

LAUDABLE GOALS, INTERESTING EXPERIMENTS, UNINTELLIGIBLE THEORIZING: A CRITICAL REVIEW OF *RELATIONAL FRAME THEORY*

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ABSTRACT: An assessment of *Relational Frame Theory* (RFT) is benefited by a distinction among goals, experiments, and theorizing/philosophizing. The goals are laudable, but not new. The experiments are interesting, but they largely involve an expansion of the concept of relational responding from equivalence to nonequivalence relations, the obvious next step. The theorizing, where RFT's bona fide novelty supposedly lies, I found to be ambiguous, opaque, and contradictory. Inasmuch as unintelligibility allowed me to understand, I found RFT to be a hypothetico-deductive and essentialistic proposal that amounts to little more than applications of basic set-theoretic (class and membership) and logical concepts (negation, material implication, biconditional) to verbal behavior.

Key words: relational frame theory, hypothetico-deductivism, essentialism, set theory, logic

The book *Relational Frame Theory* (2001) is the result of a collective effort to synthesize the multiple aspects of a proposal that, until now, has been scattered throughout journal articles, book chapters, and conferences. The book consists of two parts. Part I is entitled "The basic account" and presents the basic concepts, justification, philosophy, and experimental research of relational frame theory (RFT). Part II is entitled "Extensions and applications" and presents some of what the authors refer to as "implications" (p. 155) of RFT for a wide variety of topics (viz., development, education, social processes, psychopathology, and even religion and spirituality).

An assessment of the book is aided by a distinction among goals, experiments, and theorizing. The general goal of understanding verbal behavior is laudable, but not new. The experiments are interesting, but they largely represent an extension of the kinds of methods used in behavior-analytic equivalence-relation research to nonequivalence relations, the next obvious step, which, of course, does not diminish its importance. The theorizing (i.e., the concepts, logic,

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and justification), where RFT's bona fide novelty supposedly lies, I found to be unintelligible. Such an unintelligibility prevented me from making an unequivocal determination of *specific* goals (beyond the sketchy cliché of “prediction and control”), how they were to be achieved, and the significance of the experimental results for the achievement of any goal. I will thus focus on Part I, and, within it, on the main concepts, logic, and justification. Therefore, the present review should be seen as a conceptual/logical/philosophical *complement* to reviews that focus on the more experimental aspect of the book.

Part I consists of eight chapters. In Chapter 1, the authors do three things. First, they summarize the Skinnerian (within which they place Willard Day's and Kurt Salzinger's) and interbehavioral approaches to language. Second, they identify the philosophy of RFT with “a type of pragmatism [they] have called functional contextualism” (p. 6).¹ Third, they attempt to explain why none of the standard approaches has led to “a vibrant research program” (p. 10). In particular, their explanation regarding the Skinnerian approach arises from the idea that Skinner's definition of verbal behavior is “not functional” (pp. 12-13) and “too broad” (pp. 13-15).

In Chapter 2, the authors present the basic concepts of RFT, a central one being “relational responding,” which the authors regard as learned behavior. Specifically, it is conceptualized as an “overarching,” “generalized,” or “higher order” (p. 23) operant, where by “operant” they mean the standard Skinnerian notion of “a functionally-defined class of responses” (p. 23). Another feature of relational responding is that it is “arbitrarily applicable” (p. 25) and contextually controlled, where by “arbitrarily applicable” is meant “under the control of cues that can be modified on the basis of social whim” (p. 25). In this chapter the authors also present a classification of relational frames into “families” dubbed with terms such as “coordination,” “opposition,” and “comparison,” among others.

In Chapter 3, the authors elaborate these basic concepts into the notions of multiple stimulus relations and transformation of stimulus functions. In Chapters 4

¹ The authors' talk of “[t]he more contextualistic assumptions and approaches of the *bulk* of modern behavior psychology” (p. 5, emphasis mine) could be interpreted as asserting that behaviorism is a predominantly contextualistic philosophy. If “contextualistic” refers to Pepper's (not Popper's!) view, the assertion is a gross misrepresentation of behaviorism. For better or worse, Pepperian contextualists constitute a minority among behaviorists. That does not necessarily make Pepperian contextualism false, of course. I don't believe such a doctrine has been formulated in a sufficiently intelligible way as to allow for an unequivocal assessment. I'm only making sure that potential nonbehaviorist readers do not get the wrong impression that behaviorism is predominantly contextualistic. The authors may be relying on interpretations of radical behaviorism as a form of pragmatism (e.g., Smith, 1986; Zuriff, 1985) and believe that pragmatism entails contextualism. However, even if such interpretations were correct, talk of contextualism as a “type of pragmatism” implies that there is such a thing as noncontextualistic pragmatism (unless the authors adopt a nonstandard sense of “type”). Hence, pragmatism does not entail contextualism, for apparently one can be a pragmatist but not a contextualist. Besides, “pragmatism” in those interpretations refers to the truth theory advanced by James (1907, 1911), and it is not obvious that functional contextualism is a form of Jamesian pragmatism. In some of the ensuing commentaries, I shall follow those interpretations and use “pragmatism” to refer to Jamesian pragmatism, although in my concluding remarks I shall reject it as the worst philosophical guide one could adopt.

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through 7 all of these notions are applied to analogies, metaphors, stories (through the notion of a relation of relations), thinking, problem solving, the self, and self-directed rules. Chapter 8 is what the authors call a “*précis*” of RFT, where they “summarize some of the key features of RFT and . . . address some of the common behavioral criticisms to this approach” (p. 141).

Before I make specific commentaries, I must mention two stylistic matters that dampened my enthusiasm from the beginning. First, in the Preface, one reads that “[a] new day has dawned” (p. xii), which supposedly refers to RFT. This phrase struck me as a rather tawdry self-praise that reminded me of the obnoxious parents who say “we’ve got the best kids in the world.” There is such a thing as believing too much in a theory, no matter how provocative, interesting, or potentially fruitful. Believing too much in a theory is suspicious enough, regardless of the believers; never mind when the believers are the very authors of the theory.

In the context of such self-praise, talk of “those *special few* who will consider [their] arguments seriously” (p. xiii, emphasis mine) is baffling. What could the authors possibly mean by such an unnerving phrase? Do they really expect that only a few readers will consider their arguments seriously? If so, why? And if not, why would they talk of those “special few who will consider [their] arguments seriously”? And what do they mean by “special”? Do they mean that those “few who will consider [their] arguments seriously” will comprise an exceptional, rare, unique group, a sort of intellectual elite that is above and beyond all other research groups in behavior analysis and perhaps even psychology? To be completely honest, I found the authors’ tone here (and throughout the Preface in general) to be too pretentious. To me, such a tone denotes a lack of wisdom, which I couldn’t but see as an ominous sign of things to come.

My point with all this is that the thing I need least when I am presented with an unfamiliar theory is praise (let alone *self-praise*) for it. A theory should be evaluated in and by itself, regardless of anyone’s (especially the authors’) praise. Praise is just a marketing strategy used by editorials to sell books, and self-praise is not precisely the most objective praise. If the authors’ intention with their self-praise was to infect readers with their enthusiasm for RFT, it had the opposite effect on this reader. To be sure, the authors are not the first (and won’t be the last) ones to engage in self-praise, but that’s no excuse.

The second aspect of style was the authors’ warnings that the material in Part I was “*inherently* difficult” (p. 1, emphasis mine). The authors even say that they “have learned from experience that even sophisticated readers readily misunderstand many of the basic concepts of Relational Frame Theory” (p. 141). So, it is the readers’ fault. The theory is clear, precise, and coherent, but sophisticated readers aren’t sophisticated enough to understand it. Apparently, to understand RFT one has to possess a special kind of intellect reserved for the “special few” who are “willing to take the journey.”

To such warnings, I can only say that if RFT is really that *inherently* difficult, even its authors should have problems in understanding it. This challenge can be met in three ways. First, the authors could acknowledge that, indeed, even they find their own theory difficult to understand. Second, they could consider

themselves to possess the special kind of intellect that is supposedly required not only to understand but also to create a theory that even sophisticated readers “readily misunderstand.” Third, “sophisticated readers readily misunderstand” RFT because it *is* obscure, ambiguous, and incoherent. Perhaps, it is the writers’ fault, the possibility I will consider in the present review.

The ensuing commentaries will be of two types. In one type, I point out those occurrences of unintelligibility that I found to be particularly acute, in virtue of arising from the fundamental concepts, logic, and justification of RFT. In the other type, I criticize some of what I understood from the authors’ discourse, in spite of its unintelligibility. Given the strategy of blaming the reader for not being sufficiently sophisticated to understand an otherwise clear, precise, and coherent proposal, I anticipate that the present review will be dismissed by the authors and their followers as yet another case of misunderstanding. My only reply would be that, indeed, I do not possess the kind of intellect required to understand unintelligible proposals. Alas, I’m not sure I want to possess such an intellect.

