Ohio State University

From: 'OSU Interlibrary Services' [liblend@osu.edu]
Sent: Monday, October 08, 2007 3:28 PM
To: libartexp@osu.edu
Subject: DOCUMENT DELIVERY REQUEST 336994

DOCUMENT DELIVERY for Neil Tennant

ILLiad Transaction Number: 336994

Call Number: B11 .A71
Location: SUL Bound Periodicals

Journal Title: Proceedings of the Aristotelian Society, Supplementary Volume
Journal Vol: LVII
Journal Issue:
Journal Year: 1983
Article Pages: 79-89

Article Author: Tennant, Neil
Article Title: A defence of arbitrary objects

Notes:

If you need to contact us, please refer to this Document Delivery Transaction Number: 336994
E-Mail: liblend@osu.edu
Phone: 614-292-6211

NOTE TO SCANNING LIBRARY:
Send this Document Delivery article to patron via Odyssey. ILLiad locations should 'Mark Item as Not Found' if requested article is not available.

A DEFENCE OF ARBITRARY OBJECTS

Kit Fine and Neil Tennant

II—Neil Tennant

§0. Fine's theory of arbitrary objects is both intriguing and perplexing. I shall explore the logical structure of difficulties facing it and solutions Fine proposes. In §1 I shall outline the structure of his argument for accepting arbitrary objects, indicating various rejoinders to Fine on certain points. Then I shall take up some of these points in greater detail: in §2, the problematic status of the principle of generic attribution, and in §3 the nature of the commitment Fine would have us make to arbitrary objects.

§1. Fine arrives at his theory by successively refining his ideas to take care of certain objections. These are as follows.

Objection 1 There are no arbitrary objects
Fine's reply There are. They are abstractions. But they are not on an ontological par with individuals.

Objection 2 The theory of arbitrary objects is logically incoherent
Fine's reply The arguments behind this objection

> depend upon the failure to distinguish between two basically different formulations of the principle of generic attribution: one in the material mode; the other in the formal mode. Once the distinction is made, the arguments are seen to be without foundation.

Rejoinder In a semantically closed language, the principle of generic attribution, if itself generic, leads to incoherence in a way that makes the principle itself suspect, rather than the fact of semantic closure. I shall develop this point in §2 below.

Objection 2a Semantical rules fail for complex predications on arbitrary objects.
Fine's reply

> The statement Q(a), regardless of its inner complexity, simply has the same truth conditions as $\forall$ iQ(i).

Rejoinder All sorts of differences now emerge between predications on arbitrary objects and predications on individual objects. Moreover they emerge even when the object language is logically perfect, with no vague predicates etc. Fine admits that his proposal for evaluating disjunctions by resorting to lambda abstraction means that the lambda conversion principle fails. He concedes that 'it is impossible to achieve complete logical parity between individual and arbitrary objects'. But this is to play down the importance of the difference: to offer a picture of a progressively diminishing but never disappearing difference in logical behaviour upon successive theoretical adjustments and manoeuvres. It seems to me, however, that the gap between arbitrary and individual objects yawns just as wide as we shunt the difference around from evaluation of complex predications to principles of property abstraction etc; and that one hardly need be an 'adamant logical purist' to be disturbed by this persisting difficulty.

Objection 2b Absurdity results from taking the principle of generic attribution within its own scope.
Fine's reply Distinguish *generic* from non-generic (*classical*) conditions.
Rejoinder How? Does the fault lie only in such predicates as 'being an individual number' or 'being in the range of'? Might it be generated also by certain logical operations, such as unrestricted quantification (especially in the case of set theory)? How do we know when a given condition is generic? Fine nowhere answers this question. On p. 64 he acknowledges the problem, but offers a circular answer:

> . . . it is not as if the principle [of generic attribution] had no application. Call a language *generic* if all of the conditions obtainable by its use are themselves generic. Then many languages, of natural and independent interest, will be generic; and so the principle (G3) [for any *generic* condition Q(x), Q(a) is true iff $\forall$ iQ(i) is true] will have wide application to all such languages.

'Being generic' ought to be a decidable property of conditions expressible in the language. Only then will the principle of generic attribution have application of sure axiomatic status.

