, and, not being conjoined or bound to the diad by any con-
nection at all, lacks moreover any selfbeing or peculiar nature, physiog-
nomy, or what have you. (It is absolutely unclassifiable, falls under no
US, and has no feature or property, not even a thisness of its own,
since it is not a thing.) I can see no peculiar quality or trait which it
can contribute to or bestow on a complex which it enters. If that com-
plex contains, besides it, just another entity which is by itself real (or
even actual), I can see no new content of the purportedly new complex.
So the only case wherein there may be a difference between the fact
and the diad occurs when the fact is potential. Exemplification, then, by
clinging to the diad makes it into a potential complex. And then the

"pervading" of the fact by potentiality being nothing at all over and above potentiality and the fact, much the same difficulties can be raised again. What is in and by itself nothing, since it absolutely lacks selfbeing, properties, and so on (even its being other than actuality is nothing--neither fact nor circumstance) "pervades" a complex, giving it thereby what alone characterizes that fact and makes it different from the diad it contains. If I understand anything of this doctrine, I take it that it amounts to saying that exemplification is a way of potentiality's pervading the diad and nothing else. Exemplification thus becomes a tie between the pervading mode of potentiality and the diad formed by the meanings of the subject and the predicate of the sentence that means the atomic fact. So Bergmann has been unable to dispense with *ties*. But resorting to ties is a questionable procedure, since a tie is supposed to do the connecting work of a relation without being a relation among relations--without, that is, its tying two entities being bound to undergo the same kind of analysis which a relational fact must be susceptible to. And that is a unfair device. If such an analysis is avoidable for "tying circumstances", why not for relational facts?

All of the foregoing considerations apply equally well to the Bergmannian analysis of relational facts, since all that sorts those facts out from nonrelational ones is that the involved diad does not consist in a universal's differing from a particular but in its differing from a particular's being other than its being different from another particular. That complication alters nothing concerning what has been said so far.

Bergmann might then just as well acknowledge a tying or fastening bond between a mode and the complex it pervades, since his exemplification is in fact none other than such a bond. The ordering problem is, however, apparently solved. The complex and the mode once given, no alteration of the order with which the mode bears the tying bond (indeed, relation) to the complex can be allowed for in virtue of the categorial restrictions which stem from Bergmann's "canons". Is the price worth it? I don't think so, since it foists upon us the unbearable clog of ineffability. (A difficulty with our reconstructed Bergmannian account is that one and the same diad may be pervaded by a mode through two different "ties": exemplification, for one, and mere "pervading", for another--the latter being such that, with regard to the diads we're concerned with in Bergmannian facts, the sole pervading mode is always actuality. The difference between these two pervading links is hard to explain unless we label them as the "exemplificating" and "nonexemplificating" pervading ties--whatever such a "solution" might be worth. At any rate, a part of the same problem arises even from Bergmann's original account. Since a complex's existence along with a mode's existence doesn't suffice to determine that the complex is pervaded by that mode rather than by the opposite one, something or other must intervene, a pervading tie or, better, some solder by means of which the mode is soldered to the complex or fused into it.)

I want to take exception as well to Bergmann's way of founding order. It seems clear enough that his way suffers from all the problems besetting the usual set-theoretical manner of analyzing away order in relational facts--most obviously, the failure of the attempt. As much has been cogently argued by Hochberg [Hochberg, 1981]. By analyzing 'Booth assassinates Lincoln' as /(<Booth, Lincoln>, assassinating) exemplification/, we are availing ourselves of an implicit ordering operation by means of which we treat the subject or referent (the first term) of the

relational fact, Booth, as *former* and the relatum (the last term) as *latter*. For the 2-tuple ⟨Booth, Lincoln⟩ is nothing else but Booth≠(Booth≠Lincoln), a diversity wherein one of the terms occurs twice, the other once. Thus one term is taken first and is set over against its own differing from *some* other term. And then the gap pointed at by the quantifier 'some' is filled by the second term. There is, of course, an arbitrariness about the procedure (and that is what has been most often frowned at), inasmuch as we may also take a≠(a≠b) to be a substitution instance of the quantification ∃x(x≠(x≠b)). This is not my present concern, although I agree that the arbitrariness in question has something untoward about it. My point is that we need to take a≠(a≠b) one way or the other--either as a substitution instance of ∃x(x≠(x≠b)) or as one of ∃x(a≠(a≠x)). The asymmetry of the resulting formula thus evinces an implicit order followed in the process leading to that result. Such an asymmetry is by itself an order.

To see this more clearly, set up the 3-tuple ⟨a,b,c⟩ in accordance with Bergmann's procedure, i.e., as ⟨a,⟨b,c⟩⟩. Then that 3-tuple turns out to be the diad a≠(a≠(b≠(b≠c))). The pattern is clear: first x's difference from x's difference from something, next, that "something" becoming y's difference from y's difference from something. We stop at the last "something" if we have already inserted n-1 terms (each occurring twice), and we are out for an n-tuple. This is how "we" build up the n-tuple expression. (If, alternatively, we choose to identify ⟨a,b,c⟩ with ⟨⟨a,b⟩,c⟩, the 3-tuple turns out to be (a≠(a≠b))≠((a≠(a≠b))≠c). In a way the ordering pattern now becomes clearer, if more intricate. The first element is the one that now occurs four time; the second the one that occurs twice; the third the one that occurs once. Or, if you wish, the other way around, going right to left.) The order emerges not just in the process but in the result. If, as Bergmann's methodology rightly demands, our (grant its being ideal) language must reflect the world in its "structure", I take it that the implicit order in our linguistic pattern reveals or displays the implicit order in the real entity (or "structure") that pattern mirrors. It is then far from being true that order has been grounded in something else.

