

Scientific Ambition:
**The Relationship Between Relational Frame Theory and Middle-level Terms in
Acceptance and Commitment Therapy**

Yvonne Barnes-Holmes, Ian Hussey, Ciara McEnteggart, Dermot Barnes-Holmes, &
Mairéad Foody

National University of Ireland Maynooth

Authors' note: The authors would like to thank Dr. Miles Thompson for his insightful and constructive comments on an earlier version of this chapter.

Most natural sciences aspire to a unified theory, such as Einstein's (1945) "unified field theory" that would specify how all space and time behave under changes in the parameters of the total field. According to Wilson (2012), a unified theory requires the unity of knowledge as derived from different, but fundamentally consistent, theorizing. The mere presence of Einstein's grand ambition functions as a constant source of encouragement and guidance for physicists towards this aim. This guidance is recognizable through progress to date in terms of the natural sciences' strong adherence to parsimonious theorizing, and well-defined, testable, and coherent units of analysis.

By contrast, a unified theory of psychology seems a long way off. Wilson (2012) referred to the "archipelago" of human-related disciplines, each speaking different languages with minimal connections across islands. While it may be the case that some sections of the discipline of psychology lack ambition regarding a unified theory, it is certainly clear that any progress in this regard, even when desired, is marred in part by failure to establish consensus on overarching conceptual units of analysis that would allow us to predicate and test theories. For example, we have nothing equivalent to time, distance, or genetic inheritance. Perhaps the rarity or absence of a unified theory in psychology legitimately derives from the diversity or complexity of our subject matter. However, this seems unlikely given the extensive conceptual, and often methodological, overlap across different domains of psychology. Furthermore, Darwin's grand theory of all life on this planet certainly aimed to tackle diversity and complexity head on with a high degree of scientific parsimony.

Contextual Behavioral Science: Progress to Date

Behavioral psychology, especially of the traditional Skinnerian variety, did not lack scientific or societal ambition. On the former, Skinner (1953) first sought a unified theory of non-human animal behavior, followed by a similar approach to human behavior that more specifically included an attempt to account for the complexity of language (1957). On

societal ambition, Skinner went even beyond the limits of psychology in *Walden Two* (1948) and *Beyond Freedom and Dignity* (1971). Emerging directly from Skinner's legacy, the current approach described as contextual behavioral science (CBS) is equally lofty in its ambitions on both scientific and societal fronts to help create "*a behavioral science more adequate to the challenge of the human condition*" (Hayes, Barnes-Holmes & Wilson, 2012, p. 5). While we are indeed proud of these ambitious aims, it is important to recognize that natural sciences often have extremely high aspirations as standard (e.g., Dawkins, 1986; Hawking, 1988).

In the sections below, we begin to explore how much success we have had in CBS towards these aims. As part of these reflections, we ask questions about our scientific model, its assumptions, and how these feed our perceptions of progress. This exercise seems consistent with a recommendation by Vilardaga, Hayes, Levin and Muto (2009) that "When there are changes in the scientific practices of a field, it is periodically necessary to identify and describe a systematic position and the philosophical orientation and assumptions on which it stands" (p. 105).

The current chapter is divided into two broad sections: (1) a scientific analysis of Acceptance and Commitment Therapy's (ACT; Hayes, Strosahl & Wilson, 1999) middle-level terms, especially those comprising the hexaflex; and (2) an exploration of CBS's reticulating model between these clinical middle-level terms and the basic scientific concepts of Relational Frame Theory (RFT; Hayes, Barnes-Holmes & Roche, 2001). Within the former section, we conclude:

- A. That middle-level terms are problematic in certain contexts and these problems are not solved by simply describing the terms as "functional".
- B. The suggested functionality of ACT's middle-level terms is problematic because the terms themselves do not appear to adhere to the philosophical truth criterion of precision-and-

influence that guides functional contextualism.

In Section 2, we articulate a number of concerns we have with CBS's reticulating model. These concerns broadly center around how reticulation works and we make the following conclusions:

- A. Reticulation between basic and applied/therapeutic work is asymmetrical.
- B. RFT may readily reticulate, albeit in an asymmetrical way, with "middle-level terms" derived from perhaps *any* philosophical or therapeutic tradition.

We would like to acknowledge from the beginning that we recognize our own bias towards a basic scientific perspective, especially our ambition towards identifying empirically testable functional processes. While, on balance, we are no less committed to the critically important ambition of alleviating human suffering, it is our firm belief that a unified theory of psychology can only be achieved if basic functional processes lie at the very heart of its analysis. We are not the first to have adopted this view (Blackledge, Moran & Ellis, 2009; Hayes & Plumb, 2007; Luciano, Valdivia-Salas & Ruiz, 2012) and see it as fundamentally important in driving our ambition forward.

Section 1: A Scientific Analysis of ACT's Middle-Level Terms

In the current section, we open with a brief summary of the middle-level terms that comprise the ACT hexaflex, as well as mention of a number of additional middle-level terms commonly used by ACT practitioners and researchers. To commence, we would like to discuss what we mean by the term "middle-level". A middle-level term is a theoretically-specific, non-technical term that has not been generated within basic scientific research. In other words, middle-level terms are not "high-level" (e.g., attention) because they cohere directly with a specific theoretical account. However, they are not "low-level" or "basic" terms (e.g., reinforcement) because they have not been generated directly from experimental data. In other words, describing something as a middle-level term is a way of placing it on a

continuum between the analytic units of the basic science (of psychology) and folk psychological terms (e.g., emotion, memory, stress, etc.) within a given domain.

The ACT Hexaflex

The ACT hexaflex model pivots on the concept of psychological flexibility into which six primary middle-level terms feed: (a) being in the present moment, (b) acceptance, (c) cognitive/emotional defusion, (d) self-as-context, (e) values, and (f) committed action (Hayes, Villatte, Levin & Hildebrandt, 2011). According to ACT theory, this represents psychological well-being. The inverse model of psychological *inflexibility* that accounts for human suffering comprises the same six, but antagonistic concepts: (a) lack of contact with the present moment, (b) avoidance, (c) fusion, (d) self as concept, (e) lack of influence of values, and (f) absence of committed action in the service of values. Furthermore, ACT practitioners and researchers employ additional concepts to describe or explain psychological suffering and/or its mechanisms of change. These range from middle-level theoretically-specific terms, such as self as process, to high-level, non theory-specific terms, such as rigid attention, mindfulness, and meditation (e.g., see Blackledge & Drake, 2013; Blackledge & Barnes-Holmes, 2009; Hayes, Villatte et al., 2011).