We cannot wait to see whether a given
within the scope of the principle, will lea
undecidability of first order logic, and
matter, would appear to prevent one fr

§2. Fine distinguishes formulations of th
attribution in the material and in the for
for this is that if the principle is taken in t
Berkeley's example of odd or even
absurdity. But it is not exactly clear how
drawn. According to Fine, taking the pr
mode (as in his (G1): a Q's iff every ind

> rests on the fallacy of applying the
> only a part of the context in which th
> object appears. For although we
> $\forall$iQ(i), we cannot correctly infer Q

But the fallacy involved in the material m
be taken to be that of applying the princip
context in which the name of an arbitrar
recall Fine's own account of the matter,

> ask whether the arbitrary number *a*
> (Ex$\equiv\forall$iEi) of being even iff all in
> even. What the intuitive principle
> seems to tell us is that an arbitrary n
> the condition iff all individual num

What is this, if not applying the principle
the biconditional, rather than 'internally'
of its left hand side?
Fine's distinction between material and
principle of generic attribution can be disc
merely prohibit semantic closure. He o
something like a type restriction on applic
especially on the satisfaction and truth
in its formulations in the formal mo
emerge more clearly when formal proofs
Tarski's reason for resorting similarly to
theory of truth was to avoid the semanti
the Liar. Now with the semantical parad

differences now emerge between predi-
objects and predications on individual
emerge even when the object language is
no vague predicates etc. Fine admits that
ting disjunctions by resorting to lambda
t the lambda conversion principle fails.
s impossible to achieve complete logical
dual and arbitrary objects'. But this is to
nce of the difference: to offer a picture of a
ing but never disappearing difference in
n successive theoretical adjustments and
to me, however, that the gap between
ial objects yawns just as wide as we shunt
from evaluation of complex predications
rty abstraction etc; and that one hardly
t logical purist' to be disturbed by this

y results from taking the principle of
thin its own scope.
1 *generic* from non-generic (*classical*) con-

the fault lie only in such predicates as
umber' or 'being in the range of'? Might it
y certain logical operations, such as
ition (especially in the case of set theory)?
hen a given condition is generic? Fine
question. On p. 64 he acknowledges the
circular answer:

> the principle [of generic attribution] had
> Call a language *generic* if all of the
> able by its use are themselves generic.
> guages, of natural and independent
> eneric; and so the principle (G3) [for any
> Q(x), Q(a) is true iff ∀iQ(i) is true] will
> ation to all such languages.

to be a decidable property of conditions
guage. Only then will the principle of
ve application of sure axiomatic status.

We cannot wait to see whether a given condition, when taken within the scope of the principle, will lead to absurdity. Yet the undecidability of first order logic, and Fine's silence on the matter, would appear to prevent one from doing any better.

§2. Fine distinguishes formulations of the principle of generic attribution in the material and in the formal mode. The reason for this is that if the principle is taken in the material mode then Berkeley's example of odd or even numbers precipitates absurdity. But it is not exactly clear how the distinction is to be drawn. According to Fine, taking the principle in the material mode (as in his (G1): a Q's iff every individual Q's)

> rests on the fallacy of applying the principle internally to only a part of the context in which the name of an arbitrary object appears. For although we may affirm ∀iQ(i) ≡ ∀iQ(i), we cannot correctly infer Q(a) ≡ ∀iQ(i).

But the fallacy involved in the material mode might just as well be taken to be that of applying the principle *externally* to the *whole* context in which the name of an arbitrary object appears. If we recall Fine's own account of the matter, we

> ask whether the arbitrary number *a* satisfies the condition (Ex ≡ ∀iEi) of being even iff all individual numbers are even. What the intuitive principle of generic attribution seems to tell us is that an arbitrary number *a* should satisfy the condition iff all individual numbers do . . .

What is this, if not applying the principle to the *wider* context of the biconditional, rather than 'internally' to the part consisting of its left hand side?

