Science Without Numbers: A Defense of Nominalism. by Hartry H. Field
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Science without Numbers: A Defense of Nominalism. HARTRY H. FIELD, Princeton, N.J.: University Press, 1980. 130 p. $16.*

According to Hartry Field, "there is one and only one serious argument for the existence of mathematical entities" (5), and that is a Quinean argument from indispensability: we cannot adequately describe and explain the world around us without resort to physical theories of a sort which at least appear to make reference to such entities.

Science without Numbers is, for the most part, an extended attempt to undercut that argument. It is true that theoretical physicists talk about sets, functions, manifolds, tensor fields, and so forth. Professor Field proposes that nonetheless they do not have to. He suggests that *it should always be possible, at least in principle, to reformulate their theories so as to avoid all (apparent) reference to, or quantification over, abstract entities.* To make the suggestion plausible he discusses one example in considerable detail (Newtonian gravitational theory), and indicates how his treatment might be extended to other classical field theories posed against a flat space-time background (e.g., Maxwell's theory of the electromagnetic field).

In addition to presenting his argument for the ultimate dispensability of mathematical entities, Field briefly sketches his own positive account of the role played by mathematics in science. Specifically, he tries to explain why it is legitimate and advantageous to use mathematical theories when reasoning about the physical world, even though those theories do not refer to objects in the world and cannot be said to be either true or false.

Field's idea is that we should think of mathematical theories as *instruments* for deriving nominalistically stated conclusions from nominalistically stated premises. So construed, their use in *legitimate* insofar as they satisfy a strong consistency condition—that of being *conservative* in the logician's sense. (For in this case any nominalist conclusions derivable with their help will already be derivable from the nominalist premises alone.) Their use is *advantageous* insofar as they shorten our derivations and, perhaps, render them more intuitive. But with respect to neither legitimacy nor utility does the question of their "truth" arise.

It might seem at first of only limited interest that nominalists

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can justify the use of mathematical theories as instruments for deriving nominalist conclusions from nominalist premises. But here is where Field's principal (italicized) thesis comes in. If he is correct, the pool of available "nominalist premises" will include the constituent axioms of our most fundamental physical theories. And so it will follow that nominalists can justify even sophisticated applications of mathematics in highly theoretical contexts.

Field's remarks on instrumentalism and conservativeness at the beginning of his book are rich and provocative. They deserve the attention of anyone with an interest in the philosophy of mathematics. But in what follows I am going to concentrate on his strategy for rewriting physical theories in nominalist form.

A FIELD-STYLE REPRESENTATION THEOREM

Physicists characteristically use mathematical models to represent natural phenomena. Field believes that nominalists should be able to get at those models through the back door, using representation theorems of the sort familiar from work on the axiomatic foundations of geometry and on measurement theory.

The examples Field has in mind are classical field theories. Characteristically these hypothesize that some particular physical field of force can be represented by a mathematical field (scalar, vector, tensor, spinor) on an underlying four-manifold satisfying specified partial differential (field) equations. The points of the manifold represent "space-time points."

Newtonian gravitational theory fits this mold. But a somewhat tidier example for my purposes is the theory of the (classical, mass-zero) Klein-Gordon field.[1] Here the field is represented by a smooth scalar field $\psi: M \rightarrow \mathbf{R}$ on Minkowski space-time (M, d) satisfying the field equation $\Box\psi = 0$.[2] Of course there is more to the theory

[1] The example may sound arcane, but the use I'll make of it is very simple. Newtonian gravitational theory is a bit cumbersome as an example because Newtonian space-time geometry involves three independent elements: a four-dimensional affine structure, a spatial metric, and a temporal metric. In contrast, Minkowski space-time is fully characterizable in terms of one space-time metric. The theory of the Klein-Gordon field is the simplest classical (i.e., non-quantum-mechanical) field theory posed against the background of Minkowski space-time.