Hexaflex middle-level terms as “processes”. The last 30 years have seen a proliferation in the number of middle-level terms used in clinical psychology (e.g., distraction, endurance, hypervigilance, reactivity, rumination, etc.). These represent well-intended attempts to specify key psychological variables that can be manipulated in the therapeutic context. The contemporary question for clinical psychology is no longer only whether treatment is effective, but what makes it so. Answering this “why” question has proven much more challenging than answering the “what” questions usually addressed by outcome research (Barlow, Sauer-Zavala, Carl, Bullis & Ellard, 2013; Kazdin & Nock, 2003). ACT’s answer to the clinical process question is the hexaflex, based on the assumption

that its middle-level terms in single or in tandem represent functional behavioral processes (Hayes, Strosahl & Wilson, 2011).

The relationship between hexaflex middle-level terms and psychological flexibility. We think it important to draw attention to the fact that psychological flexibility sense of perceived wisdom that the six middle-level terms of the hexaflex in combination, or in total, represent or comprise psychological flexibility. An immediate source of confusion that may emerge here is that the lack of clarity surrounding the terms psychological *flexibility* (Hayes, Strosahl & Wilson, 2011), behavioral *flexibility* (Blackledge & Drake, 2013), and relational *flexibility* (O'Toole & Barnes-Holmes, 2009), all used by CBS researchers. Anecdotally, the current authors have noted that the term “*flexibility*” is often used (e.g., on the ACT and RFT listserves) without specifying which of the three types of flexibility is being referred to. This confusion is compounded by the fact that published works by CBS researchers have used the term flexibility in different ways. For instance, in one study employing the IRAP (as a measure of IQ), the concept of *relational* flexibility appears in the title (O’Toole & Barnes-Holmes, 2009). But, in another IRAP study (on depression), a relative change in an IRAP score is used as a measure of *psychological* flexibility as assessed by the AAQ (Hussey & Barnes-Holmes, 2012). It currently remains unclear to what extent any of these terms denote functionally similar or functionally distinct processes.

Clinical utility of ACT’s middle-level terms. Theoretically specific middle-level terms are largely the stock and trade of therapeutic models in general because they have broad and meaningful appeal. Classic examples include “exposure” and “cognitive restructuring”, and the hexaflex components are similarly characteristic of this type of term. While the merits of an individual concept may be debated (e.g., Hayes & Plumb, 2007; Hermans, Craske, Mineka & Lovibond, 2006; Luciano et al., 2012), this category of terms has undeniable utility as “shortcuts” for practitioners (Vilardaga et al., 2009). Indeed, we

strongly agree that the middle-level terms in the hexaflex are of exceptional clinical value, not only as orienting exercises for clinicians without behavioral training, but also for all clinicians trying to ensure that they deliver ACT in a manner that is likely to achieve its therapeutic goals.

Scientific utility of ACT's middle-level terms. According to Hayes and colleagues (2012), *“any disconnect between science and practice slows down practice and undermines the usefulness of science”* (p. 13). However, the connection between these two pillars has troubled both scientists and practitioners within psychology (e.g., Baker, McFall & Shoham, 2008; Hayes & Berens, 2004; Melchert, 2013; Nock, 2007). Practically all would agree that such a connection is essential, but there are many interpretations of the nature and extent of integration. For behavior therapy, Franks and Wilson (1974) adopted the rather strict requirement of adherence to “operationally defined learning theory”, as well as “conformity to well established experimental paradigms” (p. 7). Although strict, this would appear to be a reasonable demand of a therapy that is embedded in a behavior analytic tradition. However, Hayes et al. have argued that it is not possible for ACT's middle-level terms to meet this demand. In their own words, they noted the limits of these terms as follows: *“none of these are technical terms; none of them have the same degree of precision, scope, and depth of classical behavioral principles such as “reinforcement,” nor of technical RFT concepts such as “the transformation of stimulus functions””* (p. 7). In the following paragraphs, we consider why this is the case, and what constitutes a technical term from a CBS perspective.

Meeting the philosophical truth criterion. In a contextual behavioral analysis, the truth criterion is prediction-and-influence with precision, scope, and depth (Hayes et al., 2012). A focus on precision ensures that the number of analytic concepts is limited; scope ensures that the concepts have relatively broad appeal; and depth ensures that they cohere across relevant scientific domains. *All* of these elements must be satisfied if an analytic

concept is to meet the truth criterion in any contextual behavioral analysis. If a concept does *not* meet the truth criterion, it cannot be used as a “technical” term, even if it has clinical utility. Clinical utility should not therefore be conflated with scientific utility.

It is important to note at this point that from a strictly behavioral perspective, a term is not functional *in an ontological sense* (see Barnes-Holmes, 2000). For example, reinforcement is not a “real” thing. That is, it does not exist literally, but is an abstraction that has pragmatic utility based upon a substantive body of empirical evidence. This utility emerges gradually within a scientific community as its utility becomes more widely demonstrated through verifiable experience, rather than through either “truth by agreement” or “truth by democracy”. Only a limited range of terms have been deemed pragmatically useful (i.e., functional) within the behavioral tradition, including reinforcement, punishment, and stimulus generalization. For example, reinforcement constitutes a functional unit of analysis in the sense that the term denotes a causal relationship between a class of responses (e.g., lever pressing) and a class of consequences. The term causal is used here to indicate that there is an increase in the class of responses only when they produce or lead to an increase in the class of consequences. More generally, according to Vilardaga et al. (2009), functionality is defined as being “*based on sets of functional analyses based on behavioral principles based on behavioral observations*” (pp. 115-116). In what follows, we will argue that none of these specifications is the case for ACT’s middle-level terms.

The functionality of ACT’s middle-level terms

The procedure-process-outcome problem. In order to articulate the concerns we have about the suggested functionality of ACT’s middle-level terms, we would like to introduce the reader to a classic problem in cognitive psychology, commonly known as the conflation of procedure, process, and outcome (De Houwer & Moors, 2010), which leads to circular reasoning. Consider the circularity in the following example. An ACT practitioner

might say that a new client is highly fused with her psychological content (e.g., “I’m stupid”), hence defusion techniques will be needed to defuse her, and thus to reduce her level of fusion. In other words, a defusion procedure will be used to create an outcome of defusion through the process of defusion. If the procedure “worked” (e.g., the client reported a decrease in the believability of her thoughts, which the therapist interpreted as defusion), then the clinician might say that the defusion procedure had, through the process of defusion, produced the desired outcome of defusion.