This is not yet, however, the main point to be highlighted. What seems to me most important and most serious is that, by his so taking as already ordered or structured a whole whose ultimate constituents are the relation's extremes (subject and term or *referent* and *relatum*), Bergmann overlooks the fact (rightly emphasized by Russell) that relations have a *sense*. That is, in any relational fact, the relation proceeds from the referent to the relatum. The signing relation proceeds from King John to the Magna Carta, while on Bergmann's view it is the property of the already structured 2-tuple ⟨King John, the Magna Carta⟩. Needless to say, that account also bars applying the clipping-off rule. From 'Graham Greene writes *The Heart of the Matter*' we could no longer draw 'Graham Greene writes' unless, of course, the conclusion is paraphrased as implicitly quantificational--a move which, for reasons I cannot deal with here, is highly objectionable.

Failing to account from the clipping-off rule (rxz ⊢ rx) is, of course, a common weakness of all available treatments of relations except combinatory ones (which have not as yet been taken seriously enough from a philosophical point of view). Not surprisingly, combinatory approaches also can cope with other difficulties which Bergmann's account

(as well as those of Frege, Russell, Hochberg, and--up to a point--even Castañeda) fails to handle satisfactorily.[14]

SECTION 4. BERGMANN'S IL

I wish to comment on two points in connection with Bergmann's IL. First, his delinearization [Bergmann, 1981a, pp. 149-50]. The procedure consists in replacing the usual disambiguating symbols (as, in Polish notation, order or, in standard notation, a mix of order and auxiliary symbols, such as parentheses, all in an underlying string structure) with circles. It is understood that, within each circle, the contained signs can be written in any order, left to right or conversely, top to bottom or bottom to top, without a complex sign's being thereby changed. Thus, if we wish to represent a's exemplifying g, we encircle together the signs 'g' and 'a' and thus represent the diad $g \neq a$. We may then write the exemplification sign near the circle and draw another circle surrounding both the first circle and the exemplification sign. Let us abbreviate as 'p' the resulting inscription. If we wish to write 'p·q', we write 'p' and 'q' in any order and enclose them in a new circle, writing the conjunction sign next to that circle, and finally enclosing all of this within a new circle. By virtue of the ontological canons (and, therefore, by virtue of the well-formedness rules which are their syntactic counterparts) all ambiguity is dispelled. The second and fourth circles cannot represent diads, since exemplification and conjunction are not determinates. For the first and third circles are diacritical marks for being-diverse-from, whereas the other two are disambiguating auxiliary symbols like couples of mating parentheses. The remaining details can be safely ignored here.

Let me now briefly comment on all of the above. As far as I know, the first to have pointed out the linearity of language (or, more exactly, the linearity of linguistic messages) was Ferdinand de Saussure in his *Cours de Linguistique generale*. And that doctrine has been taken over by all structuralists [see Martinet, 1970, I-10, pp. 16-7]. That linearity is a function of our human natural language's being a vocal rather than, say, a gesture language, since vocality requires a temporal output and thus (as usually conceived) one dimensionality. Written language structures have thus tended to imitate (as far as possible) and tightly fit vocal language structures. Natural language's linearity is, however, far from perfect. For one thing, along with segmental signs, there are suprasegmental signs, such as prosodic factors of tone, stress, and the like, whose role is important in some languages and which add further dimensions. (Linguists have been prone to softpedal such suprasegmental signs, since they are less easy to scan and verify and since semantically they play only marginally distinctive roles in languages like English. In other languages, for example, Italian, they play much more systematically distinctive roles.) For another thing, some written representations of vocal language also (if in a restricted way) resort to suprasegmental diacritical marks like accents, etc.

Some conceptographic notations, like Frege's, are partially non-linear in character. But what most of all needs emphasizing is that linear order is often immaterial (though it is certainly there). Many languages allow a great many strings which differ among themselves in the order of the combined signs occurring in them to stand for the same meaning and so to be "felt" by language speakers as all being the same string

under different forms and variations. Consider the Latin 'Romam condid-
ere troiani', 'Condidere troiani Romam', etc., with six strictly equivalent
strings, all permutations of one another. But even in these languages
linear order matters. At any rate, this is how many or most linguists
think, since they take 'Romam' to be a syntagmatic phrase made up of
two morphemes which cannot be separated. But, of course, other analy-
ses are available (e.g., the WP approach[15] or transformational accounts).
These alternative analyses would stress the irrelevancy of linear order
in such cases.

   It would seem that the two procedures by which natural language
breaks partially loose from the sway of linear order are quite different
and ought not to be run together. Therefore, Hochberg's reply to Berg-
mann's delinearization program [Hochberg, 1981, pp. 161-2] might be
thought to be unconvincing, since all Hochberg accomplishes with it is
partially superseding the importance of linear order via resort to inflec-
tional marks. 'aRb' could be written, say, as 'a!Rb', with '!'a desinence of
the subject or referent, like a "nominative", so as to make the latter
formula strictly equivalent to 'Rba!', 'ba!R', Ra!b', and so on (six in all
again--with lack of '!' being a zero-morpheme indicating the role of re-
latum or term, i.e., a sort of "accusative").

   What I want to underline is that Bergmann's own delinearization is
not unlike that one. True, sign combinations are now allowed along two
dimensions rather than just one, thus making it possible to align two
combined symbols in infinitely many ways, not just in two as in the case
of a one dimensional language. These infinitely many ways, however,
might be regarded as falling into just eight "canonical" combinations,
disregarding smaller differences as many such differences are neglected
in the usual one-linear notation. That does not mean, however, that lin-
ear order has been done away with. It is still there, though it has be-
come a merely partial order. Besides the formerly existing linear order,
another linear order has been added along a second dimension. Do these
orders matter? It devolves upon the system's axioms and inference rules
to settle the issue. But such is the case for one-linear languages too.

