subjects of consciousness; perhaps it is considerations of this sort that caused Smith to draw such conclusions. But the fact that sensations are not intentional does not allow us to conclude that they are not psychological occurrences, nor that they are not subjects of our ordinary noninferential reports and descriptions. Thus Smith seems to oscillate between the conclusion of his argument that memory images are nonpsychological, inferentially established occurrences, and the natural (and, I believe, warranted) temptation to talk of images as genuinely psychological occurrences—though nonintentional ones. (This oscillation is made even more radical by the fact that, in claiming that having images is a kind of thinking, Smith seems to be suggesting that imaging not only is psychological in nature, but can be accurately characterized as intentional as well—thereby yielding to the temptation to presuppose that no psychological phenomenon can fail to be intentional.)

Throughout the book many suggestive observations are made concerning our use of language both in making memory claims and in making reports of our subjective experiences in remembering. Such discussions, especially where they are more or less independent of the puzzling treatment of memory images noted above, are often useful and illuminating.

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*Art, Mind, and Religion.* W. H. CAPITAN and D. D. MERRILL, editors. Pittsburgh, Pa.: University Press, 1967. 158 p. $5.00.

*Art, Mind, and Religion* turns out to be the proceedings of the 1965 Oberlin College Colloquium in Philosophy. Why doesn't the title just say so? Even without deceptive packaging, we have trouble enough searching the miscellany of colloquia and symposia and *Festschriften* for a half-remembered paper. Does the publisher imagine that art and mind and religion will sell more copies than the well-deserved reputation of the Oberlin Colloquium? I trust he is wrong. The Oberlin papers are usually interesting, and the 1965 set is no exception.

There are five papers, three of them followed by extensive comments and rejoinders.

I

The first is P. H. Nowell-Smith's "Acts and Locutions," an elaborate defense on many fronts against Austin's attempts to play Old Harry with the true/false fetish and the value/fact fetish. (Is this supposed to be art, mind, or religion?) Nowell-Smith upholds the traditional

view that what we mostly do with words is *stating*, whatever else it may be as well. Stating is not a species of act on a level with judging, warning, answering, arguing, reporting, promising, and so on. Rather, it is the genus of which the others are species. When you judge, warn, answer, argue, report, promise, . . . that *p*, you *ipso facto* state that *p*. As the plain man says: you *say* that *p*. You make a statement of purported fact; depending on the facts, your statement is true or false. Of course, Nowell-Smith does not maintain that *every* speech act falls in the genus stating; you can also ask whether *q* or tell someone to do *r*. Asking and telling also are genera, and need to be distinguished from the many species falling under them. And surely at least a few speech acts—some of the original paradigm performatives like bequeathing—will not belong to any of the three principal genera.

Nowell-Smith argues, first, that Austin's theory of speech acts, even if sound, does not require us to demote stating from a genus to a species; second, that the distinctions of the theory have not been explained clearly; and third, that our common notion of stating as a genus is grounded in ordinary language, and is not a philosophers' invention.

II

Representing art, there is Stanley Cavell's "Music Discomposed." I am not sure how well I understand him, but perhaps he is driving at this. Confronted with some of what purports to be modern music, one is apt to react in a peculiar way: "Should I trust this, should I risk taking it seriously as music? Or is it a fraud, am I being had?" The reaction is familiar. It occurs even among the critics and composers of *avant-garde* music. It is genuine and important mistrust. Yet every clear description we can give of this mistrust makes it seem foolish. The reaction is *not:* "If I invest effort, will I learn to enjoy this?" Nor: "Is this a product of human inspiration?" Nor: "Did the composer himself think this was valuable?" Nor: "Is this really unlike anything that went before?" Nor: "Is it misuse of language to call this music?" Nor: "Will this be praised a hundred years hence?" Nor: "Who will laugh at me if I praise this?" A sophisticate unconcerned with any of these questions may still feel the suspicion Cavell commends to our attention.