The process-procedure-outcome distinction helps highlight the circularity in talking about defusion in this way. Indeed, the key problem with all middle-level terms (and ill-defined mentalistic terms generally), is that it is frequently difficult to determine which of these is being targeted in any given (scientific) narrative. For example, there are defusion procedures, or at least a set of techniques collectively known by clinicians as such, and these are arranged together in the service of a common therapeutic aim. And, there may even be defusion the process which defusion procedures are designed to elicit. And, there are likely to be defusion outcomes, or at least defusion effects. Hence, used in an ACT context, defusion is used in multiple ways to refer to three different phenomena. How these three things can be separated out and discriminated accurately remains unclear. Admittedly, separating outcome from technique might be relatively straightforward, but isolating defusion as a functional process appears more problematic. As we will argue below, mediation analyses and analogue studies alone will never serve this purpose.

Blackledge and Drake (2013) summarized the two main types of analyses typically used to investigate the “functionality” of the ACT hexaflex: (a) mediation analyses, and (b) analogue studies. We will argue, however, that these forms of analyses do not necessarily provide empirical evidence that ACT’s middle-level concepts capture functional processes. Furthermore, we would argue that these types of analyses do not “prove” that the so-called

processes (functional or otherwise) are being manipulated in the context of therapy.

Mediation analyses. Various statistical techniques are often used to identify the variables that mediate outcomes, where mediation refers to whether change on one measure (e.g., scores on a questionnaire that putatively assesses defusion) explains change on another measure (e.g., reduced scores on a questionnaire that putatively assesses depression). These techniques are typically performed in situations where it is difficult to conduct functional analyses (see Kazdin & Nock, 2003, for a broader discussion). Paradoxically, the questionnaires that putatively assess these mediating constructs are often referred to as “process” measures. At this point, we think it is important to distinguish between the concept of a “process” measure used in the psychometric and/or clinical-research sense and the types of functional processes with which the current chapter is concerned. In short, we would argue that in spite of its name, a psychometric “process” measure does not necessarily capture a functional process. In fact, psychometric instruments serve at best as nothing more than proxies of psychological processes (functional or otherwise). To put it bluntly, filling out a questionnaire or completing a diary that aims to measure fusion, for example, simply captures a self-report about fusion. But it does not necessarily capture the psychological process of fusion itself. Instead, it captures the behavior of filling out a questionnaire or completing a diary.

A lot of clinical research, including that which occurs within CBS, relies on the use of proxy measures. An example is measuring suicidal ideation using a self-report questionnaire. Consistent with our earlier argument pertaining to questionnaires, the tool is not measuring the behavior of ideating, but the respondent’s report on ideating, and even this is done only in the specific context of the questionnaire. We would call this measurement a proxy. Proxies have utility because some level of reliability and validity can be ascertained psychometrically. That is, the proxy behavior (i.e., responses on a suicidal ideation

questionnaire such the Beck Scale for Suicidal Ideation; Beck, Kovacs, & Weissman, 1979) can be shown to be relatively consistent, both within the measure itself and across time (i.e., reliability). Furthermore, this proxy behavior can also be shown to be predictive of the behavior of interest, and not predictive of behaviors that are not of interest (i.e., convergent and divergent validity). However, even if a measure were shown to have all of the above (i.e., sound psychometric properties), it would remain a measure of proxy behavior rather than a direct observation of the behavior of interest. For example, reports of suicidal ideation may have utility, but they do not provide direct access to ideating as it is occurring. Proxies, by definition, remain forever proxies.

As is standard practice in clinical psychological science, mediation analyses employ proxy measures and these are also used extensively within ACT's randomized controlled trials (RCTs), especially to substantiate the claim that outcomes are mediated by psychological flexibility and the hexaflex processes that purportedly comprise it. However, the reliance upon mediation analyses raises questions about the functional distinctiveness of ACT's middle-level terms. Specifically, questions arise around the boundary conditions that must be met in order to distinguish one middle-level term from another. In other words, without knowing where one term ends and another begins, how can we know that there are *six* "processes" rather than seven, or five, and so on? Even aside from the relationships *across* middle-level terms, a question also arises about the relationship between these terms and the concept of psychological flexibility. That is, even if we were to find through mediation analyses that the six hexaflex terms (and other relevant middle-level terms) could be reduced to a single mediating variable, such as "psychological flexibility", such results would not render "psychological flexibility" a functional process. Indeed according to Nock (2007), the use of mediation analyses within clinical psychology is generally problematic because it does not provide *direct* evidence for the isolation of a psychological process. Specifically, Nock

argued that “*while statistical mediation is necessary to support the operation of a mechanism of change, it does not provide sufficient evidence for such a relation. Indeed, just as correlation does not equal causation, mediation does not equal mechanism*” (Nock, 2007, p. 5; see also De Houwer, Gawronski & Barnes-Holmes, in press). Analogously, just as Nock argues that mediational analyses do not provide direct evidence of a cognitive mechanism, we would argue that such analyses cannot provide direct evidence of a functional process.

Analogue studies. It is often suggested that analogue studies complement mediation analyses. In particular, analogue studies have been used in ACT research to scrutinize middle-level terms (e.g., hexaflex components, such as defusion) in highly controlled experimental settings (see the chapter by Levin & Villatte in this book, for example). Typically, these studies investigate outcome effects (using proxy measures) following stress-induction procedures in non-clinical populations (e.g., Foody, Barnes-Holmes & Barnes-Holmes, 2012). However, even under optimal experimental conditions (see Dymond, Roche & Bennett, 2013; and see Kazdin & Nock, 2003 for a broader discussion), analogue studies, almost by definition, must remain silent with regard to whether or not ACT’s middle-level terms refer to functional processes. This is not to argue that analogue studies are without value, but simply to underscore that they do not, in our view, move the field forward in terms of identifying basic scientific (functional) processes (e.g., Gutiérrez-Martínez, Luciano-Soriano, Rodríguez-Valverde & Fink, 2004; Kehoe, Barnes-Holmes, Barnes-Holmes, Cochrane & Stewart, 2007; Keogh, Barnes-Holmes & Barnes-Holmes, 2008; McMullen, Barnes-Holmes, Barnes-Holmes, Stewart, Luciano & Cochrane, 2008).

