PEER REVIEWED

On the role of theory and models of change in psychotherapy research

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There is a split in psychotherapy research between theory and data. The split has produced two classes of models of change: 'specific' and 'common' factor models. In this article, GIAC GIACOMANTONIO draws on the philosophy of science to identify some shortcomings of common factor models of therapeutic change, including the lack of explanatory power and the limited trajectories of rational research. He argues that these qualities are both indispensible to research and better represented in specific factor models of change. A suggested remedy is to appropriate the benefits of specific factor theories without ignoring the value of extant data.

The suggestion that all psychotherapies are equally effective is almost a century old. Rosenzweig (1936) was the first to suggest that all the psychotherapies had won the outcomes 'race' (and all deserved prizes), drawing his allegory from Lewis Carroll's *Alice in Wonderland*. Though it would be decades before researchers would pursue this possibility empirically, Carroll's Dodo Bird endures as the universal metonym for equal outcomes across different psychotherapies.

More relevant to this paper was Rosenzweig's hunch—he gave no data—that equal effectiveness implied common mechanisms of therapeutic influence. If they are all equally effective, the active ingredients must be common to all, despite the patent differences in theory, technique, and even declared goal. It is a logical slight-of-hand (what the logicians call 'affirming the consequent') to use equal outcomes as evidence for common factors (cf. DeRubeis, Brotman, & Gibbons, 2005). Far from extinction, the idea has had the greatest influence on psychotherapy research of recent decades.

Before Rosenzweig, research on therapeutic change consisted of marshalling arguments and evidence for and against the psychological theories of change. These theories of varying complexity offered *specific* claims for their uniqueness and for their unique influence on psychopathology. But with the idea of common factors, all prior theories (no matter how simple or complex) suddenly had something else in common. Their differences became less relevant than their one similarity, namely, that they assumed a technique-outcome link. The very possibility of *common factors* re-labelled all extant theories as *specific factor models*.

Common factor models oppose, by designation, specific factor models. Over the last century their respective popularities have see-sawed on the fulcrum of consilience (the fit between data and theory). The Dodo Bird hypothesis engenders scepticism for specific factor models; they seem blatantly wrong. But such scepticism is prejudice. Much research on models of change promotes common factors, without amalgamating any aspect of specific factor models, as though the prediction of differential outcome was all specific factor models embodied. Had they nothing more to offer?

This paper argues that much of value in specific factor models has been discarded, largely on the grounds of a single advantage of common factor models, i.e., that they model the Dodo Bird finding parsimoniously. DeRubeis, Brotman and Gibbons (2005) have likewise argued that the benefits of specific factor theories have been overlooked. But they fault common factors by denying the Dodo Bird: *'many cases from comparative outcome studies [demonstrate] that one particular treatment is more effective than another treatment for a specific disorder'* (p. 175). In contrast, this paper accepts the Dodo Bird finding as substantiated and directs the discussion to *theory*. After outlining some aspects of the role of theory in psychotherapy research, the shortcomings inherent in common factor models, as *theory*, are listed. Imel and Wampold (2008) have listed and then undermined a number of criticisms of their own common factors model (discussed in 2008 and elsewhere, including Wampold, 2001). They demonstrate that the common criticisms of their model are misinformed. This paper will present instead a different list of criticisms of common factor models in general (Imel and Wampold's in particular) without relying on any of the criticisms they have addressed.

> *To assert…that working alliances lead to therapeutic change is to explain almost nothing. It never touches the 'how'.*

Data, theory, and models of change

Theory is not optional

Even brief consideration of the argument that links equal efficacy to the hypothesis of common factors reveals it as an argument *from* data to theory[1]. It appears to revive the ideal of Bacon (1620) that data observed without yearning lead naturally to theoretical constructions. While this paper does not afford the space to explore fully the development of the philosophy of science over the course of modernity, some relevant consequences are spoken to by two notions:

- data are unavoidably theory-saturated (e.g., Hesse, 1980; Popper, 1994); and,
- the very perception of data without theory is impossible (Heidegger's *Vormeinung*, 1927, p. 192). Theory is not optional.

Taken together, they undermine the data-theory distinction, which is worryingly overstated in our field. Conferences section-off empirical presentations from theoretical ones. Journals advise they publish empirical research rather than theoretical research, as though the former were not entirely dependent upon the latter—the dependence is not mutual. These two propositions alone make folly of the attempt to proceed without having in hand the best of theory we can muster. Assuming their inference correct—that data collection without theory is impossible—we are obliged to declare and articulate the theoretical positions that may operate pre-reflectively when we proceed along 'purely empirical' lines. Theory lurks everywhere.

