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Functional Analysis is Dead: Long Live Functional Analysis

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Abstract

In this rejoinder, we discuss the commonalities and differences of the commentaries to our target article. Each commentary agreed with our basic message that intervention science needs to move from the DSM-governed protocols-for-syndrome approach to process-based treatments. Functional analysis has been a guiding principle since the early days of behavior therapy, but lost its dominance with the ruse of the latent disease model of psychiatry. This model gave rise to disorder-specific treatments with limited benefit to patients and science. We now have the tools and expertise to study human complexity grounded in an understanding of processes of change drawn from and fully applicable to the psychological level of analysis.

We are pleased indeed to read the opinions of such an excellent group of clinical scholars. Each commentary provided a thoughtful and suitable analysis that is likely to be a reflection of the field at large. There were some notable similarities but also interesting difference between these views.

Each commentary agreed with our basic message: Intervention science needs to move from the DSM-governed protocols-for-syndrome approach to process-based treatments. These processes need to be grounded in testable and heuristically useful theories. Interestingly, however, there were some differences in the implications and perhaps also the significance of our call toward process-based therapies.

Perhaps the most cautious view was voiced by Gerald Davison. In essence, he saw our article “more as a restatement of previous scholarship than as novel and innovative” suggesting that “it should be more clearly placed in historical and scholarly context.” We appreciate that context and both of us have written extensively about the history of our field in other venues (e.g., Barlow, Hayes, & Nelson, 1980; Hofmann, 2011; O’Donohue, Henderson, Hayes, Fisher, & Hayes, 2001). We are pleased that students or others who do not know the history will have Dr. Davison’s response available as a well-crafted and succinct reference.

Like a walk up a spiral staircase, as the field returns to old themes in intervention science, it does so from an advantaged perspective. We are better able to see in hindsight what worked and what did not. In Dr. Davison reply four-fifths of the references are more than two decades old and nearly 60% are over three decades old. The practical and scholarly context we are responding to in our target articles includes what has happen in those decades.

Functional analysis began based on Skinner's approach to the analysis of action in its historical and situational context. As we noted in the target article, it has been a guiding principle since the early days of behavior therapy and has been embraced by many notable scholars, including Dr. Davison. But something happened along the way. A search of the term "functional analysis" in Web of Science shows that if you limit the search to the fields of psychology and psychiatry the number of articles that used the term last year is virtually that same as twenty years ago. It continues inside applied behavior analysis in a limited form, but in the context of the enormous growth of intervention science generally, the interest in functional analysis is currently feeble. We join with our friend and colleague in an embrace of functional thinking but we need to deal seriously with *why* that solid start petered out and what will be different this time around.

In our opinion above all the toxic effects of a latent disease model narrowed our vision and our science, strangling functional analysis in its intellectual crib. When modern psychiatry adopted structuralism for its nosology, psychological issues (such as emotional distress, or behavioral problems) became expressions of a latent disease. It was believed by many that biological psychiatry would eventually develop drugs to effectively treat these latent diseases. Billions of tax payers' money went into randomized controlled trials to test the efficacy of specific compounds for DSM-defined disorders. Creative psychological scientists, often under the broad term CBT, developed psychological models of the DSM-defined disorders and developed treatment approaches based on them, but the net effect was to foster syndromal thinking and its latent disease model. Each year the percentage of clients receiving evidencebased psychosocial interventions decreased.

How and why did this happen? For one thing, many psychological scientists went along for the ride, driven by funding agencies and policies. Major psychological scientists served on the DSM panels. It was common for psychosocial treatments for DSM disorders to serve as separate arms of the many RCTs designed to test the efficacy of specific drugs.

This had notable scientific benefits: CBT became the most well-researched psychological intervention. It became clear that the efficacy of these drug treatments showed low treatment specificity and produced effects that were generally disappointing, while the comparator condition, often CBT, was at least as good and often better than the drugs that were tested with lower side effect profiles, produced less expensively, and often with better long term adjustment.

Those data are hugely important and provide us all with a solid foundation for moving forward, but it needs to be noted that as a public health strategy, it has so far failed. Instead of fostering a new wave of dissemination and use of evidence-based psychosocial methods, the exact opposite has occurred. It appears that once psychological scientists fully take on the assumptions of the latent disease model, it is no longer possible to be part of a serious public health discussion about human misery. In turns out that almost any outcome can be used successfully by the marketing arm of a half a trillion dollar industry. We played a rigged game structured by forces and interests foreign to our field.

