Process-driven psychotherapy does not need empirical research

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*Abstract*. This response to Michael Basseches and Patrick Luyten commentaries underlines the dialogic-dialectical and hermeneutic-phenomenological nature of process-oriented psychotherapy. To both Luyten and Basseches empirical research is necessary and the phenomenological—which studies the common, typical and essential factors of the field—is not. This is understandable, in view of the fact that they are both basically procedure-oriented. To this basic empirical-technical level they add a hermeneutic one: Basseches quite openly, Luyten in the disguised mode of a weak form of hermeneutics, in which statistics are rhetorical devices that are used not to display facts, but to support the story one wants to tell. MB is aware of the risk of manipulation at the service of the dominant ideologies—above all scientism in our time—implicit in a mode of working that is limited to the “technical” and the “practical” (or hermeneutic); therefore he adds a third level, of “critical science”, to the previous two. A critical attitude is intrinsic to the dialogic-dialectical, hermeneutic-phenomenological approach, inasmuch as it systematically brackets off all preconceptions and expectations. By contrast, it is difficult to understand how a critical science can avoid being conditioned by all sorts of preconceptions that a theory-driven scientist does not know how, or does not want to, bracket off.

*Keywords*: dialogue, dialectic, hermeneutic, phenomenology, procedure, process.

I am very grateful to Michael Basseches (hereafter MB) and Patrick Luyten (PL), as to Golan Shahar who promoted our encounter, for a dialogue on matters that I greatly cherish. Both MB and PL reject my suggestion of the existence of two *basic* and *autonomous* forms of psychotherapy, one process-oriented (existential-humanistic model, *care*) and the other procedure-oriented (medical model, *cure*), stemming from the same integrative gestalt principle: as we find both procedures and processes virtually in all treatments, we get two basically different types of therapy depending on which of them is figure and which is ground. However, the ways they argue their rejection allow us a deeper understanding of our subject-matter.

PL’s main argument is that «good psychotherapy it not a matter of either science or clinical wisdom, but of the combination of theory-driven research and clinical practice». Throughout his commentary, PL reads me as though I were proposing a divorce between science and clinical wisdom, whereas I have repeatedly stated in my paper that both process- and procedure-oriented psychotherapies, as I mean them, are scientific enterprises, and I have distinguished at length the two types of science that found the two types of therapy. PL does not take into any account this distinction, not even for criticizing or rejecting it: he simply ignores it. In so doing, he perfectly fits the definition of *scientism*, the ideology that finds a fertile ground in our field, whose shibboleths are, as reminded in my paper: (a) The only way to get valid knowledge is science, and (b) the only true science is empirical science based on theory-driven research. I will comment in a more detailed way on PL’s objections, though I see them as steeped in the above ideological position.

By contrast, MB does recognizes that there are different types of science that must be applied in the study of psychotherapy. However, he maintains that they are all necessary for the understanding of the different levels of psychotherapy (three, in his view), because they are interdependent and should not be separated in a good psychotherapy. Let us begin with the first two MB’s levels – which he calls respectively *technical* and *practical* – into which he reframes the two types of treatment that I call *cure* and *care* (as in *curing* a disorder and *taking care* of a person). At the technical level empirical sciences «study observable objects and events, treat them as reflecting lawfully related variables, formulate hypotheses entailing conditional predictions about observable events which can give rise to effective control». At the practical level, «hermeneutic methods must be used first to address practical challenges of establishing intersubjectivity of shared meanings and goals». The two levels are necessarily interdependent, in MB’s view, because «there are perils to the process when any psychotherapist *either*assumes a shared understanding of a client’s goals when it hasn’t yet been achieved, *or* fails to acknowledge and respond adequately to a client’s technicalquestions when they emerge».

