Personal Pathways in Psychotherapy Integration

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This article traces the pathways that led the author to his current integrative perspective on psychotherapy. Such perspective is described as an attempt to cope with repeated confrontations with the complexity of human functioning, as revealed by the seductiveness of major intellectual traditions, the untamable nature of clinical reality, and the challenge of unexpected empirical findings. The article also outlines the author's current and future integrative efforts, both in terms of training and research. Recommendations for the future of the integration movement, as well as the Society for the Exploration of Psychotherapy Integrations, are also suggested.

Important career choices are rarely accidental. It is frequently acknowledged that we study who we are, or what we try to run away from. As such, writing this paper on how I became an integrationist (or, more accurately, a "cognitive-behaviorist thinking integratively") has forced me to take a close look at some of my occupational (and, of course, personal) wishes and fears. Although my work in integration has been a rich source of intellectual and professional growth, I must candidly admit that part of my interest in this domain reflects a perfectionist trait: The need to know everything (or as much as possible) and to pay respect to every major contribution in the field. My difficulty to tolerate a sense of lack of control also has a lot to do with the path that I have been traveling for the last two decades (can you say obsessive–compulsive personality?!). In fact, I originally delve into the field of integration as a mean to escape a deep sense of confusion.

Responding to Lampropoulos's (this issue) kind invitation, this paper

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describes how such a sense of confusion first emerged and the steps that I have taken to put it at bay. I will also briefly outline a number of integrative projects that I plan for the future, and offer my thoughts about the future of psychotherapy integration.

BEGINNING PATHWAYS

From Actualization to Confusion

I completed my undergraduate degree in a small psychology program at the University of Sherbrooke (Quebec, Canada). The entire structure of this program was based on two basic assumptions that are at the core of the humanistic movement, or Third Force, in psychology: (a) Each Individual has an inner tendency toward self-actualization, and (b) the main facilitative factor for this tendency is the presence of validating and supportive human relationships. From a training point of view, these assumptions imply that students will maximize their learning if they control their education and if they have continued access to "student-centered" relationships with empathic and competent faculty members.

Central to this undergraduate program was the implementation of "pedagogical contracts." At the beginning of each course, students individually negotiated with their professors a contract that specified what they were going to study (content), how they were to complete the course (method), and how they were to be graded (evaluation process). Faculty did provide a syllabus for each of their courses, and the students were free to sign a contract that reflected, entirely or in part, the goals and procedures proposed in these syllabi. For each of their courses, however, students could construct and then propose a unique set of objectives and procedures. These contracts were also renegotiable at any point during the semester. During my first year, for example, I took a course on psychoanalysis with the goal of learning the theoretical foundations of this major intellectual tradition. I thought that the best way to achieve this goal was to read several books of (and about) Freud and at least one of his dissidents. At first, my evaluation was to be based on weekly discussions of my readings with the faculty (an experienced and reputed psychoanalyst). As the semester progressed, however, I felt that my readings were exposing me to an overwhelming level of information and that I needed a better way to integrate this material (no doubt reflecting the controlling and perfectionist issues mentioned above!). I therefore renegotiated my contract, whereby I kept the same reading list but dropped the weekly meeting and instead wrote a paper on Freud to be discussed at the end of the semester

(along with any additional readings). This worked well for me and for the faculty—up until he discovered that he had to read a 125-page paper and still had to discuss three books of Adler!

Of course, I took all of the courses that were required by the program (the number of which was quite limited) but what I mostly did to earn my degree was to read and write (hundreds of pages the first year and a half alone) about what I thought were the most interesting and important issues in psychology. To a large extent, I designed my undergraduate education to be structured around three phases. As mentioned above, I first devoted a large amount of time to learning about Freud and other figures of the psychoanalytic tradition. I subsequently focused my attention on humanistic authors such as Rogers, Perls, and Laing. Perhaps reflecting a mild (and mostly suppressed) rebellious trait (not rare in individuals with obsessive–compulsive tendencies), I spent the last year of my stay in this humanistic-existential program immersed in the behavioral literature (Skinner and Bandura).

As the end of my undergraduate studies approached, I began to feel a profound state of confusion. Within the scope of a few intense years, and with an equal sense of excitement and conviction, I had "tour-a-tour" identified myself as a Freudian, a Rogerian, and a Skinnerian. Just as I was about to apply to graduate programs in counseling and clinical psychology, however, I simply could not commit myself to one school of thought (I was also experiencing quite a bit of anxiety due to the fact that, at 22 years of age, I failed to possess a personal, articulated and unified view of personality, psychopathology, and psychotherapy–I now know how naïve and narcissistic this sense of failure was!). I found it exciting to argue with my peers about how Freud, Skinner, or Rogers would explain why people behave in different ways and how they change, but I was growing increasingly distressed by the realization that I could not say who was right, or which parts of their theories were the most accurate for particular circumstances. Yet, I had a vague sense that these schools of thought converged at some level, and that some of their unique perspectives complemented one another—they were, after all, trying to make sense of the same beast!