Although psychosocial interventions (especially CBT writ large) are now undoubtedly efficacious, this period had clear negative impacts on our underlying science. Protocols and manuals trumped processes and mechanisms and entire generations of psychological scientists were socialized into the assumptions of a latent disease model. The field essentially lost its behavioral roots. We moved away from identifying the crucial and controllable causal functional relationships for an individual client in the effort to succeed inside a protocol-for-disorder strategy.

But again we ask: how and why did this happen? It is here that we most part company with our colleague. We believe that the positive functional start of the evidence-based treatment movement collapsed because these early models of functional analysis failed the field scientifically and practically. Much as the way an elderly patient with a weakened immune system will succumb to any one of a number of diseases, early behavior therapy succumbed to the siren call of protocols for syndromes because it had no robust and viable alternative to offer given the limits of the day.

We did not review much of that later history either in our article, nor did our esteemed colleague. The source of failure included all of the following and more:

- the limited range of direct contingency principles;
- the lack of reliability in functional analysis;
- limited data on treatment components and kernels;
- the failure of classical statistical methods to deal with the individual;
- absence of extensive and high density longitudinal data sets;
- absence of ready technology to record client processes in situ regularly over time;
- absence of well-specified, robust, and empirically viable theories and models;
- weaknesses in the underlying basic sciences of genetics, neurobiology, emotion, culture, and cognition, among other areas;
- the lack of methods available to properly test moderation, mediation, and processes of change; and
- bulky assessment instruments not designed for repeated use.

To put it simply, the field was not ready. Now, we believe, it is.

That part is new, even if the core ideas we are arguing for certainly are not. In the words of Teeters and Dimidjian: “not being new does not mean not being important.” Other than honoring our past, the primary reason to take the history seriously is to learn from the mistakes of the intellectual cul de sacs we entered.

In hindsight, the medicalization of psychological suffering needs to be seen as the dead end it was. But that will only happen if we now rise to the challenge we had not met as a field as the DSM III arose. What biopsychosocial processes is best targeted, how, with this person

given this goal? In effect, we need a viable alternative to the DSM. That alternative will not be a better disease model, nor better micro-theories of DSM disorders. What we need are broad and effective models of human suffering and prosperity that specify the processes that that need to be changed and tell how best to do so that. That is what can rise to the public health challenge pointed to by Kazdin and Blasé (2011). If successful it will lead to the functional clustering of people and issues but not as armchair task. It needs to be data driven, multidisciplinary, and grounded in nomothetic principles driven from large sets of idiographic analyses.

The fact that NIMH promoted alternatives such as RDoC (Insel et al., 2010) shows that our future is not going to be a simple continuation of the past. RDoC is heavily guided by neuroscientists but its larger message is that a more process-based approach is back on the agenda. If we can take the next step, finally strip out the latent disease model, and confront afresh how to alleviate human problems and promote human prosperity inside a functional contextual process-oriented model, we have tools at hand that simply did not exist three or four decades ago.

Complex network approaches (Hofmann, Curtiss, & McNally, 2016), for example, give us modern methodological tools that are entirely consistent with functional behavioral analysis; and statistical methods for scaling individual data into nomothetic generalizations have been developed (Fisher, Medaglia, Jeronimus, 2018). Technological advances allow us to gather larger data sets on a single individual (e.g., though ecological momentary assessments). We know much more about functional principles of behavior, cognition, emotion, motivation, culture, genetics, epigenetic, and neuroscience. Testable and highly specified models of change exist, with large data sets relevant to understanding moderation and principles of change.

These more recent historical and contextual events have the clear potential to transform the field of intervention science in a way that was not possible only a few years ago. Thus, there might be a solution to an old problem, right around the corner. The future of intervention science is bright with exciting new possibilities, built on the solid foundations of a more distant past, but using the advancements in knowledge and methods that have occurred since. In our view, the very best way to honor our traditions and to stand on the shoulders of giants is to reach again for what was once out of grasp.