The necessity of this interdependence is questionable, in my view, for the reasons that follow. Firstly, to say that a treatment is technical or procedural is equal to say that it is *prescriptive*. The Empirically Supported Treatments (EST) use prescriptions based on empirical research, as in all Evidence Based Practices (EBP). Of course, the clinician must make an accurate diagnosis and take into account the client preferences and values, but this can be and is done in short-term treatments, typically cognitive-behavioral, usually in the first or the first two sessions. Not much more is needed, because the ground is already there: the same ground of the modern practice of medicine all over the world, where the clinician tries to secure the client informed consent and compliance as quickly as possible, mostly *with no real dialogue at all*, and the client accepts the rules of the game because everybody knows that this is how science-informed treatments work, period. As the short space given for obtaining the client compliance could hardly be called a “hermeneutic dialogue”, this very state of affair contradicts the necessity of any sort of interdependence between technical and hermeneutical approaches, for the limited but *legitimate* aims of this kind of treatment.

One could object, as PL does, that the above description of the EST is a “straw man prescriptive model” that does not fit with what mostly occurs in psychotherapy. In fact, it is true that only a minority of clinicians apply a strict medical model, while most adopt what PL calls a “tacit prescriptive approach”. Now, this “tacit” is a word that deserves our close attention; but let us first underline that PL does not deny that a treatment informed by empirical research *must be prescriptive*: as he says, «We simply need some kind of prescription; without one, there would be total chaos». Indeed, to proceed without prescriptions is only possible to those who are ready to cope with the catastrophic (for the ego) event of entering the room of therapy *without memory and desire*, as Bion suggests, or with an attitude of *not knowing* that since the origin of Western thought, with Socrates, is the foundation of genuine dialogue. This clearly does not seem to be PL’s case. Instead, he praises a “flexible” prescriptive approach, so flexible that ultimately it appears to him to be not much different from what I describe as process-oriented approach—which, in his view, demonstrates the non existence of two different psychotherapies. As tacit prescriptive approach is all about *flexibility*, we should be very clear on what we mean by this word.

The key question is: *who sits on the seat of the driver?* If it is a *theory* (or a set of theories), that theory *prescribes* what is to be done in a more or less flexible way according to the personality and the preferences of the therapist. If it is the *process*, the therapist brackets off all theoretical and personal preconceptions and expectations and *responds at their best to whatever transpires here and now* in a creative and co-constructive way, which includes the use of any procedure they happen to be familiar with that seems appropriate in the case at hand, modified at will, irrespectively of any manual or statistic. In theory-driven therapy the procedures foreseen by the theory are figure and the process is ground, and vice-versa in process-driven therapy. The latter is *highly flexible* by definition, inasmuch as the therapist aims at responding to whatever happens at any single moment. The former can be quite *inflexible*, as in some short-term therapies, or moderately but *not too flexible*, because if the procedures are excessively modified the work is no longer driven by the theory in which the therapist is trained, or the procedure simply ceases to be empirically supported. Some hermeneutic dialogue is surely advisable in flexible theory-driven therapy, whereas it is at home in process-driven therapy, where no specifically technical competence is required (this is, by the way, the second reason why I can’t see any necessary interdependence between technical and hermeneutical work in process-driven therapy).

My two kind commentators would probably not deny that the two types of treatment just outlined are two different ways of working, though they might question the belonging of the latter to the field of science, as PL explicitly does when he writes that «psychotherapy is more than the mere application of scientific findings, but also more than an art». I would say that process-driven therapy can be just art, as theory-driven therapy can be just science, but they can be both art and science without losing their respective identity, because *the science of the one is basically quite another thing than the science of the other*. In theory-driven therapy the results are expected from the competent and judicious application of a theory that has been *extra-clinically validated* (as a corollary, in procedure-oriented therapy the results are expected from the application of empirically validated procedures). The science, here, is that of the researcher who puts to test a hypothesis with the methods of empirical science. When a hypothesis passes the test, it becomes the basis of a prescription. In process-driven therapy, by contrast, there are no prescriptions to follow, and therefore no need of an empirical research to validate them. What is needed here is *good maps* to help the therapist orient themselves in the field, and a *descriptive*, *phenomenological* science to draw the maps. However, to neither of my commentators phenomenology is an option, for different reasons that I will comment on now.

