

Handbook for Theory,
Research, and Practice
in Gestalt Therapy
(2nd Edition)

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Edited by

Philip Brownell

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Handbook for Theory, Research, and Practice in Gestalt Therapy
(2nd Edition)
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Edited by Philip Brownell

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I believe it is appropriate to dedicate this book to colleagues in the global research movement in gestalt therapy. As this book will show, they are many. They are intelligent. They are accomplished, and they have enriched my life tremendously. They are the people engaged in research. They are the chapter contributors, the participants in research conferences, and the people who interact online around issues pertinent to research in gestalt therapy—and they are present, one way or another, in these pages.

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PREFACE

This book is a true second edition. There are few duplications from the first edition. They could rightfully be considered two different books, and the reader might enjoy having copies of each. Comparing the two, with the distance of an entire decade between them, is an interesting exercise. One thing that will be consistent, I'm unfortunately sure, will be my mistakes as an editor. So, I apologize to the chapter contributors. I have attempted to stand the phrasing and positioning of what these authors have given. I have tried to put the elements of their chapters into a consistent style, but I've made concessions as well. After all, the content is what is important, not so much the style. Style, punctuation, spelling mistakes, and even reference citations can be distracting—and for that I apologize—but read beyond, through, and around such things. It's what these people say that really counts.

And my goodness. Do they ever say it. This is a long book, and it is a thick book. I challenge people to read it, because it is a wonderful collection of thinkers all focused on the emerging gestalt therapy research tradition. Far from the dry and tedious subject many people might think research is, I have been revitalized in my appreciation of gestalt therapy by becoming involved with colleagues in this movement. I delight every time I witness someone surprised by the fascinating things gestalt practitioner researchers are investigating and how they are going about it.

We are moving most certainly beyond the needed, but rudimentary establishment of an empirical support base for gestalt therapy—the evidence that gestalt therapy works. We are beginning to conceptualize research into the processes of change that make gestalt effective. We are beginning to see how research can improve our training practices. We are open to expanding this research tradition to include research into the application of gestalt therapy theory and practice in organizational work and coaching.

We are also seeing the emergence of gestalt therapy research communities and the teaching and supporting of research at the institute level. As a result, ours is a much richer global gestalt community.

So, I give this book up now, having lived with it in one form or another for the last ten to twelve years.

I look out of the window in my study. The days are growing shorter. There is a coolness to the breeze that sweeps across the high desert and

gently shakes the sage brush. In the evenings I can hear the roar of Shoshone Falls. I know that in a couple of months it will be cold here, and I'll have to put the heater on when I go out to my study to write.

At that time I want to sit on the deck with my wife, Linda, and watch the raptors soar above the Canyon. I want to relax with no deadline coming down over my shoulders like a heavy harness driving me to work, to think, to be careful about words, periods, commas, dates, and the correct form of all those things.

I hope you, the reader, will enjoy this book. It is leaving me now, and after awhile I won't think about it. It will become as if someone else's work.

Then I will pull weeds and water my trees until the snow arrives.

Philip Brownell
From the rim of the Snake River Canyon,
Above Shoshone Falls,
Twin Falls, Idaho, USA
Autumn, 2018

PART ONE

A GROUND BY WHICH TO THINK ABOUT RESEARCH IN GESTALT THERAPY

CHAPTER ONE

INTRODUCTION AND FRAMING OF THE BOOK

PHILIP BROWNELL, PETER PHILIPPSON,
MADELEINE FOGARTY, VINCENT BEJA,
PABLO HERRERA SALINAS, JAN ROUBAL,
AND TOMÁŠ ŘIHÁČEK

The first edition of this book came together between 2006 and 2008. It became translated into several languages. It called for gestalt therapists to take on the challenge of establishing a research tradition for gestalt therapy, and it described both the core theory of gestalt therapy and also various approaches to research. It is now some ten years later. The gestalt community *has* undertaken to establish its own research tradition. It has held three major international research conferences and an international research methods training seminar. It has compiled a book based on these meetings,¹ and it is moving toward the creation of its own, dedicated research organization. It's fourth international research conference is set to be held in 2019, and it is already beginning to look toward 2021. This book is an update on the progress of the gestalt research movement and a true update of the first edition published in 2008.

This chapter is an introduction. First, I want to include some information from the first edition that is a good way of orienting to the issues of warrant or justification for anything, including any given practice of psychotherapy, and an orientation to evidence-based practice. Second, I want to move on to the gist of a conversation about research among some of the contributors to this book that was brought about through reflecting on the new Gestalt Therapy Fidelity Scale (GTFS) (see Madeleine Fogarty's chapter on that elsewhere in this book). What we discussed is also basic to our orientation

¹ Roubal, J. (Ed). (2016). *Towards a research tradition in gestalt therapy*. Newcastle upon Tyne, United Kingdom: Cambridge Scholars Publishing.

towards research and reflective of our state of growth at the time this book went to press.

