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PROPERTY THEORY: THE TYPE-FREE APPROACH *v.* THE CHURCH APPROACH

In a lengthy review article C. Anthony Anderson (1987) criticizes the approach to property theory developed in my book *Quality and Concept* (1982). That approach is first-order, type-free, and broadly Russellian (see also my 1979, 1983). In the course of his article, Anderson tries to defend Alonzo Church's higher-order, type-theoretic, broadly Fregean approach (Church, 1951, 1973, 1974; Anderson 1980, 1984). Like Church, Anderson accepts the two basic tenets of the book: (i) intensions are real irreducible entities, and (ii) the theory of intensional entities is part of logic, namely, intensional logic. The arguments given in the book for these tenets are logico-linguistic: 'that'-clauses are singular terms semantically correlated with propositions; gerundive and infinitive phrases are singular terms semantically correlated with properties and relations; and intensionality in logic can be traced to these logically distinctive singular terms. Anderson finds that these arguments are "extremely compelling." His worries all concern the way in which the theory of intensional entities (properties, relations, and propositions) is developed. Most of these worries were, at the time the book was written, well-known to those of us working on intensional logic. In this paper I hope to show that the worries can be handled within the approach developed in the book but, ironically, they remain serious obstacles for the Church approach. It will not be feasible to discuss all of Anderson's claims. I will confine myself to his main points: (1) fine-grained and coarse-grained conceptions of intensional entities, (2) proper names and definite descriptions, (3) the paradox of analysis and Mates' puzzle, and (4) the logical, semantical, and intentional paradoxes.

One reason for returning to this debate is that a number of other researchers – for example, Peter Aczel (1980, 1989), Kit Fine (1980), Terence Parsons (1980), Uwe Mönnich (1983), Edward Zalta (1983, 1988), Jon Barwise and John Perry (1983), Christopher Menzel (1986), Nathan Salmon (1986), Raymond Turner (1987, 1989), Genarro

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Chierchia and Raymond Turner (1988), Michael Jubien (1989), Scott Soames (1989), Michael Dunn (1990) – have developed approaches of the same general type as that which is advocated in the book. It will be good to set the record straight on the status of the conflict between these two general approaches to intensional logic: the first-order, type-free, broadly Russellian approach and the higher-order, type-theoretic, broadly Fregean approach. The former, I believe, is far more promising even when one overlooks the notorious complexities inherent in the latter.

1. FINE-GRAINED AND COARSE-GRAINED INTENSIONS

In the history of logic and philosophy there have been several conceptions of the identity conditions for intensional entities. These conceptions range from coarse-grained to highly fine-grained. The algebraic semantic method developed in the book was designed to provide a *unified* technique for characterizing these various traditional conceptions of intensional entities. The idea was to define a general notion of an algebraic model structure so that, by imposing relevant auxiliary restrictions, one could capture and then compare the various alternative conceptions. A unified semantic technique is not available on the Church approach; Church uses instead an eclectic collection of semantic techniques.

A general desideratum is that the competing conceptions should be characterized ontologically rather than linguistically. On the realist view adopted in the book – and by Church – intensional entities do not depend for their existence on language. For much the same reasons, the most basic facts about intensional entities – for example, their identity conditions – should not depend on language. Therefore, one should not use a criterion of linguistic synonymy (e.g., Church's "synonymous isomorphism" criterion for linguistic synonymy; Church, 1954) in one's ultimate statement of the identity conditions of fine-grained intensional entities. But this is what Church and Anderson do. Surely realists ought to have a language-independent way to state the identity conditions of these intensional entities. An aim of the book was to provide one.

The background view adopted in the book is that intensions are either logically simple or logically complex. Logically complex intensions are those that arise by applying a certain type of fundamental logical operation (e.g., conjunction, negation, existential generalization, singular predication, etc.) to other intensions. For example, the complex intension of being not not red arises from applying an operation of negation to the result of applying negation to the property red; that is, being not not red = neg(neg(red)). Logically complex intensions are those having logical forms. Logically simple intensions (e.g., perfectly "natural" properties and relations) are those that are not values of any of these fundamental logical operations; they have no logical form. On this view, logically complex intensions are built up ultimately from logically simple intensions (plus perhaps subjects of singular predications) by means of these fundamental logical operations. The identity criterion offered in the book is this: logically simple intensions are identical if they are necessarily equivalent; logically complex intensions are identical if they have the same complete analysis trees,[1] that is, the trees that are determined by the inverses of these fundamental logical operations (conjunction, negation, etc.) have the same structure and the logically simple intensions that occupy their respective terminal nodes are identical. This criterion, or something quite like it, is held by a large number of leading figures in the history of philosophy. (It is also accepted as a helpful heuristic by a number of the type-free theorists listed above.) An advantage of the algebraic framework is that it makes it very easy to give a precise formal statement of this criterion. A consequence of this criterion is this: each intension, when it is analyzed completely, has a unique, non-circular logical analysis, where logical analyses are identified with the aforementioned complete analysis trees. (In the limiting case, a logically simple intension may be identified with its own one-node analysis tree.) This condition places an extremely strong constraint on the identity of intensions. Early in the book (chapter 1), before the full theory is developed (chapter 8), this condition is used to mark off the fine-grained conception of intensional entities.

The Church approach does not provide a language-independent criterion for the identity of fine-grained intensions. If the above algebraic approach, which does provide such a criterion, is otherwise as acceptable as the Church approach, the algebraic approach would then be preferable. So in critical comparison of the two approaches, it would be important for a defender of the Church approach to find fault with the criterion provided by the algebraic approach. Anderson attempts to do this. His main criticism[2] is the following:

Apparently we are free to simply stipulate two definitions, say

Fx iff$_{df}$ xRb

and

Gx iff$_{df}$ aRx

where 'a' and 'b' denote different things. Now consider the proposition that aRb in Bealer's notation: $[aRb]$. Then we have, by definition, that $[aRb] = [Fa] = [Gb]$. The constituents of $[aRb]$ would seem to be, in order: a, the relation $[xy : xRy]$, and b. But the ordered constituents of $[Fa]$ and $[Gb]$ appear as $[x : Fx]$, a and $[x : Gx]$, b, respectively. We have only one proposition but three distinct analyses and hence three distinct analysis trees (p. 120).

Anderson's conclusion is that, since the proposition $[aRb]$ allegedly has three distinct analysis trees, the aforementioned condition (i.e., that each intension has a unique logical analysis) must be in error. (Later on Anderson pursues the same point for four additional pages, 132–135).

This argument is fallacious. It is an enthymeme with the following suppressed assumption:

$$\text{If } [x : Fx] = [x : xRb] \text{ and } [x : Gx] = [x : aRx], \text{ then}$$
$$[aRb] = [Fa] = [Gb].$$

But this assumption is false.[3] Consider the following intuitive counter-example (taken from my "On the Identification of Properties and Propositional Functions," 1989b). Suppose that, by definition, to fondalee is to be someone whom Jane Fonda follows. In symbols:

$$[x : fx] = [x : jFx].$$

And suppose that, by definition, to rajneesh is to be someone who follows Rajneesh. In symbols:

$$[x : Rx] = [x : xFr].$$

Consider someone to whom we have just stated the second definition but not the first. Suppose that this person is consciously and explicitly thinking that Jane Fonda rajneeshes. Must the person be consciously and explicitly thinking that Rajneesh fondalees? It certainly does not seem so. Indeed, the person might not even have the concept of fondaleeing;

in any case, the person certainly need not be consciously and explicitly employing this concept predicatively (as a property of Rajneesh). Anderson's suppressed assumption is simply false.