1 It is precisely this kind of reasoning, from surprise-data back to theoretical conclusion that made Freud the object of scorn to Popper (1963); absent is the re-testing of the conclusion, returning to the data with a (new) deliberate prediction in mind. There ought to be no accidental discoveries in the true sense. It is arguable whether this re-testing has taken place with respect to common factor models of change—models, not just isolated factors (cf. Wampold, 2001).

The nature of explanation

Beyond describing the data, a theory must also *explain* the data (Goldberg, 2004). For psychotherapy research, the explanatory power of a model is the degree to which it offers *explanation* that is not reducible to predicting outcomes from treatment-elements. To rise above description, explanation must involve constructs *in addition* to the two factors observed to be correlated. If we were to assert that water becomes ice *because* we put it in the freezer, we would fail *to explain* anything about water—the assertion is a description or observation, even if phrased as a prediction (i.e., 'do *this*, and *that* will happen'). Other constructs (e.g., temperature, molecular movement and alignment, etc.) must be introduced before the observation can be explained. For models of therapeutic change, these other constructs must be specifically psychological[2]. True explanation can distinguish usefully most specific factor models from common factor models. To assert, for example, that working alliances lead to therapeutic change is to explain almost nothing. It never touches the *how*. An explanation of therapeutic change must speak to other constructs, beyond pairing-up treatment-elements and outcome-measures.

2 For example, to say that patients improve in treatment by the Grace of God is arguably true or false, but this cannot be argued to be a psychological explanation or theory.

Illustration: John Tenniel, *Alice's Adventures in Wonderland*, 1869.

Almost all specific factor theories offer human psychologies of varying comprehensiveness. Most develop their psychotherapeutic assertions *from* comprehensive positions on mental functioning (in health and pathology). They are theoretically rich. Even the most elaborated of common factor models fails to establish a psychology. The explanations are lacking.

Lines of advance in psychotherapy

After both modelling and explaining the data, the provision of a research programme is a third desirable quality of theoretical models. (This notion follows directly from the previous one.) A well-developed theoretical explanation supplies questions, not just answers (Kuhn, 1970; Popper, 1994). The conceptual structure furnishes logically-coherent consequences at each step of the research process. Rational lines of research then open. The well-developed theory presents a matrix of concepts linked to measured constructs. In any scientific enterprise, these other mediating and qualifying theoretical constructs (known as auxiliary hypotheses) are crucial. In contrast, a 'theory' comprising description alone can be exhausted by a single empirical outcome. A 'yes' or 'no' may settle the matter. According to Lakatos (cited in Losee, 2005): *'[A research programme] is stagnating if its theoretical growth lags behind its empirical growth, that is as long as it gives only post-hoc explanations… of chance discoveries…'* (p. 104). Scissors require two blades (Lonergan, 1958).

Contemporary psychotherapy research mimics the position Lakatos describes and gives specific factor models one critical but little-cited advantage over common factor models. Even if we call specific factor models 'wrong', they specify mediating constructs that merit further investigation and refinement, while common factor models tend to lack explicit mediators. This is not to suggest that spurious mediators are superior to no mediators at all. Rather, specific factor models seem *of a kind* to be better able to supply such mediating constructs, by virtue of their complexity. We must borrow back from what we discarded if we are to advance.

Wampold's contextual model

To illustrate, let us consider one popular, well-described, and well-known exemplar of the common factor models: Wampold's *contextual model* (2001). This model demonstrates the issues listed above, namely:

- the absence of an articulated explanatory model;
- the implication of an unarticulated theoretical position; and,
- the closing of lines of research.

Aside from its popularity, Wampold's model may be considered a suitable choice because it boasts a very good fit with extant data, i.e., it attempts to explain otherwise-curious findings. In that sense, it is well-suited to illustrate these purely theoretical shortcomings, because even the most inimical critic could not mistake the theoretical issues below for simpler issues of data-fit.

In summary, Wampold dethroned specific factor models by classing them as exemplars of the medical model[3] and demonstrating the futility of the medical model in psychotherapy research. Wampold's definition of the medical model applies when theories involve:

- a specific complaint/disorder;
- a psychological explanation for that complaint/disorder;
- an identifiable and articulated mechanism of change; and,
- a specific technical intervention designed to cure the complaint by means outlined in the model, i.e., by specified mechanisms (Wampold, 2001).