[2] Some definitions: I take Minkowski space-time here to consist of a smooth four-manifold M diffeomorphic to $\mathbf{R}^4$, together with a Minkowski distance function d: $M \times M \rightarrow \mathbf{C}$ on M. One can choose standard t, x, y, z coordinates so that d is given by

$$d(p, q) = [(t(p) - t(q))^2 - (x(p) - x(q))^2 - (y(p) - y(q))^2 - (z(p) - z(q))^2]^{1/2}$$

In these coordinates the mass-zero Klein-Gordon equation $\Box\psi = 0$ is given by

$$\frac{\partial^2\psi}{\partial t^2} - \frac{\partial^2\psi}{\partial x^2} - \frac{\partial^2\psi}{\partial y^2} - \frac{\partial^2\psi}{\partial z^2} = 0$$

than this, but it will be convenient to think of it as simply determining a set of mathematical models $((M, d), \psi)$. Each represents a dynamically possible life history of the Klein-Gordon field. All the models share a common first element. But they differ in their second element.

By way of illustration I'll formulate a Field-style representation theorem for (this weak fragment of) the theory of the Klein-Gordon field.[3]

The Minkowski distance function and the Klein-Gordon field induce various "qualitative" relations on the set of space-time points. Representation theorems establish that, in a sense, it is possible to run the process backwards. One can construe the relations as primitive, impose strong constraints, and then *prove* that they are induced by a Minkowski distance function and a Klein-Gordon field. The particular theorem formulated here deals with three relations.

1. A four-place *segment-congruence* relation. (Intuitively, four points stand in the relation if the space-time (Minkowski) distance between the first and the second equals that between the third and the fourth.)
2. A three-place *scale-betweenness* relation. (Intuitively, three points stand in the relation if the value of the Klein-Gordon field at the second is (inclusively) between those at the first and the third.)
3. A four-place *scale-congruence* relation. (Intuitively, four points stand in the relation if the difference between the values of the Klein-Gordon field at the first and the second is equal in absolute value to that between the third and the fourth.)

In addition to these, two higher-order set-theoretic relations are involved.

4. The two-place *membership* relation which holds between spacetime points and sets of such.
5. A two-place *cardinality comparison* relation. (Intuitively, two sets of space-time points stand in the relation if either both are infinite, or the number of points in the first is less than or equal to the number in the second.)[4]

[3] In what follows I shall not bother to repeat the qualification in parentheses. I shall refer, simply, to "the theory of the Klein-Gordon field."

[4] The cardinality-comparison relation is supposed to bear the same relation to cardinal numbers that, for example, the segment-congruence relation bears to the space-time distance function. The relations are construed as primitive, but explicated informally in terms of the latter mathematical objects.

To give the theorem a precise formulation we must introduce a formal language. Let L be a second-order language with variables for individuals, variables for sets of individuals, and the relation symbols: '=', 'Seg-Cong', 'Scale-Bet', 'Scale-Cong', 'ϵ', '$\leqslant$' (all of the appropriate type). Let a *standard interpretation* of L be one in which 'ϵ' is assigned the (real) membership relation. Then the theorem can be phrased this way:

Theorem There exists a set of sentences T in L such that, given any standard interpretation (X, Seg-Cong$_X$, Scale-Bet$_X$, Scale-Cong$_X$, ϵ_X, $\leqslant_X$) of L, it is a model of T iff there exists an original geometrical model $((M, d), \psi)$ and a bijection $\phi: X \to M$ such that, for all points p, q, r, s in X and subsets U, V of X, the following conditions hold:

a. Seg-Cong$_X(p, q, r, s)$ iff $d[\phi(p), \phi(q)] = d[\phi(r), \phi(s)]$
b. Scale-Bet$_X(p, q, r)$ iff $\psi(\phi(p)) \leqslant \psi(\phi(q)) \leqslant \psi(\phi(r))$
c. Scale-Cong$_X(p, q, r, s)$ iff $|\psi(\phi(p)) - \psi(\phi(q))| = |\psi(\phi(r) - \psi(\phi(s))|$
d. $\leqslant_X(U, V)$ iff U and V are infinite, or card$(U) \leqslant$ card(V).

Field formulates a counterpart to this proposition in his discussion of Newtonian gravitational theory, and there sketches how one might prove it.[5] In effect he shows how one can develop the basic elements of vector calculus making reference only to the relations of congruence and parallelism between line segments. With only minor adjustments his proof sketch can be adapted to the present example.[6]

Theorems of this sort are the centerpiece of Field's "nominalization stategy." He would claim, in fact, that *theories T satisfying the stated condition deserve to be counted as nominalist reformulations of the theory of the Klein-Gordon field.* This philosophical claim is certainly controversial, and I shall soon consider several possible objections. But it should be emphasized that Field's theorems are nontrivial results of considerable technical interest whether or not they support the weight of his interpretation.