The following is a variation on the same problem. By definition, being even = being divisible by 2, and being self-divisible = being divisible by itself.[4] In symbols:

$$[x : Ex] = [x : Dx, 2]$$

and

$$[x : Sx] = [x : Dx, x].$$

An analogue of Anderson's assumption would imply, however, that the proposition that 2 is even = the proposition that 2 is divisible by 2 = the proposition that 2 is self-divisible. In symbols:

$$[E2] = [D2, 2] = [S2].$$

But this is plainly wrong. A person can be consciously and explicitly thinking that 2 is even and not be consciously and explicitly thinking that 2 is self-divisible. Although Anderson's assumption does not strictly imply this erroneous outcome, it is hard to see any natural way in which he could avoid it while holding onto his assumption. The conclusion is that Anderson's assumption is false and, therefore, that his criticism fails.

A satisfactory intensional logic should be equipped to explain how the above sorts of fine-grained intensional distinctions arise. The algebraic picture developed in the book was designed to provide just such an explanation: the indicated sorts of intensions are distinct because they have distinct analysis trees. Rather than being a fault, as Anderson alleges, the fact that analysis trees are so finely distinguished provides an explanation of intuitive intensional distinctions that Church and Anderson must suppress. In a recent paper "General and Hyper-fine-grained Intensional Logic," I have shown how to use the algebraic picture for an even more fine-grained intensional logic that explains, not only intensional distinctions like those we have just seen, but also those that arise in connection with Mates' puzzle. See section 3 below. We shall see there that, not only are these intensional distinctions intuitive, they have an unexpected theoretical utility as well.

(No doubt what led Anderson to make his erroneous assumption was his identification of properties with propositional functions. The above examples provide reason to doubt such identification, but this is not the place to pursue this point. For an extended discussion, see Bealer, 1989a and 1989b. On the view defended in those papers, properties are quite a different sort of thing from propositional functions. If this is right, then given that λ-abstracts $\ulcorner(\lambda v)A\urcorner$ denote propositional functions, λ-abstracts do not denote properties. Certain philosophers have made a practice of using λ-abstracts as a notation for properties, but this invites unnecessary confusion. Another notation is called for. I advocate using $\ulcorner[v_1 \ldots v_n : A]\urcorner$ where $n \geqslant 0$. Thus, whereas $\ulcorner\{v_1 : A\}\urcorner$ denotes the set of things v_1 such that A, $\ulcorner[v_1 : A]\urcorner$ denotes the property of being a v_1 such that A. Whereas $\ulcorner\{v_1 \ldots v_n : A\}\urcorner$ $(n \geqslant 2)$ denotes the relation-in-extension holding among $v_1, \ldots, v_n$ such that A, $\ulcorner[v_1 \ldots v_n : A]\urcorner$ denotes the relation-in-intension holding among $v_1, \ldots, v_n$ such that A. In the limiting case where $n = 0$, $\ulcorner[A]\urcorner$ denotes the proposition that A.)

So far I have been discussing fine-grained intensions. I turn now to the other historically important conception of identity conditions for intensional entities – the coarse-grained conception. According to this conception, intensions are identical if they are necessarily equivalent. I call this Conception 1. Anderson tells us that once an adequate fine-grained theory "is available, all justification for a *separate* theory of Conception 1 entities seems to disappear" (p. 123). Anderson seems to overlook the two types of justification that are discussed at length in the book. First, in metaphysics, epistemology, philosophy of science, and aesthetics there are compelling theoretical arguments for the existence of a special class of intensions that, relative to one another, are coarse-grained.[5] For example, properties and relations that are "perfectly natural." In the book such properties and relations are called *qualities* and *connections*, respectively. Besides the lengthy discussion of these matters in chapter eight in the book, see, for example: Hilary Putnam (1970), Fred Dretske (1977), Michael Tooley (1977, 1987), David Armstrong (1978, 1983), Sidney Shoemaker (1980), Chris Swoyer (1982), David Lewis (1983), Michael Dunn (1990). In recent years this has become the majority position. Rather than reviewing the now familiar arguments for this position, I will use the occasion of the present paper to give a new argument (pp. 16–18 below) to the effect that this position is needed in a fully satisfactory resolution of the paradox of analysis. Second, there is some direct intuitive support for the existence of coarse-grained intensions. Consider the aphorism, "Although the glass's being half full is the same as the glass's being half empty, there are two ways of looking at it." That is, although there is just one situation there in the world (i.e., the glass's being half full = the glass's being half empty), there are two associated thoughts (i.e., the proposition that the glass is half full and the proposition that the glass is half empty). Evidently, there is a reading on which gerundive constructions such as 'the glass's being half full' and 'the glass's being half empty' denote situations in the world, where the latter are coarse-grained intensional entities. (Someone might wonder whether these coarse-grained entities are intensional entities. To see that they are, notice that the indicated gerundive phrases contain intensional occurrences. For example, given that my gas tank is half full, all and only things that are half full are as full, proportionally, as my gas tank. But, intuitively, the glass's being half full is not the same situation in the world as the glass's being as full, proportionally, as my gas tank. After all, the latter situation might continue to obtain even as the glass and my gas tank are simultaneously being drained; the former situation would not in that case continue to obtain.)

Anderson also considers the question of how one ought to represent coarse-grained intensions semantically if one countenances them. He holds that in the present theoretical setting algebraic semantics would have nothing to offer and that "one might better use 'possible worlds' semantics" (p. 131). This proposal does not take into account two facts about the larger theoretical context. First, given that no one can with any certainty rule out the prospect that a *spectrum* of conceptions of intensional entities is warranted, there is good reason to have a *unified* semantic method in which the full spectrum of candidate conceptions can be represented and compared with one another. The algebraic semantic method does this; the possible-worlds method does not. (Nor do Church's methods.)

Second, there are well-known arguments that intensional logic ought to be *type-free* in a variety of senses. (See Bealer, 1989a, for a catalogue of the various senses of type-freedom and for arguments on behalf of them; other arguments can be found in the papers collected in Martin, 1984, and Chierchia *et al.*, 1989). Or, more cautiously, given that it is at least plausible that a type-free intensional logic is best, it would be unwise to wed oneself to a semantic method that is known not to be type-free. Now the algebraic semantic method permits one's intensional logic to be type-free in each of these senses; the possible-worlds semantic method does not. (See Bealer, 1989a, for detailed discussion of this matter. Church's intensional logic is not type-free in any of these senses.) To illustrate how the possible-worlds method is not type-free, consider the following intuitively valid sentence 'Identity is identical to identity'. In symbols: $[xy: x = y] = [xy : x = y]$. (The intuitively valid proposition that identity is identical to identity is not even expressible in Church's type-theoretic language.) This sentence comes out as logically valid in the algebraic semantics. Not so in a possible-worlds semantics. The reason is that a type-theory is already implicit in possible-worlds semantics. To see this, recall that, according to possible-worlds semantics, a relation is a function (i.e., a set of ordered pairs) from possible worlds to sets of ordered sets of items that are related to each other by that relation in the relevant possible world. The identity relation is thus a set of ordered pairs whose first elements are possible worlds and whose second elements are sets of ordered pairs $\langle u, v \rangle$ such that u is identical to v in the relevant possible world. Consider the case of the actual world. If 'Identity is identical to identity' is actually true, one of those ordered pairs $\langle u, v \rangle$ would have to be $\langle$identity, identity$\rangle$. Therefore, according to possible-worlds semantics, if 'Identity is identical to identity' is actually true, identity would be a set that belongs to its own transitive closure; that is, identity would be a set s such that $s \in \ldots \in s$. But there are no such sets according to the standard set theory in which possible-worlds semantics is constructed. (Type theorists like Church and Anderson certainly do not allow such sets.) Hence, the sentence 'Identity is identical to identity' does not come out as actually true in the standard possible-worlds semantics, and therefore, it does not come out as logically valid.

Anderson's proposal fails both in connection with the need for a unified semantic method and in connection with the need for an intensional logic that is type-free. Evidently, something on the lines of the approach in the book is required to meet these theoretical needs.

The overall conclusion so far is that the approach taken in the book provides a unified method for representing the main traditional conceptions of identity conditions for intensional entities and, despite his critical posture, the approaches favored by Anderson do not.