Throughout the review, I ambiguously refer to “the authors,” by which I mean the corresponding *chapter* authors. My reason is to avoid unnecessary repetition, for the authors of most chapters are the same as the editors (there is not a single chapter that is not coauthored by at least one of the editors, and most of the chapters are coauthored by two).

Social Mediation

Part of the justification of RFT, as presented in Chapter 1, is a rejection of the Skinnerian definition of the concept of verbal behavior as behavior whose controlling contingencies (specifically, its *consequences*) are socially mediated. In this regard, the authors write:

Skinner purported to give a functional definition of verbal behavior, but the definition turns not on the history of the organism of interest, but on the history of another organism. In no other area of behavioral thinking is a functional response class defined in this way. Except in the domain of verbal behavior, “functional definitions” are always definitions stated in terms of the history of the individual organism and the current contextual circumstances. The different definitional strategy Skinner used to define verbal behavior can lead to results that are behaviorally bizarre. (p. 12)

The authors then consider the case of a rat pressing a bar in an operant-conditioning chamber under some experimenter-arranged reinforcement schedule. Their core argument here is that such a behavior is controlled by contingencies that are socially mediated, insofar as reinforcement is mediated by an experimenter whose behavior has been socially (and, to that extent, verbally) conditioned. Hence, the argument goes, the barpressing qualifies as verbal behavior under Skinner’s definition, which they regard as “behaviorally bizarre.” The authors provide textual evidence that even Skinner acknowledged that implication (see p. 14). I have four comments in this regard. First, bizarreness is not as bad as the

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authors seem to believe. Second, the authors' definition of "verbal behavior" leads to equally bizarre results. Third, even if bizarreness were that bad, it could be substantially reduced, at least in Skinner's definition. Fourth, the authors' definition of the concept of verbal behavior seems to rely on the notion of social mediation. Let me elaborate. It should be clear that the ensuing discussion does not represent a defense of Skinner's (1957) view of verbal behavior. Rather, my main point will be that a rejection of that view, based on a rejection of its definition of verbal behavior, misses the mark.

As I see it, what Skinner did was to *expand* the domain of functional definitions. This expansion allows for descriptions of relations between the histories of two or more individuals, which is the obvious conceptual step towards functional analyses of social behavior. I will make a point about definitions towards the end of the review. For the moment, suffice it to say that they (or anything else in science and philosophy, for that matter) are not written on stone. They are means, not ends. Transferring Humpty-Dumpty's wise maxim, we must be the masters, not the slaves of our definitions. Definitions are supposed to be flexible, changeable, adaptable to our purposes, and that's what we find in Skinner's expansion. By restricting functional definitions to individual organisms, the authors have become slaves of such definitions.

What the authors call "conceptual error" (p. 13) and "key flaw" (p. 15), then, I call "conceptual flexibility," and what they call "the definitional problem" (p. 13), I call "the definitional expansion." By pretending to restrict functional definitions to single organisms, the authors show a conceptual rigidity that shuts the door to functional analyses of social behavior. To be sure, Skinner's change, *as is*, does lead to behaviorally bizarre results. However, on the one hand, bizarre results are a natural outcome of conceptual change (think of the bizarre results that have arisen from conceptual change in relativistic, quantum, and superstring theory). Bizarre results have not stopped scientists from embracing conceptual change, and I don't see why behavior analysts have to be the exception. The authors may well be right in that leading to bizarre results is a major reason why *Verbal Behavior* "did not lead to a progressive research program" (p. 11). If they are (a big "if"), far from justifying the absence of a "vibrant research program" (p. 10) based on *Verbal Behavior*, what that reason does is to expose a major mistake underlying that absence: Emphasizing bizarre results of conceptual change over the potential scientific progress it may allow.

On the other hand, take a mand episode where person A utters "Please, pass the salt" while looking at person B, and B immediately passes the salt to A. Under Skinner's definition, A's utterance qualifies as verbal behavior, not because of its topography or the fact that A is human, but because its consequence is socially mediated by B. Would the authors consider this result as bizarre? If not, then why is viewing the rat's barpressing as verbal behavior bizarre? Clearly, not because of the mediation per se (there's nothing bizarre about it) but because of the fact that rats are nonhumans and/or barpressing is not topographically linguistic. When it comes to assessing definitions of verbal behavior, then, the nature of the species and/or the responses seem to be quite important for the authors.

Moreover, under a relational-frame notion of verbal behavior, A's utterance does not qualify as verbal behavior if it does not "participate" (whatever that means) in a relational frame, which is equally bizarre. If bizarreness amounts to *pretheoretic counterintuitiveness*, rejecting an utterance such as "Please, pass the salt" is as bizarre as regarding the rat's barpressing as a case of verbal behavior. One bizarreness is thus replaced by another, so the issue becomes what bizarreness is the least bizarre, which is largely undecidable. But then again, I don't believe bizarreness is that bad. I thus agree that viewing barpressing (like not viewing an utterance such as "Please, pass the salt") as verbal behavior is bizarre, but I really don't care.

Just in case the reader (like the authors) cares, however, let me show that bizarreness can be substantially reduced in the case of Skinner's definition. The fact that the experimenter's behavior of mediating reinforcement of the rat's barpressing has been socially conditioned does not imply that the mediation is "social" (unless one is engaged in a search for the "essence" of social relations; more on essences later). The issue, then, is not that Skinner's definition of verbal behavior was too broad, but that his use of the term "social" was too broad. If we restrict "social" to certain interactions among members of the *same* species, as it is usually done in behavioral ecology and sociobiology (e.g., predator-prey relations are not typically regarded as social), the bizarre result does not obtain, at least in the kind of situation analyzed by the authors. No relation between an experimenter and a rat (or members of any other two species) thus qualifies as "social," so no behavior in the latter qualifies (technically) as verbal, no matter how much it is mediated by a socially-conditioned experimenter.

The bizarre result obtains in those cases where consequence-mediation occurs thanks to the behavior of other members of the same species. For example, consider the standard yoke arrangement where two operant-conditioning boxes are rigged such that responses in one determine the contingencies in the other. If the two boxes contain members of the same species (say, two rats, two pigeons, or two humans), the reinforcements received by the yoked subject are socially mediated by the master's behavior. The relation qualifies as social and the yoked-subject's behavior qualifies as verbal. The bizarre result arises again, but it is restricted to a more specific kind of arrangement.

One could adjust the definition of social mediation in different ways to reduce the domain of bizarre outcomes even more. For instance, one could restrict the term "social" to relations where members of the same species have a certain minimal level of spatiotemporal proximity as to allow for some sort of sensory contact (e.g., if they can see, touch, smell, and/or hear each other; sensory contact with products of each other's behavior could also be included here, to allow for mail correspondence as a form of social behavior). This restriction is also standard in behavioral ecology and sociobiology. Experimental preparations in which this restriction is satisfied are those where two or more members of the same species are placed within the same experimental environment (e.g., see Galef, 2001; Schuster, 2001). In these preparations, subject interactions do qualify as social, for which, under a social-mediation definition of

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emitted by one of the organisms and is reinforced through some response that is emitted by the other, qualifies as verbal. The bizarre result obtains once again, but it is restricted to an even more specific kind of study. Far from encompassing “*all animal operant studies*” (p. 14), the bizarre result (again, if it matters that much) is circumscribed to very few ones.

On closer inspection, however, the authors seem to adopt a social-mediation definition after all. In Chapter 2, for example, one reads:

We mean *arbitrarily applicable* simply in the sense that in some contexts this response is under the control of cues that can be modified on the basis of *social whim*. (p. 25, last emphasis mine)²

Social whim thus seems to define at least arbitrarily applicable (but apparently not nonarbitrarily applied) relational frames.³ Arbitrary applicability, in turn, defines relational frames, as stated by the authors themselves in Chapter 5: “arbitrary applicability is a *core defining feature of relational frames*” (p. 89, emphasis mine). If social whim defines arbitrary applicability and the latter defines (arbitrarily applicable) relational frames, then social whim defines (arbitrarily applicable) relational frames. This outcome is consistent with the following quote:

Relational frames are arbitrarily applicable *in the sense that* cues can be provided for relational responses based on *social whim or convention*. This is the property that makes relational framing *inherently a form of social behavior*. Indeed, *much as in Skinner (1957)*, the training history of the “*social mediator*” is *particularly important for that reason*. (p. 150, all emphases mine)

² This paragraph strongly suggests a rejection of biological constraints on verbal learning. This rejection becomes more obvious in the authors’ discussion of the arbitrary character of verbal behavior, where they say: “*Any event can be brought into any relation with any other event, verbally speaking*” (p. 47, emphases mine). If one reads “any” literally, then the unavoidable conclusion is that there are *no* biological constraints whatsoever on verbal learning, which, of course, is an indefensible tenet. If the authors did not literally mean “any,” then they must acknowledge that (aside from being misleading) there are *some* biological constraints on verbal learning, for which the latter is not as “arbitrary” as the authors seem to contend.