   The case of prosodic or suprasegmental signs is similar, since it
can be represented as adding either just one or several further dimen-
sions. In some cases the differences thus achieved are semantically ir-
relevant (although they can and often do matter from a pragmatic view-
point).

   My final comment on Bergmann's delinearization is that we ought
to conceive written language signs as writing acts rather than written
inscriptions. So the linear order for that language is a temporal one.
Likewise, we had better look upon verbal language signs as speech acts,
not as sounds. In fact, phonologists have always tended to conceive of
phonemes and signifying combinations thereof in just this way, prefer-
ring an articulatory account to an acoustic one. What means Paris is an
act of saying 'Paris'. That changes the entire outlook concerning a
sign's having occurrences within another. Literally it isn't true that a(n
act of) saying 'Peter is here' occurs in or is part of a(n act of) saying
'Peter is here or there'. Likewise, the relation between saying 'aRb' and
saying 'a', 'R', and 'b' is not accurately styled a whole-parts relation.[16]

   The second point I wish to make about Bergmann's IL concerns
diacritical marks. According to Bergmann, diacritical marks stand for

nothing but, nevertheless, discharge a disambiguating task. So, e.g., '∈(a,b)' (or '∈⟨a,b⟩', as Bergmann claims his class theory requires) stands for the circumstance of a's belonging to b, whereas '∈' itself stands for nothing. What is worth emphasizing is that whether something is a sign or a mere diacritical marks is for the syntacticist to decide as a function of his semantic (and, in last resort, ontological) convictions. A Sellarsian (or Tractarian-Sellarsian) theorist can reconstrue any given language by calling many drawn lines or ink traces mere diacritical marks, i.e., mere features of a certain complex sign's shape--not signs in their own right. And, of course, certain such ink-traces of a nominal-istically devised language, which were designed to serve as diacritical marks, may be reconstrued as denoting-signs by a realist "decipherer". (Even Sellarsian blanks can be so construed, as shown by Wilkin [Wilkin, 1979].) That detracts from the plausibility of the Tractarian claim that needless or dispensable signs are not signs (i.e., they do not denote). The claim gets trivialized by a cunning and skillful "deciphering". In a similar way and via artful "translations", Quine's ontological commitment criterion can be relativized--as Quine, above all others, is apt to admit, since this is in fact a consequence of his ontological relativity thesis. Therefore, Bergmann's IL doesn't support, as he apparently expects, his own ontological views. But please note that these comments are not de-signed to support ontological relativity or anything of that sort, but rather to show that, even though a certain way of looking at language (syntactically as well as semantically) goes hand in hand with a certain way of viewing the world, still (using a) language by itself neither com-mits us to any particular ontology nor relieves us from any such com-mitment. It commits us to some ontology or other only depending upon such methodological principles as we may espouse. This is why the very enterprise of constructing an IL seems to me futile. A symbolic notation, used along with schematic letters, is of utmost usefulness for repre-senting (some features of) spoken language perspicuously, in a catch-ing-the-eye sort of way. Thus conceived of, a symbolic notation is, how-ever, not a language. Furthermore, if an ontological doctrine can be said and made sense of, either it is said in natural language which we (hope we) understand, or else it is set forth in an artificial language which, if it can be understood at all, is to be translated into natural language. (This remark is not a denial that worthwhile insights can be gained through artificial *notations*--if they are taken in the way I have just pointed out.)

APPENDIX: WILSON'S ASSESSMENT OF THE INEFFABILITY ISSUE

The issue of ineffability, discussed above in the 1st object of Section 2, has been taken up at length by Fred Wilson [Wilson, 1983, esp. pp. 441-50]. Wilson focusses on the problem surrounding the US of particularity. I don't think that Wilson has gotten Bergmann right. Fol-lowing is how Wilson presents Bergmann's argument for the ineffability of 'a is a particular'. If a is a particular, we cannot believe that it isn't. (This lemma is proved by Wilson by an amazingly complicated argument, one which Bergmann couldn't accept in Wilson's terms, for reasons I shall leave unsaid here.) Therefore 'a is not a particular' must be ill-formed. (Wilson literally says that such a formula "in belief-stating con-texts must be considered as *ill-formed*', p. 449. But surely, if a formula is to be "considered" as ill-formed in certain contexts, it must be so "considered" in any other context whatsoever. Infringement of this rule would hugely complicate syntax and offer no worthwhile return for that

complication.) Now, if 'particular' were an acceptable term in the language, then 'a is a particular' would be a formula (even if an ill-formed one). And every formula 'p' is such that $\ulcorner p\urcorner Mp$ (see above, end of Section 1). Now there is, of course, no complex consisting of a's failing to be a particular. Hence 'a is not a particular' means nothing at all. If follows that 'particular' is not an acceptable term.

To that line of argument Wilson replies by waiving Bergmann's interpretation of the formula $\ulcorner p\urcorner Mp$'. For Wilson this formula says that the-thought-that-p causes one to utter the sentence 'p'--under certain circumstances of course. According to Wilson an ill-formed formula can be uttered, and therefore "it" can be meant. $\ulcorner p\urcorner Mp$ in no way calls for 'p' to be well-formed. All that reconstruction and Wilson's own point are at odds with Bergmann's viewpoint and also with what seems reasonable to the present writer. (That does not mean that I look upon Wilson's idea about accepting ill-formed formulae as indefensible. No, that idea makes sense. See, e.g. [Tabakov, 1983].)