Recalling Issues Related to Warrant, Evidence, and Evidence-Based Practice

In the beginning of the first edition of this book the authors raised the issue of warrant: what constitutes sufficient justification for the practice of gestalt therapy? Might it be, they wondered, the so-called evidence provided through randomly assigned clinical trials (Goodheart, Kazdin and Sternberg 2006, Nezu and Nezu 2008)? Might it reasonably include other types of "interventions," treatments, and techniques like those listed by the American Psychological Association (APA 2006)? Indeed, what constitutes the "evidence" in the construct of "evidence-based practice?" Is it process outcomes studies? Is it gestalt-informed qualitative research? Is it the common factors research or the practice-based or client-centered outcomes such as those suggested in the writings of Barry Duncan and Scott Miller (2000) or Hubble, Duncan and Miller (1999)? What is it? Do we look exclusively to the university and its largely group studies using convenient student populations or do we move beyond the university and its reduction of variables to the messy world of larger clinical practice? The answer relates to the issue of the gulf between research findings and clinical practice—the application of research to practice.

Relative Evidence

Certainty "is either the highest form of knowledge or is the only epistemic property superior to knowledge" (Reed 2008, np). In a world in which certainty escapes us, no form of evidence can rise above the need for degrees of confidence and measures of error, or random variance. In such a world, we can only have relative forms of support and more or less warrant.

Thus, while we may have a sense of the truth of a reality that is ontically independent, we only have a relative understanding of it, and even that comes from a subjective experience within it. With such a critically realist perspective as ground, what are acceptable ways of justifying one's interpretation of experience and thereby supporting one's beliefs?

Personal experience and assertion. One way is that people can contemplate the assertions of others regarding what they have experienced. This is what resides behind the use of self-report tests and the testimony of witnesses-of-fact in forensic psychology. The problem is that taking

someone's word for something doesn't seem to carry enough heft. In and of itself it does not seem justified or warranted.

Adler (2006) claimed that a testimonial chain of knowledge must arrive at a speaker who knows directly by perception in first-hand experience. The view that the reliability of testimony can be justified by inference and induction based on such testimony is called reductionism—that it reduces to the inferences. That view is opposed by anti-reductionists who hold that testimony is a source of warrant in itself. Anti-reductionists also view reductionists as holding to an individualist epistemology. Conversely, they favor a social epistemology, which holds that the possibility of knowledge we gain from testimony depends essentially on our membership in an epistemic community. It is evaluated by the context of the person giving testimony and his or her place within it.

Thus, testimony is relative, not only in terms of absolute truth, but also in terms of its context and etiology in an "epistemic community." People in such a community ask if there is social validity associated with any given inquiry (Gresham and Lopez 1996). How does it fit? Is there social significance and importance associated with research and are the interventions and procedures socially acceptable? Some will say one thing and others will say something else. There will be those who emphasize the need for internal validity (the context of the laboratory) versus those who emphasize the need for external validity (the context of the clinic).

What people say arises out of the relational matrix in any given research or epistemic community. Gestalt therapists recognize this as reference to the spheres of influence that comprise the *field*. Thus, the evidence of testimony is relative to a context.

The report from personal experience, in and of itself, is often regarded by some as constituting sufficient warrant; however, it is insufficient for others. When gestalt therapists assert the effectiveness of gestalt therapy and refer to their clinical experience, that would be acceptable to some, but when the lens of the field is widened it becomes lacking. Testimonial is a means for establishing warrant, but its degree of relativity is too high. In the context of an evidence-based practice, testimonial alone cannot stand alone.

Rejection of warrant based on foundationalism. Sometimes people will attempt to justify one belief or assertion based on another (more foundational assertion), but if that supporting assertion is not warranted, one simply creates an epistemic regress. The skeptic would maintain that such regress is inescapable, that it constitutes an infinite regress, and therefore warrant is impossible. That would make all research futile, and therein resides the flaw in the skeptical stance. It is practically unacceptable,

because within limits we can justify various kinds of beliefs and assertions and we simply must be responsible. Thus, Kvanvig (2007, np), speaking of coherentist epistemic justification stated, "This version of coherentism denies that justification is linear in the way presupposed by the regress argument. Instead, such versions of coherentism maintain that justification is holistic in character."

In Quine's (Quine & Ullian, 1978) version of coherentism, for instance, our "web" of beliefs forms an interconnection in which the structure hangs or falls as a whole. Thus, justification is a feature of a system of beliefs—a gestalt.

This is an attractive way for gestalt therapists to consider the construct of warrant, because holism is already a central component in the belief system inherent to gestalt therapy. Thus, research in support of gestalt therapy would be most helpful if it provided many strands and intersected many other strands at points in such a web of meaning.

The rejection of conclusive evidentialism. There is no way to escape the point that all "evidence" in support of practice is relative. At this point it might be helpful to establish some of the implications of that statement. Evidentialism in psychotherapy claims that unless there is conclusive evidence for the efficacy of a certain practice, one lacks warrant and should not engage in that form of practice. Addressing evidentialism in religion, Forrest (2006) observed the evidentialism suggests a full religious belief is not justified if there is not conclusive evidence for it, and since known arguments for the existence of God, including experiential ones, are probable ones, no one would be justified in having a full belief that there is a God.