Fine's distinction between material and formal modes for his principle of generic attribution can be discarded. He can instead merely prohibit semantic closure. He ought to be calling for something like a type restriction on applications of the principle, especially on the satisfaction and truth predicates occurring in its formulations in the formal mode. (This point will emerge more clearly when formal proofs are examined below.) Tarski's reason for resorting similarly to language levels in the theory of truth was to avoid the semantical paradoxes such as the Liar. Now with the semantical paradoxes, the problem lies in the natural rules of inference for the truth (or satisfaction) predicate, coupled with inferential moves licenced by the facts of self-reference afforded by semantic closure. Elsewhere[1] I have analyzed the proofs of absurdity associated with the paradoxes. They all appear to share the feature of not being normalizable. That is, the reduction sequence of a paradoxical proof does not terminate after finitely many steps. Instead it enters a loop whose periodicity depends on the logical structure of the paradoxical statement(s) in question. In the light of this, it seems reasonable to conjecture that the test of looping reduction sequences, applied to an enumeration of proofs in the semantically closed language, would yield an axiomatization of paradox.

The relevance of these remarks on paradox is this. If we take Fine's formulation of his principle in the formal mode, *but allow the language to be semantically closed*, absurdity results from a proof virtually identical to the one he gave for the case of the material mode. Thus semantic closure short-circuits the distinction between modes. In what follows I use these abbreviations:

gQ	Q is generic
a/φ	a satisfies φ
$T\varphi$	φ is true
PGA(x)	$\forall gQ(x/Q \equiv T\forall iQi)$
Ex	x is even
Ox	x is odd

PGA(x) is the principle of generic attribution for the case where x is an *independent* arbitrary object. In this case we can simply put $T\forall iQi$ on the right hand side of the biconditional, instead of resorting to talk of satisfaction by each individual in the range of the arbitrary object concerned. For an arbitrary object a, PGA(a) is axiomatic. Let us further assume that we are concerned only with natural numbers. n will range over genuine individual numbers. As an axiom schema we have $En \vee On$. In the absence of explicit criteria allowing us to determine otherwise, let us assume that PGA is a generic property. We have the following proof P of $\forall gQ(n/Q \equiv T\forall jQj)$ from assumptions PGA(a) and gPGA:

$$\dfrac{\dfrac{\dfrac{\dfrac{\dfrac{PGA(a)}{a/PGA} \quad gPGA \quad \text{i.}}{T\forall iPGAi}}{\forall iPGAi}}{\text{i.e.}\ \forall i\forall gQ(i/Q\equiv T\forall jQj)}}{\forall gQ(n/Q\equiv T\forall jQj)}$$

We continue now as follows:

$$\dfrac{\dfrac{En\vee On}{\dfrac{n/E\vee O}{n/E\vee n/O}} \quad \dfrac{\overset{(1)}{n/E} \quad \dfrac{\dfrac{\dfrac{\dfrac{gE \quad \overset{P}{\forall gQ(n/Q\equiv T\forall jQj)}}{n/E\equiv T\forall jEj}}{T\forall jEj}}{\forall jEj}}{El}}{\Lambda} \quad \overset{(1)}{n/O} \quad gO}{\Lambda}$$

Our proof of Λ is in normal form, by co semantical paradox, as mentioned ab strongly to suspect the principle of gene rather than the fact of semantic closure absurdity. The full list of assumptions on w proof above is

PGA(a), gPGA, gE, gO, $En \vee On$

Of these, only PGA(a) could be disputed clear from the way he introduced the tinction that he would dispute it. But the would do so have not been made clear and j of the reductio provided by our proof.

Another would-be reductio is the follo

f inference for the truth (or satisfaction) th inferential moves licenced by the facts ed by semantic closure. Elsewhere[1] I have absurdity associated with the paradoxes. are the feature of not being normalizable. sequence of a paradoxical proof does not ly many steps. Instead it enters a loop pends on the logical structure of the (s) in question. In the light of this, it seems ture that the test of looping reduction an enumeration of proofs in the seman- ige, would yield an axiomatization of

ese remarks on paradox is this. If we take his principle in the formal mode, *but allow tically closed*, absurdity results from a proof he one he gave for the case of the material ic closure short-circuits the distinction hat follows I use these abbreviations:

generic
isfies φ
true
(x/Q≡T∀iQi)
even
odd

le of generic attribution for the case where trary object. In this case we can simply put hand side of the biconditional, instead of isfaction by each individual in the range of concerned. For an arbitrary object a, c. Let us further assume that we are natural numbers. n will range over genuine As an axiom schema we have En∨On. In t criteria allowing us to determine other- at PGA is a generic property. We have the ∀gQ(n/Q≡T∀jQj) from assumptions

$$\dfrac{\dfrac{\text{PGA}(a)}{a/\text{PGA}} \qquad \dfrac{\text{gPGA} \quad \text{i.e.} \quad \dfrac{\text{PGA}(a)}{\forall gQ(a/Q\equiv T\forall iQi)}}{a/\text{PGA}\equiv T\forall i\text{PGA}i}}{\dfrac{T\forall i\text{PGA}i}{\dfrac{\forall\, i\text{PGA}i}{\text{i.e.}\quad \dfrac{\forall i\forall gQ(i/Q\equiv T\forall jQj)}{\forall gQ(n/Q\equiv T\forall jQj)}}}}$$