Dr. Teachman acknowledged the problem we outlined and basically agreed with our solution. However, she expressed some initial trepidation to “jump into the water” and fully embrace a process-based approach. As Teachman noted, one of the obvious problems is which processes should be considered. We have begun to describe some of them in our recent book (Hayes & Hofmann, 2018), based on the report of the inter-organizational task force on cognitive and behavioral psychology doctoral education (Klepac et al., 2012). After some thoughtful deliberation, Teachman concluded that the time is ripe to jump. We agree. Her cautionary note initially to identify and train students in empirically-supported strategies to target specific processes is well taken and is an example of how we can use what we’ve learned more recently to pursue older aims. The last time clinical scientists took it upon themselves to recommend strategies for specific psychological problems was the creation of

the infamous list of Empirically Supported Treatments by APA's Division 12. This has become perhaps the most contentious list in modern clinical science. The many reasons why this list has become so contentious should be well-known to the majority of readers (e.g., Chambless & Ollendick, 2001). The primary controversies centered around the fact that most treatments from "the list" were CBT-oriented, leaving other orientations on the side line and marginalized; the list was dominated by DSM categories; and that there was nothing in the requirements to prevent "purple-shirted desensitization" from making it onto the list due to a failure to include on process evidence. A process-based approach avoids all of these problems. It is unlikely to be associated narrowly with any specific treatment orientation since the only entry pass needed is to provide empirical support for the treatment process and the underlying theory. It does not require a latent disease assumption, in fact it encourages the abandonment of that assumption; and it inherently organized the field into functional important differences.

Theory needs to be the foundation upon we can build our intervention science, as acknowledged by Stephen Hollon. He eloquently elaborated on the theoretical significance of these processes by associating them with Robert Sapolsky's (2017) views that these process may be distinguishable by the immediate, intermediate, and distal causes. We fully agree with Hollon that modern neuroscience is likely to inform the psychological processes that are targeted in therapy. Linking these processes to evolutionary science is fully consistent with our own ideas (Hayes & Sanford, 2015) and for the first time in modern CBT so far as we are aware, our text on process based CBT includes a basic chapter on principles evolution science (Hayes, Monestès, & Wilson, 2018). Dr. Hollon's hypothesis that cognitive restructuring is linked to higher cortical processes, whereas several "third wave" behavioral processes more associated with evolutionary conserved limbic structures, seems entirely plausible and is a good example of how tests of processes of change might bring together competing wings and traditions. Recent work by LeDoux, in particular, provides, in fact, some support for this notion (LeDoux, 2000).

Finally, we appreciate Drs. Teeters and Dimidjian's call to consider the larger social and cultural context of an individual when examining the processes. The medical illness model has isolated the individual from its context, thereby creating artificial groups while ignoring essential commonalities and differences. Human suffering and well-being can only be understood in the larger context the individual is embedded. Evolution occurs at multiple levels, in multiple dimensions, and at different time scales, all nested and intertwined in a dynamical system (Hayes & Wilson, 2018). Our understanding of any one dimension, level, or time frame (such as how psychological processes foster or inhibit change within the lifetime of the individual) is dependent on our understanding of processes at other levels, dimensions, and time frames (such as how cultural contexts structure the assumptions, beliefs, and practices of the individual).

In fact, you can interpret our target article as a confrontation of exactly that problem. In a recent article on how to make intervention science more relevant (Hayes & Hofmann, in press), we noted that dissemination of intervention science is inhibited by a model that "oozes privilege." We asked "Where are the clinicians and *their* goals in this picture? Where are the individual clients and *their* needs? Where are the human beings with lives unfolding

as they are actually lived? Where, indeed, is psychology itself?” The kind of research that emanated from the latent disease model rode roughshod over personal and cultural beliefs, goals, and practices, and increasingly could only be mounted by academic medical centers in a handful of Western white countries. A process-based approach opens the door to countries, researchers, cultures, and ideas that have been silenced for too long by the hegemony of a latent disease assumption and the supposedly “homogenous” (read *decontextualized*) groups that research on these imagined entities demanded.

It is possible that treatment will become more complex in order to match the complexity of human suffering. The path ahead is unlikely to be smooth. But a process-based vision seems more likely to be progressive. We now have the tools and expertise to study human complexity grounded in an understanding of processes of change drawn from and fully applicable to the psychological level of analysis.

The spiral staircase of knowledge itself has circled intervention science back to the future. We now have the methods at hand to simplify complexity, based on principles that illuminate the functional processes that lead to the forms of human suffering and prosperity we see. We believe that the time is ripe to use them.

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