First and foremost, according to Bergmann, a complex's being such that its negation cannot be believed in no way entails that the negation of the formula denoting that complex is ill-formed. On the contrary, Bergmann takes all contradictions to be well-formed formulae. Therefore Wilson's argument cannot be Bergmann's. No, what Bergmann is attempting to convey is that, were 'a is a particular' to be sayable, its negation would also be sayable (in virtue of the contrast principle). Were such a negation sayable, then the thought whose text it is would mean something, namely, the complex consisting of a's failing to be a particular. But can any such complex entity exist? No, for it would consist in negation's clinging to a's being a particular. Since negation clings to nothing but complexes, a's being a particular would then be, not a two-in-one, which Bergmann takes it to be, but a genuinely complex entity. But it cannot be, since an infinite regress would be triggered. For in that case a's being a particular would be made up of two entities, a and particularity (to say nothing of exemplification, i.e., supposing a's being a particular to be a circumstance rather than a fact). a itself could no longer be something constituted by particularity plus a bare individuating item. (For, in that case, a itself would within itself have to possess whatever complexity could be ascribed to a's being a particular, that is to say, a's being a particular would be nothing else but a itself, within which we'd find the item plus particularity.) Or it could no longer be a bare individuating item--a *determinant*, to use Bergmann's former technical term. If it were a determinant, it would be just *this* determinant, which, by the same token, would require its being made up of both some individuating thisness and determinanthood. And so on. (Wilson's own reconstruction of Bergmann's account is in fact liable to such a regress--p. 460. He vainly tries to escape the regress "by defining a determinant to be any entity which has either the property of particularity or the property of universality". I feel unable to see how this suggestion can facilitate any way out. It's not a question about *sameness*, as Wilson says on p. 461.) Therefore, if 'a is a particular' could be said, a's being pervaded by particularity would be a genuinely complex entity, and thus an infinite regress would be triggered. Since Bergmann rejects infinite regresses, he concludes that 'a is a particular' cannot be said. On the other hand, application of the contrast principle to tautologies yields, on the ground of Bergmann's assumptions, recognizing real complexes denoted by contradictions.

Wilson is, however, quite right in saying that Bergmann's case for realism (his claim against Sellars that it doesn't suffice to posit green things, but for each green thing you need to posit greenness, on the one hand, and, on the other, some particular entity or substratum) can rightly be likened to a similar case for viewing a thing's particularity as a complex comprising both the universal, particularity, and the thing itself.

CONCLUSION

I have for years held Bergmann to be one of our age's (few) metaphysicians. I am confident he both deserves and will obtain an honored position in the history of philosophy. He has the merit of having come to grips with fundamental ontological questions, taking the bull by the horns. If his grand attempt comes out unsuccessful in the end, that is the mark of a need for a radical alternative, leagues away from the beaten track (an alternative such as the sketchy remarks in nn. 12, 14, and 16 aim at).

ENDNOTES

1   I'm thereby referring to Hugh R. King and William Charlton. See [Charlton, 1983]. Charlton's main mistake is his failing to take account of Aristotle's conceiving of prime matter as pure potency. Aristotelian potency encounters to my mind insurmountable difficulties which may be a good reason for disposing of prime matter altogether--as well as any other individuating principle conceived as "barely" particular.

2   An anonymous referee invites me to compare Bergmann's view on continuants to Hume's and to make it clear why construing physical things as series of momentary particulars is supposed to be illegitimate. Hume's views are on their own sufficiently difficult to grasp. The discussion in [Bennett, 1971], pp. 333 ff., far from being able to solve Hume's problems, fails even to grasp the heart of Hume's problem, a problem which invokes a Tractarian theme: To say that two things are identical is false, while to say that a thing is the same as itself is meaningless. In other words, if identity is a relation, it needs to relate two things, which is impossible. Notice that Bergmann somehow "solves" this problem by bestowing existence both on the identity between a and b, whatever they may be, and on the difference between a and a itself. (A more accurate examination of Hume's approach is to be found in [Hirsch, 1983]. Even so, Hirsh seems not to have grasped completely Hume's point about identity either.) Be that as it may, Hume himself regarded his reducing continuants to series of momentary entities as resorting to a mere fiction inasmuch as nothing in his ontology is a *series* over and above the series' members. Whatever we purportedly say about the series is to be understood as somehow or other (under adequate paraphrases, to be sure) applying instead to the members thereof. Withal, Bergmann wisely refrains from viewing continuants as real series of momentary entities. No such series can be a simple, so it ought to be a complex. And, if it is complex, it must be a circumstance, a fact, or a class. It can be neither a circumstance nor a fact, since (1) it can be neither potential nor actual (You can neither assert nor deny the Taj Mahal); (2) no circumstance or fact, as understood by Bergmann, could seriously be identified with a continuant body; (3) talk about any such

circumstance or fact would be quite removed from talk about an individ-
ual. Then it must needs be a class. But Bergmann rightly dislikes re-
sorting to his classes beyond strict necessity, since classes are in his
ontology derived complexes of an unconspicuous nature. Moreover, Berg-
mann's classes seem to be non-spatial, non-temporal entities. If the Taj
Mahal is a class, it cannot have been built in the 17th century. Further-
more, since the category of classes is different from the category of
particulars, it would then be senseless to assign to the Taj Mahal any-
thing we usually say about it, e.g., that it is in India--if to be in India
is, as it seems, a property of particulars, e.g., the-Taj-Mahal-now. (No-
tice that this last problem about categorial boundaries would also afflict
any identification of a series of (momentary) particulars with any Berg-
mannian complex whatever.)