I discussed this sense of confusion with Yves St-Arnaud who, as my first mentor, had taken me under his wing during most of my undergraduate studies. The most brilliant and influential figure of the humanistic movement in Quebec (see Castonguay, 2001), St-Arnaud founded the psychology program at Sherbrooke and, true to his theoretical convictions, spent infinite hours facilitating the actualization of generations of students (even if that meant having some of them become behaviorists!). He suggested that I read the newly published book of James Prochaska (1979), *Systems of psychotherapy: A transtheoretical analysis*. I remembered how excited I was to discover a cohesive and appealing conceptual framework

that integrated many clinical contributions of the major psychotherapy orientations. St-Arnaud also suggested that I apply to a master's program in counseling psychology at the University of Montreal that focused on psychotherapy integration—a new trend in the field.

As I think back at this early stage of my education, I now realize how my fear of confusion and wish for intellectual control represented powerful determinants of my involvement in integration. Such involvement represented, at least in part, a choice to deal with the anxiety triggered by the possibility that none of the major intellectual traditions that had seduced me were correct, and yet that all of them contained parcels of truth. My interest in integration also reflected my secret hope that I would be able to combine these partial truths into a new theory—a theory that would be able to account, within a unified framework, for such constructs as the Oedipus complex, actualization tendency, and operant conditioning! Although, I later rejected the viability of such a wish (Castonguay & Goldfried, 1994), I am aware that even now my understanding of integration is still, in part, a projection of my internal demons, as indicated by my recent description of this movement as a strategy to deal with the complexity and confusing nature of change (Castonguay, Reid, Halperin, & Goldfried, 2003).

From Confusion to Identification

At the University of Montreal, I was fortunate to be mentored by another brilliant thinker: Conrad Lecomte. Refusing to take part in the turf wars ("chicanes de clochers" as we say in French) that prevailed (in the late 70ss) in the field of psychotherapy, Lecomte had recruited young stars from different orientations (e.g., M.A., Bouchard, John Wright) and transformed a humanistic-oriented program into an integrative one. At first, the program's philosophy was primarily anchored in Carkhuff' s (1969) and Egan' s (1981) models, which systematically prescribed the implementation of humanistic (to foster exploration), psychodynamic (to facilitate deep understanding) and cognitive–behavioral (aimed at the acquisition of action-oriented skills) interventions. After many years of reflection, and using the self as a unifying construct, Lecomte and his colleagues later elaborated a more comprehensive (and, I believe, more ambitious) model that integrated the contributions of Rogers, Kohut, and Bandura (Lecomte, Castonguay, Cyr, Sabourin, 1992). Interestingly, the elaboration of this model was in large part a reaction to concerns voiced by students. An evaluation of the training program revealed that students felt that they had been able to acquire a diversity of therapeutic skills. As noted by Lecomte et al. (1993), however,

> a majority of them [students] felt the need to have a coherent theoretical framework of human behavior and psychopathology. Instead, they were trained within a multitheory comparison suggesting that no truth exists. The result was flexible practitioners who possessed a confused professional identity. (p. 489)

Hence, it appears as if I was not the only one whose confusion was creating a longing for some form of integration!

Looking back, I now feel that while Sherbrooke exposed me well to theories about human functioning, Montreal allowed me to implement techniques of different orientations (I left before the integration of Rogers, Kohut, and Bandura had been clearly articulated). I wish I had more opportunities to practice each of the major treatment approaches from beginning to end, but at least I had the opportunity to use interventions attached to divergent theories at different phases of therapy. Theoretically and clinically speaking, Sherbrooke and Montreal provided me with appropriate contexts for an exploration of various schools of therapy.

In terms of scholarly work, my graduate training at Montreal planted the seeds for the research program in which I have been involved for the last 20 years. Guided by Lecomte's integrative perspective, my Master's thesis consisted of a theoretical essay on factors that cut across different orientations. I first devoted a substantial amount of time reviewing the field of integration as a way to capture the context within which the study of common factors had taken place. I also spent quite a bit of effort trying to put some sort of order into the many ways common factors had been defined up to that point in time. In the midst of doing so, I came to the conclusion that common factors were incorrectly assumed to be synonymous with nonspecific variables. One chapter of my Master's thesis (which later led to a publication, see Castonguay, 1993) demonstrated how prevalent this erroneous assumption had been (even among some of the most respected leaders of the field), identified a potential source of this misleading assumption, specified some of the unfortunate consequences it had on our understanding of common factors, and proposed possible solutions to the conceptual conundrum. The bulk of my thesis, however, was a review of the major authors in the field of common factors (e.g., Frank, Garfield, Goldfried, Marmor, Prochaska, and Strupp), and an attempt to integrate therapeutic commonalities within a new model of psychotherapy. My goal was to demonstrate that so many common factors have been identified that it would be possible to elaborate a pan-theoretical model of psychotherapy solely based on them. As shown in Table 1 (see also Figure 1), this trans-theoretical model included three components (general structure, basic processes, and active dimensions), each of them regrouping a large number of common factors.