³ The authors make the distinction between “arbitrarily applicable” and “nonarbitrarily applied” in Chapter 5, but I found it to be particularly opaque. At times, it would seem to amount to a distinction between relational responding as it occurs in artificial, laboratory settings, and relational responding as it occurs in natural-language settings, respectively. At other times, the distinction would seem to refer to one between that which could have happened and that which actually happened (e.g., this subject actually responded in such and such way to such and such stimulus arrangement, but he would have responded in the same way had he been presented with a different arrangement). Also, the distinction seems to be a sharp one: “[W]hile arbitrary applicability is a core defining feature of relational frames, in fact their use in natural language is *anything but arbitrary*” (p. 89, emphasis mine). My reading is that there is *nothing* arbitrary about relational frames in natural language. However, the next two phrases read: “When relational frames are applied nonarbitrarily, the source of control is mixed. The relevant history involves both arbitrary training and nonarbitrary features of the environment, and the regulation of relational responses in natural settings usually comes from both arbitrary and nonarbitrary domains” (p. 89). So there seems to be something arbitrary about nonarbitrarily applied relational frames after all, although, again, exactly what it is eludes me.

I cannot but read this quote as asserting that social whim defines arbitrarily applicable relational frames, that it is a kind of social relation, and that it plays a mediating role. Putting it all together inevitably leads to the conclusion that the RFT definition of verbal behavior relies on the notion of social mediation as much as Skinner's. To be sure, the kind of mediation provided by social whim in (arbitrarily applicable) relational frames is different from the one provided by the contingency mediator in Skinner's definition. What is socially mediated in (arbitrarily applicable) relational frames is the *antecedent* stimulation. But social mediation seems to be at "the core" of both definitions of verbal behavior.

Immediately after the last quote, however, one reads that "[t]he history of the audience does not define the functional unit of language in RFT" (pp. 150-151). If "relational framing is *inherently* a form of social behavior" and "*the training history of the social mediator is particularly important for that reason,*" I don't see how the history of the audience (or the social history of the experimenter) does not define the functional unit of relational framing. It would seem that by "inherently" and "particularly important" the authors do not mean "defining." But what do they mean then? Moreover, what do they mean by "defining" and "define"? I shall address this issue later. For the moment, let me comment on other notions that seem to be at the core of the notion of a relational frame.

Emergence and Context

The authors specify in Chapters 2 through 5 other notions that (apparently) characterize relational frames. Key terms here are "emergent," "derived," "transformed," and "contextually controlled." It is unclear whether the first three are synonyms. If they are, using three different terms to talk about the same concept can only make the discourse more confusing. If they are not, I have no idea exactly how they differ. In any case, I suppose the authors adopt the standard sense of "emergence" found in behavior-analytic equivalence-relation experimental research, where it refers to "not explicitly trained" or "without further training." A central notion of that research thus is also central to RFT, so the latter will inherit any problem with the notion.

One problem is that many (topographically) nonverbal behavioral phenomena in nonhumans qualify as emergent in the above sense, so emergent responding is far more general. For example, nonverbal Pavlovian generalization in nonhumans qualifies as responding to stimuli that have not been explicitly trained, so it is emergent responding precisely in the above sense. Moreover, it qualifies as relational responding, for one can view subjects as responding comparatively, and hence, according to what the authors call the "relational-frame family of comparison," which:

is involved whenever one event is responded to in terms of a quantitative or qualitative relation along a specified dimension with another event. Many specific subtypes of comparison exist (e.g., bigger-smaller, faster-slower, better-worse). (p. 36)

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This definition is satisfied by nonverbal Pavlovian generalization: One event (a test frequency that has never been trained) is responded to in terms of a quantitative relation (frequency difference in Hz) along a specified dimension (frequency) with another event (the trained frequency).⁴ The specific subtype of comparison here is “higher pitch-lower pitch.” The authors thus are mistaken if they assert that emergent responding is confined to verbal behavior, unless by “emergent” they mean something above and beyond “not explicitly trained.”

As for contextual control, it is a well-established nonlinguistic-conditioning phenomenon, especially in Pavlovian conditioning (see Balsam, 1985), so there is nothing especially new or linguistic about it. Furthermore, nonlinguistic Pavlovian (e.g., Hulse, Cynx, & Humpal, 1985) and operant (Thomas, 1985) generalization are context-dependent. If contextual control is critical to nonverbal Pavlovian and operant generalization, and if one regards them to be critically involved in verbal behavior, then evidently contextual control is critical to verbal behavior. The authors are thus asserting the obvious if they assert that contextual control is critical to verbal behavior. However, they are mistaken if they assert that contextual control is confined to verbal behavior, unless, again, by “contextual” they mean something different from the standard usage of the term in context conditioning.

The authors might concede that emergence and contextual control are not confined to verbal behavior but still reject that contextually-controlled Pavlovian and operant nonverbal generalization qualify as genuine cases of relational responding. That is to say, they might view relational responding as the kind of emergence and contextual control uniquely found in verbal behavior. Or they might even concede that relational responding is not confined to verbal behavior but argue that verbal relational responding is different from nonverbal relational responding. In any case, what is the difference between the verbal and the nonverbal? The behaviorally sensible answer would be that verbal relational responding is a certain *phenomenon*, a kind of *result* that is obtained by applying a kind of *method*. If that is the case, RFT (much like behavior analysis) largely amounts to a *methodological* proposal, for which it might as well be called “Relational Frame Method” (RFM).⁵ But then why call it RFT instead of RFM? Specifically, what does the “T” in “RFT” mean?

⁴ Of course, much depends on what one means by “in terms of.” Moreover, it is unclear how the authors distinguish between “responding in terms of” and “responding to,” or even if “responding to” applies to relational responding at all. In the absence of an explicit definition of what is meant by “in terms of,” talk of a rabbit blinking “in terms of” a frequency difference is perfectly legitimate.

⁵ Here I am using the terms “methodological” and “method” in the sense familiar to experimental psychologists, which refers basically to the method of an experimental study. Philosophically speaking, “methodology” roughly signifies “critical reflection on method” (or “method theory”), where “method” refers to a mode of thought or way of justifying beliefs. In the next section, I will adopt this philosophical sense, in the context of the induction-deduction issue.

Theories, Inductivism, and Hypothetico-Deductivism

The authors address the issue in the first section of Chapter 8, under the heading “Theory.” According to their characterization, RFT is a theory in that it is “an abstraction built up for a functional analytic approach to derived stimulus relations” (p. 144). More explicitly:

The relationship between behavioral principles and behavioral theories parallels precisely the relationship between behavioral observations and behavioral principles. In both cases, the shift is *from the specific to the general case*. . . . Relational Frame Theory is a theory in this specific sense. (p. 144, emphasis mine)

Even more explicit is the following statement: “In keeping with the *inductive nature* of behavior analysis, this concept of the relational operant will gain or loose strength through basic and applied research, rather than logical analysis *per se*” (p. 148, emphasis mine). The authors thus present RFT as a theory precisely in that it is an *inductive generalization*. This usage of “theory” follows Skinnerian conventional wisdom (e.g., Chiesa, 1994, pp. 134-143; Skinner, 1969, pp. vii-xii), so the ensuing criticism transcends the book, insofar as it applies to that wisdom. But the criticism is especially applicable to the book, for two reasons. First, the authors take that wisdom to an extreme that, contrary to their self-proclaimed anti-foundationalistic stance (see p. 34), involves an ontological assumption. Second, the authors are inconsistent with their self-proclaimed inductivistic stance. Let me elaborate.

The criticism in question is that the Skinnerian usage of “theory” amounts to little more than a *redefinition* of the term. As is well known, the standard sense of “theory,” the one rejected by Skinner (1950), was central to logico-positivistic philosophy and reserved for statements about things, properties, or events that were *unobservable in principle* (through *any* kind of observation method).⁶ So why

⁶ This sense was adopted by Mach (1893/1960), who (in contrast to radical behaviorists) never proposed to redefine the term “theory.” In particular, he never used it as a synonym for inductive generalization. If he had, such a thing as the problem of theoretical terms would have never arisen in the philosophy of science. Nor did he reject that sense of “theory” *per se*, but rather its metaphysical import. In contemporary jargon, he was an *anti-realist* or *instrumentalist*, not an anti-hypothetico-deductivist. The same basic sense of “theory” was adopted by the logical positivists, who elaborated it through the application of the new, post-Aristotelian, formal logic (see later). In their elaboration, a theory became a conjunction $T \wedge C$, where T denoted a set of theoretical laws, “ \wedge ” the logical operator “and,” and C a set of correspondence rules. Theoretical laws consisted only of theoretical terms, which referred to things, properties, or events that were observable only indirectly, through the application of highly sophisticated instruments. Correspondence rules were the logical bridges that allowed for the deduction of empirical laws, which comprised the theory’s predictions (or explanations, as the case might be), from the theoretical laws. Empirical laws consisted only of observational terms, which referred to things, properties, or events that could be directly observed, through either the natural sensory organs or relatively unsophisticated observation instruments. From this perspective, an important part of the philosopher’s job consisted in reconstructing scientific theories in the language of formal logic, to unveil their theoretical laws and correspondence rules, and to determine their logical consequences (although, strictly speaking, these were not parts of theories).