Specificity is central to this definition. A specific intervention will treat a specific disorder by specified means. The connection between disorder and technique is both articulated and intelligible. Specific factor models are examples of Wampold's medical model, not because they derived originally from medicine and psychiatry, but because they conform to this definition.

Wampold presents a convincing argument for the superior consilience of his contextual model characterised by:

- an emotionally charged relationship that engenders expectations of change;
- a healing context for the treatment relationship, in which *'the client believes that the therapist will provide help and will work in the client's best interests'* (p. 268);
- the presence of a plausible rationale for the client's symptoms that is consistent with the client's extant worldview; and,
- interventions that adhere to the aforementioned rationale and have the support of both parties.

Wampold argues that the contextual model fits the data better than the medical model in terms of absolute efficacy, relative efficacy, specific effects, general effects, allegiance and adherence, and therapist effects. Most of his book, *The Great Psychotherapy Debate,* is devoted to demonstrating that meta-analyses and other studies can be seen to support his contextual model. A wealth of data is on his side. The case is compelling.

But consilience is insufficient for a model of change. Theories are underdetermined, making data-fit itself of common currency, (however indispensible). As outlined above, we must turn to other criteria to gauge the quality and utility of a theory. I argue that the consilience of the contextual model comes at too great a price and fails to replace a number of the essential qualities of the medical model it aims to supplant.

Where oh where is the explanation?

The contextual model describes no *mechanisms* of change. An explanation for how psychotherapy cures—for how these particular contexts cure—is missing. As a model, it gives adequate descriptions of the conditions that must obtain, yet no link is offered between contexts and outcomes except that of contiguity—nothing beyond mere empirical findings. Patients with depression, or anxiety, or anything else, allegedly get better *because* they are placed in these contexts.

If theory is not optional, then we must address the inherent but

3 The Person Centered model of Rogers being a possible exception (Wampold, 2001, p. 27).

unarticulated theory in the contextual model. Philosophy of science warns that where no link is declared, one will (must) be inserted. An implied theory is unavoidable and not to be mistaken as a worthy substitute for a well-developed and articulated theory. Let us begin with the rudiments of theoretical assertions that might be inferred from the contextual model.

Implied mechanisms. The contexts imply mechanisms, and expectation is key. We are told that the client *'**expects** the relationship to develop as he or she divulges emotional and psychologically sensitive material'* (Wampold, 2001, p. 268, emphasis added). This outlines a first step, but like Beck's treatment of the role of the working alliance (Beck, Rush, Shaw, & Emery, 1979) it explains only the accumulation of material for exposure to the treatment proper. Each factor in the contextual model (none more explicitly than the first context, i.e., an emotionally charged relationship… etc.) speaks to this expectation, to a structure containing the *expected* trappings of healing, laid on the foundations of the client's beliefs about his or her problems. The notion that expectation produces cures is everywhere implied, but nowhere declared and nowhere explained.

A chief weakness of the contextual model is the absence of clearly specified and explained relationships between contextual factors and therapeutic outcomes. It was suggested above that explanation is scarce in common factor models of change and that the contextual model[4] demonstrates the point. While data-fit may be good (or at least better than the fit of many specific factor models) inadequate specification at the level of theory fails to supply an explanation of change and to generate hypotheses of change-mechanisms that can be tested.

Implied theory. Another potential danger is the 'common sense' explanation that researchers and practitioners may leap to when a suitable explanation is not proposed. It is easy to overlook the absence of an explanation, because these contexts appeal to psychotherapists' common sense. *'Of course'* a healing relationship with agreed-upon means to achieving agreed-upon goals will be therapeutic. But, again, *how*?

It is not that Wampold proposes a model divorced from organising theory (e.g., Wampold et al., 2001, p. 270; Wampold, 2001). Rather, he follows Rosenzweig (1936) in asserting that the *presence* (not the *content*) of an organising theory is a crucial (and common) factor in producing change. The usual definition of a 'theory' is made a commodity, an item *within* a list of requirements. What counts in the contextual model is the presence of contexts.

But this very position itself is a theoretical one. The implication of theory is unavoidable. Theory cannot be contained *within* an assertion about therapy, as if the assertion itself could escape without casting a theoretical shadow. In other words, Wampold's important distinction between the ingredient and the context serves only to identify both as factors. The 'contexts' have no claim to escape the status and scrutiny of specifiable (and in *that* sense 'specific') factors. Offering us 'the presence of a plausible rationale' explains nothing, but to identify 'presence' as an agent of change is to designate it as a specific ingredient and to imply an associated specific factor model. A context of expectation is no exception. Specific factors are synonymous with articulated models, and therein lies the inevitable paradox of trying to escape theories that specify.