At the cost of two years of reading and writing, my Master's thesis helped reduce some of my previous sense of confusion by providing an opportunity to integrate a large number of the clinical contributions of

Table 1. Common Factors in Psychotherapy

Framework	Basic processes	Dimensions
Participants (client & therapist)	Interpersonal Influence: -Persuasion -Operant conditioning	Communication: -Forms (verbal and non-verbal)
Therapeutic setting	-Modeling	-Rules (e.g., specificity, coherence)
Assessment procedures	Therapeutic Relationship:	Content (e.g., anxiety)
Therapeutic contract	-Therapist interpersonal skills	Methods of intervention:
Treatment Stages	-Therapeutic alliance and transference	-Techniques and procedures
		-General strategies of intervention
	Engagement: -Emotional -Cognitive -Behavioral	

different orientations. The completion of my thesis also increased the respect I already had for the complexity of psychotherapy, as well as for the tremendous number of efforts that had been made in order to understand such complexity. Without even addressing the role of variables unique to particular forms of psychotherapy, my thesis involved 51 pages of references, more than 400 in all (can you say OCP?!). My thesis also gave me an opportunity to delineate what I considered the most fruitful directions for

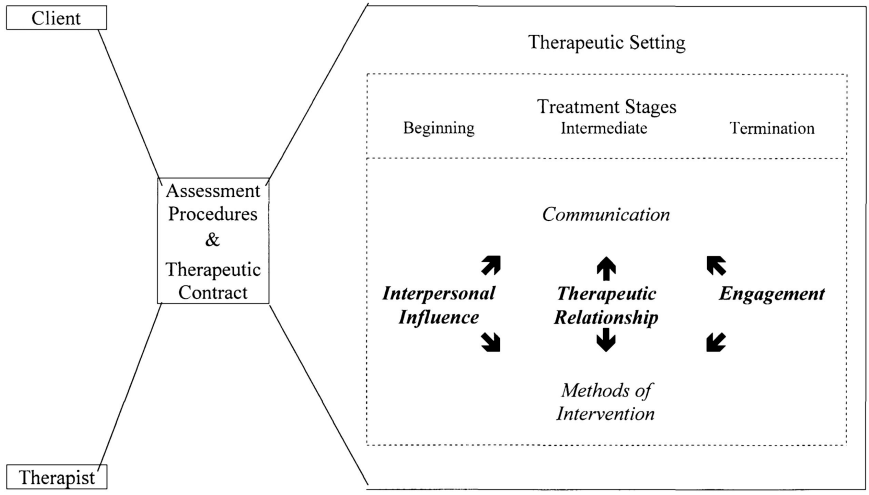

Figure 1. Transtheorical model of psychotherapy.

future research, including the factors that I was hoping to investigate if given the chance of to conduct doctoral studies.

While working on my Master's thesis, I began to put together a series of colloquia on rapprochement and integration in psychotherapy. The first one involved talks by C. Lecomte and S. Garfield. For the second symposium, I invited reputed scholars from Quebec (W. Reid, Y. St-Arnaud, and J. Wright), as well as Marv Goldfried. These colloquia served as the basis for an edited book (Lecomte & Castonguay, 1987), the first one in French on integration. The book covers many of the important historical, theoretical, clinical, empirical, and epistemological issues related to eclecticism, common factors, and integration in psychotherapy. It also includes a series of chapters for which authors were asked to identify the contributions that their own approach (humanistic, behavioral, and psychodynamic) could offer to other orientations, and vice versa. The success of the first two colloquia paved the way for a more ambitious one, which featured two important figures of each of the major orientations: Les Greenberg and Laura Rice (representing the humanistic approach), Marv Goldfried and Lee Birk (representing the cognitive–behavioral perspective), and Hans Strupp and Paul Wachtel (representing the psychodynamic approach).

Once I completed my masters, I took a position in a cognitive–behavioral program for sexual offenders at the Institute Philippe Pinel of Montreal. I had neither planned to work with rapists and pedophiles nor intended to become a cognitive behavior therapist. The position, however, fell into my lap when I desperately needed a job. It also paid well, and allowed me to acquire invaluable research and clinical experience from Chris Earls, a known expert in that field. Soon after beginning my new position, I decided that staying at Pinel for a few years would allow me to save a substantial amount of money for graduate school, while giving me the perfect opportunity to learn English (Earls was an American), and plenty of time to study for the GREs. Despite my best intentions, however, I failed miserably at these three goals (Instead, I used all of my money to buy a brand new car, Earls improved his French, and my GREs scores might well have ended up in the record books—on the low side of the curve, that is!).[1]

[1] My disastrous GRE scores were hardly a surprise to me. To be candidly honest, I organized the last two colloquia mentioned above in large part because I wanted to go to grad school in the United States but was convinced that I would do extremely poorly in the GRE exams. I thus reasoned that my only chance to work with a leading researcher in reputable school was to establish a relationship with such individual prior to applying to grad school. This is why I invited Marv Goldfried twice—it paid off handsomely, as he agreed to fight for my admission with his colleagues, even when they suggested (and I refused) to take the GRE again—they were that bad!

My years at Pinel did allow me to see first hand some of the strengths and limitations of cognitive–behavioral therapy for the assessment and treatment of severe psychological problems. I further learned about cognitive-behavior therapy (CBT) by doing my Ph.D. at Stony Brook, which for many was, and still is, a recognized Temple of CBT. After years of tutelage from leading experts (such as M. Goldfried, T. Carr, T. D'Zurrila, D. O'Leary, S. O'Leary, and H. Racklin), I was forced to admit that I had become a cognitive–behavioral therapist — even though many people at Stony Brook thought that I really was (and/or accused me of being) a Freudian.