3   The larger complex in question (the one consisting of another
complex being pervaded by a mode) might be supposed to exist while
the negation thereof failed to exist. But then the complex would be un-
sayable. Unsayable complexes are, according to Bergmann, alone in being
such that their negations do not exist. Yet no such entity is a complex
proper, since negation is a clinging subdeterminate which clings to any
complex whatever, thus bringing about a new and existent complex--
whether potential or actual, according as the clung-to complex is actual
or potential.

4   It is, of course, very hard to pinpoint the locus of an argument
in the *Tractatus*. The argument under consideration emerges in 2.02 ff.
and in a somewhat different way in 3.23-3.24 (about which see [Pears,
1981], especially the discussion of H. Ishiguro's interpretation, pp. 77-8).
I have analyzed that argument at length in Chapter 13 of Section I of
[Peña, 1985b], pp. 288-95, and in [Peña, 1985c]. Indirectly the same point
can be buttressed by ascertaining that in the *Tractatus* every speak-
able-about entity is perforce a simple one (see 3.21 ff, 2.03, 2.072, 2.01).
Furthermore, Wittgenstein's criticism of Russell's theory of judgment (in
5.541) also adds to that general line of argument. For a different view
of 2.0211 see [White, 1976].

5   See the Appendix of this paper where I go into Wilson's discussion
of Bergmann's tenets about ineffability.

6   It has been suggested to me that Bergmann could make use of
Meinong's moves, which he examined earlier in his book, *Realism*. Now,
resorting to a Meinongian view of possibility or the like is to my mind
at odds with Bergmann's strong realism. For, as I have pointed out in
Chapter 11.4 of Section I of [Peña, 1985b], devoted to Meinong, Mei-
nong's thought is to be regarded as a mingling of two kinds of essen-
tialism ('Essentialism' here being taken in this non-Quinean sense: a
doctrine according to which some truths are about no existent entity at
all): alethic essentialism (which was started by Aristotle) and ontic es-
sentialism (which was started by the Stoics). Routley and the "free logi-
cians" have consequently developed Meinong's thought in the former di-
rection (each in his own way). Grossmann, Castañeda, and Rapaport have
rather tended to set up Meinongian ontic-essentialist theories. (The rel-
evant references are to be found in [Peña, 1985b].) Bergmann rightly (to
my mind) rejects any essentialism whatsoever. On the other hand, (part
of) what Meinong has to say about possibility points towards a kind of
acceptance of degrees of being. I myself find this line of thought con-
genial, but I cannot imagine how it could be reconciled with Bergmann's

philosophy. Let me summarize the grounds of my rejection of Bergmann's modes. What I find distasteful is the difference between actuality and existence as regards complexes. (Bergmann is there in the company of philosophers like A. Plantinga with the distinction between *existing* and *obtaining*. See [Plantinga, 1974].) But, of course, I'm far from oblivious to the consequences of identifying them (which the logical atomists did). Yet my own enterprise is keen on taking up such identification and then avoiding unacceptable results through appropriate measures (thanks to the recognition of degrees and aspects of truth or existence. See below, n. 12.). On Bergmann's plight in consequence of acceptance of (fully) existing and yet (merely) potential entities, see my objection 3 in Section 2 of this paper.

7   Except, of course, in a devaluated sense of 'can' or 'could', i.e., the Leibnizian one point out above at the end of Section 1. We may say that a complex which is actual could be potential inasmuch as we thereby are saying nothing else but that it is contingently true that the complex is actual, where 'contingently' means just 'synthetically' (i.e., in such a way that the complex's mode is inaccessible to us). We have no evidence for which mode pervades the complex, but this does not hinder us from *feeling* sure that in fact the complex is pervaded by the mode of actuality or by the mode of potentiality, according as we feel sure that the complex is the case of not (as we would commonly put it).

8   What I mean is that any logically incoherent theory can be patched up so as to hide its logical flaws by means of inserting a number of reduplication clauses. Aristotle himself more than once (mis)uses 'hei' clauses. Such a (mis)use is the Achilles heel of Medieval Aristotelians. On facing the incompatibility between God's unchangeableness and necessity on the one hand and the contingency of the created universe, they, e.g., used to say that one and the same entity, God's decision to create, is, as God's decision, necessary and unchangeable but, as decision of creating this or that, contingent. (See my book [Peña, 1981], passim. Sundry references will be found there to Aquinas, Suarez, and other Aristotelian metaphysicians concerning their approach to problems in philosophical theology. In [Peña, 1985b] I've gone into the misuse of reduplicative clauses in ontology proper by all of the Aristotelian lineage, from the Stagirite himself onwards.) The reduplicative device has rightly been jettisoned by contemporary (analytical) philosophers, though it still emerges sometimes in the bloated prose of non-analytic philosophers. I am aware of attempts at bestowing logical respectability on reduplicative clauses. Late Medieval and Post-Renaissance Scholastics endeavored to whet the tool by positing a number of different senses of reduplicative clauses, i.e., clauses starting with one of the particles 'qua', 'quatenus', 'in-quantum', 'ut', 'utpote', 'prout', 'secundum', and so on: the reduplicative sense proper, the specificative sense either *proprius* or *minus proprius*, either *per se* or *per accidens*, and so on. See Aristotle, *Prior Analytics* I, ch. 38 (49a12–b3). Also Joannes a S. Thoma (1948), *Ars Logica*, Prima Pars Summularum 1.2c.24, p. 57. For recent accounts of reduplication see P. Geach, "Nominalism", in [Geach, 1972]; [Angelelli, 1978]; [Fine, 1982]; [Back, 1985]. However much ingenuity may be displayed by its friends, I remain firmly convinced that reduplication has had its day. See [Wiggins, 1980], p. 89.