This is the crux of the problem with regards to the efficacy of psychotherapy. Some might claim that belief in gestalt therapy's efficacy/effectiveness is not justified unless one has conclusive evidence to support its practice (and that is impossible).

Some in the EBP movement take an evidentialist approach to warrant. I once met a psychologist trained in a strict application of such evidentialism. She found herself in a dilemma. She needed to conduct assessments for, and provide therapy to, an offending population, but she could not find specific instruments and interventions that were documented in the research literature for her particular population (a certain culture of people on a particular island nation where virtually no specific research had been conducted). Thus, she needed to operate with a relative degree of confidence, extrapolating from the research literature that she could find. This, however, flew in the face of her training, a training asserting the limits of application based on the model of empirically supported treatments

(ESTs). ESTs not only describe intervention procedures, but also describe the appropriate populations for which such procedures apply. Thus, she was lost. She could not, in good conscience, do the job for which she was hired in accord with the training that she had received.

Consequently, the magnitude of evidence necessary to attain warrant is a relative quantity, and it cannot be ascertained in isolation. In every case, it must be assessed in connection with other components of a given situation. Warrant is contextual and the evidence that is available and applicable is relative to one's context.

Evidence-Based Practice

The American Psychological Association adopted a working definition of evidence-based practice, and they asserted that *evidence-based practice in psychology* (EBPP) is the integration of the best available research with clinical expertise in the context of patient characteristics, culture, and preferences (APA 2006). They went on to make a critical distinction between empirically supported treatments and evidence-based practice and to open up multiple and relative streams of support as "evidence:"

ESTs start with a treatment and ask whether it works for a certain disorder or problem under specified circumstances. EBPP starts with the patient and asks what research evidence (including relevant results from RCTs) will assist the psychologist in achieving the best outcome. (APA 2006, 273)

They went on to say that ESTs are specific treatments that are shown to be efficacious in controlled circumstances, but that EBPP include a broader range of clinical options that include clinical interview and assessment, case conceptualization, and the therapy relationship itself. Evidence-based practice includes multiple streams of research evidence—including but not limited to the controlled efficacy studies.

The APA task force pointed to a range of research designs that all contribute to the body of knowledge relevant to evidence-based practice. They include clinical observation, qualitative research, systematic case study, single-case experimental designs to examine causal factors in outcome with regard to a single patient, process-outcome studies to examine mechanisms of change, effectiveness studies in natural settings, Random Controlled Treatments and efficacy studies for drawing causal inferences in groups, and meta-analysis for observing patterns across multiple studies and for understanding effect sizes. With regard to any particular treatment intervention, the task force identified two considerations: does the treatment work, does it cause change—a question of its efficacy, which is most related

to internal validity, and does it generalize or transport to the local setting where it is to be used—a question of its effectiveness, which is most related to external validity.

In spite of the variety of these methods, globally a number of problems have been observed with EBPPs. They are limited in regard to the generalizability of the results in their empirical supports, and that leaves a lack of confidence in them among clinicians. Furthermore, clinicians are often distant in many ways from the processes involved in such research, and the results have low transportability to clinical practice. In addition, evidence-based movements overemphasize treatments and treatment differences, ignoring outcome results on psychotherapy demonstrating variation among psychologists, the impact of relationship, and other common factors (Wampold and Bhati 2004).

In contrast, Practice-based Evidence (PBE) provides a bridge for this gap between research and practice (Evans, Collins, Barkham, et.al. 2003).

Practice-Based Evidence

Practice-based evidence has been characterized as a bottom-up process of gathering data that relies on the experience of practicing clinicians to inform treatment (Dupree, White, Olsen and Laflour 2007). It is research that takes place at the level of the practice. Practice-based research networks (PBRNs) have been utilized to cooperate among clinician-researchers across diverse organizations in preventative medicine (Green 2007); such PBRNs seek to increase external validity and the generalizability of results. The mental health system in one locality, for instance, discovered that linking EBP with the research strategies associated with practice-based evidence (PBE) could improve service to clients. Outcome measurements were used to bridge between EBP and PBE, and they were based upon objective factors and clients' perceptions of care, often utilizing standardized measures at referral, during moments of assessment, the beginning of therapy, at discharge and then again at some interval following. In the agencies in question, this process became systemic and often provided useful clinical information as well as a read on client progress (Lucock, Leach, Iveson, et.al. 2003). Wade and Neuman (2007) found that integrating research skills into clinical processes could correlate clinical practices with treatment outcomes, providing helpful feedback to clinicians regarding the effectiveness of their methods. Unfortunately, they also observed that the average clinician lacks the time, resources, and expertise to work out such an integration without support. Several studies in the United Kingdom argued for utilization of an outcomes instrument known as the Clinical

Outcomes in Routine Evaluation (CORE) to assess the effectiveness of treatments from such a bottom-up, practice-based perspective (Barkham, Mellor-Clark, Connell 2006; Stiles, Leach, Barkham, et.al. 2003; Barkham, Margison, Leach 2001, Mellor-Clark, Barkham, Connell, et.al. 1999).