As for when I worked at Pinel, I did not go to Stony Brook because of its emphasis on behavior therapy. To some extent, one might say, I became a CBT by default. After many years of immersion (or exposure!) in CBT environments, I progressively became comfortable developing case formulations and treatment plans within a CBT framework. With more seminars, clients, and supervision, I grew more aware of the nuances associated with many concepts underlying this approach, and more respectful of the difficulty of applying its methods consistently and competently. Speaking the CBT jargon became second nature, and while I frequently struggled with "the return of some repressed materials" (e.g., transference, countertransference, resistance, growth, immediacy) I was impressed by the impact that CBT had on many of my clients. I did kick and scream, both at Pinel and Stony Brook, against what I saw as narrow and rigid foci in the CBT tradition, and I never perceived cognitive and behavioral constructs as truths, as I felt a number of my colleagues did. But while it took me a long time to admit to myself and others that I was a CBT, it eventually became clear to me that the best way to maximize what I could learn from faculty members and my peers was to involve myself as deeply as possible into what CBT had to offer.[2] What I did not expect at the time, although it now makes perfect sense, is that with my professional identification with the myths and rituals (Frank, 1961) of one particular approach came a wonderful bonus: A further dissipation of the nagging sense of confusion that I had been struggling with for many years.

The reason I went to Stony Brook (to do a PhD in a language I then barely spoke, I might add) was to work with Marvin Goldfried. The idea of

[2] I remember the state of shock on the face of some of my peers when in the middle of discussion (most probably during my third year at Stony Brook) I let slipped out (no pound intended) that I identified myself as a CBT ("You, CBT? You've been annoying us for years with that Freudian non-sense!"). Perhaps it takes a long time to put aside, even if only temporarily, intellectual traditions that has imprinted one's mind at a relatively young age. Perhaps, also, there is some benefits (i.e., reinforcement) that comes with being different in an academic settings–as I did when I was Skinnerian at Sherbrooke and Freudian at Stony Brook (the rebellious trait associated with OCPD, I guess)

choosing him as a graduate advisor, I must admit, did not come to mind the first time I came across his work. Rather, it came after I met Sol Garfield in 1983. Garfield had read a preprint of a poster I was about to present at a meeting of the American Psychological Association, which described the trans-theoretical model mentioned above. It was clear to him that my perspective on common factors was very close to Goldfried's recent work. "Goldfried?" I said, "Isn't he a cognitive–behavioral person?" To which Garfield replied: "Yes, but he is now involved in the creation of a network of folk interested in psychotherapy integration and he just edited a book on the topic. You should read the first chapter of his book as it really fits nicely with some aspects of your paper."

I bought Goldfried's (1982) *Converging Themes in Psychotherapy* a few months later at the APA convention and immediately read Goldfried and Padawer's (1982) chapter. I vividly remember walking (jumping would be more like it!) across my hotel room and saying out loud "that's it," "that's the way to go" when I got to the section on common ingredients in psychotherapy. Garfield was definitively right. I was, at the time, immersed in the writing of my Master thesis and I felt that Goldfried's conceptualization of common factors as general principles or strategies of change was fitting nicely within one aspect of my model (i.e., as examples of active dimensions of interaction). It also seemed logical to me that investigating these strategies would provide the best avenue for future research. I was convinced by Goldfried's eloquent argument that searching for common factors at the level of techniques prescribed by different treatment protocols would likely lead to trivial findings, and that profound philosophical divergences would render fruitless the search for similarities at the level of theories of human functioning underlying major orientations. Instead, the general strategies of intervention delineated by Goldfried (e.g., establishing a therapeutic relationship, fostering a new view of self, facilitating corrective experiences) reflected an intermediate level of abstraction (between the philosophical underpinnings and technical repertoires of different approaches) to investigate common factors. This conceptualization allowed for the recognition of variables unique to a particular approach, but it also implied that many of the so-called specific techniques (interpretation, reformulation, cognitive restructuring) were idiosyncratic manifestations of general principles of change.

From my reading of Goldfried's earlier work on behavioral assessment and cognitive–behavioral treatments, I knew that he was a sophisticated, influential, and prolific researcher. My review of the common factors literature had also convinced me that even though integration was gaining popularity in the field, few scholars had yet taken the bold step of developing an extensive research program on any aspect of this movement. If I was to empirically study common factors as part of my doctoral studies

(which would be the logical step following my Master's thesis) and if the investigation of strategies or principles of change was the most promising path for future research, then working with Goldfried was the best thing I could hope for with respect to my doctoral training. Having learned from Garfield about Goldfried's leading role in creating the Society for the Exploration of Psychotherapy Integration (SEPI), and after reading more of his conceptually elegant and clinically insightful writings on integration, I gained tremendous respect for his exquisite scholarship, creative mind, influential leadership, paradigm-breaking (my term for "thinking outside the box") vision of the field. It thus became imperative that I invite him to Montreal to present in my colloquia on integration so that I could work my way into studying with him at Stony Brook!