We continue now as follows:

$$\dfrac{\dfrac{\dfrac{\text{En}\vee\text{On}}{n/\text{E}\vee\text{O}}}{n/\text{E}\vee n/\text{O}} \qquad \dfrac{\dfrac{\overline{n/\text{E}}^{(1)} \qquad \dfrac{\text{gE} \quad \overset{\text{P}}{\forall gQ(n/Q\equiv T\forall jQj)}}{n/\text{E}\equiv T\forall j\text{E}j}}{\dfrac{T\forall j\text{E}j}{\dfrac{\forall j\text{E}j}{\dfrac{\text{E}1}{\Lambda}}}}}{} \qquad \dfrac{\overline{n/\text{O}}^{(1)} \qquad \dfrac{\text{gO} \quad \overset{\text{P}}{\forall gQ(n/Q\equiv T\forall jQj)}}{n/\text{O}\equiv T\forall j\text{O}j}}{\dfrac{T\forall j\text{O}j}{\dfrac{\forall j\text{O}j}{\dfrac{\text{O}2}{\Lambda}}}}}{\Lambda}\,(1)$$

Our proof of Λ is in normal form, by contrast with proofs of semantical paradox, as mentioned above. This leads me strongly to suspect the principle of generic attribution itself, rather than the fact of semantic closure, to be the source of absurdity. The full list of assumptions on which Λ depends in the proof above is

PGA (a), gPGA, gE, gO, En∨On

Of these, only PGA (a) could be disputed by Fine. Indeed it is clear from the way he introduced the generic/classical distinction that he would dispute it. But the grounds on which he would do so have not been made clear and justified independently of the reductio provided by our proof.

Another would-be reductio is the following:

$$\dfrac{\dfrac{\dfrac{\dfrac{\forall j(Ej\vee Oj)}{T\forall j(Ej\vee Oj)} \qquad \dfrac{g(E\vee O) \quad \forall gQ(a/Q\equiv T\forall jQj)}{a/E\vee O\equiv T\forall j(Ej\vee Oj)}}{a/E\vee O}\,(*)}{a/E\vee a/O} \qquad \overset{\text{PGA(a):}}{\dfrac{\dfrac{\dfrac{\dfrac{\overset{(1)}{\overline{a/E}} \quad \dfrac{gE \quad \forall gQ(a/Q\equiv T\forall jEj)}{a/E\equiv T\forall jEj}}{T\forall jEj}}{\forall jEj}}{El}}{\Lambda}} \qquad \begin{array}{c}\text{similarly}\\ \text{for O in}\\ \text{place of E}\\ \vdots\\ \Lambda\end{array}}{}\,(1)$$

The suspect move here is of course (*). From the fact that an arbitrary object satisfies a disjunction, it does not follow that it satisfies one or other of the disjuncts. Fine notes as much, observing that the semantical rule for evaluating disjunctions fails for statements about arbitrary objects. But I think the point comes out more vividly in a proof theoretic context. The last proof shows just how abruptly ordinary reasoning about objects can be stopped dead in its tracks by the indeterminacy of the arbitrary. Far from being a theory about arbitrary *objects*, it is rather a theory of arbitrary obstacles. We lose the distributive laws for satisfaction by objects across logical operators; thus arguably being deprived of a notion of objecthood at all.