Firstly, to PL empirical science seems to be the only one that deserves the name of science. However, I want to remind him again, as I did some years ago (Carere-Comes, 2007a) in a discussion over a paper that he co-authored with Blatt and Corveleyn, these words by Jerome D. Frank from the epilogue—his scientific legacy—of the third edition of *Persuasion and Healing* (1991, p. 299): «Over the years I became increasingly puzzled by the contrast between the methodological sophistication of the research conducted by many outstanding psychotherapy researchers and the triviality of most of their findings… Suddenly the light dawned. As a type of persuasion, psychotherapy might be more closely allied to rhetoric and its close relative, hermeneutics, than to behavioral science!... Could the fundamental limitation of psychotherapy research be that researchers have been trying to apply to the realm of meanings methods created to elucidate facts?» Frank pointed out that empirical research does yield results, but these results are mostly trivial—i.e., they «while statistically significant, have been too weak to influence clinical practice. Moreover, many unnecessarily elaborate experimental designs have merely examined minor variations in methods or concepts or confirmed common-sense conclusions» (*ibid*., p. 300). The triviality of these results explains why empirical research hardly has an impact on clinical practice. Clinicians usually don't even read the empirical research papers, which they find irrelevant to their work. Are most clinicians lazy, ignorant, and arrogant or is there something very wrong in the idea of considering empirical research as the proper or unique mode of research for psychotherapy?

In our 2007 discussion I also pointed out the influential opinion of Robert Abelson, a leading psychological statistician who, among many others, argued that the function of statistics is not to display the *facts* but to tell a coherent *story*. In the same discussion and in the same vein, Zvi Lothane observed that «Blatt, Luyten and Corveleyn are not positivistic: they are hermeneutic from the get go» (Carere-Comes, *ibid.*). In other words, their positivism is just a facade for a (weak) form of hermeneutics, to the extent that statistics are rhetorical devices that are used not to display facts, but to support the story they want to tell. I say that this is a weak form of hermeneutics because, as a disguised one, it lacks the scientific status that it has in MB’s perspective, which is openly hermeneutic. In his three-level system, the first (“technical”) is one in which procedures validated by empirical research are used, and the second (“practical”) is one in which intersubjectivity of shared meanings and goals is established. In MB’s view, only in the context of a shared understanding of a client’s goals can the therapist adequately respond to technicalquestions when they emerge. I agree that this is correct in a prescriptive or procedural perspective. My agreement is based on the conviction that, although statistics are mostly used as a rhetorical device, nevertheless they are not *completely* irrelevant, and can be *legitimately* employed in the limited scope of procedure-oriented psychotherapy. For those who, like PL, are at a loss if they cannot base their work on prescriptions, this mode makes them feel on a solid ground, which is obviously beneficial to the treatment. It is also possible that the sense of security that the therapist gains on that base, and transmits to their client, is the main factor that makes theory-driven therapy work (Wampold, 2001). On the other hand, while I agree with MB on the interdependence of empirical and hermeneutical factors in procedure-driven psychotherapy, such interdependence has hardly a meaning when the driver is the process and procedures are used in a non-prescriptive mode, if and when they are used (see Hoffman’s quotation that I put not by chance at the start of my paper, and my commentators have overlooked in their commentaries).