Worse yet are the aspects of the model that serve to prevent it becoming the well-developed and articulated theory required. It is not clear why the contextual model as described needs to supplant the medical model. First, the medical model contains features arguably synonymous with science, including the identification and articulation of mechanisms of change. (It is worrying that a model is presented *in opposition* to this criterion.) Second, are the reasons peculiar to the contextual model: Why can it not be subsumed under the medical model as described? To consider these in turn:

Implied science. An indispensible element of theory, of explanatory models, is that the cause-and-effect relationships be spelled-out clearly. Yet by Wampold's definitions, attempts to articulate or develop such a theoretical explanation of the contextual model serve to defy it. The medical model is *defined* with the property that *'there exists a psychological explanation for the disorder, problem, or complaint'* (2001, p. 203). Thus emerges the conundrum that the more contextual factors are defined and refined (i.e., understood), the less they support the contextual model's *distinction* from the (specific) medical one. The contextual model begs to remain rudimentary. It comes to require (by its own definition) to be above the investigation of direct cause-and-effect mechanisms for fear of infidelity. Surely this quality (ascribed by Wampold to the medical model only) is indispensible.

More curious than this requirement, is that there appears, on reflection, nothing in the contextual model to *require* dispensation with the medical model. The contexts argued as curative factors have no inherent ontological status demanding they be seen as anything other than specific factors themselves, however unarticulated their implied mechanisms may be. In a telling quote, Wampold (2001) insists they inhabit a different stratum from specific factors:

'If one considers the contextual model

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4 Wampold is somewhat ambiguous about whether his contextual model is a common factors model (contrast 2001, p. 26 with p. 148 or p. 206). Clearly I treat it here as one.

to be at the same level of abstraction as other psychotherapeutic theories, then one could design studies comparing a particular approach…with a contextual model approach. This is not possible, however, because one cannot construct a manualized contextual model treatment. In another sense, all treatments are examples of contextual model treatments…' (2001, p. 27).

Wampold's rhetoric positions the contextual model as almost untestable. He demands his contexts reside on a level of abstraction that precludes direct comparison with competing models. He demands they be seen as meta-theory. But why? Surely these factors are not beyond the scope of operationalisation and measurement. Wampold (2001) argues convincingly that the distinction between technique and its context is paramount[5]. So in *that* sense, one might agree that his model is not *specific*. But more worrisome is his exclusion of comparative testability. We cannot compare the contextual model with other models of change, leaving only tests for correlation between contexts and outcome. This second point (in the last sentence of the quote) is the more problematic, because it either flirts with the possibility that the contextual model explains everything (in which case it explains nothing), or it places the contextual model beyond the possibility of being directly disproved. Either way it offers little in the way of explaining therapeutic change.

Addressing Wampold's critique

It is incumbent on us to address the criteria by which Wampold evaluated the contextual model as superior. His criteria were all versions of consilience. Consilience is an indispensible quality for a scientific theory (Thaggard, 1978; Losee, 2005). But, as outlined above, it is not the only criterion of consideration. I have emphasised explanatory facility and auxiliary hypotheses as equally indispensible, though they are often overlooked in common factor models.

A further word on the nature of consilience is warranted. Every theory offers consilience. It is true that some boast 'better' consilience than others, but the definition of 'better' is itself a theoretical exercise. There are no theoretically neutral grounds on which to stand, from which to make comparisons. Theories are underdetermined; the same data can be seen to fit a potentially infinite number of theories. Where data were once seen to imply or to force a certain theoretical position, the question of what data are is no longer so simple. As outlined above, data are not perceivable without one or another conceptual expectation. It is not that we fail to shrug-off these conceptual frames, thus polluting our data-gathering, but rather, that data-gathering is simply not possible without them (Hesse, 1980). Data demand bias. And each specific factor theory has its own explanations for its relative lack of consilience.

The fate of models of change is that they cannot hope to claim superiority on the basis of consilience alone. *Every* specific factor theory offers an explanation for the Dodo Bird effect. While these explanations may be ranked by the feeling of conviction they produce (Stoicism), such distinctions must not be mistaken as purely qualitative, or ultimate, or final.