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not use “theory” in this standard sense? Why redefine the term? Of course, everyone has the right to use any term in any way. However, the issue is not whether one *can* use any given term in any way one pleases, but *why* do it. So why use “theory” as a synonym of “inductive generalization,” aside from merely having the right to do it?

To me, the redefinition in question is a self-granted license to claim that one’s science is as theoretical as the next person’s, as a defense against the criticism of “anti-theory.” In the hands of behavior analysts and RFT researchers alike, and to perloin the authors’ metaphor (see p. 89), the word “theory” has become a veritable “weasel word” used as an “escape hatch” to evade that particular criticism. The authors’ assertion in this respect is most symptomatic: “The resulting misperception that behavior analysis rejects theories is ironic because it is one of the most theoretically oriented fields in all of psychology” (p. 143). What is ironic is that such an assertion is a clear example of the fallacy of equivocation: Conveniently shifting the meaning of a term to make a point. By the trivial device of labeling inductive generalizations “theories,” they are able to conclude that behavior analysis is “theoretical,” for which considering it as “anti-theoretical” is a “misperception.” Playing Devil’s advocate, I would reply something like this: “Well, you are *calling* what you do ‘theory.’ But I am not criticizing you for not *using the word* ‘theory.’ I am criticizing you for being an anti-hypothetico-deductivist. Using ‘theory’ in a different sense, as legitimate as that might be, does not answer my criticism. We might as well drop the term and my criticism would remain unanswered.”⁷

The nontrivial defense, of course, is to directly reject hypothetico-deductivism (e.g., Sidman, 1960, pp. 4-7), without committing the fallacy of equivocation or getting ensnared in the semantics of the term “theory.” That is, unless one is searching for the essence of theories. Incredibly, the authors engage in such an endeavor, and this is the first reason why my criticism to the Skinnerian usage of “theory” is especially applicable to its perpetuation by the authors. On page 143, one reads: “This is the *essence* of theory from a behavior analytic point of view” (emphasis mine). What could the term “essence” possibly mean here?

⁷ To be sure, the term has been redefined in post-positivistic philosophy of science, but not to refer to inductive generalizations, express an anti-hypothetico-deductive stance, or escape from the anti-theory criticism via the fallacy of equivocation. The logico-positivistic program was never realized, not only because of the excessive power of symbolic logic, which made the program practically unrealizable, but especially because of the extreme ambiguity with which logical-positivists made the observational-theoretical distinction. This situation eventually led to a rejection of the logico-positivistic (sometimes called “received”) view of scientific theories in favor of alternative views, of which the predominant ones are the structuralist (e.g., Balzer, Moulines, & Sneed, 1987; not to be confused with linguistic or psychological structuralism) and the semantic view (e.g., Suppe, 1989). The main objective of such redefinitions has been to obtain a clearer, more precise, and philosophically more fruitful concept of a scientific theory. Behaviorists (and psychologists in general) are still discussing about the need (or not) of theories under a view that has been amply shown to be inherently unintelligible and, when made intelligible, ultimately inadequate.

As is well known, the traditional sense is the Aristotelian one, according to which an essence of x is a property without which x could not exist.⁸ This sense certainly involves an ontological assumption, that is, that essences exist objectively (i.e., wholly independently of any knowing subject) and that theories can have them. But if the authors adopt this sense, they contradict themselves when they say, “the pragmatic qualities of contextualistic thinking preclude foundationalism and other kinds of *ontological assumptions*” (p. 34, emphasis mine).⁹ The only way to eliminate the contradiction and keep an anti-foundationalistic stance is to reply that “essence” is being used in a non-ontological sense, but what sense could that be? No answer is to be found in the book. The authors might reply that by using the term “essence” they are not presupposing anything, but in the absence of an explication of that term, such a reply would be little more than hand-waving.¹⁰

The second reason why my criticism of the Skinnerian sense of “theory” is especially applicable to the book arises from the fact that, in the first section of

⁸ This meaning is so standard that it is the official one found in a standard dictionary: “the permanent as contrasted with the accidental element of being; the individual, real, or ultimate nature of a thing” (*Merriam-Webster’s Collegiate Dictionary*, 10th Ed., 1998). A more precise and technical sense is found in the notion of necessity *de re*, which can be expressed in the formalism of modal logic as follows: For an individual x and property F , F is an essential property of x if, and only if, $\Box(\text{Ex} \rightarrow Fx)$, where “ \Box ” denotes “necessarily,” “E” denotes “exists,” “ \rightarrow ” is the material-implication operator (read as “if . . . , then”; see later), x is an individual variable, and “ Fx ” signifies “ x is F .” The *de re* character of this formula is given by the occurrence of a free variable (i.e., a variable that is not within the scope of a quantifier) within the scope of a modal operator (in this case, “ \Box ”). Under this interpretation of “essence,” the authors’ assertion that inductiveness is essential to theories, on the one hand, endows theories with existence and takes them as individuals, and, on the other, assumes (under a possible-world semantics of modality) that in every possible world where they exist, they are inductive generalizations. Such assumptions raise formidable difficulties, so ascribing essences (especially to theories) is not something to be done cavalierly.

⁹ The contradiction becomes even more evident if one reads the continuation of this quote: “Operants are analytic units that analysts adopt for specific purposes—*they are not things*” (p. 34, emphasis mine). Obviously, to assert that an operant (or anything else, for that matter) is not a thing is as much an ontological assumption as asserting that an operant is a thing. The negation of an ontological assertion is itself an ontological assertion.

¹⁰ A frequent non-ontological usage of “essence” refers to “a property that is critically relevant.” But why is it so relevant? A typical non-ontological answer would be something like “because it is what I am interested in,” or “because it is what I have chosen to focus on.” But such answers reduce the whole issue to a matter of personal interest, where discussion is futile and even gratuitous. The pragmatic answer would be that an essence is any property whose predication is useful in achieving certain goals. However, that answer only justifies choosing certain properties as essences, without excluding belief in their objective existence (the general belief in essences and the choice of specific essences are quite different matters). In fact, that belief has been quite useful for generations of philosophers, so the kind of ontological foundationalism the authors reject is amply warranted even within pragmatism. The fact that the authors (apparently) do not find ontological foundationalism useful does not make it false, for there is nothing in pragmatism that requires expediency to be collective. Usefulness to one person is sufficient to warrant a belief’s truth under pragmatism. One could view truth as a matter of degree and argue that a belief is truer the more people who find it useful. But what criterion should we use to decide whether or not a given person finds the belief in question useful? And what kind of people should we include in such a determination? Should we focus only on a certain verbal community? Which one? Over what period of time?

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Chapter 8, the authors try to distance RFT (and behavioral “theories” in general) from hypothetico-deductive theories:

Behavioral theories are quite different from the types of theories one usually finds in non-behavioral psychology. Hypothetico-deductive theories attempt to model the underlying mechanisms that mediate the contextual and behavioral features that are directly observed in a given domain. As such, these theories tend to cross levels of analysis. For example, the behavioral domain is often explained by neurological phenomena or inferred mental processes. They are tested using *predictive verification* or falsification. (p. 143, emphasis mine)

I cannot but take this quote to imply that prediction is a goal of hypothetico-deductive theories, unless that by “predictive verification” the authors mean something *fundamentally* different from “prediction.” But isn’t it also a goal of behavior analysis? The authors believe so:

Behavioral analytic theories are analytic abstractive (Hayes, 1995). *Analytic abstractive theories* are simply organized sets of behavioral principles that are used to help *predict* and influence behaviors in a given response domain. (p. 143, last emphasis mine)¹¹

They also seem to regard prediction as a goal of RFT:

. . . Dymond and Barnes *predicted* that if picking stimulus B1 after making one response was reinforced, a subject, without further training, would then choose the following:All four subjects performed as *predicted*. (p. 60, emphasis mine)

. . .generic *predictions* have been made with regard to the types of histories that are required for relational framing to emerge (p. 148, emphasis mine).