To fully appreciate the foregoing point, consider the number of errors underlying the logic of analogue studies as the basis for identifying functional processes. An analogue study usually selects a specific component from a larger treatment package (e.g., such as those tested in RCTs) with the goal of creating a procedure that can be studied in a controlled

environment. Although this seems perfectly logical, the very first step of selecting the component constitutes the first error. This selection is based upon a presumed correspondence between the designated component (selected as a “good candidate”) and a target “functional” process. For example, we might select a word repetition task as a good example of *defusion-the-procedure* in order to target *defusion-the-process*. The error thus involves conflating procedure with process. In making the first error, we also make a second in assuming that there “is” such a functional process (i.e., defusion). The third error is in assuming that the procedure in question will provide *direct evidence* of any functional process (i.e., does not rely on proxy measures). The fourth error lies in assuming that the relationship between *that* procedure and *that* outcome demonstrates the specific process one attempted to target in the first place (rather than some other process).

Even if the foregoing errors are fully recognized, it may still be tempting to seek convergent evidence of the target process by seeking similar evidence from another (defusion) task (e.g., the floating leaf exercise). This constitutes the fifth and final error: that is, assuming that having two “defusion” procedures that yield a similar outcome will provide better evidence for defusion-the-process. This simply involves repeating the first error noted above. Indeed, no number of non-functionally defined procedures that lead to similar outcomes will provide direct evidence for defusion the process. In short, while analogue studies can answer the “what” questions (e.g., what procedures produce what outcomes), they cannot answer the “why” questions (e.g., why do those procedures produce those outcomes), insofar as the “why” question is about processes rather than about procedures or outcomes.

Summarizing and Illustrating the Dilemma as We See It

At this point, we would like to summarize our points from the paragraphs above:

A. ACT’s middle-level terms most frequently refer to outcomes or procedures, and not functional processes.

B. Attempts to provide evidence that middle-level terms refer to functional processes involve mediation analyses and analogue studies, neither of which has provided direct evidence of functional processes.

In the paragraph below, we offer a metaphor that we hope captures at least some of the points above regarding our concerns with middle-level terms, such as those in the hexaflex. The reader should note that the original rocket metaphor was created by Blackledge, Moran et al. (2009) for a broader purpose, and the current metaphor is an adaptation of same for a more specific purpose.

A team of researchers built a rocket – a metal tube that used controlled explosions to hurl itself into space. The team became well respected in the field of building rockets. They knew all about rocket construction and its details, and even had a coherent philosophy of rocket building to which all of their construction adhered. Most importantly, they knew the rockets could indeed fly: many of their rockets made it into orbit.

A second team of researchers had the same aim of building rockets that could go to space. They too had a coherent philosophy on good rocket building, to which they adhered. However, they had very different ideas about how to build and test rockets.

In short, everyone agreed that building rockets was important, but the two teams could not agree on what specifically made a good rocket, although both teams thought it had something to do with “speed”.

The first team theorized that the concept of speed was influential to rockets successfully reaching space. They articulated this theory and generated testable hypotheses. For example, a more powerful engine would result in a higher speed, and a rocket must fly faster than a specific minimum speed to be successful. They tested these and related hypotheses across repeated trials involving many different speeds. They concluded from these tests that speed must be manipulated carefully in order for the rocket to successfully reach space. More importantly, they arrived at a basic scientific principle: “speed = distance divided by time”, and from this they were able to work out the minimum speed necessary for a rocket to escape the Earth’s atmosphere.

The other team of researchers also speculated that the concept of speed played a role in rocket travel, but they had a very different approach to its measurement. They employed crowds of spectators to collect data from the ground during the launch of all their test rockets. They asked each spectator questions such as “How fast was the rocket, from 1/Not speedy to 10/Very speedy?” Based on the responses of large samples of spectators, across many rocket trials, and using excellent statistical analyses, the researchers confirmed their view that speed had a role to play in rocket travel, but were unable to work out how much speed was needed for a rocket to escape the Earth’s atmosphere. As a result the second team built many rockets that made it into space, but many that did not.

If you wanted to develop a therapy to solve a complex psychological problem, which of the two models would you adopt? The strategy adopted by the first team would require great patience and time, and many clients might suffer in the interim. However, you would persevere in abstract testing of the critical concept of X, in the knowledge that X was not only essential to your therapy, but that it was the key process to be manipulated in order for you to create change and, more importantly, to understand how change occurs. Ironically, while many of these tests would not even involve participants with the same complex psychological problem, the large body of data would allow you to understand the critical concept of X within therapy and how to manipulate it precisely. In the wider context, your understanding of X could also be generalized to develop other applications of X in other areas of psychology.

The strategy adopted by the second team would require much less patience and time, and as a result fewer clients would suffer in the interim because you could conduct your treatment as soon as possible. However, although you have developed a broad knowledge of the concept X, your limited experimental testing would not afford precise understanding nor manipulation of X. As such, it would become apparent as more and more clients were treated that your treatment is less effective than initially thought, because the limitations in your knowledge of X do not permit you adequate flexibility when novel scenarios emerge.

In a nutshell, we believe that ACT's middle-level terms are an example of the second strategy. They are undoubtedly useful in therapeutic change and they may even reflect the core functional processes that are essential to changing human psychology. This achievement is supported by considerable evidence of psychological change and both mediational analyses and analogue studies of how this change might have happened. However, it is important to note that researchers from a variety of therapeutic traditions have expressed growing dissatisfaction with this general approach (Barlow et al., 2013; Nock, 2007). We too believe

that this strategy will not be enough in the long run because it cannot provide *direct* evidence of the *core* process(es) at work.

We strongly believe that while theorizing in ACT has been of enormous clinical benefit to date, its scientific potential is limited. Many more sufferers of the human condition may indeed access treatment in the interim. However, there may be even more that cannot be helped in the future because the core processes have not been isolated and therefore cannot be targeted appropriately. As basic research scientists, we are no less dedicated, metaphorically, to the flying of rockets, even though we may not pilot them ourselves. We believe that building and flying rockets is an incredibly complex scientific endeavor that will not be serviced adequately without understanding the core processes; without testing these rigorously and directly; and without understanding the precise manipulation of these processes in the construction of each individual rocket.