Common factors in general

As noted above, the contextual model is no special case of the issues discussed, and it has been used to illustrate widespread problems in the discipline. By way of another example, the working alliance is far and away the most studied common factor (Castonguay, et al., 2006, Castonguay & Beutler, 2005; Horvath & Bedi, 2002; Martin, Garske, & Davis, 2000; Hubble, Duncan, & Miller, 1999; Horvath, 1994; Horvath & Luborsky, 1993; Gaston, 1990; Gomes-Schwartz, 1978), yet it suffers the same alarming theoretical homelessness. If a therapeutic alliance is correlated with outcome—even assuming a causal relationship, however controversial that may be (compare Webb, DeRubeis, Amsterdam, Shelton, Hollon, & Dimidjian, 2011, with Crits-Cristoph, Gibbons, Hamilton, Ring-Kurtz, & Gallop, 2011)—it requires a theory to know how an alliance is created and how an alliance works. These two problems are telling:

The first has untold consequences for training programmes. Without sufficient theory we cannot teach therapists how 'to have' a good alliance with a patient (Castonguay, et al., 2006). Again, theory is not optional, though it can remain unarticulated. Therapists are left to common-sense explanations of how therapeutic alliances are made, which may default to making friends with the patient, or behaving at least partially as friends. It is common to see case presentations where colleagues speak of creating an alliance in the early phases of treatment before focusing on the therapeutic tasks, as though alliance spoke to a relationship different from the therapeutic one.

The second problem hurts the scientist more than the practitioner. We cannot explain how an alliance cures without reference to a de facto theory. This point has been made in the previous discussion of the contextual model. Let us add only that with a theory-free exploration of alliance, we run the danger of merely expanding the usual list of alliance elements (bond, goals, and means) to include

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5 The *'provision of new learning experiences, as an example, will not be therapeutic unless the client perceives the therapy to be taking place in a healing context in which he or she as well as the therapist believe in the rationale for the therapy'* (2001, p. 26).

discrete techniques, e.g., '*exploration, reflection, noting past therapy success, accurate interpretation, facilitating the expression of affect, and attending to the patient's experience*' (Ackerman & Hilsenroth, 2003, p. 28). This fails still to furnish a well-developed psychology, and contributes little or nothing to improving psychotherapy effectiveness (Silberschatz, 2009).

Alternative uses for specific factor models

I argue that specific factor theories must be re-introduced into the research on therapeutic change. The well-known shortcomings of specific factor theories cannot be overlooked, but as noted above, each specific factor model implies a theoretical position on the Dodo Bird effect, and each provides some degree of consilience. Most, if not all, specific factor theories come with a richness of theoretical edifice that offers precisely what I have argued is lacking in common factor models. Most offer:

(a) explanations beyond descriptions;

(b) explanations that are (more) declared than implied from a psychological point of view; and,

(c) sufficient theoretical richness to foster lines of research inquiry (via mediating factors).

In other research (Giacomantonio, 2012), I have found, for example, that the theory of self psychology (Kohut, 1971, 1984) offers a model of how equal outcomes may belie multiple mechanisms, with multiple corresponding types of change, each of which involves expectably similar symptom-change. From this perspective, we might wonder whether the Dodo has used the wrong criteria to evaluate the race. Variance might yet be measurable where none has been found, if indeed the equality of symptom change can be differentiated along other, equally therapeutic, factors. This possibility rests on the theoretical distinction between symptoms and structure (see for example Grande, Dilg, Jakobsen, Keller, Kraweitz, Langer, Oberbracht, Stehle, Stennes, & Rudolf, 2009). Many research possibilities open from this one consideration. One cannot yet comment on the robustness of this prediction in empirical studies, but it might be enough for the current paper to acknowledge that specific factor theories can offer more than predictions of their own clinical superiority.

Summary

Psychotherapy research faces something of a crisis. The split between theory and data, and between specific factor and common factor models of therapeutic change have created something of a stalemate. I have suggested that indispensible qualities of scientific theories abound in specific factors models, yet remain scarce in common factor ones. I sought to illustrate this point with a well-known exemplar: Wampold's contextual model. It is significant that Wampold wrote as though his contextual model did not require the same development and elaboration of mechanisms as any other. Or perhaps more precisely, as though a specific theoretical model were not always inherent in any such argument of contextual factors. I hope to draw attention to the lack of explanatory power and to the related closing of lines of research. I have argued for a re-consideration of specific factor theories on the grounds that (a) they emerge de facto whenever models are honed, and (b) they offer qualities that common factor models (inherently) lack. This is not simply a criticism of particular (extant) common factor models. It is an argument for why the definitions of common factor models and specific factor models make this split unavoidable.

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