According to their own characterization of hypothetico-deductive theories, then, there is something quite hypothetico-deductive about RFT (and behavior analysis in general) after all. Their attempt to distance RFT from hypothetico-deductive theorizing is thus inconsistent with their characterization of theories as inductive

¹¹ The continuation of this quote is worth examining: “They are, in other words, *coherent* sets of functional analyses” (emphasis mine). But in virtue of what is a set of functional analyses “coherent”? Is it *internal* coherence, that is, coherence *among the principles*? If yes, how is it to be determined? The only answer is *the absence of contradiction*, in virtue of the nonviolation of the principle of “the excluded middle” (or “bivalence”), the fundamental principle of (bivalent) deductive logic. This principle asserts that either p or not- p , where p is any statement, assertion, or proposition. However, such a principle is *purely logical* and even *rationalistic*, for it is to be *axiomatically* accepted as true, “‘under the natural light of reason,’ as the philosophical saying goes.” And before the reader raises the possibility of reducing deductive to inductive logic, consider that such a reduction would presuppose a principle of induction that would have to be accepted on purely rationalistic grounds as well, on pain of triggering an infinite regress of induction meta-meta-meta . . . principles. An emphasis on coherence thus commits oneself at least to bivalent logic and, to that extent, to deduction as a mode of justification. So much for the alleged “inductive nature” of RFT (or behavior-analytic theories in general).

generalizations used for prediction, unless “prediction” takes different meanings throughout their discourse.

A way to resolve the above contradiction is to clarify that an inductive generalization can be as predictive as a hypothetico-deductive theory. The difference is the way in which predictions are *justified*. Under an inductive methodology (now I use “methodology” in its standard philosophical sense; see Note 5), predictions are justified by appealing to *past experience*.¹² In contrast, under a hypothetico-deductive methodology, predictions are justified as *logical consequences* (or “implications”) of certain premises adopted as working hypotheses. A famous example is Einstein’s (correct) prediction of the discrepancies between the apparent and real positions of certain stars during the solar eclipse of 1919.

If RFT were inductive in the standard philosophical sense of the term, its predictions would be justified inductively. However, the justifications of the authors’ predictions seem to be anything but inductive. For instance, consider the continuation of the penultimate quotation above:

Dymond and Barnes predicted that if picking stimulus B1 after making one response was reinforced, a subject, without further training, would then choose the following:

1. C1 following “one response.” This would happen *because* C1 and A1, and B1 and A1 were in frames of coordination and thus C1 would acquire the same function as B1 by virtue of a transfer of function through the frame of coordination.
2. B2 following “no response.” This would happen *because* B2 was less than A1 and A1 and B1 were equivalent. Thus, B2 would acquire a response function that is less than the B1 function. (p. 60, emphases mine)

¹² More precisely, an inductive argument is one whose premises refer to *observed* things, properties, or events, and whose conclusion refers to things, properties, or events of the same (or sufficiently similar) kind that have not been observed at the moment the argument is advanced. Here’s a typical example: This rat increased its barpressing rate after being reinforced for pressing the bar; hence, the next rat I reinforce for pressing the bar will also increase its barpressing rate. Note that this inference goes from the particular (the first rat) to the particular (the second rat). This characterization of induction goes against the psychologist’s habit of defining induction as inference from the particular to the general. Strictly speaking, inferences from the particular to the general comprise a *proper subset* of all inductive inferences (insofar as the particular involves observed cases and the general involves unobserved cases; of course, one can make particular statements about unobservables and general statements about observables, so the terms “particular” and “general” do not unequivocally fix the technical notion of induction). All inferences from the particular to the general thus qualify as inductive. However, as illustrated in the rat example, not all inductive inferences go from the particular to the general. The emphasis on inductive inferences that go from the particular to the general is due to their involvement in a certain account of empirical laws, which have been (and still are) a central topic in the philosophy of science. Deductive inferences can also go from the particular to the particular (e.g., If this metal is heated, it will expand; this metal is heated; therefore, this metal will expand), so not all of them go from the general to the particular. Another difference is this: In an inductive argument, the truth of the conclusion is not guaranteed by the truth of the premises, while in a deductive argument the truth of the conclusion is guaranteed (or “inherited”) by the truth of the premises.

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Clearly, such justifications do not have the inductive form “because we have observed many times in the past that subjects under such and such conditions have responded in such and such ways.” Hence, they are not inductive generalizations, at least in the standard technical sense of the term. On the contrary, they are deductive, in that they are *logical consequences*. This interpretation is consistent with the way the authors introduce Part II of the book: “In Part II we attempt to explore some of the *implications* of Relational Frame Theory in specific domains” (p. 155, emphasis mine). The predictions in question seem to be as much “implications” of RFT as those explored in Part II. RFT does not qualify as an inductive generalization insofar as “implication” means (as it usually does) “logical consequence.”

The hypothetico-deductive character of RFT is further evidenced by the authors’ use of the notion of *falsification*. For example, in their introduction to Part II, one reads: “If these points are wrong, the theory is *falsified in a traditional sense of the word*” (p. 155, emphasis mine). Saying “*a traditional sense*” implies there is more than one traditional sense. The only “traditional sense” of which I am aware, however, is Popper’s (1935/1959), whose methodology is known precisely as “hypothetico-deductivism.” If the authors refer to another “traditional sense,” I have no idea what it could possibly be.

In Popperian methodology, falsification is strictly a logical relation between certain theoretical laws (taken as premises) and certain empirical predictions (taken as logical consequences of the theoretical laws).¹³ Such a methodology is based on the deductive-inference rule known as “modus tollendo tollens.”¹⁴ In contrast, inductive generalizations cannot be falsified (nor corroborated, in Popper’s technical sense of the term) but only confirmed (technically, made inductively more probable) or disconfirmed (made inductively less probable).

Popper was notable for his adamant rejection of induction, which he, following Hume (1739-40/2000), viewed as an irrational practice. By regarding RFT as “falsifiable” in the “traditional” sense, the authors commit themselves to hypothetico-deductivism and a rejection of inductivism. Their proposal thus suffers from a deep incoherence, for it would seem to combine two radically opposed methodologies.

¹³ Popper adopted the standard logico-positivistic notion of scientific theories (see Note 5), so he did not disagree with the logical positivists in this particular respect. What he rejected was inductivism, the doctrine that all scientific beliefs are to be justified inductively. More precisely, he rejected Carnap’s (e.g., 1945) characterization of the principle of induction, which involved the application of probability theory to compute degrees of confirmation of beliefs, relative to certain evidence. In this characterization, a belief is more rational the larger its degree of confirmation. In addition to rendering induction as irrational, Popper complements his rejection of inductivism with the idea that probability and information are inversely related.

¹⁴ According to this rule, one can validly negate the antecedent by negating the consequent of a conditional. That is to say, $[(p \rightarrow q) \wedge \neg q] \rightarrow \neg p$ is tautological, where p and q denote any two assertions, “ \wedge ” conjunction (“and”), and “ \neg ” negation (“not”).

Set Theory and Symbolic Logic

In my next commentary, I claim that core notions of RFT (relation and mutual entailment) are set-theoretic and logical. I thus submit that RFT largely involves the application of set-theoretic and logical concepts to verbal behavior. Let me start my justification of this claim by pointing out a difficulty with the authors' usage of the terms "class" and "relation."

The authors distance RFT from the "traditional stimulus class-based account" (p. 61), "class-oriented method" (p. 65), and "class-based interpretation" (p. 69). They also say that in RFT "the concept of a stimulus relation begins to dominate over the concept of stimulus class" (p. 59). They even say that one cannot "deal with [certain] data using only class concepts" (p. 62). On the other hand, however, one reads that "[a] relational frame *is* a specific *class*" (p. 33, emphases mine), "derived relational responding is a *class-based* concept" (p. 35), "relational frames are *classes* of relational behavior" (p. 71), and "[t]he RFT approach invokes a purely functional concept of an operant, and the term "overarching operant *class*". . . is used to emphasize that fact" (p. 146, all emphases mine). So class-based accounts, methods, and interpretations are to be rejected (or, at least, are insufficient), but relational frame is a class-based concept.

One way of making sense of the authors' discourse here is to conclude that they appeal to class as well as nonclass accounts, methods, and interpretations. Perhaps they view relational frames as involving stimulus relations *and* response classes. But then, the notion of a relational frame would become a class *and* a nonclass concept, which is oxymoronic. The authors could reply that the concept of a relational frame has a class component and a nonclass component. However, exactly how the two components relate *conceptually* (especially if they are mutually exclusive) remains obscure.