Section 2: CBS: Towards a Unified Theory

In this section, we open with the need for clarity around how the CBS community can harness each of its elements to progress the science towards a unified theory. We consider whether the recently proposed “reticulating model” (Hayes, Long, Levin & Follette, 2013) can facilitate this agenda. Specifically, we address the question of how successfully we can close the gap between RFT and ACT. We finish by considering future directions for basic research in the service of a unified theory for CBS.

The Pursuit of a Unified Theory

Given that a primary ambition of CBS lies in the construction of a unified theory of human suffering and its treatment, its ultimate goal must be towards a unified theory of psychology by uniting multiple levels of analysis under a single theoretical umbrella. Towards this latter aim, the field can only progress with constructive discussions about how its different elements can, and should, interact with one another. However, it is important to

emphasize that our own view is that these discussions should be open and respectful to avoid any sense of prescription about how any elements of the field should conduct their business. For us, it is more a matter of the community clarifying pragmatic avenues through which coherent progress can be made.

Discussions about unification are not unique to CBS, but can be seen within many areas of psychology. For example, De Houwer et al. (in press) have recently discussed how different levels of scientific analysis within cognitive psychology can effectively communicate with one another (see also Marr, 1982). Many similar debates have taken place within various therapeutic traditions, although it is well known that progress has been limited (e.g., Blackledge, Moran et al., 2009; Barlow et al., 2013; Egan, Wade & Shafran, 2011).

For CBS, the key question concerns how basic researchers and clinicians can exchange information in a meaningful and progressive way. The methods of doing this are often reduced to what are loosely referred to as “top-down” versus “bottom-up” models (Hayes & Plumb, 2007). Some of the problems inherent in the top-down approach are reflected in our discussion of middle-level terms in Section 1. In contrast, a bottom-up approach seeks to first identify basic processes before using them to build a conceptual analysis. Again, we noted some of the problems associated with an exclusively bottom-up approach in the previous section. In our view, however, the contrast between top-down and bottom-up models is often overly simplistic and potentially unhelpful. Indeed, Hayes et al. (2013) appear to have more accurately characterized the relationship as one of “reticulation” motivated by mutual interest in the service of fostering a productive relationship between basic research and applied therapeutic interests.

Most psychologists would agree, in principle, with the potential benefits of a productive relationship between basic research and therapy. However, this does not suggest that everyone's work must be directly in the service of this relationship. For example, basic

researchers often do basic research that seems irrelevant to clinicians, and clinicians do effective therapy that is of no interest to basic researchers. These individual endeavors of the scientist and the practitioner should be treated with the utmost respect, and should not be shoehorned into a broader organizational or philosophical agenda. After all, the point of a unified theory is to enhance the field as a whole, not to be pursued as an end in itself. As Hayes et al. (2013) have argued, “only a very small number of researchers need to be willing to pursue both sides of the issue (basic and applied) to allow an overall team to cooperate” (p. 876).

In the paragraphs below, we discuss whether the reticulating model of treatment development¹ suggested by Hayes et al. (2013; see also Hayes et al., 2012) will serve CBS’s prosecution of a unified theory of psychology. First, we summarize the reticulating model. Second, we elaborate the mechanisms of reticulation implied by the model, as we see them. For example, what forms of information pass between basic scientists and clinicians, and does comparable information pass in each direction? Third, after outlining our concerns with the model, we conclude that it does not offer a fair reflection of the current relationship between basic research and practice in CBS and, more importantly, that this relationship may not appropriately facilitate the pursuit of a unified theory.

A Reticulating Model in CBS

According to Hayes and colleagues (2012), the working model that captures CBS currently is “a reticulated (that is, web-like) model of scientific and practical development, in which theoretical and technological progress occurs at multiple levels but in an interconnected way” (p. 6). The current chapter focuses on the relationships among what Hayes et al. refer to as philosophical assumptions, analyses, and theory (see Figure 1, adapted

¹ We focus here on a reticulating model of treatment development specifically, rather than basic science and its application more generally for two related reasons. First, Section 2 is concerned specifically with the relation between RFT and ACT as the basic science of CBS and a psychotherapeutic application, respectively. Second, the reticulating model proposed by Hayes et al. (2013) pertained specifically to psychotherapeutic development.

from Hayes et al. 2013).

=====

INSERT FIGURE 1 AROUND HERE

=====

Figure 1. Adapted from Hayes et al.’s (2013) “The contextual behavioral science model of treatment development” (p. 872).

In Figure 1, for CBS, we assume that “Basic Theory” refers to basic behavior analytic concepts such as reinforcement and punishment, and the technical terms found in RFT. “Applied Theory”, we assume, refers for example to ACT’s hexaflex model of psychological flexibility (Hayes, Strosahl, et al., 2011). Mutual interest based analyses of “Middle-Level Terms” might therefore include analogue studies and mediation analyses of defusion, acceptance, etc. “Basic Analysis” might refer to, for example, the study of derived relational responding as a model of semantic relations in natural language (e.g., Dymond & Barnes, 1995).² In summary, for a unified theory of CBS, the bridge to be crossed is specifically, but not exclusively, between RFT and ACT.

Reticulation is Asymmetrical

Hayes and colleagues’ (2013, see also 2012) reticulating model appears to assume a transfer of information between basic science and applied theory in a bidirectional, symmetrical manner. Put simply, these authors suggest that ACT can inform RFT as much, and in the same manner, as RFT can inform ACT. The implication is that they are equally and mutually beneficial. In other words, Hayes et al. suggest that the “traffic” that goes from ACT to RFT contains functional middle-level terms that have clinical and psychological

² In making a clear distinction between basic and applied analyses, it is important to note that we are at risk of oversimplifying the nature of research conducted within CBS. For example, studies have employed the IRAP as a measure of fluency in relational responding (which is clearly a basic science focus) and sought to determine if this fluency correlates with measures of middle-level terms such as the AAQ (e.g. Hussey & Barnes-Holmes, 2012). One could argue that such research lies somewhere between the two extremes of basic and applied.

precision and utility. As such, they tell us something important about psychology and particularly about the human condition, and therefore warrant empirical analysis. In return, the traffic that goes from RFT to ACT comprises empirical support for ACT's middle-level terms through a language of relational translation (e.g., an RFT "interpretation" of a given defusion procedure). For Hayes et al., this is a special relationship built upon supposed functionality and shared philosophical roots.