Under Goldfried's mentorship, I learned how to study the process of change. One principle of change in particular took predominance in my empirical efforts. Using the Coding System of Therapist Feedback (Goldfried, Newman, & Hayes, 1989), Goldfried, my lab mates (especially Adele Hayes and Sandra Kerr) and I conducted a number of investigations on therapist's feedback (which, we have assumed, is in part aimed at changing client's view of self and world). In a preliminary study, for example, we found that while both psychodynamic-interpersonal therapy (PI) and CBT focused on intrapersonal and interpersonal aspects of the client's functioning, differential patterns of relationship emerged with respect to these foci of intervention and outcome (Kerr, Goldfried, Hayes, Castonguay, Goldsamt, 1992). Specifically, an intrapersonal focus tended to be related to client's improvement in CBT but not in PI. Reversibly, we found that a focus on interpersonal issues tended to relate to change, but only in PI. Although preliminary, these results suggested that PI therapists might be able learn from CBT by considering how they focus on intrapersonal issues, while CBT therapists might benefit from learning how PI focus on interpersonal issues.[3]

Collaboration with other graduate students (Pat Raue and Susan Wiser) allowed me to expand my empirical investigation of the process of change by including the (nonbehavioral) concepts of working alliance and emotional experiencing. With my dissertation, I integrated what I knew in

[3] A later study was conducted as an attempt to replicated these preliminary findings (Castonguay et al., 1998). In contrast with Kerr et al.'s (1992) study, we found that a focus on combined intrapersonal issues tented to relate to outcome in PI but not in CBT. On the other hand, we also found that a focus on a specific intrapersonal issue (i.e., therapist's connection between two components of the client's functioning, such as the link between thoughts and emotions) was only predictive of outcome in CBT. Furthermore, an interpersonal focus was still predictive of improvement in PI only. Although further studies should be conducted before a definite statement can be made, these differential relationships with outcome again seem to suggest that each treatment might benefit by considering the potential contribution of the other.

process research by investigating the predictive value of the CSTF, working alliance inventory, and the experiencing scale in cognitive therapy for depression (Castonguay et al., 1996). The results indicated that the psychodynamic concept of alliance and the experiential construct of experiencing were predictive clients' improvement. Interestingly, the findings also indicated that therapists' adherence to the cognitive rationale and techniques (as measured by a specific item of the CSTF) related to outcome, but negatively so.

Further quantitative and qualitative analyses, however, suggested that the focus on cognitive rationale and techniques was not detrimental per se. Rather, such focus seemed to interfere with change when used in an inappropriate context. Specifically, content analyses revealed that cognitive therapists tended to increase their adherence to the cognitive rationale and techniques as a way to deal with alliance ruptures (e.g., by trying to convince the client of the validity of cognitive theory when the client expressed doubt about the relevance of this perspective for his or her situation). Such increased adherence frequently appeared to lead to further reluctance from clients, which in turn appeared to trigger more attempts from therapists to justify the validity of cognitive theory and/or the effectiveness of cognitive interventions, which were frequently followed by more reluctance or resistance from the client. What was observed, in other words, was that the use of interventions (at the core of cognitive therapy) to deal with alliance ruptures seemed to exacerbate relationship problems rather than resolving them.

While at Stony Brook, I also continue to work on clinical and theoretical issues related to psychotherapy integration. (e.g., Goldfried & Castonguay,1992, 1993; Goldfried, Castonguay, & Safran, 1992). Thus, even if my practice was limited to CBT, my writing and research was about integration. As a sign of having developed a sense of professional identity, I eventually came up with a label to describe myself as a clinician and researcher: A cognitive-behavior therapist open to other orientations—this is still the way I introduce myself to colleagues and students.

From Identification to Consolidation and Integration

As my sense of identity was taking roots, I began to expand my clinical work in two directions. On the one hand, I was committed to generalize my cognitive–behavioral skills to different clinical roles, treatment modalities, and client populations. On the other hand, I wanted to enlarge my technical repertoire. At the end of my stay at Stony Brook, I jumped into the opportunity to serve as a supervisor for less experienced clinicians and I

tried my hand at couples and family therapy—all of this while being primarily guided by CBT principles. When the time for an internship came around, I was hoping to find myself in an environment that would value my CBT expertise, yet able to provide me with systematic training in psychodynamic and humanistic orientations. I found what I was looking for at the Counseling and Psychological Services Center at Berkeley. My post doctorate at Stanford also provided me with an excellent setting to further develop my knowledge and skills in the same direction, as I was fortunate to be mentored by Len Horowitz (one of the foremost authorities in psychodynamic therapy and interpersonal theory) and Stewart Agras (one of the pioneers of behavior therapy).

At both Berkeley and Stanford, I was encouraged to use CBT skills in individual therapy, group treatment, and outreach activities for a variety of clinical problems (e.g., anxiety disorders, eating disorders). These new experiences helped me to consolidate my CBT identity. I also arranged to be supervised by non-CBT clinicians so that I would be able to engage in therapeutic interventions that were mostly seen as taboo at Stony brook (e.g., transference interpretation, working with resistance, exploring what is taking place in the here-and-now of therapeutic process, facilitating emotional deepening, uncovering and processing developmental issues). I was also conscious of protecting my hard earned sense of identity by conceptually assimilating these "forbidden issues" within neo-behavioral theories. Goldfried's (1985) in vivo concept, for instance, allowed me to talk about transference within a framework I found comfortable, and Arnkoff's (1981) insightful work provided me with helpful heuristics to use the client-therapist transactions (the "here and now" of the therapeutic relationship) in order to facilitate cognitive change. Experiencing emotions made "behavioral sense" when seen in the context of the need for full exposure of fear structures to foster lasting change in anxiety disorders (Foa & Kozac, 1986). Linehan's (1993) elegant concept of balance between change and acceptance gave me a much-needed rationale to begin dealing with what my supervisors called adaptive and maladaptive resistance. Linehan's dialectic behavior therapy, as well as Beck' (Beck, Freeman, et al., 1990) and Young's (1990) contributions with regard to personality disorders, made it possible to recognize unfortunate legacies of early environments on core (and frequently implicit) views of self, and the profound impact of these views on relationships with others.