9   As I shall point out below, in the Appendix, Bergmann can avail himself of an argument against words standing for US which wouldn't tell against the thinkability of contradictions. What my contention in the

5th Objections intends to convey is that, so far as unthinkability goes, I find a≠a no less unthinkable than a's utterly failing to be a particular.

[10]   Bergmann is, of course, widely known and quoted as positing bare particulars. As far as his later work is concerned, however, we need to make a distinction: Items and US are bare, propertyless; but particulars proper, i.e., such two-in-one's as are each made up of both an item and a US, are not bare. The notion of bareness may be understood in different ways. Some people say that for a thing to *have* properties it needs to be in and of itself bare, propertyless. This is how many people view the Tractarian objects (see, e.g., [Goddard and Judge, 1981]). Nevertheless, it's one thing to say that, underneath its having or exemplifying a property, a thing in itself is without properties and accordingly bare (and in that sense it's true to say that Tractarian things are bare-- their ipseity, thing-as-content, having no features). It's quite another thing to say that an entity (of sorts) lacks any properties whatever, whether "in itself" or in any other way (though in fact there is no "any other way" in such cases). The latter, stronger sense is the one that applies to Bergmann's bare entities, viz., items, US, all other subdeterminates, complexes. As for the former, weaker sense, it arises from worrying about what a thing *is* aside from its entering into relations with other things. Even if Tractarian internal relations, combinabilities, are constitutive of the things, Wittgenstein feels that, even so, the thing itself must be a *substratum* which as such is bare. Such, at least, is my interpretation--and apparently it is Goddard and Judge's too. To be sure, Wittgenstein would be right, if, as he believes, the identity of indiscernibles were false, two different things being thus able to share, in the same degree, all their properties and relations, even their internal ones (i.e., their respective forms). The worry is not unjustified in itself, but I think there is a far better solution to the problem than positing bare things in any sense (see below, the end of n. 12). As for Bergmann's own bare entities, some of them, viz., complexes, i.e., facts and circumstances, although propertyless, still have a "feature". They are either analytic or synthetic. (Recall, however, as was pointed out above, at the end of Section 1, for a complex to be analytic(ally true or false) is nothing else but for its being pervaded by its mode (actually or potentiality, respectively) to be accessible to us.) I take it that those features are not properties. How could they be properties within the framework of Bergmann's ontology, unless that framework were either shattered or through and through made over? But then what are they? Some odd kind of "over-US" or the like which "pervade" complexes as modes do or as US pervade items in such a way that any such pervading is nothing over and above the pervaded and pervading entities? Withal (but not unrelatedly, of course), complexes enter a (*pseudo*)relation of sorts, since they are meant (by thoughts). F. Wilson in [Wilson, 1983] criticizes Bergmann on this account, showing the difficulties the Bergmannian "relation" of meaning bristles with. I think that those who know the Aristotelian-Scholastic tradition will recognize here, as elsewhere in Bergmann, some time-honored devices: relations of reason, connotations which are not really relations, nonpredicamental relations. My own attempt at finding a solution to the issues which prompt resort to such devices will consist in avoiding what seem to me subterfuges. But I feel in honesty bound to admit that the task is far from easy. Otherwise, there would be no need to cavil about people's falling back on such procedures, once a clear, unproblematic alternative was found. The solution I myself have to offer is not either easy or problematic.

[11]   See [Unger, 1979a], [Unger, 1979b], [Unger, 1980]. Unger's sorites constitute the most relevant challenge to classical logic put forward of late (although he himself doesn't view things in that way. Quine has cleverly made it out). See [Quine, 1981]. Taking up that challenge is going to be one of the two main grounds justifying my own approach, the other ground being the need for an account of relations free of the two Aristotelian liabilities, viz., categorial boundaries and the assumption of something such as prime matter or the like. (These are flaws, since they, each and both, entail ineffability.)

[12]   Let me briefly hint at what my combinatory, noncategorial account amounts to. In my world--to borrow Bergmann's expression--every entity is at the same time a determination and a state of affairs. Each being is the same as its being, i.e., its existence. This is why everything is a fact, a state of affairs. Therefore, a name standing alone means the same as the sentence resulting from concatenating the name with 'exists'. Nominalizations of sentences mean the same as the sentences in question; there is merely a stylistic difference as well as (in some languages) a surface-structure syntactic difference of distribution. (I'm aware of objections like this one: 'To see someone is not the same as to see the fact that he exists'. Isn't it? Why? I surmise that such objectors are mistaking pragmatic restrictions--as to the wording of our linguistic messages according to in what contexts they are uttered--for semantic differences.) Furthermore, a fact is a determination. Facts in general fall into two kinds: *transient* and *intransient*. The former are such as are meant by sentences which can be augmented by a subjoined direct object (e.g., such facts as someone's smoking, lending, painting, borrowing, writing, loving, eating, breathing, playing, and so on). Any fact is now identified with a determination, the determination of being an entity into which the fact "passes", i.e., of being an entity meant by an expression which can be as a direct object attached to a sentence meaning the fact in question. I take it that an intransient fact is a determination that determines itself, where for x to determine z is nothing else but for z to exemplify x. This is why one lives one's life, dies one's death, and so on. Therefore, a's bearing relation b to c is nothing else but for c to be determined by a's being determined by b. Thus all atomic sentences are of the subject-predicate type. Non-atomic sentences are handled in a similar manner. Negation is a predicate: not-p is nothing else but for (the fact that) p to exemplify nothood. Conjunction is a predicate too, a relation such that p-and-q is nothing else but p's exemplifying and-q, where and-q is nothing but for q to exemplify andhood (i.e., conjunction itself). (A partly similar treatment is to be found in [Fitch, 1976]. See also Fitch's reply to Geach's objections, ibid., pp. 201 ff.) Quantifiers are introduced in a combinatory manner. Two primitive combinators are introduced as primitive symbols. For any sentence 'p' containing an expression 'b', we define the b-abstract of 'p'. The b-abstract of p is the determination of (being like b in) being such that p. An axiom-schema is laid down according to which, for any two expressions, 'b' and 'c', containing no occurrence of either of those two combinators and any sentence 'p', c exemplifies the determination of the b-abstract of p insofar as it is the case that p' (where 'p' and 'p' ' are exactly alike except that 'p' ' contains free occurrences of 'c' at all and only those places where 'p' contains free occurrences of 'b'. Such is the axiom of separation for determinations. Thus the system manages to escape incoherence (deliquescence). Still, a number of problems remain. My account identifies each relation with its domain, which entails that, if two relations share their domain, they are the same. That difficulty is solved through