For us, the traffic that goes from ACT to RFT is an orienting exercise that highlights phenomena, domains, and classes of behavior, such as "fusion", "literality", or "psychological flexibility". In return, the expansion of RFT's remit is guided by the consideration of how its established functional processes might be at work in these areas. For example, we might ask "is relational flexibility as measured on an IRAP akin to the type of psychological flexibility that is described in ACT?" In short, the traffic that goes in each direction between RFT and ACT is not symmetrical. We will elaborate on this below.

Before continuing with our traffic metaphor in the context of exploring reticulation, we would like to use the current paragraph to express our concerns about the RFT translations of ACT middle-level terms that are increasingly prevalent in the CBS literature. In short, an RFT *interpretation* does not equate to an RFT *analysis*. For example, Blackledge and Barnes-Holmes (2009) defined defusion as "*well established verbal stimulus transformations being disrupted via the displacement of contextual conditions that control relational responding in general*" (p. 49). Although this description appears to be consistent with the language of RFT, one could question the extent to which it could direct clear functional (experimental) analyses of relational responding. In this respect, RFT interpretations that remain nothing but interpretations have little more to offer than relying exclusively on middle-level terms. Let us put it another way, a functional-sounding interpretation is not a functional analysis (see Hayes & Brownstein, 1986). We consider this

to be an important point as behavioral psychology has already witnessed the problems caused by conflating the two (e.g., Skinner, 1957).

Is Asymmetrical Reticulation the Same as “Bottom-up”?

Historically, the science of behavior analysis has advocated a bottom-up approach (e.g., Skinner, 1938) and some have argued that CBS continues in this tradition (e.g., Hayes & Plumb, 2007; Vilardaga et al., 2009). However, other authors within CBS have pointed to a number of generic limitations in adopting a bottom-up approach (Hayes et al., 2012). These are as follows:

1. Scientific progress is slow.
2. Basic scientists may not be interested in investigating concepts of primary interest to clinicians.
3. Even if basic researchers and clinicians have shared interests in the same concepts, the complexity of the scientific analysis may limit its direct translation to clinical practice.

In short, Hayes et al. (2012) argued that the key dilemma facing the relationship between basic science and therapy is more practical than theoretical. With these limitations in mind, the authors proposed the reticulating model of scientific progress for CBS.

Given the difficulties inherent in trying to reticulate a basic science approach (e.g., RFT) with a therapeutic model that espouses non-functional middle-level terms (e.g., ACT), we would argue that this dilemma is, on the contrary, more theoretical than practical. First and foremost, science is not guided by the proximal demands of therapeutic work. Second, if we are pursuing a unified theory, we will only know that we are succeeding when we can observe expansion of the basic science. While the process involves asymmetrical reticulation with middle-level terms, it cannot rely solely on hijacking functionally precise terms and using them to construct interpretations of therapeutically-important phenomena. Rather, scientific progress is defined by the expansion of the basic account. As such, in our view,

progress is not entirely *driven* from the bottom-up, but is *accomplished through it*.

The End of the Special Relationship?

For us, the relationship between RFT and ACT is not necessarily a special one. We have already made useful developments in RFT by drawing on middle-level terms from other domains of psychology as an orienting exercise. Examples include associations versus propositions (Smyth, Barnes-Holmes & Barnes-Holmes, 2008; Hughes & Barnes-Holmes, 2011), theory of mind (McHugh, Barnes-Holmes & Barnes-Holmes, 2004), and implicit cognition (Hughes, Barnes-Holmes & Vahey, 2012). Once we assume the position that ACT's middle-level terms are not necessarily functional – and are therefore comparable in value to orienting terms from any other area of the discipline – we must also accept that a basic science can reticulate with any middle-level terms insofar as they are heuristic or orienting. Similarly, CBS's basic researchers (i.e., relational frame theorists) might be oriented by *any* therapeutic model, such as Cognitive Therapy, Cognitive Behavioral Therapy, or Mindfulness-Based Cognitive Therapy (see Remue, De Houwer, Barnes-Holmes, Vanderhasselt & De Raedt, 2013; Nicholson & Barnes-Holmes, 2012; Hussey & Barnes-Holmes, 2012, respectively, for examples).

It is of course plausible, given that RFT and ACT have shared philosophical roots, that ACT may require less expansion, modification and/or discard in order to be aligned more closely with RFT than a therapy with philosophically disparate roots (see Barlow et al., 2013 for a broader treatment of the modification of therapy in light of basic research findings). Critically, however, conceptualizing this greater alignment as a move towards integrating ACT with RFT ignores the potential for wider consolidation of psychotherapeutic strategies. Put simply, the point of a unified theory is to bring closer together basic science and its applications *more generally*. Furthermore, we would argue that this type of relationship between a basic science and the full breadth of its professional domain is precisely the type of

web-like model that has been proposed for CBS (Hayes et al., 2012).

How to Advance the Basic Account towards a Unified Theory

We would like to be clear that we are not opposed to the eloquent model of reticulation that Hayes et al. (2012, see also Hayes et al., 2013) have proposed for CBS. Indeed, depending upon how one defines reticulation, we would agree that this is preferable to a “purely” bottom-up approach and clearly more desirable to a “top-down” strategy. Admittedly also, we do not yet have an RFT model of human suffering. Nonetheless, we feel it essential to remain exact and coherent in the concepts we use for both science and practice. Indeed, the field of behavioral psychology is predicated on this. While the call for unification through basic science is not unique (e.g., Blanton & Jaccard, 2006; Hayes & Berens, 2004; Kazdin & Nock, 2003; Lindsley, Skinner & Solomon, 1953; see also Baker et al., 2008; Henriques, 2013; Melchert, 2013), we believe that the core conceptual units and their properties proposed by RFT, and those that may yet be forthcoming, have something unique to offer the understanding of the human condition and how psychological suffering might be alleviated.

Throughout the current chapter, we have argued strongly for the advancement of basic science. At this point in time, the evidence for RFT’s key conceptual unit of relational responding seems robust (e.g. Dymond & Barnes, 1995; Dymond & Barnes, 1996; Healy, Barnes-Holmes & Smeets, 2000; McHugh, et al., 2004; Stewart, Barnes-Holmes & Roche, 2004; Whelan, Barnes-Holmes & Dymond, 2006). In our view, it is now time to go beyond demonstration research on individual frames and the transformation of functions. Specifically, we think an important direction for future research will be to conduct experimental functional analyses of the role that RFT’s verbal units, and their various properties, play in human suffering and its alleviation.