Fueling the assimilation of these previously foreign clinical issues was the convergence that was taking place between my practice and my research. The benefit that I was observing from my client's deepening of their emotions was not only consistent with one of my dissertation findings mentioned above but it was also in line with the findings of a process study on CBT for eating disorders that I conducted during my post doctorate

(Castonguay et al., 1998). At about the same time, Adele Hayes's research was showing that a focus on attachment with early caregivers during cognitive therapy was not only linked with outcome but predicted relapse two years after treatment (Hayes, Castonguay, & Goldfried, 1996). Hayes et al.'s (1996) findings also suggested that therapists' focus on real interpersonal interactions might have a more positive impact than a focus on clients' thoughts about interpersonal relationships.

Parallel to these unexpected and challenging research findings, the need to assimilate noncognitive behavioral interventions came from repeated confrontations with the untamable reality of clinical practice. Working with particularly difficult cases during my internship and post doctorate made me aware that I was committing therapeutic mistakes similar to what my qualitative analyses on my dissertation seemed to reveal: When confronted with alliance ruptures, I tended to dig my heals and keep using the techniques prescribed by my supervisor or dictated by my case formulation. Interestingly, I also noticed that when I stopped doing so, reflected on what had taken place, and then shared my experience (including a recognition of my rigidity or failure to understand), tension in the room eventually decreased and ruptures frequently got resolved. Not too long after these personal observations, I came across the work of David Burns (1990) and Jeremy Safran (Safran et al., 1990; Safran & Segal, 1990) about the identification and repair of alliance ruptures. Their writing presented an exquisite rationale for the strategies that seemed to work for me: recognize and focus on alliance ruptures, explore feelings related to the alliance tears, and explore my contribution to the relationship problems.

The first independent study that I conducted after taking my academic position at Penn State was the logical extension of my dissertation. The study was aimed at investigating whether humanistic and interpersonal techniques could be integrated within cognitive therapy in order to adequately resolve alliance ruptures and possibly improve the effectiveness of cognitive therapy. Specifically, therapists were trained to follow the guidelines of traditional cognitive therapy, except when confronted with markers of alliance ruptures. At such point, they were required to use Burns' and Safran's techniques to address the relationship problems, after which they were to resume CT. As described elsewhere (Castonguay et al. 2004) , , the findings of this preliminary study suggested that this integrative cognitive therapy is not only superior to a waiting list but appears to compare favorably with traditional cognitive therapy.

What the data and clinical experience that I had accumulated from the end of my doctoral program until the beginning of my academic career were indicating was that I could improve my effectiveness as a CBT therapist by integrating techniques of other orientations. Even until that point, however, my integrative efforts had a piece meal flavor. While I was

feeling more and more comfortable in implementing a larger variety of techniques (e.g., repairing alliance, deepening emotion), it was at times very difficult to articulate a rationale for a treatment plan that prescribed the use of CBT and non-CBT intervention methods. Thus, with the increase of my technical repertoire, also came an old foe crawling back: the uneasy sense of confusion triggered by my failure to achieve a cohesive integration of what I was doing and why I was doing it.

From a Piagetian sense, I needed to move from assimilation (incorporating new issues within old schema) to accommodation (modifying old schema to integrate conceptual anomalies). At one level, what I needed was a clinical model, that is, a framework allowing me to integrate CBT and non-CBT clinical contributions. At another level, I was searching for a theory of human functioning that would be able to integrate dimensions traditionally disregarded by CBT, while being complementary to the clinical Model I was looking for. I found the first framework by returning to what brought me to the United States: Goldfried's principles of change. I found the second model in Safran's expansion of cognitive theory. As described in the next section, these two models are at the core of my current efforts in training and research.

CURRENT AND FUTURE PATHWAYS

For the sake of clarity, I will summarize separately my training and research efforts. I hasten to say, however, that these efforts mutually influence one-another.

Training

Current Efforts

A few years after my arrival to Penn State, I was asked to teach a graduate seminar on behavior therapy. Typically, the preparation of a course that one has never given before (a "new prep") is an anxiety-producing chore, especially if one has not yet been tenured! However, I remember being quite excited by this request. Indeed, I felt that it would give me an excellent opportunity to integrate and further articulate my thoughts about the way I was doing clinical work.

Rather than assigning readings to be discussed every week, I decided to built a more practicum oriented, or a "how to conduct CBT" type of course. I also wanted to cover both the strengths and limitations of CBT.