resort to a tensorial infinite-valued logic. This is, at any rate, called for by sundry considerations, e.g., those connected with Unger's sorites (see n. 11 above). Within that framework existence is nothing but the relation of *being-exemplified-(by)*. Hence, for a to be determined by b is nothing else but for b to bear the relation of existence to a. 'To exist' is thus viewed as a "transitive" verb. No technical details of my treatment can be gone into here. As far as what has been hitherto said goes, not all difficulties between relations are thus solved. The Russellian problem remains of how a's exemplifying b is to be distinguished from b's exemplifying a. Since there are no categorial differences, aren't we compelled to resort to some third, hidden entity, meant by concatenation and belonging to a higher category? We might try to sidestep such a difficulty by acknowledging for any entity two symbols which mean it but in two different ways. But that by itself wouldn't entirely dispose of the difficulty. My final solution consists in viewing a's exemplifying b not as a (static) combination of a and b through exemplification, but as a (nontemporal) process: existence passing from b into a. (And I am prepared to espouse an account of *passage* or transiency which is both contradictorial (thus resorting to fuzzy paraconsistent logic) and also accepts an ontological analysis involving some kind of infinite regress.) Likewise the sentence 'ba' is to be regarded not as a combination of two symbols, 'b' and 'a', nor of course as a *tertium quid* having nothing to do with either, but as some passing from 'b' into 'a'. (See below, n. 16.) A passing from a thing into another is neither in the former nor in the latter, but yet it is in both. Yes, that is a contradiction. What I daresay is to be gathered from the riddles surrounding relations is that those riddles cannot be coped with in a noncontradictory way. Still, there are paraconsistent logics (especially the fuzzy contradictorial system referred to hereinabove and put forward both in [Peña, 1984] and more thoroughly developed in [Peña, 1986a]). A concise presentation of the philosophical ideas underlying my treatment (but with no examination of some of the thorniest puzzles about relations taken account of in these notes) is to be found in [Peña, 1986b]. Let me bring this note to its close by pointing out that my ontology seems able to waive bare entities altogether. For one thing, the contrast principle is given up--as are any categorial boundaries. For another, identity of indiscernibles is kept. For still another, every entity is in my ontology viewed as something towards other entities, thus ruling out any worry about what the entity is either beyond or this side of (or underneath, or whatever) the things it is towards. This point can be explained as follows. A relational fact is a state of affairs consisting in some entity, a, bearing some relational determination, b, to another entity, c, e.g., Robespierre's friendship towards Saint-Just. For any such fact, there is the subrelational fact that a exemplifies b, e.g., Robespierre's friendship (which is, as well, a determination of other people). Obviously, a subrelational fact is characterized by "towardness", since it exists towards whatever it determines (in fact, such is the case of any determination). Now any entity, x, is the subrelational fact that x exists, i.e., that x determines--and so a fact which is towards whatever is determined by x. Therefore, everything is characterized by towardness and is nothing at all without or beyond its towardness. Towardness is existence. Hence every entity is the same as its own towardness. No ontology can be farther from metaphysical isolationism than this one.

[13]   Wilfrid Sellars developed his nominalistic account in a number of papers. See, e.g., [Sellars, 1949], [Sellars, 1963a], [Sellars, 1963b], [Sellars, 1963c]. A thorough implementation of Sellars' ideas is to be

found in [Sicha, 1974]. The most in-depth discussion of Sellars' ideas to date are to be found in [Hochberg, 1978] and [Hochberg, 1984]--which, among other essays, includes the by now famous "Mapping, Meaning and Metaphysics". See, however, Sellars' reply to that essay in [Sellars, 1977]. A very interesting discussion of Sellars' purported way of avoiding even the linguistic temptation to raise Bradley's paradox is [Wilkin, 1979].