Suppose for the moment that Field's philosophical claim is accepted. Then one can illustrate his early remarks about the use of mathematical theories as instruments for deriving nominalist conclusions from nominalist premises.

[5] Actually, Field's initial formulation is a bit different, He first works with a "cardinality comparison quantifier" rather than the present relation. But later he shows how, at least in the contexts of concern to him, the former can be traded off for the latter.

[6] Within the context of Minkowski space-time geometry, the relation of parallelism (between line segments) can be defined in terms of congruence. (This is not true in Newtonian space-time geometry.)

Let T be a "nominalist reformulation" of the theory of the Klein-Gordon field (satisfying the condition of the theorem). It is understood as a theory whose subject matter is not the Minkowski distance function and the Klein-Gordon field, but rather the set of space-time points and several primitive relations into which they enter.

Let S be some fact about the Klein-Gordon field which admits a "nominalist reformulation" S_L in L. [For example, S might be the assertion: If the Klein-Gordon field is constant over some interval of time (as measured by some inertial observer), then it is constant over all of space-time. This can be expressed in terms of 'Seg-Cong' and 'Scale-Bet' using only first-order quantifiers.[7]]

Now suppose Field's nominalist physicist wants to prove S_L from T. One efficient method involves a "platonist" detour through S. First he invokes auxiliary mathematical hypotheses (including mathematical existence assertions) which allow him to prove in the manner of Field that the relations of segment-congruence and scale-congruence are induced by a Minkowski distance function and a Klein-Gordon field on space-time. Then, invoking elementary facts about partial differential equations, he proves that the Klein-Gordon field necessarily satisfies the condition of S. Finally he translates his result back into L and concludes that S_L holds.

If I understand him correctly, Field's idea is that this derivation can be justified without assuming that the physicist's auxiliary mathematical hypotheses are "true." It is sufficient that they be conservative. For in this case the detour through metrics, fields, and partial differential equations is avoidable. S_L is a direct logical consequence of T.

IS THE LANGUAGE L TOO WEAK FOR PHYSICS?

I want now to consider Field's (italicized) philosophical claim from the previous section (523). Once again let T be a set of sentences in L which satisfies the condition of the representation theorem.

Field would like to offer T in rebuttal to the argument from indispensability (as applied to the theory of the Klein-Gordon field). But, if it is to qualify as the promised "nominalist reformulation" of the theory, at least three conditions must be met:

1. L qualifies as a "nominalist language."

[7] Somewhat more geometrically, S might be formulated as follows: Given any two parallel simultaneity slices (i.e., spacelike hypersurfaces), if the Klein-Gordon field is constant between them, then it is constant everywhere. To express this in first-order form one can replace reference to simultaneity slices with reference to triples of space-time points which determine the slices.

2. All assertions concerning the space-time distance function and the Klein-Gordon field which are essential for the purposes of science ("describing and explaining the world," or whatever) can be reformulated in L.
3. Given any sentence in L, if it is derivable from the theory of the Klein-Gordon field in its original formulation (using standard mathematics), then it is a logical consequence of T.

Condition 3 is guaranteed by the representation theorem and is not problematic. (If a sentence in L comes out true under interpretation in all geometric models of the original theory then, by the theorem, it must be true in all models of T.) But the others are clearly controversial. Let me first consider condition 2.

Of course the condition as it stands is vague. Still, one can make some progress toward evaluating it by indicating types of assertions concerning the space-time distance function and the Klein-Gordon field which can*not* be reexpressed in L.

Recall that, as I characterized the theory of the Klein-Gordon field, it determines a set of models of form $((M, d), \psi)$ where (M, d) is Minkowski space-time and ψ is a smooth real-valued function on M satisfying the Klein-Gordon equation $\Box\psi = 0$. Theorems about this set can be classified by structure. Among the possibilities are these:

A. Propositions which report generic features of individual models. [One example was cited above: Given any model $((M, d), \psi)$ and any two parallel simultaneity slices in (M, d), if ψ is constant between the slices then it is constant everywhere in M.]

B. Propositions that establish the existence of models with special features. [A trivial example is: There exists a model $((M, d), \psi)$ in which ψ is non-constant. More interesting are propositions that establish the existence of models in which the field exhibits specific wave characteristics.]