Unlike PL, MB gives hermeneutics a major place and a scientific dignity, building on Habermas’ work. Basically, as he says, «interpretations are validated as intersubjective knowledge to the extent that they are consensually validated as “making sense” and acted upon in a well-coordinated manner.» Again I agree, up to a point. In a dialogue we had some years ago (Carere-Comes, 2007b), MB and I concurred with the view that a dialogue entails holding one’s convictions at a critically reflective distance and being willing to put them at stake. In MB’s view, however, this happens only on the basis of *what the participants in the dialogue bring to it*. In my view, that means more a dialectic than a dialogue proper. The difference is that in a dialectical confrontation each of the participants is willing to be challenged by the views of the other and to change their own as a result of the confrontation, whereas in a genuine *dia-logos* one is willing to *suspend* one’s preconceptions and expectations to open a space in which the *logos*, the inner logic of the process can reveal itself. We could say that dialectic is active-constructive whereas dialogue is receptive-contemplative, as the *male* and the *female* sides of a genuine communication. This is to say that MB’s position to me is *necessary but not sufficient*. Dialectic is akin to hermeneutic as dialogue is to phenomenology. Hermeneutic is a *co-construction* of shared meanings as phenomenology is a *revelation* of the essential traits of things.

In contrast to what Habermas and MB believe, I join the empiricists and the phenomenologists in the conviction that hermeneutics cannot found a science in its own right, because it cannot transcend the level of intersubjective agreement to reach the *things* as they are, either as *objects* (as in the objective knowledge of empirical science) or as *essences* (as in the eidetic sciences based on the phenomenological method). I agree with MB that empirical methods are appropriate when the telos is that of predicting and controlling events—a telos that is legitimate in prescriptive psychotherapy. Yet I, among many others, don't share this telos. Rather than predicting and controlling events, what I need is «the development of *understandings* which may assist people to anticipate events, by sensitizing them to possibilities… this kind of anticipation occurs with an appreciation that the field may include will and choice» (McLeod, 2001, p. 57). According to McLeod, and to myself, this approach (*anticipating possibilities, not predicting and controlling events*) is common to both hermeneutics and phenomenology. But if on one side hermeneutics cannot transcend subjectivity, on the other phenomenology does not take enough into account that we are interpreting beings who live in an interpreted world. Therefore, the hermeneutic and phenomenological approaches must work synergically. I agree with McLeod: «*the roots of all qualitative research lie in hermeneutics and phenomenology*» (ibid. p. 56). In any case, hermeneutics cannot stand alone by itself, if it claims to be more than a literary discipline. Left to itself, hermeneutics is lost in a flight of interpretations of interpretations, with no solid ground ever to rest. Therefore, it has to build either on empirical science—MB’s choice, quite opportune in technical or procedural psychotherapy—or on eidetic science—my choice, conducive to process-oriented psychotherapy. Only the latter, based on hermeneutic-phenomenological research, is in my view properly *dialogic-dialectical*.

The basic difference between MB’s and my approach can be made clearer with an excerpt from our 2007 conversation. MB wrote: «If one tries to understand a set of phenomena with a framework of understanding, it is like using a filter where you catch some things and not others. Someone approaching things with a different framework/filter will catch other things. If the two people compare what they caught, they will have a richer appreciation of what's "out there", and perhaps they can even redesign a filter to catch the important things that both previous filters caught. But if the idea of phenomenology is to approach a phenomenon with no theory, no structures with which to assimilate what one finds, no filters, isn't that like sticking a fishing net in a river with no netting, just the ring, insuring that one won't catch anything at all?» I replied: «You don't throw away your filters; you just put them aside for a while. You try to empty your mind, to create a 'clearing' in which something of the true nature—the essence—of things can be seen. The crucial difference is that in the hermeneutic approach your try to grasp something, to catch something with your interpretive nets; whereas in the phenomenological approach you don't try to catch or capture anything, you just try to be as calm and silent and open as possible, so that something can reveal itself, can manifest itself to you (phenomenon means what appears, what is manifested). Two examples from our field: Kleinian psychoanalysts who go on hammering their interpretations into their patients' heads work in a pure constructionist way: they try to have their patient replace their non adaptive filters with the analyst's presumably more adaptive ones (most behavior therapists work in the same way). By contrast, Rogerian therapists who avoid interpretations but listen empathetically, try to create a space in which new insights can happen spontaneously: this is a phenomenological way of working. I am not saying that the one is better than the other. Both work. But the really good thing is when you have both arrows in your quiver, and use them alternatively or in combination in response to your patient's needs.»