Concluding Comments

We would like to close the chapter by drawing attention to our overriding sense that CBS is a living tradition. We would be disappointed if anything in this chapter served to restrict or constrain the scientific or therapeutic interests of the community or its individual members. It takes courage and creativity to expand science or therapy and/or to take them in a new direction, but doing so is probably essential for achieving progress. For us, progress also involves clearly articulating one's own view and aspirations, and discussing these openly as they relate to the views and aspirations of others. Sometimes these exchanges can be disheartening but they are nonetheless honest and productive. If the community of CBS is to move further in the direction of its ambition towards a unified theory this seems a humble place to start.

References

- Baker, T. B., McFall, R. M., & Shoham, V. (2008). Current status and future prospects of clinical psychology: Toward a scientifically principled approach to mental and behavioral health care. *Psychological Science in the Public Interest*, 9(2), 68-103.
- Barlow, D. H., Sauer-Zavala, S., Carl, J. R., Bullis, J. R., & Ellard, K. K. (2013). The nature, diagnosis, and treatment of neuroticism: Back to the future. *Clinical Psychological Science*. Advance online publication. doi: 10.1177/2167702613505532
- Barnes-Holmes, D. (2000). Behavioral pragmatism: No place for reality and truth. *The Behavior Analyst*, 23(2), 191–202.
- Beck, A. T., Kovacs, M., & Weissman, A. (1979). Assessment of suicidal intention: The Scale for Suicide Ideation. *Journal of Consulting and Clinical Psychology*, 47(2), 343-352.
- Blackledge, J. T., & Barnes-Holmes, D. (2009). Core processes in acceptance and commitment therapy. In J.T. Blackledge, J. Ciarrochi, & F.P. Deane. (2009) (Eds.). *Acceptance and commitment therapy: Contemporary research and practice* (pp. 41-58). Sydney: Australian Academic Press.
- Blackledge, J. T., & Drake, C. E. (2013). Acceptance and commitment therapy: Empirical and theoretical considerations. In S. Dymond & B. Roche (Eds.), *Advances in relational frame theory and contextual behavioral science: Research and application* (pp. 219-252). Oakland, CA: New Harbinger.
- Blackledge, J. T., Moran, D. J., & Ellis, A. E. (2009). Bridging the divide: Linking basic science to applied psychotherapeutic interventions - A relational frame theory account of cognitive disputation in rational emotive behavior therapy. *Journal of Rational-Emotive & Cognitive-Behavior Therapy*, 27, 232-248.
- Blanton, H., & Jaccard, J. (2006). Arbitrary metrics in psychology. *American Psychologist*,

61(1), 27–41.

Dawkins, R. (1986). *The blind watchmaker*. New York: W. W. Norton & Company.

De Houwer, J., Gawronski, B., & Barnes-Holmes, D. (in press). A functional-cognitive framework for attitude research. *European Review of Social Psychology*.

De Houwer, J., & Moors, A. (2010). Implicit measures: Similarities and differences. In B. Gawronski, & B. K. Payne (Eds.), *Handbook of implicit social cognition: Measurement, theory, and applications* (pp. 176-193). New York, NY: Guilford Press.

Dymond, S., & Barnes, D. (1995). A transformation of self-discrimination response functions in accordance with the arbitrarily applicable relations of sameness, more-than, and less-than. *Journal of the Experimental Analysis of Behavior*, 64, 163-184.

Dymond, S., & Barnes, D. (1996). A transformation of self-discrimination response functions in accordance with the arbitrarily applicable relations of sameness and opposition. *The Psychological Record*, 46, 271-300.

Dymond, S., Roche, B., & Bennett, M. (2013). Relational frame theory and experimental psychopathology. In: S. Dymond & B. Roche (Eds.) *Advances in relational frame theory and contextual behavioral science: Research and application* (pp. 199-218). New Harbinger: CA, USA.

Egan, S. J., Wade, T. D., Shafran, R. (2011). Perfectionism as a transdiagnostic process: A clinical review. *Clinical Psychology Review*, 31(2), 203-212.

Einstein, A. (1945). A generalization of the relativistic theory of gravitation. *Annals of Mathematics*, 46(4), 578-584.

Franks, C. M., & Wilson, G. T. (1974). *Annual review of behavior therapy: Theory and practice*. New York: Brunner/Mazel.

Foody, M., Barnes-Holmes, Y., & Barnes-Holmes, D. (2012). Empirical investigation of the

- single-sentence paradigm as a method of stress induction. *International Journal of Psychology and Psychological Therapy* 12(2), 127-138.
- Gutiérrez-Martínez, O., Luciano-Soriano, C., Rodríguez-Valverde, M., & Fink, B. C. (2004). Comparison between an acceptance-based and a cognitive-control-based protocol for coping with pain. *Behavior Therapy*, 35, 767-783.
- Hawking, S. (1988). *A brief history of time*. New York: Bantam Books.
- Hayes, S. C., Barnes-Holmes, D., & Roche, B. (Eds.). (2001). *Relational frame theory: A post-Skinnerian account of human language and cognition*. New York: Plenum Press.
- Hayes, S. C., Barnes-Holmes, D., & Wilson, K. G. (2012). Contextual behavioral science: Creating a science more adequate to the challenge of the human condition. *Journal of Contextual Behavioral Science*, 1(1-2), 1-16.
- Hayes, S. C., & Berens, N. M. (2004). Why relational frame theory alters the relationship between basic and applied behavioral psychology. *International Journal of Psychology and Psychological Therapy*, 4(2), 341-35.
- Hayes, S. C., & Brownstein, A. J. (1986). Mentalism, behavior-behavior relations, and a behavior-analytic view of the purposes of science. *The Behavior Analyst*, 9(2), 175-190.
- Hayes, S. C., Long, D. M., Levin, M. E., & Follette, W. C. (2013). Treatment development: Can we find a better way? *Clinical Psychology Review*, 33(7), 870-82.
- Hayes, S. C., & Plumb, J. C. (2007). Mindfulness from the bottom up: Providing an inductive framework for understanding mindfulness processes and their application to human suffering. *Psychological Inquiry: An International Journal for the Advancement of Psychological Theory*, 18(4), 242-248.
- Hayes, S. C., Strosahl, K., & Wilson, K. G. (1999). *Acceptance and commitment therapy: An experiential approach to behavior change*. New York: Guilford Press.