III

Representing mind, there is Hilary Putnam's "Psychological Predicates," one of several papers [1] in which Putnam presents the hy-

[1] Others are: "Minds and Machines," in *Dimensions of Mind,* ed. S. Hook (New York: NYU Press, 1960); "Brains and Behavior," in *Analytical Philosophy: Second Series,* ed. R. Butler (Oxford: Blackwell & Mott, 1965); "The Mental Life of Some

pothesis that mental states—pain, for example—are not brain states and not behavior dispositions, but rather *functional states* of organisms. A functional state is a state specified implicitly by its place in a *functional description* of the organism: a true statement to the effect that the organism possesses states $S_1, \ldots, S_n$ governed by a network of laws of the form:

> If the organism is in state $S_i$ and receives so-and-so sensory imput, then with so-and-so probability the organism will go into state $S_j$ and produce so-and-so motor output.

An example of a functional description (in which the transition probabilities are all 0 or 1) is given by the machine table for a Turing machine, provided we regard the tape not as part of the machine but as an external source of inputs and recipient of outputs. I take it that a (nonphysiological) psychological theory—or better, the Ramsey sentence thereof—is a functional description, and that state-names introduced by that theory name functional states.

Putnam offers his hypothesis that pain is a functional state as a rival to the hypothesis that pain is a brain state; hence he takes it that whatever reasons count in favor of the functional-state hypothesis count also against the brain-state hypothesis. Putnam announces that his strategy "will be to argue that pain is not a brain state, not on *a priori* grounds, but on the grounds that another hypothesis is more plausible." In fact, he starts by *defending* the brain-state hypothesis against some attempted *a priori* refutations.

I do not think Putnam has shown that pain cannot be *both* a brain state and a functional state, these being identical. The concept of any functional state as such does, of course, differ from the concept of any brain state as such. But Putnam is alive to the possibility that different concepts might be concepts of the same state; this observation is part of his own defense of the brain-state hypothesis against a priori objections. Suppose pain is indeed a certain functional state $S_{17}$ in an appropriate functional description; suppose the description is realized *inter alia* by the human brain states $B_1, \ldots, B_n$, respectively. Those are the states that are lawfully related to one another, and to suitable human inputs and outputs, by the proper transition probabilities. Why not conclude that pain $= S_{17} = B_{17}$ (in the case of humans)? On this view, a functional state is better called a *functionally specified* state, and might happen to be a functionally specified brain state.

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Machines," in *Intentionality, Minds, and Perception*, ed. H.-N. Castañeda (Detroit: Wayne State Univ. Press, 1966); "Robots: Machines or Artificially Created Life?" this JOURNAL, LXI, 21 (Dec. 12, 1964): 668–691.

Putnam argues that the brain-state hypothesis (and with it, the functionally-specified-brain-state hypothesis) ought to be rejected as scientifically implausible. He imagines the brain-state theorist to claim that all organisms in pain—be they men, mollusks, Martians, machines, or what have you—are in some single common nondisjunctive physical-chemical brain state. Given the diversity of organisms, that claim *is* incredible. But the brain-state theorist who makes it is a straw man. A reasonable brain-state theorist would anticipate that pain might well be one brain state in the case of men, and some other brain (or nonbrain) state in the case of mollusks. It might even be one brain state in the case of Putnam, another in the case of Lewis. No mystery: that is just like saying that the winning number is 17 in the case of this week's lottery, 137 in the case of last week's. The seeming contradiction (one thing identical to two things) vanishes once we notice the tacit relativity to context in one term of the identities. Of course no one says that the *concept* of pain is different in the case of different organisms (or that the *concept* of the winning number is different in the case of different lotteries). It is the *fixed* concept expressed by 'pain' that determines how the denotation of 'pain' varies with the nature of the organism in question. Moral: the brain-state theorist cannot afford the old prejudice that a name of a necessary being (such as a state) must name it necessarily and independently of context.

IV

Representing religion, there are two papers. The first is George Nakhnikian's "St. Anselm's Four Ontological Arguments." [2] The four arguments, "strongly suggested, if not explicitly found, in St. Anselm," are presented in modern dress and examined one by one. Their validity is unchallenged; however, each is held to depend on at least one premise that the fool has no reason to accept. Can we fools who have said in our hearts "There is no good ontological argument" rest content with Nakhnikian's diagnoses? Not, I fear, in every case.