What I feel missing in MB’s approach is the basic attitude that Bion connotes with the formula *F in O*, which means *faith in the unknown*, or in the *logos* (the inherent logic) of the process. A therapist cannot obviously be process-driven if they don’t trust the built-in capacity of the process to take the lead of the therapeutic relationship. To the extent that a therapist has F in O, their main goal is to be as much as possible *in tune* with the process; to the extent that they don’t have it, they must use their theoretical filters in order to predict and control as much as possible the events, or stick their interpretive nets to catch more adaptive meanings than the client’s. The process is not just the course of events that takes place in the field of possibilities generated by the unique encounter of a client and a therapist. This conception of the process would be quite acceptable in a purely hermeneutic-constructivist perspective. But there is more to it. What makes the process trustworthy and reliable, and allows the therapy to be process-driven, is the *experience* repeated again and again that a course of self-knowledge, personal growth, self-realization and liberation happens just *being present to whatever is, accepting it, embracing it, flowing with it*. The process may include all sorts of interactions, required or suggested at each step by the context and facilitated by good maps of the factors that are common to any relationship of care, with *no need* of the more or less flexibly implemented specific procedures or technical actions that are the hallmark of theory-driven psychotherapy. I have explained in my paper why phenomenology is the right method for drawing good maps and empirical science is not. Neither of my commentators has objected to this point.

One final note on MB. He and Habermas are aware of the risk of manipulation at the service of the dominant ideologies—above all scientism in our time, I would say—implicit in a mode of working that is limited to the “technical” and the “practical”; therefore they add a third level, of *critical science*, to the previous two. A critical attitude is *intrinsic* to the dialogic-dialectical, hermeneutic-phenomenological approach, inasmuch as it systematically distances, suspends or brackets off all preconceptions and expectations. By contrast, I fail to understand how a critical science can avoid being conditioned by all sorts of preconceptions that a theory-driven scientist does not know how, or does not want to, bracket off.