We have seen from the proofs above that if the language is semantically closed then the principle of generic attribution (in the formal mode) leads to absurdity unless one denies both that the principle itself expresses a generic property and that arbitrary objects behave like individuals with respect to satisfaction of predicates. Let us concur with the latter denial for the time being, thereby refusing to admit the last 'proof' of absurdity. I want now to make a constructive suggestion which might enable Fine to avoid having to specify what generic properties are, and indeed enable him to formulate the principle of attribution without restriction to generic properties. It appears that merely indexing levels is all that is needed to avoid the first proof of absurdity given above. Let us assign predicates to levels in the obvious way. Let PA_n be the generically unrestricted but type restricted principle

$$\forall Q_{n-1}\,(a/_nQ\equiv T_n\forall iQ_{n-1}i)$$

Howsoever we now try to write down a version of P observing type restrictions we fail. The type restrictions can be gram-

matical or inferential. That is, we can ei
typed formulae as ill-formed, or incorrec
fallacious. To illustrate the effect of either
us try to write down a version of P start

$$\text{(II)}\,\dfrac{PA_1(a)}{a/_1PA_1} \qquad \text{i.e.}\ \overline{\forall Q_0 \ldots}$$

$$a/ \ldots$$

$$\dfrac{T_1\forall iPA_1i}{\forall iPA_1i} \quad \text{(II}$$

$$\vdots$$

etc.

Type restrictions are violated repeated
generalization over zero level predicat
predicate. (II) does not raise the leve
predicate from 1 to 2 as it should, giv
argument a predicate of level 1. (III) h
formula already of level 1. We thus thorou
of passage to Λ by this route; and this witl
the notion of generic properties and with
the principle of attribution to generic pr

§3. On a Carnapian distinction[2] between
questions about existence, one might d
there are arbitrary objects. This Fine doe
about the applications of his theory make
professed willingness to go along with a pro
that would put his new found crystals into
way to a positive answer to the external que
was, after all, the utility of applicatio
framework' involved. Yet Fine describes
negatively what he calls the 'ontologically
about existence. He likens himself to the n
the ultimate necessity of number talk for o
But if Fine wishes to deny the ultimate n
arbitrary objects for our scientific purposes

$$\text{PGA}(a):$$

$$\dfrac{\dfrac{\ }{a/E}(1) \qquad \dfrac{gE\ \forall gQ(a/Q \equiv T\forall jEj)}{a/E \equiv T\forall jEj}}{\dfrac{\dfrac{T\forall jEj}{\forall jEj}}{\dfrac{El}{\Lambda}}} \qquad \begin{array}{c}\text{similarly}\\ \text{for O in}\\ \text{place of E}\\ \vdots \\ \Lambda\end{array}$$

$$\dfrac{\dfrac{\ldots T\forall jQj)}{\ldots Ej \vee Oj)}}{} \qquad \text{———————————————— (1)}$$

e is of course (*). From the fact that an
ies a disjunction, it does not follow that
er of the disjuncts. Fine notes as much,
nantical rule for evaluating disjunctions
ut arbitrary objects. But I think the point
ly in a proof theoretic context. The last
bruptly ordinary reasoning about objects
in its tracks by the indeterminacy of the
eing a theory about arbitrary *objects*, it is
itrary obstacles. We lose the distributive
by objects across logical operators; thus
ved of a notion of objecthood at all.
the proofs above that if the language is
en the principle of generic attribution (in
s to absurdity unless one denies both that
esses a generic property and that arbitrary
ndividuals with respect to satisfaction of
ncur with the latter denial for the time
g to admit the last 'proof' of absurdity. I
nstructive suggestion which might enable
o specify what generic properties are, and
o formulate the principle of attribution
generic properties. It appears that merely
that is needed to avoid the first proof of
e. Let us assign predicates to levels in the
be the generically unrestricted but type

$$\ldots {}_n\forall iQ_{n-1}i)$$

y to write down a version of P observing
ail. The type restrictions can be gram-

matical or inferential. That is, we can either count incorrectly typed formulae as ill-formed, or incorrectly typed inferences as fallacious. To illustrate the effect of either kind of restriction, let us try to write down a version of P starting with $PA_1(a)$:

$$\dfrac{\text{(II)}\ \dfrac{PA_1(a)}{a/_1PA_1} \qquad \text{i.e.}\ \dfrac{\dfrac{PA_1(a)}{\forall Q_0(a/_1Q_0 \equiv T_1\forall iQ_0 i)}}{a/_1PA_1 \equiv T_1\forall iPA_1 i}\ \text{(I)}}{\dfrac{T_1\forall iPA_1 i \quad \text{(III)}}{\forall iPA_1 i}}$$

$$\vdots$$

$$\text{etc.}$$

Type restrictions are violated repeatedly. (I) instantiates a generalization over zero level predicates with a first level predicate. (II) does not raise the level of the satisfaction predicate from 1 to 2 as it should, given that it has as an argument a predicate of level 1. (III) has T_1 applying to a formula already of level 1. We thus thoroughly forfeit our rights of passage to Λ by this route; and this without having to invoke the notion of generic properties and without having to restrict the principle of attribution to generic properties.