2. PROPER NAMES AND DEFINITE DESCRIPTIONS

The larger theoretical strategy of the book was to devise an approach that could accommodate a wide spectrum of competing philosophical and linguistic theories. The semantical representation of competing theories of the identity conditions for intensional entities was a case in point. Theories of proper names and theories of definite descriptions are two more examples.

Proper Names. Chapter two "Intensional Logic" begins with the construction of a formal language like the one described a moment ago. This language (called L_ω) has an intensional abstraction operation: if A is a formula, $\ulcorner[v_1 \ldots v_n : A]\urcorner$ is a singular term (for any $n \geqslant 0$). Intensional abstracts may be embedded within intensional abstracts any finite number of times, and they may contain externally quantifiable variables. At this stage of the book, neither names nor definite descriptions were adjoined to L_ω. The point was to provide a base from which a wide spectrum of competing theories could be accommodated. I stated:

> L_ω contains no primitive names. My strategy with regard to primitive names will be to proceed in two stages. First, I will study the logic of intensional language without names; that is, I will study the logic of L_ω as it stands. Once this task is completed, I will take up the question of how to treat names. There are two main competing theories of names – Frege's theory and Mill's theory. According to Frege's theory, names have descriptive content; according to Mill's theory, they do not. In sections 38–9 it is shown that, given either theory, names can be successfully treated in the setting of L_ω (p. 44).

Specifically, in section 38 it is argued that, if Frege's theory is correct, treating names as abbreviated definite descriptions would suffice to handle apparent substitutivity failures involving them; in section 39 it is argued that, if names do not have descriptive content, then apparent substitutivity failures involving them may be explained using a combination of semantic and pragmatic features. (This latter approach was subsequently adopted by Nathan Salmon, 1986.) I deliberately did not take a stand on which treatment of names should be adopted. For my purposes there was no need to take a stand.

Anderson's main criticism is this:

> Consider:
> (1) Pierre believes that Blaise believes that $\neg(\exists n, x, y, z)\ (n > 2\ \&\ x^n + y^n = z^n)$,
> (2) Fermat's Last Theorem $= [\neg(\exists n, x, y, z)\ (n > 2\ \&\ x^n + y^n = z^n)]$.
> There certainly *seems* to be a reading of (1) and
> (3) Pierre believes that Blaise believes Fermat's Last Theorem,
> such that (3) does not follow from (1) and (2). Yet the inference goes through in Bealer's logic as:
> (1′) $uB^2[vB^2[A]]$,
> (2′) $w = [A]$,
> (3′) $uB^2[vB^2w]$,
> where A abbreviates '$\neg(\exists n, x, y, z)\ (n > 2\ \&\ x^n + y^n = z^n)$' and '$u$', '$v$', and '$w$' are to be replaced by names (p. 126).[6]

This, however, does not accurately report the position developed in the book.

As just indicated, if names have descriptive content (as Frege, Church, and Anderson believe), sentences involving names are to be treated as synonymous to sentences containing definite descriptions. For example, 'Fermat's Last Theorem' would abbreviate '$(\iota w)F^1(w)$', where 'F^1' expresses the descriptive intension that Fregeans posit; similarly, for 'Pierre' and 'Blaise'. Accordingly, the indicated readings of (1)–(3) would be represented thus:

(i) B^2(Pierre, [B^2(Blaise, $[A]$])

(ii) Fermat's Last Theorem$= [A]$

(iii) B^2(Pierre, [B^2(Blaise, Fermat's Last Theorem)]

which, respectively, are synonymous to:

(i′) $B^2((\iota u)P^1(u), [B^2((\iota v)B^1(v), [A]])$

(ii′) $(\iota w)F^1(w) = [A]$

(iii′) $B^2((\iota u)P^1(u), [B^2((\iota v)B^1(v), (\iota w)F^1(w))]$

where '$(\iota w)F^1(w)$' has narrow scope. Since (iii′) does not follow from (i′) and (ii′), (iii) does *not* follow from (i) and (ii). So, on this way of representing (1)–(3) (which would be the way to which Anderson is committed), the inference from (1) and (2) to (3) does *not* go through in the logic developed in the book.

On the other hand, if names do not have descriptive content, Anderson's datum – namely, that (3) does not *seem* to follow from (1) and (2) – is dealt with pragmatically in terms of Gricean rules of conversation. Specifically, when we read or hear sentences (1)–(3) in an actual context, we understand three propositions such that the third does *not* follow from the first two. To identify these three propositions, we must take into account features of the context as well as purely semantical features. In section 39 "Pragmatics" there is a discussion (in connection with a closely related example) of the sort of propositions these would be and how (*mutatis mutandis*) to represent them so that the third does *not* follow from the first two. (Incidentally, Church, 1954, proposes a pragmatic treatment for a somewhat similar problem – namely, his fortnight/period-of-fourteen-days puzzle – so Church, and presumably his followers, have no good reason to reject a pragmatic solution in the present case.)

Thus, when the position developed in the book is represented accurately, Anderson's criticism just does not apply.

Definite descriptions. I will return to the issue of names in a moment. But first I will consider definite descriptions. Suppose (with Frege, Church, and Anderson) that names have descriptive content. Names could then be treated as abbreviations for definite descriptions. If, however, definite descriptions are treated as *defined* expressions, one encounters a well-known problem: there are a number of equally plausible definitions that, although logically equivalent, are intuitively not perfectly synonymous. Russell himself proposed more than one such definition. This leads Anderson to reason thus: "Now if as in the present case, alternative analyses are available which are not synonymous with one another, then no one of them can correctly be said to be *the* thing which is meant by some expression in an intentional context. This seems to show that Bealer cannot deal with the fundamental problem of intensional logic – *prima facie* failure of substitutivity of co-denoting terms" (p. 127). Anderson calls this the Problem of Multiple Analysis.

Anderson's argument contains a fallacy. The argument is once again an enthymeme whose suppressed assumption is false. The suppressed assumption is that within the framework of the book definite descriptions must be treated as *defined expressions*. This assumption is false. The reason is obvious: Within the framework of the book 'the' can be treated as a *primitive operator*. This was emphasized in the book, and it was indicated how to do this for two of the leading primitive-operator treatments, namely, Evans' and Frege's.[7]