Although many people have bridged the gap between EBP and PBE with outcome studies, surveys, and qualitative studies to discover patterns in actual practice, one of the research designs identified by the APA task force serves as both a form of evidence in support of EBPs and as a form of PBE. That is the single case time trial, otherwise also known as case-based time-series analysis, or single case, timed series research design (SCTS). Borckhardt, Nash, Murphy, et.al. (2008) pointed out that the

...practitioner-generated case-based time-series design with baseline measurement fully qualifies as a true experiment and that it ought to stand alongside the more common group designs (e.g., the randomized controlled trial, or RCT) as a viable approach to expanding our knowledge about whether, how, and for whom psychotherapy works. (p. 77)

They also pointed out that the APA Division 12 Task Force on Promotion and Dissemination of Psychological Procedures recognized such time-series designs as important and fair tests of both efficacy and/or effectiveness. Thus, the single-case research design can do a great deal for gestalt therapists. It is a design individual gestalt therapists can utilize at the level of the clinic to track the process of therapy with individual clients, and if they collect the data across several clients, they can make observations about patterns emerging in the way they practice. Further, aggregates of several gestalt therapists using the same designs could be used to observe still larger patterns.²

Would these patterns provide conclusive evidence that gestalt therapy works? No. However, they would contribute to a growing body of relative warrant.

What Makes Good Research: A Conversation

We have come a distance from the nature of the situation when the words above were first written. Single case, timed series research is well underway. The first such project arose out of conversations at the first research conference at Cape Cod in 2013. As mentioned, the first research

² In fact, SCTS design has been used recently to establish the efficacy of gestalt therapy in working with patients suffering from anxiety (Herrera, P., Mstibovsky, I., Roubal, J., Brownell, P., 2018.)

paper, reporting on this first project, has been published (Herrera, et al., 2018). The practice-based research network started through that first study has been expanded in a large, funded research project involving groups from Europe, North America, Mexico, South America, Australia, and Russia, all using the single case experimental design.³ We have also addressed the need for more accountability in our research by the creation of a fidelity scale for gestalt therapy. The research movement has momentum, and at times it seems that everyone in the gestalt community is suddenly concerned to appear as if they are interested in or actually involved with research. But what is it that makes for good research?

Following the writing of the chapter in this book on treatment manuals, a group of people who have been instrumental in leading gestalt research discussed research-related issues when considering the development of the Gestalt Therapy Fidelity Scale (GTFS), which is also described later in a chapter in this book. The discussion is captured in part here as an example of the kinds of discussions needed in the gestalt community and because of the points people mentioned.

After Philip Brownell pointed out that the chapters in the book being suggested as a treatment manual included the newly formed GTFS, Peter Philipsson responded, and the discussion ensued. Peter said that Phil pointed to the GTFS, adding that it was created in part by expert raters in the field of gestalt therapy and had been validated as a descriptive list of things gestalt therapists do when they are doing gestalt therapy.

He continued: People who were at the panel in the Paris conference know that I believe that Madeleine's "fidelity scale" is fatally flawed and would pass through any reasonably competent person-centered therapist. Worse still, the "validation" did not compare the Gestalt people with anybody close to that person-centered side. Worse still, I was told after the panel by people who were involved in the validation that this objection was raised by them, and neither addressed nor published as a caveat in the literature on the scale, which in the *real* world of research would mean the journals involved would repudiate the research and question the motives of the authors.

Worse still, it is now becoming easy to say that the scale is a "list of things gestalt therapists do," and I have seen that approach taken on a training workshop at a major Gestalt training institute. Put aside the idea of

³ This is a project funded by the John Templeton Foundation and administered through Brigham Young University. "Spiritually Integrated Processes in Gestalt Therapy: An Investigation of Faith, Awareness, and Spontaneity in Psychotherapy." Philip Brownell (Principal Investigator), Mark Reck, Jelena Zeleskov Djoric, Pablo Herrera Salinas, and Hannah Acquaye (core team).

making gestalt therapy (or psychoanalysis from which it came) a list of things to do, the list is almost entirely “one-person psychology,” with an overwhelming bias towards paying attention to what the client does (e.g. noticing the client's physical experience, not the therapist's). The work which I would do and see as most fully gestalt therapy, the kind of work described by Gianni, Jan, Michela, Margherita, would not be passed as gestalt therapy by using the list.

I am strongly in favour of research in gestalt therapy, and I am not even against manualization or a fidelity scale, but it has to be gestalt therapy that is being researched, not that gestalt therapy has to play what Fritz Perls called the 'fitting game' to fit a more easily researched dilution of gestalt therapy, and then in order to get supported as a training institution, an institute will have to show that this is what they teach, and so on. The real story of this scale is that to capture what gestalt therapy is is a difficult task, maybe also that there is still a lot of one-person psychology going on out there among the expert raters, or that it was easier for the initial proposers of list items (of which I was one) to speak about the one person aspects than to put the relational aspects into succinct words.