Now some final remarks on PL’s commentary. He writes: «It is neither appropriate nor productive to speak of a “holy war” in the psychotherapy domain, as Carere-Comes does.» As a matter of fact, many speak of “holy war” or “power war”, both among professionals in our field (e.g., Norcross, 2006) and outsiders (see the article in the L.A. Times quoted in my paper). To me it is both appropriate and productive to decry this state of affairs, but I have nothing to object to PL seeing this state differently. Then PL writes: «These findings [concerning self-help and internet-based interventions] in themselves cast doubt on the universality of the psychotherapy model that Carere-Comes is proposing… as there is no therapeutic relationship in most of these interventions—at least, definitely not the kind of therapeutic relationship that Carere-Comes deems necessary for any type of psychotherapy to be effective.» The main thesis of my paper is that there are two *legitimate* basic models of psychotherapy, one process-oriented and the other procedure-oriented, *not* the one universal model that PL says I am proposing. He adds: «This aside, the model that is described is prescriptive as well. It is the clinician prescribing to other clinicians and researchers what good psychotherapy should look like!» I don’t prescribe anything. I just say that clinicians can choose (at least) between two legitimate models of psychotherapy. In case they choose the process-driven, they work in the least possibly prescribing way, because they choose to be driven by the process, not by any prescriptive theory or technique. PL goes on to write: «He focuses only on what does not seem to be effective, wrongly concluding from findings concerning the Dodo bird verdict (that the outcomes of all bona fide treatments are roughly equal) that specific techniques, interventions, and theories do not matter… In fact, the importance of offering a theoretically coherent, consistent, and continuous treatment is currently considered to be the main factor explaining the efficacy of current evidence-based treatments, particularly treatments aimed at severe psychopathology.» I never said that specific techniques, interventions, and theories do not matter. I am not sure that clients need such things, but it is indubitable that *many therapists* do. As Wampold (2001) convincingly showed, almost any theory will go, provided that it is coherent, consistent, and continuous, and above all the therapist *believes* in its efficacy and succeeds in transmitting their belief to their client. Then PL asks: «A case of cryptomnesia, Carere-Comes’s emphasis on mirroring, validation and empathy?» If I understand PL’s question, I would suffer from a form cryptomnesia because I might have forgotten that the importance of mirroring, validation and empathy has emerged first and foremost from the humanistic tradition, forerunner in the domain of detailed process**-**outcome research. In fact, I have not forgotten the pioneering research conducted by Rogers and his group. The story of that research is retraced by Orlinsky & Russel (1994) as the story of a failure. Rogers started as a phenomenologist, but at a point he thought that his work was not scientific enough. Then he began to do what after him many or most empirical researchers have done: he tried to isolate in the transcripts of sessions the basic therapeutic factors that he had discovered. As the context of discovery was not enough for the positivistic mind dominating his time, he moved to the context of justification. He wanted to give a scientific support to his discoveries through a process of objectifying, measuring, and proving. The result was a pile-up of theoretically irrelevant facts with very low clinical significance. The effort to formalize the principles of the therapy transforming them in standardized techniques brought about in fact an impoverishment and a distortion of his method. Rogers was indeed a pioneer in this field too. Many researchers followed in that line, producing a great deal of frustrating and contradictory results. The paradox pointed out by some was that «the more accurate is a research, the less useful it is to the clinician, because the methodological rigor required by the experimentation distances it too much from everyday clinical practice, which is inevitably not very rigorous and “polluted” by myriads of hardly controllable factors» (Migone, 2008).

The little relevance of empirical research for clinical practice has prompted many to discard it completely as a valid method to investigate psychotherapy. Yet this has only fueled the harsh struggle between supporters and opponents of it. To avoid this it would be fair to admit, on the part of the opponents, that empirical research can produce some kind of useful prescriptions, as PL has conveniently reminded us. If up to now this production has been much lower than expected, this does not mean that we cannot expect much better results from future research. It is not hard to imagine, may be in one or a few centuries from now, the existence of a computer-aided (or even computer-made) psychotherapy in which a treatment will be tailored to a client’s needs with a high grade of correspondence to the many variables relevant to that case, with a revision of the case every few sessions and retailoring of the treatment correspondingly. This would be, in my view, the perfect implementation of the medical model. I have nothing to object to those who work today in that direction, both useful and legitimate. This medical form of treatment will always be chosen by many, maybe most, practitioners and clients. The rest of us, who love the unpredictability and creativity of a dialogic-dialectical relationship that furthers a process of self-knowledge and personal growth, will always be able to choose the other way—at least in a democratic society. And there will also be those who will want to hybridize the two modes, in virtually infinite ways. I like to imagine a world in which everyone will be able to choose the one or the other way, or any combination of the two. What we surely don’t need is the reciprocally disrespectful contemporary condition outlined in my paper, in which on one side they charge the others of being old-time believers, sectarians, or charlatans if they do not accept to submit their work to empirical test, and on the opposite front they charge those who privilege systematic research of being authoritarian objectivists, inhuman determinists, or insurance companies lackeys.

I apologize for not having paid all the attention they deserve to all the criticism raised by my commentators. I could only do my best to respond to their arguments that I found more challenging. I am very grateful for the way they have confronted my ideas, even if I don’t seem to have changed them a bit right now. To me, as to many others, it takes time. Rereading the exchanges I had with MB and PL back in 2007, I realize now that I have to thank the former for helping me get rid of a residual naif objectivism from which I still suffered at that time, and the latter for the more respectful consideration of empirical research that I have been able to grow in the meantime.

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