In fact all three of the leading primitive-operator treatments of descriptions – (1) Evans', (2) Prior's, and (3) Frege's – can easily be dealt with in the framework of the book. (This issue is discussed at length in my paper "A Solution to Frege's Puzzle," 1993.) (1) According to Evans (1977a, b), the definite-description operator is treated as a primitive binary quantifier $\ulcorner$[the x]$\urcorner$ that combines with a pair of formulas to yield a new formula. For example, $\ulcorner$The F is $G\urcorner$ has the form $\ulcorner[\text{the } x](Fx : Gx)\urcorner$. To incorporate Evans' theory into the semantics, we simply restrict ourselves to algebraic model structures in which the set of logical operations contains a binary operator **the** that takes pairs u, v, of properties to propositions such that, for each possible extensionalization function H in the model structure, $H(\mathbf{the}(u, v))$ =TRUE iff, for some w in the domain of the model structure, $\{w\} = H(u) \subseteq H(v)$.[8] (2) Consider next the treatment suggested by Prior (1963).[9] On analogy with $\ulcorner$some $F\urcorner$ and $\ulcorner$every $F\urcorner$, $\ulcorner$the $F\urcorner$ is treated as a restricted quantifier $\ulcorner[\text{the } x : Fx]\urcorner$ that combines with a formula to yield a new formula. For example, $\ulcorner$The F is $G\urcorner$ has the form $\ulcorner[\text{the } x : Fx](Gx)\urcorner$. To obtain a semantics for descriptions on Prior's treatment, we restrict ourselves to model structures in which the set of logical operations contains a unary operator **The** that takes properties to properties of properties in accordance with the following: for all properties u and v in the domain of the model structure and for all possible extensionalization functions H in the model structure, $v \in H(\mathbf{The}(u))$ iff, for some w in the domain of the model structure, $\{w\} = H(u) \subseteq H(v)$. Then, the proposition that the F is G = $\text{pred}_s(\mathbf{The}([\text{x} : \text{Fx}]), [x : Gx])$. Here pred_s is the operation of singular prediction. (One could deal with $\ulcorner$some $F\urcorner$ and $\ulcorner$every $F\urcorner$ analogously.) (3) Consider, finally, Frege's treatment of descriptions (which is the treatment Church and Anderson accept). On this treatment, $\ulcorner$the $F\urcorner$ is an ordinary singular term having the form $\ulcorner(\iota x)(Fx)\urcorner$, where $\ulcorner(\iota x)\urcorner$ is a primitive unary operator that combines with a formula to yield a singular term.[10] The simplest way to incorporate this treatment into the semantics is to assume (with Russell) that $\ulcorner$The F is $G\urcorner$ is false if there does not exist a unique thing of which $\ulcorner F\urcorner$ is true.[11] Then, to obtain a semantics, we simply restrict ourselves to algebraic model structures in which the set of logical operations contains an individual-conceptforming operation ***the*** and an operation of descriptive predication pred_d.[12] The operation ***the*** takes, say, the property of being an F to the property of being the F. (More formally, for all properties u in the domain of the model structure and for all possible extensionalization functions H in the model structure, $w \in H(\boldsymbol{the}(u))$ iff $H(u) = \{w\}$.) The operation pred_d takes, say, the property of being G and the property of being the F to the proposition that the F is G. (More formally, for all properties x and y in the domain of the model structure and all possible extensionalization functions H in the model structure, $H(\text{pred}_d(x, y)) =$ TRUE iff $\emptyset \neq H(y) \subseteq H(x)$.) This is all there is to it. Even for Frege's syntactic treatment of definite descriptions, we obtain a semantics that is vastly simpler than that of Church and Anderson.

The conclusion is that within the framework of the book it is easy to treat 'the' as a primitive operator, as Anderson demands. Thus, Anderson's suppressed assumption (which was explicitly rejected in the book) is mistaken. The criticism – namely, that the theory in the book falls prey to the so-called Problem of Multiple Analysis – simply does not apply.

The above remarks on the representation of definite descriptions illustrate how the algebraic semantic technique constitutes a *general semantics*. The representation of adverbs and attributive adjectives provides another instructive illustration. But for that another day.

Names again. Let us return to the matter of proper names. Do proper names have descriptive content, as Frege, Church, and Anderson believe? Donnellan (1970) and Kripke (1972) have provided powerful arguments that they do not. It is a great embarrassment to Fregeans that no satisfactory descriptive content for ordinary proper names can be found. What are the distinct descriptive senses of 'Hesperus' and 'Phosphorus'? How do we have epistemic access to them? If satisfactory answers are not forthcoming, the traditional Fregean solution to the standard substitutivity puzzles has the status of a mystery solution. The simplest explanation for the complete failure to find the hypothesized descriptive contents is that they simply do not exist and that the Mill–Kripke theory is right. As indicated above, if the Mill–Kripke theory is right, substitutivity puzzles involving names can be explained pragmatically rather than semantically. In this case, names may be treated as a special kind of undefined singular term whose semantical behavior is like that of a free variable with a fixed assignment. Accordingly, 'that'-clauses containing names would denote Russellian *singular propositions*. In algebraic semantics, it is easy to represent such propositions: a singular proposition may be represented as the result of applying the operation of singular predication (pred_s) to a property and an object. By contrast, the Church–Anderson approach, at least as it stands, does not provide for singular propositions, and providing for them would require a massive revision of the Church–Anderson syntax and semantics. So once again the Church–Anderson approach appears to be deficient: its commitment to the Fregean descriptive-content theory has the status of a mystery solution; at the same time, it is highly unsuited to accommodate the alternative Mill–Kripke theory.

(Incidentally, since writing the book I have seen a way to provide a semantical, as opposed to pragmatic, solution to these substitutivity puzzles. This solution is given in "A Solution to Frege's Puzzle," 1993. However, there are significant barriers to incorporating this solution into the Church–Anderson approach.)

3. THE PARADOX OF ANALYSIS AND MATES' PUZZLE

The Paradox of Analysis. The following example illustrates the propositional-attitude version of the paradox of analysis:

(1) x knows that whatever is a circle is a circle.

(2) x does not know that whatever is a circle is a locus of points in the same plane equidistant from a common point.

(3) Being a circle = being a locus of points in the same plane equidistant from a common point.

Intuitively, (1) and (2) are simultaneously satisfiable, and (3) is evidently true. However, (1) and (3) entail the negation of (2) in a wide variety of intensional logics (e.g., Church, 1974; Anderson, 1980, 1984; and the intensional logic proposed provisionally in chapter 2 of the book prior to the discussion of the paradox of analysis in chapter 3). Something has to give. In the book, I advocated making certain modifications in the intensional logic. Anderson's response is quite different; he challenges the data: "The resolution of the alleged paradox in connection with such examples would seem to be to deny the corresponding property identity" (p. 140) – that is, to deny (3).[13] If it is to be adequate, Anderson's resolution must, of course, be general. That is, he must hold that ⌜Being an x such that Fx is the same thing as being an x such that $\ldots x \ldots$⌝ is always false unless it is wholly trivial.

I have just been using gerundive phrases ⌜being a ϕ⌝ to formulate the paradox of analysis and to report Anderson's resolution. However, infinitive phrases ⌜to be a ϕ⌝ could have been used instead. Specifically, there would still be a puzzle if (3) were replaced with the following:

(4) To be a circle is to be a locus of points in the same plane equidistant from a common point.

And Anderson's resolution of this puzzle would be to hold that (4) is false and, more generally, that ⌜To be an x such that Fx is to be an x such that $\ldots x \ldots$⌝ is always false unless it is wholly trivial.

Anderson's resolution of the paradox of analysis is highly implausible. Certainly there is a natural reading according to which (3) is true. Likewise, there is a natural reading according to which (4) is true. Surely, to be a circle is to be a locus of points in the same plane equidistant from a common point. If not, what is it to be a circle?

Why does Anderson reject a commonplace truth like this? He thinks that the so-called Problem of Multiple Analysis forces that conclusion. This is how he reasons. The following is just as plausible as (3):

(3′) Being a circle is the same thing as being a closed plane figure of constant curvature.

Likewise, the following is just as plausible as (4):

(4′) To be a circle is to be a closed plane figure of constant curvature.

Because there is no way to choose between (3) and (3′), neither is correct. Likewise, because there is no way to choose between (4) and (4′), neither is correct. But this is a fallacy. All of them are correct.

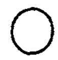
Anderson's argument is again an enthymeme whose suppressed assumption is false. The assumption is that (up to synonymous isomorphism) there can be *at most one* correct property identity of the form Being an F is the same thing as being a $\ldots$⌝ and, likewise, (up to synonymous isomorphism) there can be *at most one* correct property identity of the form ⌜To be an F is to be a ...⌝. Anderson intimates that the theory in the book is committed to this false assumption, but this is certainly not the case, as I will explain in a moment.

Before I elaborate on these points, notice that Anderson's resolution of the paradox of analysis evidently has further implausible consequences. Suppose we ask someone to define what it is to be a circle. Surely the person would meet our request by asserting either (4) or (4′). But according to Anderson's view, neither defines what it is to be a circle, for neither is true. But if neither (4) nor (4′) defines what it is to be a circle, what on earth could? Are we to conclude that it is not possible to define what it is to be a circle? Evidently, Anderson's view has this implausible consequence. The situation appears to be even worse. Consider the following highly intuitive schema:

If Fx iff$_{\text{def}}$ $\ldots x \ldots$, then to be an x such that Fx is to be an x such that $\ldots x \ldots$.