Peter then pointed to his article in the *British Gestalt Journal* in which he inscribed his objections as raised during the panel at the third international gestalt therapy research conference in Paris, France, which was held in 2017. Vincent Beja, one of the co-conveners for that conference, responded.

Vincent Beja: I am resonating with Peter's comments on the GTFS. The question of the person-centered therapist has been one of my earliest concerns regarding the scale's validity. As I was one of the 60 people involved with the poll, I can say that there wasn't much space to think outside the box, which would have made the whole process much more hazardous in reaching a tangible result.

Peter Philippon: Thanks Vincent. I don't know if there are ways to set up a more relational scale. It is not my area of expertise. If not, we need to face that and its consequences rather than falsifying what we do to fit it into an easier paradigm.

Vincent Beja: Well Peter, I don't think that using the GTFS is falsifying our practice. We collectively have to be cautious and critical in using already made categories of any kind. Science reduces the richness, thickness, of what it studies, but in the process we may gain valuable insights. We need to stay aware to the fact that science is not “the truth” nor “the real.”

Peter Philippon: Dear Vincent, if the practice is relational, and the scale doesn't support a relational practice but one that is easier to describe, and is already being used in some places as a paradigm of good practice, I don't know what else it is. Quite simply, since my practice has become more and more relational, its effectiveness has increased, and our connection to the latest neuroscience research has deepened (I write about this in my BGI

article). We have the prospect of putting in place a scale that takes us away from what I see as both most central and most effective in our therapy.

Then, Philip Brownell joined the conversation more fully. He and Peter share a long history of interacting with one another and so their style with each other does not hold back and seems more confrontational. However, as admitted by both people, it simply reflects a ground of familiarity that allows them to be more direct.

Philip Brownell: As far as the GTFS you [Peter] say some challenging things. The fidelity scale is not an exhaustive test of all things gestalt. It cannot capture the nuances of theory; it relates to theory but it is not the theory itself. As such it cannot satisfy our hunger for such nuances of theory. Second, it only attempts to be an observer of the therapist. *That* is the nature of a fidelity scale. It speaks as if to ask, “What is the therapist doing? Does that look like gestalt therapy?” So, when you complain that it doesn’t capture the nuances of *relational* gestalt therapy (which by definition has to look not at what the therapist does but at what the interaction between therapist and client does), then it seems to me like complaining that an apple is not an orange. Of course you are correct. It isn’t. Third point: the GTFS *was* created by expert raters, and then it was validated. As Vincent points out, the science is not the truth; at best our research can observe some things that have some measure of correlation, some measure of causality, some emerging themes, etc. No, we do not live in a positivistic, or as Husserl put it a “naturalistic,” day in science. We realize we know in part, but we assert that we *do* know. So, the GTFS does discern when people are practicing what many in the gestalt world would recognize as being gestalt therapy. As you imply toward the end of what you wrote, perhaps we should look at that. Perhaps a more worthwhile consideration would be what the GTFS found out rather than trying to knock it down as if it were a liar who needed to be discredited and rejected. Fourth, you may have a point about not discriminating between gestalt and person-centered; one form of validity is called discriminative validity, and so further research could be done to investigate that. And to this point of validity, there are several kinds of validity, so tearing down the “validity” of the GTFS *in general* is not helpful. The point being that it can have high validity in one way and not so high in another. But that is something that can still be investigated.

As I say in the chapter on manuals, what we are doing as we really start gearing up in the generation of research is to mark out fields of research within the larger gestalt therapy movement. What starts out simply wanting to show that gestalt therapy “works” must evolve into specialties (if you will)—focal points in ongoing research in which people dig down into tenets of our theory and check them out, develop ever more useful and valid tools by which to evaluate our practices, our teaching and training methods, and by which we evaluate our sacred cows.

So, we have made a start with this section of the book that I am suggesting as a treatment manual. My goodness, *no*. It's not perfect. It certainly can be improved upon and I can imagine a whole new genre of gestalt literature: gestalt treatment manuals focused on work with specific populations. The wonderful book edited by Gianni, Jan, and Michela is in that direction. However, what I think is necessary is that we turn from tearing down and destroying the work of others and turn toward making something better ourselves. If you don't have the expertise to create a better, more relational fidelity scale, then partner with people who make scales and together with others make something better. That is how research is done; it's very much team oriented, groups of people working together to create. As such, ironically, the process of doing research has to be relational. At least in my opinion.

Asynchronous process poses some unusual demands. People must be patient, remember what someone said and come back to it later. Sometimes this can be jarring until one recalls the previous day or even the previous part of the same day. Consequently, Phil recalled something Peter had said about the GTFS not reflecting a relational style, and he went back to it.