§3. On a Carnapian distinction[2] between external and internal questions about existence, one might deny (externally) that there are arbitrary objects. This Fine does. But his *acharnement* about the applications of his theory makes one suspicious of his professed willingness to go along with a programme of reduction that would put his new found crystals into logical solution. The way to a positive answer to the external question about existence was, after all, the utility of applications of the 'linguistic framework' involved. Yet Fine describes himself as answering negatively what he calls the 'ontologically significant' question about existence. He likens himself to the nominalist who denies the ultimate necessity of number talk for our scientific purposes. But if Fine wishes to deny the ultimate necessity of talk about arbitrary objects for our scientific purposes (namely, achieving a better understanding of truth conditions and valid reasoning in ordinary and mathematical discourse), we can legitimately ask what the point is of developing the theory in the first place. If we already have our reduction in the orthodox view, why go inventing theories about new categories of ill-behaved objects that are to be reduced to it? And if the reduction is to go a different way, why not aim for its terminus straightaway?

What is Fine's position *qua* semantical theorist with regard to the internal question about existence? He calls the question 'ontologically neutral' and answers it affirmatively. He likens himself to the nominalist philosopher of mathematics who is convinced that number talk is dispensable, but useful, and who indulges himself in first order arithmetical assertions. The modern theorist will live with his objects, but not really commit himself to them.

I shall not raise here problems that might beset Fine should he wish to use set theory to do the arbitrary object model theory required to treat the set theorist's own sayings of the form 'Let x be an arbitrary set . . .'. Instead I wish to draw attention to a position not yet considered as far as ontological issues are concerned. Is the native speaker (as opposed to the semantic theorist studying his utterances about some subject matter) in any way committed to the existence of arbitrary objects? I would say not. Assume the native speaks an ordinary first order language with no branching quantifiers. If we are concerned to interpret his utterances by elucidating their truth conditions then it seems we have no way of committing *him* to arbitrary objects that we might invoke to work out what follows from his general claims about the subject matter in question. It is only if the native reasons by means of certain locutions that can be understood in no other way than as referring to arbitrary objects that it becomes plausible to regard him as 'internally' committed to their existence.

Suppose now that I am a first order theorist whose surface linearizations of natural deductions in tree form judiciously eschew all apparent reference to arbitrary objects. For example, instead of saying

> Let a be an arbitrary object . . . Then Fa. So, since a was arbitrary, for all x Fx

I say rather

> Take a . . . Then Fa. But I assumed
> for all x Fx

Each of these manners of speaking adeq
inferential move of ∀-introduction

$$\begin{array}{c} \vdots \\ \underline{\;Fa\;} \\ \forall xFx \end{array}$$

where a does not occur i
which Fa depends

The latter gloss is deep and robust. The f
bloating. If I, in the position of the native,
robust renderings of the moves in my lang
wrong to claim

> I have as much reason to affirm th
> objects in this [internal] sense as t
> affirm that there are numbers.

Fine says (p. 57) that

> the question 'what are they?' may be
> non-philosophical way, as a request
> what objects one is talking about.

And he goes on immediately to to say or

> than refer to the kind of role that
> intended to play.

We may ask here whether he is talking a
arbitrary objects or by parts of language fo
those objects are (perhaps misguidedly)
sider set theory. In his more philosophi
acterizes arbitrary objects as abstractions li
Let us not pause to ask how, in his more p
would characterize arbitrary sets. Let u
context of set theory, whose *intentions* abou
relevant here. Presumably not those of th
intentions might laudably be to describ
uncluttered by arbitrary members, sets t

of truth conditions and valid reasoning in atical discourse), we can legitimately ask veloping the theory in the first place. If we duction in the orthodox view, why go out new categories of ill-behaved objects d to it? And if the reduction is to go a t aim for its terminus straightaway?