Certainly this schema accurately reflects how we talk. However, because on Anderson's view the consequent of this schema is always false unless wholly trivial, it follows (by contraposition) that on Anderson's view there are *no* non-trivial definitions. This consequence is in sharp conflict with accepted standards in logic, mathematics, philosophy, and science, where non-trivial definitions are commonplace. The conclusion is that Anderson's proposed resolution of the paradox of analysis is unreasonable.

Let us therefore turn to the theory of properties and concepts proposed in the book. Recall that on that theory each intension is either logically simple or logically complex. Logically simple intensions do not have any logical form; they are identical if necessarily equivalent. Logically complex intensions are those having logical form; they are identical if their complete analysis trees have the same structure and their respective terminal nodes are identical. Logically simple n-ary intensions ($n \geqslant 1$) are the real properties and relations in the world; logically complex n-ary intensions ($n \geqslant 1$) are mere concepts. The *concept* of being a locus of points in the same plane equidistant from a common point is different from the *concept* of being a closed plane figure of constant curvature; these logically complex intensions have quite different analysis trees. Nevertheless, *to be* a locus of points in the same plane equidistant from a common point is *to be* a closed plane figure of constant curvature. This is what it is to be a circle. It is a genuine property of things in the world; for example, it is present here:



This intension has no logical form; as far as logical form is concerned, it is simple. Hence, it is distinct from the above concepts, which do have logical form.[14]

This theory of properties and concepts leads to a natural solution to the above propositional-attitude version of the paradox of analysis. (1) and (3) do *not* entail the negation of (2) because the proposition that whatever is a circle is a locus of points ... and the proposition that whatever is a circle is a circle are distinct. The former proposition is formed by applying relevant logical operations to the property of being a circle and the concept of being a locus of points .... The latter proposition is formed in the same way except that the property of being a circle takes the place of the concept of being a locus of points.... Because the property and the concept are distinct, so are the two propositions. Now because (1) and (3) do not entail the negation of (2), (1)–(3) are simultaneously satisfiable, and the paradox is resolved.

This theory also allows us to say how a definition $\ulcorner Fx \text{ iff}_{\text{def}} \ldots x \ldots \urcorner$ can be correct but non-trivial: it can be correct because the property of being an x such that Fx = the property of being an x such that $\ldots x \ldots$; it is non-trivial because the property of being an x such that $Fx \neq$ the concept of being an x such that $\ldots x \ldots$.[15] Moreover, we can say how both $\ulcorner Fx \text{ iff}_{\text{def}} \ldots x \ldots \urcorner$ and $\ulcorner Fx \text{ iff}_{\text{def}} \underline{\quad x \quad} \urcorner$ can be correct without being synonymous: they can both be correct because the property of being an x such that Fx = the property of being an x such that $\ldots x \ldots$ = the property of being an x such that $\underline{\quad x \quad}$; they are non-synonymous because the concept of being an x such that $\ldots x \ldots \neq$ the concept of being an x such that $\underline{\quad x \quad}$. Anderson's problem of multiple definitions is thus solved without the implausible consequences inherent in Anderson's proposed resolution.

Quality and Concept was designed to represent this more complex ontology of *properties* and *concepts*. The details might need to be adjusted in one way or another; however, (something like) this general picture appears to be required in order to explain the fact that some intensions have a multiplicity of non-trivial definitions. (This is what I meant by my remark in section 1 above that additional justification for the ontology of properties and concepts arises in connection with the paradox of analysis.) On Anderson's theory, by contrast, one seems driven to deny the plain fact that some intensions have a multiplicity of non-trivial definitions. Of course, there is more to say about the paradox of analysis. I will return to it in a moment. In preparation I need to discuss some related puzzles.

Mates' Puzzle. The puzzle advanced by Mates (1950) is evidently different from the propositional-attitude version paradox of analysis, for it does not trade on a person's ignorance of *definitions*. Mates holds that, for any distinct sentences D and D',

Nobody doubts that whoever believes that D believes that D.

and

Nobody doubts that whoever believes that D believes that D'.

can always diverge in truth value no matter how strict one's criterion of synonymy. For example, let D be 'Somebody chews' and D' be 'Somebody masticates'. In symbols: '$(\exists x)Cx$' and '$(\exists x)Mx$'. Surely, on any plausible criterion these two sentences have a common meaning. One response to Mates' puzzle is to solve it pragmatically, on analogy with the aforementioned pragmatic solution to substitutivity puzzles involving proper names. (Section 39 in the book presents a discussion of this sort of pragmatic solution. In broad outline this is the same sort of solution that Church, 1954, advocates; see his discussion of the fortnight/period-of-fourteen-days example.) A second response to Mates would be to construct a new intensional logic that admits even more fine-grained intensional distinctions. According to this new intensional logic, even though the propositions denoted by '$[(\exists x)Cx]$' and '$[(\exists x)Mx]$' would be identical, the propositions denoted by the following more complex intensional abstracts would not:

(a) $[(\forall u)(B^2 u, [(\exists x)Cx] \rightarrow B^2 u, [(\exists x)Cx])]$

and

(b) $[(\forall u)(B^2 u, [(\exists x)Cx] \rightarrow B^2 u, [(\exists x)Mx])]$.

How can this be?

There are two proposals. One is based on a complicated variation of traditional Fregean ideas (this is the proposal Anderson advocates[16]), and the other is based on differences in *logical form*. According to the former proposal, although the ordinary senses of 'Somebody masticates' and 'Somebody chews' are the same, their indirect senses are different. That is, although the proposition that somebody chews = the proposition that somebody masticates, the concept of being the proposition that someone chews $\neq$ the concept of being the proposition that someone masticates. In turn, the propositions denoted by (a) and (b) are distinct. The problem with this neo-Fregean proposal is that, without further explanation, it is a mystery solution. Given that 'Something chews' and 'Something masticates' are synonymous and given that they have the same syntactic form, what on earth could distinguish the concept of being the proposition that somebody chews and the concept of being the proposition that somebody masticates? Spell out the difference, and tell us how we have epistemic access to these allegedly distinct concepts. Unless this can be done, merely alleging the existence of new primitive Fregean senses is not theoretically acceptable. The situation is quite analogous to a dogmatic insistence on a traditional Fregean semantics for names in the face of the Donnellan–Kripke critique.

The second proposal (which is related to a suggestion made in Putnam, 1954) is to exploit differences in *logical form* between the two complex intensional abstracts (a) and (b). Specifically, the predicate 'C' is *repeated* in the former abstract but not in the latter; so the former has the logical form $\ulcorner[(\forall u)(\ldots 1 \ldots \rightarrow \ldots 1 \ldots)]\urcorner$ whereas the latter has the logical form $\ulcorner(\forall u)(\ldots 1 \ldots \rightarrow \ldots 2 \ldots)]\urcorner$. The idea is to build a new hyper-fine-grained intensional logic around the following general principle: non-elementary intensional abstracts are to be co-denoting only if they have exactly the same logical form. (An intensional abstract is elementary if it has the form $\ulcorner[v_1 \ldots v_n : F^n(v_1 \ldots v_n)]\urcorner$, where $\ulcorner F^n \urcorner$ is a primitive predicate and $n \geqslant 1$.) It turns out that the semantics and axiomatic presentation of this theory can be formulated within the general algebraic approach. Moreover, unlike the neo-Fregean response to Mates, this response is not a mystery solution. It is plausible that differences in logical form should be responsible for differences in meaning. This solution has been developed in my paper "General and Hyper-fine-grained Intensional Logic." (It goes without saying that the prospect of developing this solution within Church's framework is forbidding.)

Now a special advantage of this solution to Mates' puzzle is that it can be used to solve a puzzle that is "midway" between Mates' puzzle and the propositional-attitude version of the paradox of analysis. Namely, it explains how the following three formulas can be simultaneously satisfiable:

x knows that to be a circle is to be a circle.

x does not know that to be a circle is to be a locus of points ....