"If" is a small word with big implications. It introduces a conditional phrase, and if you accept the condition, then you have to deal with its consequences. I don't accept the condition you continue with after you use "if." I don't reject it either. I don't believe enough has been demonstrated to be able to assert one way or the other that the GTFS "doesn't support a relational practice." This is simply an assertion on your part, one person at this point backed up by some people in France. What does "support" in your assertion actually mean? What are you asserting? Are you actually saying that there is nothing in the GTFS that relates to a dialogical relationship? That there is nothing in the GTFS that touches on what happens when the therapist considers field dynamics or that nothing takes place, nothing in the GTFS in which experiments are carried out? Really? There is nothing connected to relationship in the phenomenological observations and descriptions of the therapist?

As I've said before, the GTFS is not a statement or development of theory. You have to know that theory to be able to understand what it is tapping into. And if you do, then I believe you can find relationship and intersubjective relational dynamics between its lines (given that the very nature of this beast AS a fidelity scale is focused on what the therapist does). As such, the fidelity scale does not address the effectiveness or efficacy of the therapy itself. It just asks, "Was this gestalt therapy?" I believe that what Madeleine and her colleagues have done is to chisel out of the swamp of theory about gestalt therapy, and the ever-present contention that there *is* no defining description of gestalt therapy possible (for various reasons in our gestalt ethos and history), a good-enough description of it.

For me it works well enough until something better comes along. And we are going to be using it in the research project on spiritually-integrated processes in gestalt therapy. We will use it in a novel kind of way. We will put its various descriptions in at the front; that is, we will list them and ask gestalt therapists to check off which ones they used in that session. Then, for at least one session, we will record the session and have someone use the fidelity scale to be able to claim (hopefully) that what was conducted was indeed gestalt therapy. So, later on one thing that would be possible is to see what the emphases in gestalt practice might be over a broad array of geographic and cultural regions (i.e. how much are people “checking off” this or that, here or there?). We will also be able to compare what therapists thought they were doing with what they were observed doing. And, no, *that* will not be perfect either.

Then, Tomáš Řiháček, a European researcher and colleague of Jan Roubal, entered the discussion.

Tomáš Řiháček: Hi Phil and all. Thanks for sharing your and Jan's chapter which is, in my opinion, a very nice take on the subject. We live in an era of manualization and we have to adapt to this fact in order to survive (as Gestalt therapists). Would it be possible to share also the fidelity scale with us who are not familiar with it yet?

Philip Brownell: Dear Tomáš, the chapter in the book on the fidelity scale is actually a summary of a more elaborate article that Madeleine and her colleagues have submitted to a peer-reviewed journal (possibly *Psychotherapy Research*). The summary describes briefly the way in which the GTFS has taken shape, and it gives the format of it that was used in the validation studies. It refers to previous articles about it in the *British Gestalt Journal* and also the *Gestalt Journal of Australia and New Zealand*. I will share it as I have the manual chapter, in pdf form with the same caveat to quote only from the original, which is not out yet (and will be in probably two or three months).

Tomáš Řiháček: Phil, thanks for a prompt reply. Before I immerse myself in the chapter, one more thought—parallel to a gestalt-specific fidelity scale, there are other options that can be considered (which are not to replace the idea of GTFS but to widen the scope of our considerations). One of them is the *Psychotherapy Process Q-Set* (PQS) developed by Enrico Jones and used by Stuart Ablon and others. It is a 100-item observer-rated descriptive measure of the psychotherapy process. Although it was not initially developed to study treatment fidelity, it was used in that way in a couple of studies. A treatment-specific prototype was constructed using a group of experts and the degree of fit between this prototype and an actual psychotherapy session was then used to measure therapist adherence (i.e., treatment fidelity). There are prototypes developed for CBT, interpersonal therapy, and psychodynamic therapy (and maybe others). Of course, the downside of using such a measure is that it may not contain items crucial for

defining gestalt therapy. However, an advantage of using such an instrument is the comparability across studies on diverse modalities. Just something to think about.

Then Peter went back to something Phil said about the role of expert raters in the construction of the GTFS.

Peter: No, it was not 'created by expert raters'. A number of I guess kind of experts in gestalt therapy (including me) were asked to send in statements of what we would see ourselves doing in being Gestalt therapists. That would tend to produce more statements about the things it is easy to make statements about, and some more diverse statements about what is more complicated. The Delphi process would then weed out the latter, and leave the simple, but not sufficiently discriminative statements.

But there was nothing wrong with the original statement-gathering. Yet it does not necessarily mean that the scale is able to discriminate properly what is or is not gestalt therapy. That would be why there was a need for a second stage, a validation. I think where it went wrong was that the evaluation didn't take into account the kind of lack of discrimination which might be a weakness in the scale. And then it went further wrong in not publishing the caveats that came out of the validation process. So rather than showing that a piece of work is highly likely to be gestalt therapy, all it shows is that it is not definitely not gestalt therapy!

Phil: You raise important issues. Read the summary I just sent. There was a process, and part of the process was the generation of descriptions by expert raters, and then along the way the refinement of that initial set. I am comfortable asserting that the process reflects a consensus of expert raters. No, there was not a meeting of expert raters who all sat in a room and hashed out together the GTFS. I don't think it works that way.