ion *qua* semantical theorist with regard to about existence? He calls the question ' and answers it affirmatively. He likens alist philosopher of mathematics who is er talk is dispensable, but useful, and who first order arithmetical assertions. The ve with his objects, but not really commit

problems that might beset Fine should he to do the arbitrary object model theory et theorist's own sayings of the form 'Let x .'. Instead I wish to draw attention to a sidered as far as ontological issues are ive speaker (as opposed to the semantic utterances about some subject matter) in the existence of arbitrary objects? I would native speaks an ordinary first order nching quantifiers. If we are concerned to ces by elucidating their truth conditions e no way of committing *him* to arbitrary invoke to work out what follows from his the subject matter in question. It is only if means of certain locutions that can be r way than as referring to arbitrary objects ble to regard him as 'internally' committed

I am a first order theorist whose surface ural deductions in tree form judiciously eference to arbitrary objects. For example,

trary object . . . Then Fa. So, since a was l x Fx

I say rather

> Take a . . . Then Fa. But I assumed nothing about a. So, for all x Fx

Each of these manners of speaking adequately represents the inferential move of $\forall$-introduction

$$\frac{\begin{matrix}\cdot\\ \cdot\\ \cdot\\ Fa\end{matrix}}{\forall x Fx}\quad \text{where a does not occur in any assumption on which Fa depends}$$

The latter gloss is deep and robust. The former is ontologically bloating. If I, in the position of the native, persevere with deeply robust renderings of the moves in my language game it is simply wrong to claim

> I have as much reason to affirm that there are arbitrary objects in this [internal] sense as the nominalist has to affirm that there are numbers.

Fine says (p. 57) that

> the question 'what are they?' may be taken, in an ordinary, non-philosophical way, as a request for an explanation of what objects one is talking about.

And he goes on immediately to to say one can do no better

> than refer to the kind of role that arbitrary objects are intended to play.

We may ask here whether he is talking about a role played by arbitrary objects or by parts of language for whose interpretation those objects are (perhaps misguidedly) being invoked. Consider set theory. In his more philosophical mood Fine characterizes arbitrary objects as abstractions like sets or propositions. Let us not pause to ask how, in his more philosophical mood, he would characterize arbitrary sets. Let us instead ask, in the context of set theory, whose *intentions* about roles to be played are relevant here. Presumably not those of the set theorist. For his intentions might laudably be to describe the universe of sets uncluttered by arbitrary members, sets that do not depend for their existence on, or derive their character from any role he intends them to play. So the intentions must be those of the semantic theorist.

Now *is* it satisfactory to answer the ordinary non-philosophical questioner who asks 'what are arbitrary objects?' by telling him what role one intends for them as a semantic theorist? Would one be satisfied, upon asking 'what are spirits?' if one's Azande informant told one what role he intended spirits to play in his theory of bad weather and juvenile delinquency? Surely our retort would be that the description of intention is not enough, and that there are further independent methodological constraints to be placed on the theory before a satisfactory answer might be forthcoming. Thus I am interested not so much in the role Fine intends arbitrary objects to play as in the genuinely explanatory role (if any) that they have to be allowed to play if we are to account successfully for the logico-linguistic intentions of native speakers. These intentions are to model and describe reality, and this sometimes in highly schematic fashion, as when they reason about it at first order.

I say 'schematic' for good reason. The orthodox construal of Fine's phrase (p. 65) 'names for arbitrary objects' is 'placeholders for names of actual individuals'. We can extend the discussion above of the rule of $\forall$-introduction to make this clear. The subproof is schematic in a in the following sense. Any term t may be substituted for appropriate occurrences of a in the subproof so as to yield a proof of Ft from the original assumptions. (Similar considerations apply to the subproof in the rule of $\exists$ -elimination.)[3] Thus instead of taking the parameter a as a name for an arbitrary object we may consider it as a placeholder for names of actual individuals. Talk 'on the surface' of arbitrary objects a when presenting the proof may thus be construed as remarks about the logically hygienic pattern of occurrences of the parameter a within the proof, given or planned. We thus have a wholly syntactic option for systematically understanding the behaviour of 'names for arbitrary objects'. At the other extreme, arbitrary objects themselves, if admitted, might be better assimilated to the domain of cognitive psychology. The original discussions of the 'general triangle' were highly psychologistic.[4] One might regard arbitrary objects as incomplete mental representations—not so much objects *of* thought as objects *within* thought, by

means of which we reason about the wo

We may even be able to merge the two
best cognitive account will be one that tr
tations as clusters of predicates, or 'p
relational scheme. Fine himself expects