C. Propositions that make essential reference to more than one model. [An example of this sort is a theorem establishing that the theory of the Klein-Gordon field is deterministic: Given any two models $((M, d), \psi)$ and $((M, d), \psi')$ and a simultaneity slice H in (M, d), if ψ and ψ' agree on H and if their time derivatives (i.e., directional derivatives orthogonal to the slice) agree there, then ψ and ψ' agree everywhere.]

Now *within the language* L *Field can at best reformulate theorems in the first category*. Moreover, this would still be true even if L were enriched to allow reference to other "qualitative relations" (besides segment-congruence, scale-betweenness, and scale-congruence) induced by the space-time distance function and the Klein-Gordon field. Field's nominalist physicist cannot assert that it is possible for the Klein-Gordon field to be nonconstant.[8] He cannot assert that the field evolves deterministically. Indeed he cannot do anything except assert general truths about what goes on within arbitrary models.

This does not establish that condition 2 above is *false*. It does show that Field is committed to a narrow and austere appraisal of what is essential for the purposes of science. Some might say that it is *so* narrow as to undermine interest in his claim that one can do science without abstract objects.

IS THE LANGUAGE L TOO RICH FOR NOMINALISM?

Consider next the claim that L qualifies as a "nominalist language." Several objections readily come to mind. According to the first, the description is inappropriate because L admits second-order quantifiers. It naturally seems illegitimate for nominalists to quantify over both space-time points and *sets* of such.

Field himself anticipates this objection and gives a long, complex response. I am not sure that I fully understand it, but at least very roughly it has the following structure. Field accepts the objection as it stands, but suggests, first, that it can be partially deflected if the second-order variables in L are interpreted as ranging over *regions of space-time* rather than sets of space-time points.

> If our nominalist accepts Goodman's calculus of individuals, then the introduction of points carries with it the introduction of regions: for a region is just a *sum* (in Goodman's sense) of the points it contains. And even if one does not accept the calculus of individuals in general—even if one thinks that there are entities that can't meaningfully be "summed"—there seems to be little motivation for allowing points and yet disallowing regions. . . . So it seems to me that regions are nominalistically acceptable (37; italics in original).

The deflection is only partial, he acknowledges, because the new interpretation leaves untouched the character of the logical-consequence relation for L. And he believes that nominalists should at least be *reluctant* to accept a "logic" that is neither recursively axiomatizable nor compact.

The second stage of the response involves a fork. Field first *conjectures* that the full strength of L is not necessary for physics and

[8] To be sure he has the available the sentence: $(\exists x)(\exists y)\ \neg$ Scale-Cong(x, y, x, x). But this captures only the quite different statement that the Klein-Gordon field *is* (in all cases) nonconstant.

that one can make do with a weakened first-order version.[9] (I'll skip the details.) Then he claims that, even if the conjecture fails, it does not follow that nominalism falls prey to the argument from indispensability. He argues that, if it comes to a choice between giving up nominalism and swallowing the logical resources of L, the latter may well be preferable.

> . . . although there are certainly advantages to using only a compact and recursively axiomatized fragment of logic in developing physics, there are also advantages to keeping one's ontological commitments to a minimum; and the situation that we would be in (on the assumption that nominalism can't be made to work without going beyond first-order logic) is that we would have to make a choice as to which of two desirable goals is more important. It seems to me that the methodology to employ in making such decisions is a holist one: we should be guided by considerations of simplicity and attractiveness of overall theory. It seems totally unreasonable to insist on sticking to the requirement that logic be kept compact and recursively enumerable, *whatever* the costs for ontology; it is the simplicity of the *overall conceptual scheme* that ought to count (97; italics in original).

I am rather puzzled by the way Field argues the second prong of his fork. He seems to be responding to the objection that the logical strength of L is in some general sense undesirable or unattractive. (At one point he writes that "the invocation of anything like a second order consequence relation is *distasteful*" (38; the italics are mine). I should have thought that the objection is more pointed—that nominalists are not *entitled* to the logical resources of L. Presumably, if a "logic" is not recursively axiomatizable (and not compact) its logical-consequence relation cannot be recovered in terms of a formal derivation system. So, the objection goes, a nominalist cannot understand the assertion that sentence S_L is a logical consequence of theory T. (What could it mean to say that S_L is true in all set-theoretic models of T?)