I notice you pointing out things that are certainly part of the research process. The one about creating simplified statements (and leaving out the more complex ones). You know, it's not just in research that we encounter such things. I find it happening in the editing process, whether I'm editing someone else or they are editing me. Complexity in written word is difficult to follow. In philosophy it's called being "thick." Research only tolerates "thick" in the discussion of the results of its process. The method itself must be simple—simple so that people can follow, not read into it what they *think* the writer meant or interpret the complexity, go off on tangents because of associations, etc. Research in this way, as I've been trying to say, is not theoretically satisfying in the way that many of us have been writing. I think the research has to address the theory in the literature review and discussion sections of what we do, but in the methods section it's just not going to satisfy [people who want theoretical precision and complexity].

The point about discriminative validation is important. And that study should follow. What the GTFS claimed was to confirm criterion validity. I think it actually confirmed construct validity. So, what that means is that on

the basis of what gestalt therapists themselves claim, in theory, is gestalt therapy (i.e. their constructs), it found that the GTFS accords with such claims. I would need to see what kinds of therapies were used to discriminate between gestalt and not gestalt. The summary does not say that, but from other explanations I can see there was a comparison between three gestalt people on the one hand (Michael Clemens, Bob Resnick, and Gordon Wheeler) and Jeff Young representing Schema Therapy on the other. That is not enough for discriminative validity.

Then Peter took a different tack and went beyond the direct issue of the GTFS to issues of research in the gestalt community more generally.

Peter: The problem is that what is called “research” in this company is not real. Real research is trying to *invalidate* the accepted truth, to see if it is robust. I don’t see any of that, more trying to use research as a new form of marketing of gestalt therapy. The “validation” of the Fidelity Scale was trying to validate it, not choosing the comparisons that are most likely to *invalidate* it, as real science would do. And weasel words like “we will have to do that later” merely point to the problem. In any reputable research set-up, the scale would not be used as part of research projects until that had happened. This is so obvious that it takes a particular kind of blindness not to see it.

I want to support research, but more than that, I want to support honour and integrity, and I am not seeing that shown. I’m sorry about the hard words, but that is how I see it.

Phil: I am glad—very glad—that you speak in the way you do. Nothing false about me saying that Peter. For years and years I found people rejecting the need for research in the Gestalt community and then as if suddenly, around 2013 or so it just tipped. That is simplifying but it seemed as if everyone began jumping on the research bandwagon. I knew the resistance had not simply disappeared. So I am happy when it comes out in the open. It feels like when a person can breathe again.

You said that research in this company is not real. Aside from that being an absurd assertion, it is insulting to the integrity of good people who are indeed doing real research. You are a good man, brilliant thinker, and clear voice. You don’t need to make sweeping generalities like that. Madeleine’s research is real research. I don’t know if you actually read any research literature, but if you did you would find near the end of every article statements describing what was not done or what was incomplete or could have been done better and certainly suggestions about what might be done next. Or be done later. Real research is always just a piece of the puzzle and there is always—*always*—something to be done later.

Then Pablo offered his perspective on the situation and the conversation that had been unfolding.

Pablo: Dear colleagues, I just want us to remember the original aim of the fidelity scale: to help us survive the era of empirically validated therapies and not be banned—excluded from mental health policies. What’s the role of the fidelity scale in that context?

1. To avoid disappearing we need to show some evidence of our efficacy. As Peter correctly states, this is not a pure scientific goal, it’s more a sales pitch to the local authorities and policy-makers.
2. To show evidence of our efficacy, one of the methodological requisites is to have some evidence of treatment fidelity. Currently our only evidence is “the therapist has Gestalt training.” This is barely acceptable, and the fidelity scale (even with its limitations) is vastly superior as treatment fidelity proof.

I wish we could concentrate more on true scientific studies that challenge our beliefs and practices; I would like to dedicate most of my research towards that goal. However, this other kind of research (the efficacy study) is vital to be able to survive and also have funds to do the more interesting kind of research. This has been said to us by Leslie Greenberg and other prominent researchers who have supported us at the research conferences.

Regarding the limitations of the fidelity scale, I completely agree that they should be stated in the papers as that is a basic scientific practice. Also I remember that the scale originally compared gestalt therapists with Kellog's transformative chairwork (or some other therapist who uses a similar method), so we have some discriminative validity. Also, it’s a big mistake to use the scale for something that it doesn’t intend to do, like showing how gestalt therapy must be done or within our community to exclude colleagues who work differently. The scale is “*just*” a methodological instrument to do efficacy studies, nothing more and nothing less than that.

And Jan Roubal added his perspective.

Jan: Hi Pablo, I completely agree with you. And, I just want to add a personal experience: participating in the video ratings of the GTFS was really inspiring and a discussion-provoking experience for the whole of our team of trainers. For example, it provoked us to see the difference between behavioral markers of Gestalt therapy that, quite surprisingly, clearly distinguish this approach from other approaches, and therapists’ competency to use them in a way supportive for the therapeutic relationship. It was very learningful to see: yes, this therapist is practicing Gestalt therapy as an approach, and he is at the same time not doing it in a competent way. It led us into a discussion of how to train competent therapists instead of therapists who simply “do Gestalt”, as we often hear. Well, this is just one example. Enriching experience, thank you Madeleine!