Furthermore, I intended to demonstrate that some contributions of other approaches could address several of the weaknesses of CBT. For a while, however, I was not able to find an angle that could convince students that one could be a heretic and yet be conceptually consistent! How could I persuade them that a number of humanistic, systemic, interpersonal, and psychodynamic techniques (most of which I was actually using) were complementary to many CBT interventions? Then, it dawned on me that the best way to solve this problem was to base the seminar around the principles of change that I had been investigating for the decade—sometimes, I guess, things are so close that you can't see them clearly!

This seminar is only briefly outlined here (my training efforts in integrative psychotherapy, including this CBT seminar, have been described in detail elsewhere. (see Castonguay 2000). Its main message is that although particular approaches of psychotherapy are associated with specific sets of techniques, many of these techniques reflect principles of change that cut across different orientations (e.g., establishing a positive relationship, providing a new view of self, facilitating positive experience, providing a continuous test with reality). Put in another ways, several of the therapeutic procedures that are assumed to be unique to particular orientations are topographically different, yet they are also likely to be functionally equivalent. Consistent with a core underpinning of CBT, one could argue that the model that I use to integrate clinical contributions of different approaches is based on a functional analysis of intervention methods.

The seminar, then, systematically trains students in the most prevalent techniques that have been developed by CBT proponents, while organizing these intervention methods around each of the principles of change identified by Goldfried. Students are also exposed to clinical observations and empirical data suggesting that in certain circumstances or with particular clients these techniques may be either ineffective or counterproductive. Students are then exposed to psychodynamic, humanistic, or systemic interventions that serve the same function but are likely to be more effective in these situations.

When addressing the therapists' efforts to provide a new perspective of self, for instance, students are trained in cognitive restructuring techniques (e.g., searching for evidence, generating alternatives to client's interpretation) that are used to directly and explicitly replace negative thoughts with more positive beliefs. Based on the empirical work of Beutler (e.g., Beutler et al., 1991), however, students are also informed that such directive interventions can be problematic for some types of clients, that is, individuals that are reluctant to be controlled. With numerous examples and role-plays, students are then instructed on how one can make use of less directive modes of interventions to first validate and then invite highly

reactant clients to explore him/herself the veracity of his or her thinking. I have found the use of humanistic techniques (such as selective reformulation of clients' views) to be particularly helpful in this regard.

In describing ways to provide clients with a different views of self, students are also exposed to empirical evidence suggesting that an exclusive focus on clients' cognition could limit the benefit client can derive from therapy, including in cognitive therapy. Several process studies—some of them described above—have indeed demonstrated the relationship between outcome and CBT emphasis on emotional, developmental, and interpersonal dimension of functioning (e.g., Castonguay et al., 1996; Hayes et al., 1996; Jones & Pulos, 1993). Hence, students are taught how and when to use psychodynamic, interpersonal, and humanistic interventions that focus on emotion, the past, and on maladaptive relationship patterns.

To say that a cognitive-behavior therapist can use some non-CBT interventions because they are functionally equivalent to certain CBT procedures, or because they have been shown to predict outcome in CBT is one thing. To make sure students will be using such techniques and feel that they can legitimately call themselves CBT (as opposed to eclectic, for example), is another. As discussed in Castonguay (2000), this is where Safran's theoretical work is so helpful. Safran's theory expands upon traditional cognitive models in a way that allows an integration of a wide range of dimensions of functioning (interpersonal, emotional, developmental, and conflictual) that a CBT therapist is likely to work with if he or she bases his or her treatment plan on principles of change rather than on a list of techniques traditionally prescribed in CBT protocols. Safran's model builds on Beck's construct of schema by emphasizing that the nature of such schema is intrinsically interpersonal (i.e., we can't think about who we are without referring to others), that our core views of self have been developed within emotionally laden and conflictual interactions with early caregivers, and that these views are not only likely to distort one's perception of self and others but also pulls for others' reactions (including the therapist) that are unlikely to meet one's needs.

Future Directions

I have been teaching this graduate seminar for several years now, and it is likely to remain an important component of my teaching for several years to come. A major incentive for continuing to teach this seminar is the fact that I am planning to use it as the basis for a book that Marv Goldfried and I have talked about writing for several years. This book will (hopefully)

describe how we practice and how future therapists can be trained in CBT while thinking in an integrative perspective. In fact, the syllabus of my CBT seminar was based on an outline that I wrote for the book.

As part of my future plans for training, I am also hoping to expand the use of the principles of change beyond the scope of my seminar and into the clinical training program of my department (at Penn State). This integrative program, also described in Castonguay (2002), would organize the students' training within four more or less distinct phases: Preparation (acquisition of basic clinical skills), exploration (systematic but limited clinical exposure to major psychotherapy orientations), identification (specification, even if only temporary, of a preference to one orientation), and consolidation (expansion of skills related to an identified orientation with respect to various populations, settings or roles). Right from the beginning of their training, students would be exposed to the notion of principles of change. By emphasizing the complementarities and convergences among different orientations, these principles of change are likely to reduce the students' sense of confusion especially during the preparation and exploration phases of the training. The same principles would force the student to remain aware of the limitations (along with strengths) of their preferred orientation while being engaged in the phases of identification and consolidation. Needless to say, these four stages reflect a systematization of various aspects of the developmental trajectory that my own training took. Other phases of this training (assimilation and accommodation) would be left for internship and postgraduate experiences.