> that to each set of individuals fro
> arbitrary object with that set as its

thereby in effect equating arbitrary object
in all applications of which he has given u
appear that only *definable* subsets matte
interpret his arbitrary objects syntactic
conditions on their ranges?[5] Is not Fi
orthodox a post-Fregean in devising a se
theory of arbitrary objects, without offerin
objects as models of cognitive processes
psychological—or indeed any kind of—r

NOTES

[1] cf. 'Proof and Paradox', *Dialectica* 36 (1982) 265-296

[2] Cf. Carnap, 'Empiricism, semantics and ontology', *Re*
(1950) 20-40.

[3] For more detail see my *Natural Logic* (Edinburgh, 19

[4] For references to an extensive literature see Beth,
Dreieck"' *Kantstudien* 48 (1956/57) 361-380.

[5] One is reminded here of Hilbert's ε-terms and Hailpe
Leisenring, *Mathematical Logic and Hilbert's ε-Symbol* (Ma
Hailperin, 'A theory of restricted quantification, I and I
(1957) 19-35 and 113-129.

derive their character from any role he
So the intentions must be those of the

to answer the ordinary non-philosophical
vhat are arbitrary objects?' by telling him
for them as a semantic theorist? Would
asking 'what are spirits?' if one's Azande
hat role he intended spirits to play in his
r and juvenile delinquency? Surely our
he description of intention is not enough,
rther independent methodological con-
n the theory before a satisfactory answer
. Thus I am interested not so much in the
trary objects to play as in the genuinely
y) that they have to be allowed to play if
essfully for the logico-linguistic intentions
ese intentions are to model and describe
imes in highly schematic fashion, as when
t first order.
r good reason. The orthodox construal of
ames for arbitrary objects' is 'placeholders
dividuals'. We can extend the discussion
$\forall$-introduction to make this clear. The
n a in the following sense. Any term t may
opriate occurrences of a in the subproof so
from the original assumptions. (Similar con-
e subproof in the rule of $\exists$ -elimination.)[3]
ng the parameter a as a name for an
ay consider it as a placeholder for names of
lk 'on the surface' of arbitrary objects a
oof may thus be construed as remarks about
pattern of occurrences of the parameter a
en or planned. We thus have a wholly
stematically understanding the behaviour
y objects'. At the other extreme, arbitrary
admitted, might be better assimilated to
ve psychology. The original discussions of
were highly psychologistic.[4] One might
ts as incomplete mental representations—
of thought as objects *within* thought, by

means of which we reason about the world of actual objects.

We may even be able to merge the two accounts. Perhaps the best cognitive account will be one that treats mental representations as clusters of predicates, or 'pigeonholes' within a relational scheme. Fine himself expects

> that to each set of individuals from *I* there will be an arbitrary object with that set as its range

thereby in effect equating arbitrary objects with subsets of *I*. But in all applications of which he has given us any inkling it would appear that only *definable* subsets matter. So can we not reinterpret his arbitrary objects syntactically as the defining conditions on their ranges?[5] Is not Fine himself being too orthodox a post-Fregean in devising a semantic or referential theory of arbitrary objects, without offering schemes of arbitrary objects as models of cognitive processes with some smack of psychological—or indeed any kind of—reality?

NOTES

[1] cf. 'Proof and Paradox', *Dialectica* 36 (1982) 265-296

[2] Cf. Carnap, 'Empiricism, semantics and ontology', *Revue internationale de philosophie* 4 (1950) 20-40.

[3] For more detail see my *Natural Logic* (Edinburgh, 1978) pp. 42-3, 46, 65-9.

[4] For references to an extensive literature see Beth, 'Über Lockes "Allgemeines Dreieck"' *Kantstudien* 48 (1956/57) 361-380.

[5] One is reminded here of Hilbert's ε-terms and Hailperin's restricted variables. See Leisenring, *Mathematical Logic and Hilbert's ε-Symbol* (MacDonald, London, 1969) and Hailperin, 'A theory of restricted quantification, I and II', *Journal of Symbolic Logic* 22 (1957) 19-35 and 113-129.