- Hayes, S. C., Strosahl, K.D., & Wilson, K. G. (2011). *Acceptance and commitment therapy, second edition: The process and practice of mindful change*. New York: Guilford Press.
- Hayes, S. C., Villatte, M., Levin, M., & Hildebrandt, M. (2011). Open, aware, and active: Contextual approaches as an emerging trend in the behavioral and cognitive therapies. *Annual Review of Clinical Psychology*, 7, 141-168.
- Healy, O., Barnes-Holmes, D., & Smeets, P. (2000). Derived relational responding as generalized operant behavior. *Journal of the Experimental Analysis of Behavior*, 74(2), 207-227.
- Henriques, G. (2013). Evolving from methodological to conceptual unification. *Review of General Psychology*, 17(2), p. 168–173.
- Hermans, D., Craske, M. G., Mineka, S., & Lovibond, P. F. (2006). Extinction in human fear conditioning. *Biological Psychiatry*, 60, 361-368.
- Hughes, S., & Barnes-Holmes, D. (2011). The dominance of associative theorizing in implicit attitude research: Propositional and behavioral alternatives. *The Psychological Record*, 61, 465-496.
- Hughes, S., Barnes-Holmes, D., & Vahey, N. (2012). Holding on to our functional roots when exploring new intellectual islands: A voyage through implicit cognition. *Journal of Contextual Behavioral Science*, 1, 17-38.
- Hughes, S., & Barnes-Holmes, D. (2013). A functional approach to the study of implicit cognition: The IRAP and the REC model. In B. Roche & S. Dymond. (Eds.). *Advances in Relational Frame Theory & Contextual Behavioral Science: Research & Applications* (pp. 97-128). New Harbinger: CA, USA.
- Hussey, I., & Barnes-Holmes, D. (2012). The Implicit Relational Assessment Procedure as a measure of implicit depression and the role of psychological flexibility. *Cognitive &*

Behavioral Practice, 19(4), 573-582.

- Kazdin, A. E., & Nock, M. K. (2003). Delineating mechanisms of change in child and adolescent therapy: methodological issues and research recommendations. *Journal of Child Psychology and Psychiatry* 44(8), 1116-1129.
- Kehoe, A., Barnes-Holmes, Y., Barnes-Holmes, D., Cochrane, A., & Stewart, I. (2007) Breaking the pain barrier: Understanding and treating human suffering. *The Irish Psychologist*, 33, 288-297.
- Keogh, C., Barnes-Holmes, Y., & Barnes-Holmes, D. (2008). Fused or defused? Getting to grips with what your mind is telling you. *The Irish Psychologist*, 35, 1-8.
- Luciano, C., Valdivia-Salas, S., & Ruiz, F. (2012). *The self as the context for rule governed behaviour*. In L. McHugh and I. Stewart (Eds.), *The self and perspective taking: Research and applications* (pp. 143-160). Oakland, CA: New Harbinger.
- Lindsley, O., Skinner, B.F., Solomon, H.C. (1953). *Studies in behavior therapy (Status Report I)*. MA: Waltham Metropolitan State Hospital.
- Marr, D. (1982). *Vision*. San Francisco: W. H. Freeman.
- Melchert, T. P. (2013). Beyond Theoretical Orientations: The Emergence of a Unified Scientific Framework in Professional Psychology. *Professional Psychology: Research and Practice*, 44(1), p. 11–19
- McHugh, L., Barnes-Holmes, Y., & Barnes-Holmes, D. (2004). Perspective-taking as relational responding: A developmental profile. *The Psychological Record*, 54, 115-144.
- McMullen, J., Barnes-Holmes, D., Barnes-Holmes, Y., Stewart, I., Luciano, C., Cochrane, A. (2008). Acceptance versus distraction: Brief instructions, metaphors and exercises in increasing tolerance for self-delivered electric shocks. *Behaviour Research and Therapy*, 46, 122-129.

- Nicholson, E., & Barnes-Holmes, D. (2012). Developing an implicit measure of disgust propensity and disgust sensitivity: Examining the role of implicit disgust propensity and sensitivity in obsessive-compulsive tendencies. *Journal of Behavior Therapy and Experimental Psychiatry, 43*, 922-930.
- Nock, M. K. (2007). Conceptual and design essentials for evaluating mechanisms of clinical change. *Alcoholism: Clinical and Experimental Research, 31*, 4S-12S.
- O'Toole, C., & Barnes-Holmes, D. (2009). Three chronometric indices of relational responding as predictors of performances on a brief intelligence test: The importance of relational flexibility. *The Psychological Record, 59*, 119-132.
- Remue, J., De Houwer, J., Barnes-Holmes, D., Vanderhasselt, M. A., & De Raedt, R. (2013). Self-esteem revisited: Performance on the implicit relational assessment procedure as a measure of self- versus ideal self-related cognitions in dysphoria. *Cognition & Emotion, 27*(8), 1441-1449.
- Skinner, B. F. (1938). *The behavior of organisms: An experimental analysis*. Oxford, England: Appleton-Century.
- Skinner, B. F. (1948). *Walden two*. New York: Macmillan.
- Skinner, B. F. (1953). *Science and human behavior*. New York: Free Press.
- Skinner, B. F. (1957). *Verbal behavior*. Englewood Cliffs, NJ: Prentice-Hall.
- Skinner, B. F. (1971). *Beyond freedom and dignity*. New York: Knopf.
- Smyth, S., Barnes-Holmes, D., & Barnes-Holmes, Y. (2008). Acquired equivalence in human discrimination learning: The role of propositional knowledge. *Journal of Experimental Psychology: Animal Behavior Processes, 34*, 167-177.
- Stewart, I., Barnes-Holmes, D., & Roche, B. (2004). A functional-analytic model of analogy using the relational evaluation procedure. *The Psychological Record, 54*, 531-552.
- Vilardaga, R., Hayes, S. C., Levin, M. E., & Muto, T. (2009). Creating a strategy for

progress: a contextual behavioral science approach. *The Behavior Analyst* 32(1), 105-33.

Whelan, R., Barnes-Holmes, D., & Dymond, S. (2006). The transformation of consequential functions in accordance with the relational frames of more-than and less-than. *Journal of the Experimental Analysis of Behavior*, 86, 317-335.

Wilson, D. S. (2012). Making contextual behavioral science part of the united ivory archipelago. *Journal of Contextual Behavioral Science*, 1, 39-42.