To be a circle is to be a locus of points....

And it solves a puzzle that Anderson calls the Paradox of the Synonymy Relation. Namely, it explains how the following three formulas can be simultaneously satisfiable:

x knows that the concept of being a vixen is the concept of being a vixen.

x does not know that the concept of being a vixen is the concept of being a female fox.

The concept of being a vixen is the concept of being a female fox.

These solutions do not require positing (as Anderson's does) mysterious distinctions between the indirect senses of synonymous predicates; instead, they are based on readily intelligible distinctions in logical form.

A Simpler Puzzle. Despite these successes, there is a *simpler* type of substitutivity puzzle, discussed in the book, that cannot be solved with the above sort of machinery. (Anderson calls this type of puzzle

"Bealer's Puzzle.") Consider any two predicates that express the same intension, for example, 'chew' and 'masticate'. (Or choose some predicate 'C' and then just stipulate that a new predicate 'M' expresses the same intension as that which is expressed by 'C'.) Consider someone x "halfway" along in the process of picking up the use of 'masticate' by hearing others use it. There are conversational contexts in which x could correctly (not to say literally) characterize his or her cognitive state by asserting:

> I am now sure that whatever masticates chews, but I am not yet sure that whatever chews masticates.

In this example, the two intensional abstracts '$[(\forall z)(Mz \rightarrow Cz)]$' and '$[(\forall z)(Cz \rightarrow Mz)]$' have the *same* logical form: $[(\forall z)(1(z) \rightarrow 2(z)]$. So the above hyper-fine-grained theory does not help to elucidate what x's cognitive state is. Nor does Anderson's theory that, although synonymous, 'chew' and 'masticate' have distinct indirect senses. How, then, are we to represent x's cognitive state?[17] In view of Church's pragmatic treatment of the fortnight/period-of-fourteen-days example, Church – and presumably Anderson – would answer with something like the following:

(A) x is sure that whatever satisfies the English predicate 'masticate' chews, but x is not sure that whatever chews satisfies the English predicate 'masticate'.[18]

This is a reasonable proposal, but there is a problem, which has been in the literature for a long time now (Burge, 1975, 1978, 1979; Evans, 1982; Schiffer, 1987, 1990). Suppose that x is a child (or a slow-learning adult) who has no articulate command of the metalinguistic concepts we take for granted. In particular, x has no mastery of a device (e.g., quotation names, phonetic descriptions, etc.) for designating expressions, and x has no articulate command of concepts from linguistic theory such as the syntactic concept of a linguistic predicate or the semantical concept of satisfaction or the concept of the English language. Furthermore, when we try to teach x these bits of linguistic theory, x has great difficulty learning them. (Indeed, x learns to use the new predicate 'masticate' much more readily.) However, a few years later when we try again to teach x these things, he learns them quickly. This shows, so the worry goes, that the above characterization of the child's cognitive state represents him as having reached a stage of cognitive develoment beyond that which we can plausibly attribute. Because this worry has *prima facie* cogency, Church and Anderson need an answer to it. However, Anderson has nothing to say about the worry even though it is discussed at some length in the book.[19]

I know of only one way to deal with this worry that is consistent with the standard parsing of attitude sentences such as (*A*) adopted by Church, Anderson, and myself. The idea is to formalize our common informal practice of using *hyphenation* to mark certain fine-grained intensional distinctions. Here is an illustration of this practice. In presenting a sentenial treatment of belief such as Quine's, we feel compelled to use the hyphenated expression 'believes-true-as-a-sentence-of-English' rather than the unhyphenated expression 'believes true as a sentence of English'. The use of hyphenation is intended to indicate that 'belives-true-as-a-sentence-of-English' is to be taken as if it were a primitive predicate. A plausible hypothesis is that an analogous use of hyphenation is appropriate if we wish to attribute to x a cognitive state commensurate with his developmental stage. Specifically, instead of using (A) to characterize x's cognitive state, those who favor the suggested pragamatic approach (perhaps Church himself) ought to use (something like) the following:

(A′) x is sure that whatever satisfies-the-English-predicate-'masticate' chews, but x is not sure that whatever chews satisfies-the-English-predicate-'masticate'.

Since (A′) is not equivalent to (A), this avoids the problem of mistakenly attributing to x the cognitive state reported by (A), which (if the above worry is sound) is a cognitive state that x will have only at a cognitively more advanced developmental stage.

In the book, this idea was formalized in two steps. First, the language L_ω was enriched with an *underlining notation* which was to be the syntactic counterpart of hyphenation. Second, an ontological distinction was posited between "unanalyzed concepts" and "analyzed concepts." The idea was that underlined expressions (i.e., the counterpart of hyphenated expressions) were to express unanalyzed concepts, whereas non-underlined expressions (i.e., ordinary, non-hyphenated expressions) were to express analyzed concepts. This semantics for the underline notation works out formally. However, I now see that the ontology of analyzed and unanalyzed concepts is unnecessary. An ontologically more economical semantics for the underline notation can be given within the hyper-fine-grained framework used above in connection with Mates' puzzle. All that is needed is a semantical representation of distinctions such as the distinction between the following:

$$[(\forall z)(\underline{\text{Satisfy}(z,\ \text{English},\ \text{'masticate'})} \rightarrow \text{Chew}(z))]$$

and

$$[(\forall z)(\text{Satisfy}(z,\ \text{English},\ \text{'masticate'}) \rightarrow \text{Chew}(z))].$$

To represent this distinction, one simply adds clauses to the semantics stipulating that an intensional abstract such as '$[(\forall z)\ (\underline{\text{Satisfy}(z,\ \text{English}, \text{'masticate'})} \rightarrow \text{Chew}(z))]$' is to denote the proposition $[(\forall z)(M(z) \rightarrow \text{Chew}(z))]$, where '$M$' is a new primitive predicate stipulated to expresses the concept [z: Satisfy(z, English, 'masticate')]. (This example is typical and easily generalizes.) This semantics yields the desired result. For in the hyper-fine-grained framework, even though the concepts [z: M(z)] and [z: Satisfy(z, English, 'masticate')] are identical, the indicated propositions are not because they differ in logical form. That is,

$$[(\forall z)(M(z) \rightarrow \text{Chew}(z))] \neq [(\forall z)(\text{Satisfy}(z, \text{English}, \text{'masticate'}) \rightarrow \text{Chew}(z))].$$

Therefore,

$$[(\forall z)(\underline{\text{Satisfy}(z, \text{English}, \text{'masticate'})} \rightarrow \text{Chew}(z))] \neq [(\forall z)(\text{Satisfy}(z, \text{English}, \text{'masticate'}) \rightarrow \text{Chew}(z))]$$

as desired.[20]

A moment's reflection shows that the latter distinction is just a more complicated instance of the type of intensional distinction Anderson rejected in his effort to criticize the book's fine-grained conception of intensional entities (viz., the distinction between the proposition [Fa] and the proposition [*Rab*]; see section 1 above). There is irony here. Even though Anderson rejects intensional distinctions of this type, they are evidently needed to solve a family of elementary puzzles that confront Church and Anderson even if, for the sake of argument, we suppose Anderson's view that synonymous predicates have distinct primitive indirect senses. This conclusion was alluded to earlier (in section 1) when I noted that this type of intensional distinction would prove to have an unexpected theoretical utility in dealing with the paradox of analysis.

4. THE PARADOXES

In the book I made it clear that I was not proposing a resolution of the paradoxes. I believed then (and I continue to believe) that no ideal resolution is known although much important new work has been done and continues to be done. It is not unreasonable to hold that an ideal resolution may never be discovered by human beings.[21] My stated thesis was rather modest, namely, that a wide range of styles of resolution can be incorporated into the intensional logics constructed in the book. To do this, one singles out a distinguished logical constant for the *predication relation* and introduces associated candidate axioms for it.[22] Anderson seems to accept this thesis; he provides no arguments against it.