In connection with this objection, consider again the example used to illustrate Field's remarks about conservativeness. The claim there was that the detour derivation of S_L from T making use of auxiliary mathematical hypotheses is justified insofar as those hypotheses are conservative. For then, according to Field, one can always prove S_L from T alone. But how, in the absence of a formal derivation system, is a nominalist supposed to do *that* (in general)?

[9] One possible candidate is a language in which 'ϵ' is dropped, an inclusion relation '$\subseteq$' is added, and the (first-order) variables are interpreted as ranging over space-time regions. "Points" would then be introduced as regions without proper subregions; and the three geometric relations would be restricted to points.

He certainly cannot present a semantic argument about the properties of arbitrary models of *T*.

My purpose here is not to press the objection, nor to deny the possibility of an adequate response. My point is simply that I cannot find one in Field's remarks about holism.

A second possible objection to the claimed nominalist status of L would apply just as well to a weakened first-order version of the language. It holds that it is illegitimate for nominalists to quantify over space-time points or space-time regions.

Field also anticipates this objection. He himself believes that it is legitimate for a nominalist to quantify over these objects only if a certain philosophical doctrine, which he calls "substantivalism," is correct.

> There are, to be sure, certain views of space-time according to which the quantification over space-time points or space-time regions really would be a violation of nominalism. I'm speaking of *relationalist* views of space-time, as opposed to the *substantivalist* view. According to the substantivalist view, which I accept, space-time points (and/or space-time regions) are entities that exist in their own right. In contrast to this are two forms of relationalist view. According to the first (*reductive relationalism*), points and regions of space-time are some sort of set-theoretic construction out of physical objects and their parts; according to the second (*eliminative relationalism*), it is illegitimate to quantify over points and regions of space-time at all (34; italics in original).

Field does not take it on himself to argue for substantivalism. But he does record in passing his belief that relationism (in either form) is untenable, quite apart from any views one may have on nominalism. He does not think it possible to formulate physical theories —specifically classical field theories— without quantifying over space-time points,[10] and he does not believe that any reductive analysis of them has ever been satisfactorily completed, even given a "full blown platonist apparatus of sets" (35). At the very least, Field insists, substantivalism is a serious view, and is accepted "by the majority of the 'new wave' of space-time theorists" (35). So it is no liability that he is committed to the view by his nominalization strategy.

I find this response somewhat unsatisfying. It is not that I believe that Field has an obligation to give a full argument for substantivalism or that I subscribe to some version of relationalism.

[10] To avoid 'and/or' I'll phrase things in terms of space-time points. Nothing here turns on the difference between a space-time point and an (atomic) region with no proper subregions.

Rather it seems to me that his way of approaching the objection side-steps what is most important.

Suppose it is agreed that space-time points are "entities that exist in their own right." Still, philosophers with nominalist scruples might well be uncomfortable with them. They certainly are not concrete physical objects in any straight-forward sense. They do not have a mass-energy content (unlike, for example, the Klein-Gordon field itself). They do not suffer change. It is not even clear in what sense they exist *in* space and time.

Field takes for granted the distinction between concrete, physical objects on the one hand and abstract objects on the other. But I, for one, begin to lose my grip on the distinction when thinking about such things as "space-time points." It would have helped me to understand his conception of nominalism if Field had explained how he draws the line and made clear why space-time points are so much *better* than, for example, sets and qualities.[11]

ARE FIELD'S EXAMPLES REPRESENTATIVE?

There are other objections that one might raise to the claim that L is a "nominalist language." But rather than pursue them I want to mention a final difficulty which is quite different in character from those discussed so far. *Field's nominalization strategy, even if successful in some cases, almost certainly fails when applied to other physical theories of interest.* His example (Newtonian gravitational theory) and mine (the theory of the Klein-Gordon field) are both very special.

I am not thinking here of the difficulties that arise when one

[11] Field does argue in one footnote that space-time points are *causal agents.* This, perhaps, would suggest why they are nominalistically acceptable. But the argument seems strained to me.

> Note incidentally that according to theories that take the notion of a field seriously, space-time points or regions are full-fledged causal agents. In electromagnetic theory for instance, the behavior of matter is causally explained by the electromagnetic field values at unoccupied regions of space-time; and since, platonistically speaking, a field is simply an assignment of properties to points or regions of space-time, this means that the behavior of matter is causally explained by the electromagnetic properties of unoccupied regions. So according to such theories space-time points are causal agents in the same sense that physical objects are: an alteration of their properties leads to different causal consequences (114).