[14]   As I have said, the clipping-off rule can be formalized like this. From 'rxa' to conclude 'rx': If s.o. eats something, he eats; if he loves s.o., he loves. In English there are but few verbs which can with the same meaning be used both as active and intransitive verbs. Other languages (Greek, Latin, Romance languages) can virtually always delete the direct object of a sentence with the verb in the resulting sentence having the same meaning as in the given one. Yet that linguistic evidence doesn't amount to a proof of the correctness of the clipping-off rule. A number of philosophers (e.g., Frege) and linguists contend that a sentence like 'Brian loves' is to be viewed as an elliptical one, the direct object, 's.o.', having to be understood. Many linguists (among them Lyons, in [Lyons, 1968], pp. 350 ff.) think that (at least many) transitive verbs hide a deep structure causative form. Those linguists implicitly acknowledge just one transitive verb, 'to cause', whose direct object is always a nominalized sentence or something of that ilk. My treatment instead takes 'to love' as primitive in the active sense, but as an intransitive verb, i.e., as a one-place predicate. For Robin to be loved by Brian is for Brian's love to determine (i.e., be truly attributed to) Robin. Accordingly, all relational verbal phrases are in deep structure one-place VV.PP. But then you no longer can ignore the question of how 'Brian loves Robin' is related to 'Brian loves'. Understanding in the latter sentence an elided existential quantifier can be justified within the ergative treatment, but the move is, otherwise, far from obvious. What is more, if 'Brian loves Robin' results from adjoining 'Robin' to 'Brian loves', it is clear that the latter cannot be a mere abbreviation of 'Brian loves s.o.'. Obviously, if Brian loves, he loves s.o. or something, but those are two different facts and needn't be equally true or real, even if they are both real. The latter may be much more existent. The first philosopher to have exploited the clipping-off rule was Plato. (In a more general way: any "complement" can be excised without the sentence thus resulting being utterly false when the given sentence was, to some extent or other, true. See [Peña, forthcoming].) Notice, finally, that, if you agree that 'Brian loves' is an elliptical way of saying 'Brian loves s.o. or something', which I don't, you are also bound to accept my clipping-off rule, although then in a trivialized way. On the other hand, you may spurn my countenancing a one-place predicate, 'love', and then say that the rule makes no sense. The few English cases apparently bearing the rule out will be explained away through paraphrase (i.e., through *catalisis*, the converse procedure to ellipsis). But then, for those cases at least, you accept my rule in the aforementioned trivialized way. And you ought to face up to the fact that in other languages such verbs are not exceptional, but in fact include *all* active verbs. Ellipsis is thus to be fallen back on more widely--too widely indeed, since it seems reasonable to keep deep structure as close to surface structure as possible. On the ontological side of the issue, notice that for Brian to love is for love to determine him, i.e. (see note 12 again), for existence to be passing from love into Brian, which may happen in a smaller degree than love's passing from Brian into, say, Robin--or anyone else for that matter.

15   Problems faced by linguists are not unlike our own philosophical
problems. Thus for inflected languages a number of alternatives are
canvassed, each fraught with its own difficulties. One of those syntactic
alternatives is the WP approach (from 'Word Paradigm'), according to
which a word like the Latin, 'hostium', is to be taken as a noncomposite
whole, instead of being regarded as a string of morphemes meaning 'en-
emy', 'plural', 'genitive', then somehow amalgamated or blurred. Among
linguists having developed the WP approach, I can mention R.H. Robins.
See an introductory account in [Matthews, 1970], pp. 107-10. The prob-
lem with WP is that the whole segment 'hostium' needs to be regarded
as a partless unit and yet also appears somehow to result from two or
more adjoined segments even if these segments are "then" fused or
welded in some way.

16   It has not been light-heartedly or with airy insouciance that I've
chosen to regard written language signs as acts of writing. All I have
been taught ran counter to such a move. My main ground is the one re-
ported in this paper. So long as you regard the sentence 'ba' as a
whole made up of two parts, 'b' and 'a', you face the following trilemma.
Either (i) you regard 'b' and 'a' as signs of different categories, which
raises the question of whether the entities respectively denoted are of
different ontological categories (an unsayable claim, as is well-know, and
one giving rise to such difficulties as the predicament of Frege's con-
cept-correlates); or (ii) you regard them as signs of different *syntactic*
category bearing different semantic relations to the entities they re-
spectively "stand for" (each in its own way of "standing for"), which
raises the difficulty that two signs standing for the same entity, say
subject b and predicable B, will have *nothing at all* in common, with no
systematic way of obtaining one of those symbols out of the other; or
else (iii) you regard them as belonging to the same category and saying
that what marks the subject-role of 'a' is just the place it occupies,
which in turn gives rise to this difficulty: marking a syntactic role (or
"function") by a position is a syntactic procedure equivalent to other
means used in other languages, like affixing some kind of functional
morpheme to the expression to which the position in question has been
assigned. (See, e.g., [Martinet, 1970], sections 4-10, pp. 109 ff.) All of
this amounts to resorting implicitly to a third sign. My problem with
this is not exactly a Bradley regress, but the fact that, if we resort to
a third (elliptical) component, we need then to explain something similar
to what we had initially tried to explain. If the elliptical functional mor-
pheme is some kind of "desinence", say '!', to be attached to the "sub-
ject", i.e., in our case to 'a', then the sentence 'ba' now becomes 'ba!'
(equivalent, of course to 'a!b'), which, in fact, means that we have a
sentence one of whose components is 'b', the other component being 'a!'.
We now need to analyze the inner structure of 'a!' (since '!' must stand
for something or other), i.e., to explain what is the "subject" therein
and how its role is marked. Thus we may apparently conclude that 'ba'
is an incomposite, riftless atomic whole. But, of course, that conclusion
can be refuted. What then? I suggest that a sensible way out is to re-
gard the sentence 'ba' as a process, a passing from writing 'b' to writ-
ing 'a', without ever being a mere writing 'b' or a mere writing 'a'. As
the travelling body can be viewed as being at no place and yet also at
each place included in the whole stretch of its travelling (see my con-
tradictorial treatment of Zeno's paradox of the arrow in [Peña, 1983c]),
likewise during writing--which is movement after all--the writer or the
writing hand or pen is never at the starting position, never at the final

one, never in any part of the whole course, and yet also during each part of the whole interval it is at all the parts of the whole course. Writing 'ba' is not writing 'b' and then 'a'. It is a single movement, a continuous passing from writing 'b' to writing 'a' (yes, an act may be a passage from another act into a third one).

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