In the book I emphasized that the predication theories I would be presenting were only *illustrations* of how to incorporate some previously known style of resolution.[23] The illustrative theory of predication I chose is based on (i) Zermelo's iterative-hierarchy resolution of the set-theoretical paradoxes, and (ii) a context-relativity resolution of the semantical and intentional paradoxes (rather like those advocated by Charles Parsons, 1974, and Tyler Burge, 1979b). I chose this illustration largely because the underlying ideas were very familiar and, I thought, more or less palatable to my readers.

I also stated that there were other illustrations which would have been very easy to present, for example, illustrations based on fixed-point ideas developed by Fitch (1948, 1963, 1980), Gilmore (1974), Feferman (1975), and Kripke (1975).[24] Although I personally preferred those ideas, I believed that readers would not be so receptive to an illustration based on them as they would be to an illustration based on the more familiar iterative/context-dependency ideas. I thought that readers, most of whom would have been raised on Quinean extensionalism, would already be generally resistant to the thoroughgoing intensionalism in the book and that they would therefore not be receptive to those less familiar proposals on the paradoxes. This was mistaken. Philosophical opinion has taken a major intensionalist swing in the past decade, and there are now a considerable number of researchers working on type-free resolutions of the paradoxes. When I wrote the book, the time was ripe for presenting a type-free predication theory. It would have been easy to do so, and the book would have been better for it.

This misreading of the audience was unfortunate, for some otherwise sympathetic readers got the mistaken impression that the general approach in the book is tied to iterative/context-dependency ideas. Regrettably, Anderson's article perpetuates this impression. He goes on for ten pages elaborating familiar shortcomings in the iterative/context-dependency resolution. It was because I knew of such shortcomings that I stated that this style of resolution was not ideal and that it should be taken only as an illustration of how to incorporate a given resolution into the intensional logics developed earlier in the book.[25] Anderson's article thus has the effect of misrepresenting the book on the issue of the paradoxes. A helpful plan for the article would have been: (1) to state that the illustration given in the book predictably inherits the sort of shortcomings commonly known to be present in the iterative/context-dependency resolution, (2) to give a representative example of this sort of shortcoming, (3) to indicate that these particular sorts of shortcomings could be avoided by incorporating instead one of the type-free resolutions (which were mentioned in the book and which had become quite popular by the time Anderson's article was written), and (4) to discuss the relative merits of the type-free approach and his own rigidly typed approach.

There is an irony in Anderson's presentation. Namely, most of the shortcomings he points out in the iterative/context-dependency resolution have counterparts (indeed, infinitely many counterparts) in the ramified type-theoretical resolution of the paradoxes that he and Church advocate. As I stated in the book, "The idea that the semantical and intentional paradoxes can be resolved by making explicit contextually invoked limitations on the universe of discourse ought to sound familiar. For the ramified theory of types embodies a special case of this very idea. Indeed, the modified [i.e., context relativized] ZF-style and GB-style logics for L_ω with Δ [i.e., my symbol for the predication relation] may be viewed as natural generalizations of ramified type theory" (p. 100). And in an accompanying note I say, "For a discussion of an analogous relationship between ordinary ZF and simple type theory, see pp. 266–86, Quine, *Set Theory and Its Logic*, revised edition" (p. 262). However, unlike the Church–Anderson approach to predication theory, the approach taken in the book is not wedded to this style of resolution. As I have indicated, type-free resolutions can just as easily be incorporated. When they are, the well-known sorts of shortcomings, of which Anderson makes so much and which have counterparts in his own approach, are avoided.[26] The important point is that, whereas type-free resolutions are easily incorporated into the approach taken in the book, the Church–Anderson approach is permanently hamstrung because the ramified type-theoretical approach is built into its very syntax.

There is further irony in Anderson's criticisms. Much of what he says in the article cannot even be stated in the language of his ramified type theory. Page after page, he talks in a *general, type-free* way about intensions (concepts, properties, relations, propositions), types, identity, predication, truth, necessity, validity, belief, paradox, and so forth. But there is no way to say such things in his ramified type theory. If Anderson's official theory were true, much of his article would be meaningless. How would Anderson reply? He would try to invoke the type-theorists old standby – "typical ambiguity":

> The type theorist can reply that such [apparent use of general, type-free terms] should be regarded as displaying typical ambiguity (in the sense of Whitehead and Russell). This is the legacy of the paradoxes. Particular arguments involving these apparently typeless notions can be treated by extending the type theory as far into the transfinite as the occasion demands (Anderson, pp. 124–125).

But this statement suffers from the same defect: it is a type-free statement about type theory and the transfinite hierarchy of types. Such a statement cannot be made in type theory. Indeed,there is no statement in type theory that captures the type-theorist's doctrine of typical ambiguity: such a statement would require talking in a general type-free way about types, but type-theory has no variables that range generally over types. Necessarily, type-theorists cannot within their theory consistently say what they want to say.

Are those of us who advocate a type-free approach any better off? We certainly are. This is not the place to attempt a detailed explanation.[27] Suffice it to say that, by extending the Fitch–Gilmore–Feferman–Kripke techniques, one can obtain theories that in relevant ways are capable of describing their own semantics. I have in mind, for example, the theories proposed by William Reinhardt (1986), Vann McGee (1991), and Brian McDonald (1992).[28] It is relatively straightforward to adapt these type-free theories to the intensional logics developed in the book. These constructions make it at least plausible that there can be a type-free intensional logic capable of representing our theoretical thought and talk – including our theoretical thought and talk about our theoretical thought and talk. This important prospect is just thrown away at the outset in the Church–Anderson approach.

5. CONCLUSION

I have considered Anderson's main points. One after another they have not held up; on the contrary, it is the Church–Anderson approach that has proven to be deficient even where Anderson thought it was stronger. (This is not to mention the long list of other defects in the Church–Anderson approach.) Although Anderson makes a number of lesser points, they can be rebutted with equal vigor. The conclusion seems inescapable: the future does not lie with the Church–Anderson approach.

NOTES

[1]A complete analysis tree is one in which the logical analysis of the nodes cannot be carried further. For example, if the concepts A-ness, B-ness, C-ness, and D-ness are logically simple, the concept of being an x such that $(Ax\&Bx) \vee (Cx\&Dx)$ has three *incomplete* analysis trees (in addition to the degenerate one-node tree consisting of the concept itself), but it has a unique *complete* analysis tree.

[2]Anderson gives two other criticisms which can also be shown to turn on confusions.

[3]Care must be taken here in these matters. The proposition that aRb is not the same as the proposition that a and b stand in the relation R. If these were the same proposition, the associated comprehension principle would be a complete triviality: $(\exists r)$ (a and b stand in relation $r \leftrightarrow aRb$). For the same sort of reason, the proposition that Fa is not the same as

the proposition that a has the property F (where F is the relational property $[x: xRb]$). Nor is the proposition that Gb the same proposition as the proposition that b has the property G (where G is the relational property $[y: aRy]$).

[4]If there is doubt about these identities, the same point may be made with new primitive predicates 'eves' and 'sdivides' introduced by stipulative definition.

[5]Anderson holds that this question might not be a question of logic but merely a question of fact: "Maybe it is just a fact that no two 'simple' intensions are logically equivalent" (p. 160). Surely Anderson does not mean contingent fact, for that would contradict the *ante rem* Platonism he inherits from Church. Suppose that he means necessary fact. If it is necessary that logically simple intensions are identical if logically equivalent, that would be a logical truth according to one time-honored tradition. According to this tradition, which we see in Aristotle and several medieval logicians, logic (properly understood) includes a theory of categories (or a theory of terms). Among other things, a theory of categories provides elementary necessary truths about the identity conditions of entities of each category. These are truths of logic according to this conception of logic. Ironically, (a vestige of) this conception is implicit in the theory of types itself: according to Russell and Church, for example, type distinctions are logical distinctions. It should also be noted that a notion of natural property can be defined in relevant logic; see Dunn (1990).