Another possibility is that the authors only reject the notion of a *stimulus* class (endorsing, instead, the notion of a stimulus *relation*) but accept the notion of a *response* class. This interpretation would be consistent with the authors' distancing from the "traditional stimulus *class*-based account" (p. 61, emphasis mine), their belief that "derived stimulus *relations* constitute the core of verbal behavior" (p. 43, emphasis mine), and their acceptance of "functional response *class[es]*" (e.g., p. 149, emphasis mine). But why do the same reasons for rejecting stimulus-class not apply to response-class analyses, accounts, methods, and interpretations as well? Why does the concept of response *relation* not "dominate over" the concept of response class? Do the notions of "relational responding" and functional response class not rely on the notion of a response relation? Should the concept of a response relation not be at least *allowed* in behavior analysis (even of language)?¹⁵

¹⁵ Sidman (1994, pp. 85, 112, 122) rejects the view that response *equivalence* is *necessary* for stimulus equivalence, based on a rejection of the notion of response mediation advanced in the paired-associate literature. However, he acknowledges that response equivalence may well be *sufficient* for stimulus equivalence: "differential responses can mediate the emergence of new stimulus relations" (p. 222). So even Sidman, who is against the necessity of response equivalence

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Whatever their answers, the authors' key assumption here would seem to be a distinction between classes and relations, which implies that relations are not classes. However, relations are conceptualized as classes (or sets) in set theory.¹⁶ Such a conceptualization should be perfectly legitimate within RFT, for three reasons. First, equivalence relations in behavior-analytic research are explicitly defined in the language of set theory (see Sidman, 1994). Second, RFT is a direct descendant of that research, insofar as the former expands the latter to nonequivalence relations. Third, if one defines equivalence relations set-theoretically, one must also define nonequivalence relations set-theoretically, for the latter are defined as the absence of reflexivity, symmetry, and/or transitivity. Nonreflexivity, nonsymmetry, and nontransitivity are as much set-theoretic concepts as reflexivity, symmetry, and transitivity.¹⁷ In this manner, RFT (like equivalence-relation accounts in behavior analysis) largely amounts to an application of set-theoretic concepts to a description, explanation, account, or whatever, of verbal behavior. An emphasis on nonequivalence relations does not inoculate RFT from that possibility.

Of course, I see nothing inherently wrong with using set-theoretic concepts, as long as it is properly justified. Usages in pure (as opposed to applied) mathematics are extensive and well justified. The same can be said about usages found in formal philosophy, especially in metalogic (see Hunter, 1971), metamathematics (see Kleene, 1952), mereology (e.g., Leonard & Goodman, 1940), and the structuralist view of scientific theories (see Note 6). These usages involve certain kinds of analyses of *linguistic products* of certain kinds of activities (viz., pure mathematics, pure logic, or scientific theorizing). In general, the usage of set theory is a technical method in philosophical analysis (Pollock, 1990).

Usages in empirical science, however, are quite a different matter, for they involve characterizing (describing, conceptualizing, interpreting, explaining, or whatever) phenomena and processes (as opposed to linguistic products) set-

for stimulus equivalence, allows the concept of a response equivalence and, to that extent, of a response relation. Besides, response relations include response equivalence as well as *nonequivalence*, so rejecting the necessity of response equivalence for stimulus equivalence does not entail rejecting response relations altogether.

¹⁶ Here I refer to *crisp* (as opposed to "fuzzy") set theory. Also, I use "class" and "set" interchangeably, for the technical distinction made in set theory is irrelevant here. Set-theoretically, a relation is a *set of ordered pairs*. An ordered pair is a set of the form (x, y) , where x and y denote any two individuals. In an ordered pair, the order of occurrence of its members makes a difference. That is to say, $(x, y) \neq (y, x)$, in contrast to $\{x, y\} = \{y, x\}$ in the case of unordered pairs. This feature is what allows a set-theoretic definition of any relation, for what counts in relations *extensionally* is the order in which proper names occur in the sentences that satisfy the relation in question. For example, the relation of love can be set-theoretically defined as the set of all ordered pairs (x, y) that satisfy the scheme " x loves y ," where x and y are any two proper names (or definite descriptions). The relation is nonreflexive (there are many people who do not love themselves), nonsymmetric (love may not be corresponded), and nontransitive (that x loves y and y loves z does not entail that x loves z), so it counts thrice as a nonequivalence relation.

¹⁷ The absence of symmetry can take three different forms: Asymmetry, nonsymmetry, and antisymmetry. As far as I know, no behavior analyst (the authors included) has specified exactly which forms apply across failures of behavioral symmetry. Such a specification would seem to be especially relevant for an emphasis on nonequivalence relations.

theoretically.¹⁸ To the best of my knowledge, the first case is equivalence-relation research in behavior analysis, where descriptions, interpretations, and predictions are routinely made in terms of the “acquisition,” “emergence,” or “formation” of (stimulus and/or response) classes (e.g., Sidman, 1994; Zentall & Smeets, 1996). I view RFT as the second case.

The beginning of that trend can be traced to Skinner’s (1935/1961) seminal notion of operants as classes, which has become an integral part of the conceptual core of behavior analysis (e.g., Catania, 1973; Lee, 1981; Schick, 1971; Segal, 1972; Zeiler, 1977, pp. 222-223). A set-theoretic conceptualization of (equivalence and nonequivalence) relations is a natural development of this notion (after all, if one conceptualizes the phenomena of interest as classes, one might as well take advantage of a formal theory of classes in order to make one’s statements in a clearer and more precise manner). Admittedly, such a development was not easy to fathom in the 1930s, for set theory was not well known at that time. However, it eventually became known to anyone with a high-school education since the 1960s, thanks to the “Modern Mathematics” or “New Math” (the application of the Bourbaki set-theoretic reconstruction of mathematics during the 1930s and 1940s to the teaching of mathematics). Using set-theoretic concepts in behavior analysis thus was bound to occur sooner or later.

Are such usages unjustified? I tend to think so, but this is not the proper place to elaborate this claim. After all, the authors are just following behavior-analytic conventional wisdom in this particular respect, so they can only be blamed for uncritically perpetuating it. Suffice it to say that the rest of the empirical sciences have done perfectly well without set-theoretic concepts. The reason is twofold. On the one hand, talk of class membership adds nothing above and beyond the understanding we achieve through an empirical study of properties and how they relate to each other. By asserting that a given continuant (an atom, a molecule, a cell, a chair, an organism) or a given event (a stimulus, a response) is a member of a certain class we do not understand it any better than by saying that it has such

¹⁸ The process/product distinction raises difficult issues I cannot discuss in detail here. Suffice it to say that one’s position on this distinction depends largely on whether or not one wants to separate between the process/product and the cause/effect distinctions. Such a separation, in turn, relies on a more fundamental one: *events* versus *continuants* (the metaphysical technical term for things, viewed as physical objects that are relatively static, permanent, enduring, or persistent through time). If one does not make those separations, then talk of processes and products is just another way of referring to cause-effect relations (the point also applies to “functional” relations). If one wants to make the distinction in question, one way would be to view processes as causal chains of events, and products as continuants. Under this interpretation, then, a product does not qualify as an effect, for effects are events (a standard view in causality accounts) and events are not continuants (a standard view in metaphysics). Buildings, cumulative recorders and records, nests, video and sound records, marks or traces of any kind (e.g., footprints, brake skid marks, burn marks, printed material like books and papers; computer files are more difficult to categorize), craters, sediments, and pellets qualify as products under this interpretation. Rains, thunders, lightnings, earthquakes, concerts and recitals, responses, and stimuli (including droppings of pellets into feeders) are events (that can be causes and/or effects) but not products. Of course, this interpretation does not exclude the intuition that products are in some sense related to processes. However, the interpretation does exclude treating the relations between processes and products as causal (or functional).

and such properties that relate and change in such and such way. If nonequivalence relations are viewed as classes, the same point applies to RFT (see Tonneau, 2001a, p. 9, for the same point applied to equivalence relations).¹⁹

On the other hand, if one seeks precision and clarity in the formulation of *scientific* principles, applying mathematics will do just fine (scientific modeling is *applied* mathematics). To be sure, set theory is routinely used in *pure* mathematics. Moreover, under logicism, mathematics can be reduced in part to set theory (and in part to formal logic; see below). However, mathematical expressions very quickly become cumbersome when recast in the language of set theory, which can only make the empirical scientist's work more difficult (for example, the numeral "2" is set-theoretically defined as " $\{\emptyset, \{\emptyset\}\}$," where " \emptyset " denotes the empty set). The philosopher, in contrast, is forced to use set theory largely because his reflections include the nature and foundations of pure mathematics and because a major philosophical view on pure mathematics is, precisely, that it can be reduced to set theory (plus predicate logic). Besides, using pure mathematics as a (meta-)language to reflect upon pure mathematics can easily lead to circularity and vicious regress. Only in the context of a very special kind of analysis (viz., Gödel's incompleteness studies) can pure mathematics be used to reflect upon itself, and even here set theory is used extensively.