Field here construes an electromagnetic field as "an assignment of properties to points or regions of space-time." I suppose one can characterize a field this way, but then one could characterize a sofa similarly. The important thing is that electromagnetic fields are "physical objects" in the straightforward sense that they are repositories of mass-energy. Instead of saying that space-time points enter into causal interactions and explaining this in terms of the "electromagnetic properties" of those points, I would simply say that it is the electromagnetic field itself that enters into causal interactions. Certainly this is the language employed by physicists.

moves from scalar theories to more complicated (tensor or spinor) classical field theories. Nor am I thinking about the problems generated when the assumption of a flat space-time background is dropped and curvature is allowed. I am prepared to believe that, if one introduced the right "qualitative relations," *some* sort of Field-style representation theorem would always be possible. It is the entire category of classical field theories which is special.

Let me mention two examples where Field's strategy would not seem to have a chance: classical Hamiltonian mechanics, and ordinary (nonrelativistic) quantum mechanics. (Quantum-mechanical field theories would provide further examples.)

As in the case of the theory of the Klein-Gordon field, it is simplest to identify Hamiltonian mechanics by its determination of a class of mathematical models.[12] Each model is of form (M, Ω_{ab}, H) where M is an even-dimensional manifold, Ω_{ab} is a symplectic form on M, and H is a smooth, real-valued ("Hamiltonian") scalar field on M. The points of M represent "possible dynamical states" of a given mechanical system. (Ω_{ab} and H jointly determine a "Hamiltonian vector field" which characterizes the dynamic evolution of the system.)

Now Field can certainly try to trade-in Ω_{ab} and H in favor of "qualitative relations" they induce on M. If successful, he can reformulate the theory so that its subject matter is the set of "possible dynamical states" (of particular physical systems) and various relations into which they enter. But this is no victory at all! Even a generous nominalist like Field cannot feel entitled to quantify over *possible dynamical states*.

The point here is very simple. Suppose Field wants to give some physical theory a nominalist reformulation. Further suppose the theory determines a class of mathematical models, each of which consists of a set of "points" together with certain mathematical structures defined on them. Field's nominalization strategy cannot be successful unless the objects represented by the points are appropriately physical (or non-abstract). In the case of classical field theories the represented objects are space-time points or regions. So, Field can argue, there is no problem. But in lots of cases the represented objects *are* abstract. In particular this is true in all "phase space" theories.

Quantum mechanics is even a more recalcitrant example than Hamiltonian mechanics. Here I do not really see how Field can get

[12] The following abstract geometric characterization of Hamiltonian mechanics has the advantage that it gives Field a toe hold. It makes the theory look as much like the theory of the Klein-Gordon field as possible.

started at all. I suppose one can think of the theory as determining a set of models—each a Hilbert space. But what form would the recovery (i.e., representation) theorem take? The only possibility that comes to mind is a theorem of the sort sought by Jauch, Piron, *et al.* They start with "propositions" (or "eventualities") and lattice-theoretic relations as primitive, and then seek to prove that the lattice of propositions is necessarily isomorphic to the lattice of subspaces of some Hilbert space. But of course no theorem of this sort would be of any use to Field. What could be worse than *propositions* (or *eventualities*)?

It is clear that I am not yet convinced of Field's principal thesis in *Science without Numbers* (the thesis first italicized above, page 523). Nonetheless I am much impressed by his book. It has a significant technical result at its core. It presents a strikingly original approach to central issues in the philosophy of mathematics. It is full of interesting passages on secondary topics. (I have not even mentioned a technical appendix in which Field discusses the relation between consistency and conservativeness.) And it is written with a spare, clean prose style that I find most attractive. It is a very fine work indeed.

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NOTES AND NEWS

The Institute of Communication and Cognition in Belgium announces a conference, "Pragmatics and Education," to be held in Ghent, March 21-25, 1983. Contributions are invited from a variety of disciplines, including educational psychology, linguistics, philosophy of language, and special education. Papers should be no more than 10 pages; abstracts of 100 words should be submitted by November 1. These should be sent to, and further information can be obtained from, Fernand Verdamme, Communication and Cognition, Blandijnberg 2, 9000 Ghent, Belgium.

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