[6]Anderson's numbering of the displayed sentences is changed for uniformity.

[7]I stated: "Russell answers Frege's question by means of a two-part *syntactic* theory. First, he holds that, if '$a = b$' is true but different in meaning from '$a = a$', then 'a' or 'b' is an overt or covert description or extensional abstract. Secondly, Russell holds that definite descriptions and extensional abstracts are incomplete symbols" (p. 161). "Treating 'a' or 'b' as an incomplete symbol is the essence of the second part of Russel's answer to Frege's question. Russell's contextual definition of definite descriptions (and extensional abstracts) is really incidental. For one could eliminate instances of Frege's puzzle simply by treating definite-description and extensional-abstraction operators rather like quantifiers, i.e., as primitive *formula-producing* operators (versus singular-term producing operators)" (p. 270n.). "Russell's theory of descriptions is not essential to the program in the text. It would be possible, though more complex, to treat definite descriptions much as Frege does. However, that would force me to enrich my algebraic model structures with appropriate new logical operations to handle definite-description concepts" (p. 267n.).

[8]This treatment of descriptions is spelled out in Bealer and Mōnnich (1989).

[9]Related treatments are suggested by: Grice (1969), Sharvy (1969), and Montague (1973).

[10]In the language of Church (1951) $\ulcorner$the $F\urcorner$ has the form $\ulcorner\iota(\lambda x.Fx)\urcorner$, where '$\iota$' is an operator that combines with a propositional-function term to form an individual term. The difference is immaterial to the issue under discussion in the text.

[11]The semantics would be straightforward but more complicated if we assumed (with Frege) that $\ulcorner$The F is $G\urcorner$ is neither true nor false if there does not exist a unique thing of which $\ulcorner F\urcorner$ is true.

[12]In my paper "A Solution to Frege's Puzzle" it is shown that the operation of descriptive predication is already implicit in Frege's theory of senses when that theory is viewed from the algebraic perspective rather than from Church's propositional-function perspective.

[13]Anderson (pp. 140 ff.) tries to focus the discussion on synonymy and *my* conception 2. But synonymy and *my* provisional fine-grained intensional logic are beside the point. The paradox is stated without reference to synonymy, and it is a *prima facie* problem for any of a wide range of provisionally plausible intensional logics including Church's and

Anderson's. The only way out is to deny (1), (2), or (3) or to modify one's intensional logic to accommodate (1)–(3). Anderson does not do the latter, and he certainly does not deny (1) and (2). So his view is that (3) *simpliciter* is false.

[14]How sparse are these logically simple intensions? From a formal point of view, the theory presented in the book is completely neutral on this question (although in the informal discussions in the book I adopt the view that they are sparse). This fact permits people who believe that these logically simple intensions are not at all sparse to make use of the formal theory presented in the book. Various readers have misunderstood this point. The significance of this point for the present discussion is this. For the resolution of the paradox of analysis offered in the present paper it does not matter whether these logically simple intensions are sparse or not; all that matters is that there are logically simple intensions and that a multiplicity of logically equivalent complex intensions (concepts) correspond to each one of them.

The gerundive phrase $\ulcorner$being an x such that $\phi\urcorner$ and the infinitive phrase $\ulcorner$to be an x such that $\phi\urcorner$ denote properties. These property abstracts are distinct from the associated concept abstract $\ulcorner$the concept of being an x such that $\phi\urcorner$. In the book, after these intuitive distinctions are introduced, the notation is enriched (pp. 190–195) so that property abstracts are thereafter represented with $\ulcorner|x : \phi|\urcorner$ and concept abstracts are represented with $\ulcorner[x : \phi]\urcorner$. (For uniformity with the discussion in the text I have made a slight typographical modification here.) In this notation, (3) is symbolized with '$|x : \text{Circle}(x)| = |x : \text{Locus of points} \ldots|$' and (4) is symbolized with '$|x : \text{Circle}(x)| = |x : \text{Closed plane figure} \ldots|$'. On the semantics given for this notation, these property identities come out true as desired. At the same time, the following property/concept non-identities come out true as well: $|x : \text{Circle}(x)| \neq [x : \text{Locus of points} \ldots] \neq [x : \text{Closed plane figure} \ldots] \neq |x : \text{Closed plane figure} \ldots|$. As I am about to explain in the text, these identities and non-identities permit one to solve the propositional-attitude version of the paradox of analysis, and they permit one to explain how definitions $\ulcorner Fx \text{ iff}_{\text{def}} \ldots x \ldots\urcorner$ can be correct but non-trivial and how there can be a multiplicity of correct but non-synonymous definitions.

[15]This remark is not intended to provide general necessary and sufficient conditions for what it is to be a definition. (E.g., the remark is not intended to tell us why '$Fx \text{ iff}_{\text{def}} \neg\neg Fx$' fails to be a definition. For this, more is needed.) Rather, the intention of the remark is this. On the assumption that we already know what it is to be a definition, the remark tells us what it is that makes a definition *true*, and it explains how a true definition can nevertheless be *non-trivial*.

Incidentally, there is a second way in which a definition can be correct but non-trivial. See Note 20.

[16]See his remarks about Mates on p. 137 and p. 143. This proposal is of a piece with Anderson's proposed solution of what he calls the Paradox of the Synonymy Relation. To solve the latter, he says, "We can allow that two expressions are *synonymous* and yet their first ascendants [i.e., once embedded occurrences] need not be synonymous" (p. 142).

[17]The following is another instance of this type of puzzle. Suppose that a beginning logic student says, "I do not fine it intuitive that ((if A, then B) iff (A only if B))." How are we to represent this person's cognitive state if the proposition that, if A, then B = the proposition that A only if B?

[18]In my "A Solution to Frege's Puzzle," 1993, another solution is given, but it is a solution that cannot be incorporated into the Church approach. Nor does this solution obviate the

need for the kind of intuitive intensional distinction that we are about to discuss in the text, i.e., the distinction between the concept of satisfying the English predicate 'masticate' and the concept of satisfying-the-English-predicate-'masticate'.

[19]The importance of this problem and its relevance to his discussion of the paradox of analysis was pointed out to Anderson in correspondence long before the publication of his article.

[20]This apparatus allows us to identify another way in which a definition might be correct but non-trivial: x satisfies-the-English-predicate-'masticate' iff$_{def}$ x satisfies the English predicate 'masticate'.

[21]Mates (1983) holds that there exists no real resolution.

[22]I stated, "Until we find an ideal resolution of the paradoxes of predication, we may therefore follow this maxim: to obtain a workable resolution of these paradoxes, determine the best resolution of the paradoxes in first-order set theory and then adapt it to the setting of intensional logic with predication" (p. 96).

[23]I stated, "For illustrative purposes I will now sketch how such adaptation works in the case of the two most familiar resolutions of the first-order set-theoretical paradoxes, namely, Zermelo's resolution and von Neumann's resolution" (p. 96). I went on to say, "The same thing can be done for Quine's resolutions and for the more recent Fitch-Gilmore–Feferman resolution. (An idea analogous to Fitch's original insight lies behind Kripke's resolution of the Epimenides paradox in 'Outline of a Theory of Truth'.) For adaptation to the logic for L_ω with predication, Gilmore's lucid paper 'The Consistency of Partial Set Theory Without Extensionality' is ideal" (p. 259, n.).

[24]See the second quotation in the previous note.

[25]This was made plain to Anderson in correspondence long before publication of his article.

[26]This can be done so that the resulting model contains a hierarchy of ZF style properties plus a non-well-founded universe of properties – including universal properties, etc. Of course, the resulting theories have new difficulties of their own. No completely ideal resolution is yet known.

[27]See, e.g., Martin and Woodruff (1975), Kripke (1975), Gupta (1982), Feferman (1984), Bealer (1989a), Bealer and Mönnich (1989) for relevant discussions.

[28]There is also the approach of Haim Gaifman (1992).

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