Another central notion of RFT is entailment. This notion, however, can be expressed rather directly in symbolic logic. The authors themselves leave this possibility open: "An *abstracted frame of opposition* is seen in *symbolic logic* with the concept of the "*logical not*" (p. 36, emphases mine). So, why not say that an "abstracted frame" of entailment is seen in symbolic logic with the concept of material implication? As is well known, negation and material implication are

¹⁹ Contrary to what the reader might intuit, asserting that an individual possesses a certain property is neither the same as, nor does it entail, nor is it entailed by, asserting that it is a member of a certain class. Take, for example, the assertions "Socrates is human" and "Socrates is a member of the class of humans." The first one presupposes that there is an individual called "Socrates" and that such an individual exemplifies or possesses a certain property (human-hood) but not that there is a class of humans. The second assertion presupposes that there is an individual named "Socrates" and a class named "humans," without presupposing the property of human-hood. One can certainly combine set theory with predicate logic (a standard practice that is seldom accompanied by the present clarification) and obtain what is known as a definition by abstraction. Such a definition, however, only specifies properties as *criteria* for picking individuals out and regarding them as members of a certain class. Strictly speaking, a definition by abstraction does not directly define a set, but rather the conditions that a certain individual must satisfy in order to be considered as a member of a set. A set is directly defined only by extension. To assume that the two kinds of assertion in question are equivalent, aside from conflating properties with individuals, which leads to much confusion, amounts to (or, at least, leaves the door open for) adopting two very problematic assumptions. First, that the identity of a class can be determined by properties, which goes against the set-theoretic axiom of extensionality (that the identity of a set is determined by its members, not their properties). Second, that for every property P there is a class of individuals that possess P . The second assumption is the so-called "unrestricted axiom of comprehension," which, as is well known, leads to a contradiction (Russell's famous paradox). After the contradiction was discovered, the axiom was formulated in a restricted form by Zermelo in 1908 (see Bernays, 1958): There is at least one set A whose members are members of another set U and have property P .

sufficient to define all other relations found in propositional logic, including the biconditional, which can be seen as an “abstracted frame” of mutual entailment.

Viewing logical negation, material implication, and the biconditional as “abstracted” relational frames does not contradict the above outcome. On the contrary, it further strengthens my claim that RFT is either propositional logic (plus set theory) applied to verbal behavior or propositional logic (and set theory) itself. If logical negation, material implication, and the biconditional are in effect “abstracted frames” of opposition, entailment, and mutual entailment, respectively, and if by “abstracted” one means “generic,” then clearly propositional logic encompasses RFT (RFT is an application of symbolic logic). And if logical negation, material implication, and the biconditional are themselves *relational frames*, then RFT and propositional logic are indistinguishable. In this sense, RFT becomes LFT (Logical Frame Theory).

I see nothing inherently wrong with using logical concepts either. On the contrary, they have proven to be quite useful, especially when combined with set-theoretic concepts, as shown in pure mathematics and formal philosophy (see Pollock, 1990). Usages of logical concepts in the empirical sciences, however, are much less frequent. To the best of my knowledge, the first case is cognitive science, especially traditional (symbolic) Artificial Intelligence, where the processes of interest themselves are viewed as being logical in nature. RFT has now arisen as the second case, only that it involves applying logical concepts to (verbal) *behavioral* rather than mental processes. In this manner, cognitivism and behaviorism are brought closer together, but only superficially. A deep and yet unresolved chasm remains: The ontological status of mental properties. In cognitivism, mental properties are viewed as fundamentally different from and irreducible to (albeit “supervenient” upon) physical (specifically material or structural) properties. In behaviorism (and, to that extent, RFT), mental properties *are* behavioral properties. To the extent that RFT rejects property dualism, it should strike cognitivists as more behaviorism and not be fooled by the authors’ usage of logical concepts.

The authors could retreat into pragmatism and retort that it is ultimately the reader’s choice whether to be a behaviorist or a cognitivist and that whatever he finds useful would be fine with them. Indeed, it is the reader’s choice, but that’s precisely the problem. Even if all readers were pragmatists (a big “if”), some will find it more useful to think cognitivistically and others behavioristically. Pragmatism will not make the ontological chasm go away. To make that happen, the authors would have to convince the majority of psychologists to sweep it under the rug. Alternatively, if the authors decide not to ignore the chasm and remain faithful to their behavioristic upbringing, they would have to convince the majority of cognitivists that the behavioristic side of the chasm is the correct one. To put it pragmatically, the majority of cognitivists would have to be convinced that thinking behavioristically is more useful than thinking cognitivistically. Such a task would require convincing cognitivists that their goal (to understand the workings of the mind) is less worthy than the behaviorist’s goals (the control and prediction of behavior). On both tasks, I can only wish the authors good luck.

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When set theory and symbolic (propositional and predicate) logic are combined (as they usually are in pure mathematics and formal philosophy), the resulting expressive power is formidable. Such a power is sufficient to define all the families of relational frames defined by the authors (see pp. 35-39) and much more.²⁰ That power also leads to the explosion of relations the authors find so “unbelievable” (e.g., see pp. 62-63).²¹ The authors’ amazement is nothing more than a rediscovery of that power. The possibility of recasting RFT as set theory plus symbolic logic applied to verbal behavior also accounts for the variety of applications allowed by RFT (see Part II), for both languages are topic-neutral.

²⁰ Coordination frames can be described in terms of equivalence relations. Distinction is opposition (logical negation) without the specification of an “appropriate response” (p. 36). Comparison frames can be described in terms of nonequivalence relations (e.g., larger-than). Hierarchical relations can be described through the set-theoretic notion of a proper subset (although the “part-whole” and “attribute-of” relations raise deep and formidable issues). Temporal and spatial relations can be subsumed by the comparison family (earlier-than, closer-to; undoubtedly, as the authors trivially acknowledge, ordering temporally is different from ordering spatially, insofar as space is different from time, but temporal and spatial ordering can be described as two different types of comparison). Conditionality and causality are cases of entailment. And deictic relations can be described by translating personal pronouns into either proper names or, if ambiguous, definite descriptions (see Russell, 1905). For instance, the assertion “I am taller than you” can be translated into “Peter is taller than Paul” (or, if “Peter” and “Paul” are ambiguous, “the person who has such and such characteristics is taller than the person who has such and such characteristics”). These translations are odd but not empty, for some people do refer to themselves in the third person.

²¹ In (bivalent) predicate logic, one can build $2^{(2^P)}$ truth tables with P propositions, as given by $P = \sum_{i=1}^m n^{k_i}$, where m the number of predicates, n the number individual constants, and k_i the

number of places of predicate i . For example, with just one monadic predicate and two individual constants (or two monadic predicates and one individual constant), one can build 16 different truth tables (among which are negation, material implication, conjunction, inclusive and exclusive disjunction, and biconditional). In set theory, the number of relations between sets A and B is given by the size (number of elements) of the power set (the set of all subsets) of the Cartesian product $A \times B$ (the set of all sets of ordered pairs that can be formed with A and B). For example, let $A = \{a, b, c\}$ and $B = \{d, e, f\}$, where $a, b, c, d, e,$ and f denote particular objects. We can thus write,

$$A \times B = \{(a, d), (a, e), (a, f), (b, d), (b, e), (b, f), (c, d), (c, e), (c, f)\}.$$

The Cartesian product is noncommutative, so we can also write,

$$B \times A = \{(d, a), (d, b), (d, c), (e, a), (e, b), (e, c), (f, a), (f, b), (f, c)\}.$$

The union of both products results in a domain set C consisting of 18 ordered pairs. The size of the power set of C is given by $2^{S(C)}$, where $S(C)$ is the size of C . We thus obtain a total of 2^{18} possible nonequivalence relations between two sets of only three elements each. Even if one refused to count singletons of ordered pairs as relations and subtracted $2^{18} - 18$, the number would still be decently large. Equivalence relations can be obtained when only one set is involved. If A is the set of interest, $A \times A = \{(a, a), (a, b), (a, c), (b, a), (b, b), (b, c), (c, a), (c, b), (c, c)\}$, for a total of 2^9 possible relations. Whether or not $\{A \times A\}$ qualifies as the only equivalence relation depends respectively on whether or not $x \neq y$ for any pair (x, y) . To the best of my knowledge, the standard way of defining reflexivity, symmetry, and transitivity does not stipulate that $x \neq y$, so a subset such as $\{(a, a), (b, b), (c, c)\}$ qualifies as an equivalence relation. Take for example the relation of colleague, which is an equivalence relation. One can perfectly say that if John is a colleague of John, then John is a colleague of John. One can also say that if John is a colleague of John and John is a colleague of John, then John is a colleague of John. To exclude such cases as empirically trivial, the behavior analyst would have to add the stipulation that $x \neq y$ to the standard set-theoretic definitions of reflexivity, symmetry, and transitivity.

Such neutrality would also explain why RFT could be appealing to behaviorists and cognitivists alike. In particular, set theory and logic are metaphysically neutral, so they do not force any position on the mind-body problem (but see Tonneau, 2001b, pp. 117-120).