Phil came back to the things that Pablo observed and also responded to Peter's previous comments about invalidity.

Phil: Dear Pablo, few things are just one thing or another. While I agree that we are engaged in a political process to provide proof to policy makers that Gestalt therapy works, I think we are also doing science—real research. As such we can learn things. Our work needs to be about more than rubber stamping Gestalt practice as we've known it.

I think it is a false dichotomy to place public policy making at the other end from "true science/research." For one thing, if there is no real science, no respected research, then no one is going to build policy on fantasy. There is pressure and political action, and there is group think and so forth, but there is also valid outcomes research. You need one to do the other. So, it may be that in the research movement we have various focal points, interests, and committees. One committee or sub-group could be dedicated to advocating for gestalt practice using the outcomes generated by others. And, by the way, that is another reason to finally create our own research organization—to have such advocacy.

[To Peter]The issue of invalidity is not the same as what I think you are referring to. Internal invalidity is when there is something wrong with a person's research design or when the assumptions don't pan out. There is a kind of invalidity as well in hypothesis testing. Rejecting the null hypothesis is used to establish the validity of the hypothesis. So, people intentionally attempt to refute their assumptions and discredit their findings. Is that what you mean? Hypothesis testing is still done of course, but it is far less important these days than establishing the strength of the significance, which is associated with the effect size.

I don't think real research is trying to invalidate the accepted truth. Rather, we live in an age in which abduction (recourse to the best explanation among competing hypotheses and paradigms) is more the norm (than even Kuhn's ideas about paradigm change; one dominant idea swaying all other options). Here is a description of this taken from an excellent article in *Nature* (How scientists fool themselves – and how they can stop, by Regina Nuzzo, 10/7/15).

"One solution that is piquing interest revives an old tradition: explicitly considering competing hypotheses, and if possible working to develop experiments that can distinguish between them. This approach, called strong inference, attacks hypothesis myopia head on. Furthermore, when scientists make themselves explicitly list alternative explanations for their observations, they can reduce their tendency to tell just-so stories."

But I think what you were referring to is the idea of falsifiability rather than some kind of invalidity or invalidating. Karl Popper advocated falsifiability, but I prefer the thinking of physicists Alan Sokal and Jean Bricmont. They said that science does not progress by refuting ideas but by taking note of

ideas that pan out. There is a good description of this in the Wiki on “Falsifiability:”

In their book *Fashionable Nonsense* (published in the UK as *Intellectual Impostures*) the physicists Alan Sokal and Jean Bricmont criticized falsifiability on the grounds that it does not accurately describe the way science really works. They argue that theories are used because of their successes, not because of the failures of other theories. Their discussion of Popper, falsifiability and the philosophy of science comes in a chapter entitled "Intermezzo," which contains an attempt to make clear their own views of what constitutes truth, in contrast with the extreme epistemological relativism of postmodernism.

Sokal and Bricmont write, "When a theory successfully withstands an attempt at falsification, a scientist will, quite naturally, consider the theory to be partially confirmed and will accord it a greater likelihood or a higher subjective probability. ... But Popper will have none of this: throughout his life he was a stubborn opponent of any idea of 'confirmation' of a theory, or even of its 'probability'. ... [but] the history of science teaches us that scientific theories come to be accepted above all because of their successes." (Sokal and Bricmont 1997, 62f)"

Then Phil turned more directly to Peter’s previous assertions.

[Peter said] “The ‘validation’ of the Fidelity Scale was trying to validate it, not choosing the comparisons that are most likely to *invalidate* it, as real science would do. And weasel words like 'we will have to do that later' merely point to the problem. In any reputable research set-up, the scale would not be used as part of research projects until that had happened. This is so obvious that it takes a particular kind of blindness not to see it.”

So, first off: blindness? To whom or what are you referring?

Of course the effort was to validate the GTFS. Of course. And there is nothing wrong with that. But I think what you are implying is that people were not honest, that they were choosing weak opponents, making things easy on themselves and not being ruthless in their comparisons with other modalities so that they could arrive at the destination they had already decided they needed to achieve. Think about it. The effort to invalidate it could have been just as dishonest if that had been the ultimate goal, and then what? Neither kind of dishonesty is worthy of the people, the real people about whom we are talking.

And I’m sorry but research is accomplished in steps; so, it’s not blindness to indicate what might have to be accomplished “later.” You don’t want to appreciate it, apparently, but research advances in steps, a little here and a little there. I think we have established that more could be done. My bet is that it will be done. And by honest people doing credible research.

Peter: I know the words. Philosophy of science, Popper, Kuhn, Feyerabend, Lakatos, etc. were part of my doctoral thesis. I am not here talking about falsifiability. Gestalt therapy is certainly falsifiable. I am saying that the way