My deepest misgivings concern Nakhnikian's rejoinder to the second argument. He says that, whereas existence may be a property, there is no such property as necessary existence; and therefore the premises

(1) It is possible that there exists a being whose nonexistence is impossible.
(2) If it is possible that $x$ does not exist, and it is possible that there exists a being whose nonexistence is impossible, then it is possible that there exists a being greater than $x$.

[2] Of which another version is available in Nakhnikian's *Introduction to Philosophy* (New York: Alfred A. Knopf, 1967).

of the second argument are false. In the first place: (2) is a conditional, and (1) is a conjunct of its antecedent; so how can (1) and (2) *both* be false? But more important: why is there no such property as necessary existence? We are invited to notice that in

He has the necessary courage to face the danger

we are ascribing just courage, not the property of necessary courage.

Granted. It by no means follows that we cannot find some better way to ascribe the property of necessary courage—or of necessary existence. These properties give no trouble on the usual semantic analyses of quantified modal logic in terms of possible worlds and the inhabitants thereof: the property of necessary existence is the property belonging to just those possible individuals which are present (or have counterparts, if you want to be wary of inter-world identifications) in every possible world.

It will not do to replace "God has the property of necessary existence" by "It is a necessary truth that God exists," as Nakhnikian proposes; the substitute does not mean at all the same as the original, as St. Anselm already knew. Imagine that there are just two possible worlds, the actual and one other. Suppose the actual Occupant of the role of God is present (or has a counterpart) in the other world, but not in the role of God; in fact, the role of God is unoccupied there. Then the original is true and the substitute false. Next suppose instead that the actual Occupant of the role of God is absent (and has no counterpart) in the other world; but the role of God is occupied in the other world by some very different being. Then the original is false and the substitute true. So far as I can see, both counterexamples are consistent, though perhaps inconsistent with truths of theology.

## V

Ninian Smart's "Mystical Experience" [3] questions R. C. Zaehner's distinction between two types of mystical experience: monistic and theistic.[4] There are certainly two types of *reports* of mystical experience: a monistic mystic reports an experience of "realising the eternal oneness of one's own soul," a theistic mystic reports an experience of encountering God. If you are content to take the mystics at their word, you conclude that the two types of reports report two types of experience; that is Zaehner's hypothesis. Smart advises more caution,

[3] Part of "Interpretation and Mystical Experience," *Religious Studies*, 1 (1965): 75–87.

[4] Presented in R. C. Zaehner, *Mysticism Sacred and Profane* (New York: Oxford, 1957), and elsewhere.

and puts forth an alternative hypothesis to explain the two types of reports: perhaps the mystics all had more or less the same experience, but they interpreted and reported it differently. Smart is somewhat inclined to accept his hypothesis, but does not defend it; he merely contends that Zaehner's hypothesis is unproved.

I do not know what possible evidence could strongly support Smart's hypothesis over Zaehner's; we cannot expect mystics to become proficient at giving purely phenomenological accounts of their experience, guaranteed free of extraneous interpretation. (It's hard enough for *ordinary* experience!) But there could be evidence strongly supporting Zaehner's hypothesis. Suppose we find a mystic who reports:

> At times $t_1$, $t_3$, and $t_5$ I had an experience of realizing the eternal oneness of my soul; at times $t_2$, $t_4$, and $t_6$ I had an experience of encountering God; the two experiences are very different indeed.

I would be interested to know if there has been any such versatile mystic; if there has, he cannot very well be explained on Smart's hypothesis.

Smart's hypothesis presupposes that, although the mystic's reports may be contaminated with interpretation, the reported experience itself is not. Either the interpretation is retrospective, or the mystic's total experience can somehow be analyzed into two distinct parts: a core experience, and interpretation thereof. If interpretation is inseparable from the mystical experience, it cannot happen that two mystics have the same experience but interpret it differently. In that case Zaehner's hypothesis wins by default.

Epistemologists have long struggled, with little success, to make sense of the notion that ordinary perceptual experience can be analyzed into a sensory core and interpretation thereof. Phenomenologists of mysticism might do better, but we should not count on it. (They are worse off than the epistemologists; at least we have some notion what the sensory core of visual experience is supposed to be, viz., a pattern of light and color.) Perhaps philosophers are wasting their time on the question how, if at all, experiences can be analyzed into parts; this may be a question to be settled by the construction and testing of psychological theories, not by conceptual analysis.

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