In view of the above considerations, I cannot but conclude that RFT's versatility is largely due to its usage of set-theoretic and logical concepts. Such a versatility, therefore, is unsurprising. After all, set theory and logic capture basic grammatical features of natural language. In fact, to a great extent, both arose as abstractions from everyday language. If RFT is recast as set theory plus symbolic logic applied to verbal behavior, it becomes little more than a return to natural language from its abstract forms found in those two languages. But then again, that is what philosophers (with set theory plus symbolic logic, in reference to linguistic products) and cognitivists (with symbolic logic, in reference to mental processes) have been doing all along.

Definitions

I want to finish by drawing attention to the phrase "by definition," which is frequently found throughout the book (e.g., see pp. 14, 23, 29, 59, 155). In the standard Aristotelian view, definitions are supposed to specify essences. It is one of the meanings of "definition" found in *Merriam-Webster's Collegiate Dictionary* (10th. Ed., 1998): "a statement expressing the essential nature of something." Moreover, according to the *Oxford American College Dictionary* (2002), "by definition" means "by its very nature; intrinsically." I have already anticipated the essentialistic character of RFT in relation to the authors' position on theories. Here, I want to show that such a character seems to be more general than that. This outcome, of course, is not surprising. If one endows theories with essences, one can do the same with virtually anything else.

Under the Aristotelian view, definitions have an apophantic (assenting) and apodeictic (necessary and universal) character. RFT arises as a deeply essentialistic proposal insofar as the authors adopt the Aristotelian view of definitions. This interpretation is supported by the fact that the authors present their definitions as *the* ones that *should* be adopted. For example, regarding verbal behavior, the authors write: "Some psychologists who have been exposed to the RFT approach fail to see why verbal events *should be so defined*" (p. 44, emphasis mine), where the endorsed definition prescribes that "[v]erbal behavior is the action of framing events relationally" (p. 43). Such a prescriptive view of definitions, where terms are taken to have rigid meanings, is symptomatic of essentialism, insofar as the search for such meanings can be sensibly justified only as a search for essences or natures.

The authors thus seem to believe they have captured not only the essence of theories, but also the essence of verbal behavior, language, cognition, metaphor, story, development, thinking, problem-solving, understanding, self, and even spirituality. I see nothing inherently wrong with being an essentialist (see Ellis,

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2001).²² However, on the one hand, essence ascription raises formidable issues (see Note 8). On the other hand, as I argued before, being an essentialist and claiming ontological neutrality is incoherent.

An alternative is the modern view (see Suppes, 1957, pp. 151-173), where definitions are mere stipulations of meanings, with no ontological import. From this view, to define is simply to explicitly declare the way in which certain terms will be used within a certain discourse, with no intention of specifying essences (and, to that extent, with no intention of excluding other definitions). Since definitions in this view are taken to be devoid of metaphysical import, they do not have an apophantic (let alone apodeictic) character. The only role of definitions under this view is to *abbreviate* the discourse by using shorter terms as surrogates for longer expressions (e.g., instead of “organization at the edge of chaos,” one could simply say “life”). The implication is that “definitions are theoretically superfluous” (Whitehead & Russell, 1910, p. 12) and, to that extent, theoretically dispensable. Under a modern view of definitions, the authors’ theoretical proposal amounts to little more than a trivial and sterile exercise in meaning stipulation.

Concluding Remarks

In sum, I found the concepts, logic, and justification of RFT to be unintelligible, and when not, incoherent, misguided, trivial, and sterile. Of course, in the presence of unintelligibility, any criticism can be rendered as “misunderstanding.” Being unintelligible thus can be quite a useful practice, which is all-too-consistent with a commitment to pragmatism, at least of the Jamesian type. Such a commitment would explain the authors’ positive tropism towards unintelligibility, for Jamesian pragmatism is as unintelligible as a doctrine can be. Criticisms towards the relativism underlying Jamesian pragmatism are well known (see Harris, 1992), so I will not rehearse them here. I will just quote Kirkham’s (1992) apt assessment of William James as a philosopher:

clarity and consistency were not James family traits. Part of the problem is that James philosophically grew up in the later nineteenth century, an era in which ambiguity, indirection, and rococo encrustations of metaphor were standard features of philosophical expression. . . .Argumentation that preceding and succeeding generations would count as textbook indulgence in the fallacy of equivocation was elevated to a philosophic method in the world of James’s intellectual upbringing. James’s views never recovered from this intense early irradiation of nonsense. His was a mind capable of asserting, within a few pages, two definitions of truth enjoying not a word in common, without so much as acknowledging the difference, let alone explaining it. . . .He could embrace extreme subjectivism. . .and yet, when indicted for that sin, insist with wounded

²² I tend to think essentialism is far more difficult to justify in biology and psychology than in physics and chemistry. Rejections of essentialism within evolutionary biology are well known, at least regarding conceptualizations of species as natural kinds. Essentialism, however, may well be more defensible in molecular biology, although such a possibility would raise the issue of how to make both branches of biology metaphysically harmonious.

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innocence that he was an objectivist. In one bizarrely guileless passage he refers to himself as a relativist and proclaims that relativism does not involve a denial of absolutism. . . . Although he complained repeatedly that his critics misunderstood him, sometimes referred to their objections as “slanders” . . . , and once attributed to them “an inability almost pathetic, to understand the thesis which they seek to refute” . . . , there were times when he grudgingly conceded that some of the fault might lie with his own careless expression and/or intrinsic flaws in his philosophy itself. At such moments, however, he as often as not retreated into special pleading so outrageous as to strike the reader as comical. There are the places, for example, where he dismisses proofs of the contradictoriness of his views as mere technicalities. . . . (pp. 87-88)

I ignore the extent to which the authors are committed to Jamesian pragmatism. They cite James (pp. 130, 239) but not his major works on pragmatism and truth (James, 1907, 1911). I do know, however, that Jamesian pragmatism is the most discussed form of pragmatism in philosophy and science (behaviorism and behavior analysis included; e.g., see Smith, 1986; Zuriff, 1985). So much, that “pragmatism” is almost synonymous with “Jamesian pragmatism” (Charles S. Peirce renamed his doctrine “pragmaticism,” in order to distinguish it from Jamesian pragmatism, with which he had important disagreements [1905/1955]).

If functional contextualism is a form of Jamesian pragmatism, then I cannot but see the book as a result of the kind of intellect described by Kirkham, where nonsense is rampant (largely a means for convincing the unwary and coping with criticisms from the cautious) and the reader is blamed for the writer’s carelessness. In this sense, the book strikes me as yet another application of Harry S. Truman’s maxim “If you cannot convince them, confuse them.” Under Jamesian pragmatism, anything goes, even nonsense, as long as it is useful to someone. Such is the misery of Jamesian pragmatism, for which behaviorists (and scientists in general) would be better off by distancing ourselves from it as quickly and far as possible.

If functional contextualism shares nothing with Jamesian pragmatism, then why regard the former as “a type of *pragmatism*” and even assert that “[c]larity about the goals of analysis is critical to contextualists because goals specify how a *pragmatic truth criterion* can be applied” (p. 6, emphasizes mine)? It is hard not to interpret talk of the “pragmatic truth criterion” as a commitment to Jamesian pragmatism. Of course, even if functional contextualism had nothing to do with Jamesian pragmatism, I would still regard the book as an exercise in ambiguity, contradiction, indirection, misdirection, and opacity, only that its roots would remain a mystery to me. A commitment to Jamesian pragmatism would at least give a hint as to the intellectual origins of the authors’ unintelligibility.

In any case, the authors’ own characterization of the material presented throughout Part I as “arcane and esoteric” (p. 1) couldn’t be more adequate. In Roget’s International Thesaurus (6th Ed.), both adjectives are found under the categories of “secret” (where one finds synonyms such as “hidden,” “hermetic,” “unrevealable,” “undivulgable,” “undisclosable,” “unutterable”), *recondite* (where

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one finds synonyms such as “abstruse” and “obscure”),²³ and “supernatural” (where one finds synonyms such as “paranormal,” “superhuman,” “superphysical,” “unearthly,” “unworldly”). The definition of “esoteric” found in *Merriam-Webster’s Collegiate Dictionary* (10th. Ed., 1998) is “designed for or understood by the specially initiated alone; of or relating to knowledge that is restricted to a small group; limited to a small circle.” Under these interpretations of the authors’ own words, RFT seems to me to be as much a cult as anything else, which is in tone with their talk of “those special few who would consider [their] arguments seriously” in the Preface.

Whether RFT requires a commitment to Jamesian pragmatism, or can be guided by an intelligible philosophy or stand alone, remains to be seen. In any case, if the result is a clearer, more coherent, less trivial, and more precise proposal that is offered with more humility, I would be willing to give it another look.

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²³ To be fair to the authors, under “recondite” one also finds synonyms such as “abstract,” “transcendental,” “profound,” and “deep,” of which “abstract” is the least pretentious. If by “esoteric and arcane” the authors just meant “abstract,” they could have used the latter term, for it is shorter and far less vulnerable to the interpretations I am using here.

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