Research

Current Efforts

Safran's model and Goldfried's principles of change are also playing an important role in my research and scholarly activities. In terms of research, most of my attention is currently focused on the empirical investigation of an integrative therapy for generalized anxiety disorder (GAD) that colleagues and I have developed at Penn State (These are more than colleagues: A true mentor, Tom Borkovec has provided me with advices, guidance, support and friendship for years and, as the best possible partner for life, Michelle Newman has mentored all aspects of my professional development). The specific goal of our research team is to increase the effectiveness of CBT, which so far stands as the most effective form of intervention for GAD (Borkovec & Ruscio, 2001). For methodological reasons described elsewhere (Newman, Castonguay, Borkovec, & Molnar,

2004), our treatment protocol involves the sequential combination of two separate components: (1) CBT; and (2) interpersonal/emotional processing therapy (I/EP). Based in large part on process findings described above, the techniques included in our I/EP segment have been chosen because they target change processes that fail to be adequately addressed in many CBT protocols, such as facilitating emotional deepening, resolving conflictual issues (integrating wishes and fears), repairing alliance ruptures, exploring past relationship, and correcting maladaptive current interpersonal problems (e.g., helping clients recognize and change their contribution to relationship difficulties). As described in Newman et al., (2004), Safran's model has provided us with a general rationale for a cohesive integration of these nonbehavioral and behavioral interventions.

NIMH has twice funded our research program. The first grant allowed us to test the feasibility of adding interpersonal, developmental, and emotional interventions to a traditional CBT protocol. The promising results (both in terms of process and outcome) of this first study have led to a large clinical trial, which is comparing the efficacy of our integrative treatment with pure CBT. In both studies, my primary responsibility has been to investigate the process of change with respect to several facets of the therapist contribution's, client's experience, and their interaction. Not surprisingly, some of the principles of change that I have been investigating since graduate school feature prominently in the list of process variables assessed (e.g., therapist's focus of intervention, working alliance).

The same principles of change are currently guiding yet one more aspect of my scholarly work. With Larry Beutler, I recently co-chaired a Task Force sponsored by Division 12 (Clinical) of the American Psychological Association and the North American Society for Psychotherapy Research. The goal of the Task Force was to examine the empirical literature related to personal (client and therapist characteristics) technical, and relational factors in psychotherapy, as well as to delineate principles of change that are relevant to the treatment of four types of clinical problems (dysphoric disorders, anxiety disorders, personality disorders, and substance use disorders). Involving some of the most influential leaders in psychotherapy research, the Task Force has led to a book that summarizes what we currently know about the process of change, generates empirically derived guidelines for clinical practice, and identifies future research directions for improving the effectiveness of psychotherapy (Castonguay & Beutler, 2006).

Future Directions

There is no doubt in my mind that I will continue my involvement in the Penn State research program on integrative therapy for GAD. In

addition, future directions of my research are likely to be shaped by a National Practice Research Network (NPRN) that my colleagues (Jeremy Safran, Tom Borkovec, and David Krauss) and I are developing. Based on an active collaboration between clinicians and researchers, the goal of this network is to facilitate clinically relevant, scientifically rigorous, and relatively inexpensive studies. Among the first studies that we intend to conduct is a replication and extension of the research on integrative therapy for depression described above (Castonguay et al., 2004). Specifically, our goal is to determine whether training therapists in resolving of alliance ruptures will increase their effectiveness. In contrast with my preliminary study, this investigation would not be restricted to one disorder (depression) and one orientation (cognitive therapy). Rather than being conducted in a controlled setting, the proposed study would also be conducted in a natural setting.

It is more than likely that the future directions of my research program will also be influenced by the conclusions of the Task force that I recently co-chaired with L. Beutler. Considering the goals of this Task Force, it is hardly surprising that its conclusions suggest ways to better understand principles of changes that cut across many orientations. Thus, I predict that a substantial amount of my professional energy will continue to be devoted to the elucidation of the same variables that first captured my research interest in psychotherapy.

FUTURE OF PSYCHOTHERAPY INTEGRATION AND SEPI

As announced earlier, I am ending this paper with some predictions and suggestions about psychotherapy integration and SEPI. I have conflicting views about the future of the integration movement. On the one hand, I find it reassuring, and not the least surprising, that most psychotherapists (at least in the United States) define themselves as integrative or eclectic (Mahoney, 1991). It is unlikely that this will drastically change in the near future. While I believe that major theoretical orientations (humanistic, cognitive–behavioral, psychodynamic, and systemic) are here to stay, I cannot think of any reason why a majority of therapists will suddenly turn to one of them as their only source of theoretical identification. On the other hand, I cannot help but think there are some "writings on the wall" that integrationists are not paying attention to, and that our blindness to important professional and social pressures are likely to cause major damage to psychotherapy integration as a respected tradition, and SEPI as a reputed organization. These writings on the wall, I believe, are calling for more attention to training and research.

As discussed elsewhere (Castonguay, 2005; Castonguay, et al., 2003), systematic training programs have been developed within each of the major orientations. Furthermore, for each of these orientations one can easily point to well-known training institutions, or "Temples." This is hardly the case for the integration movement. In my view, the lack of systematic, if not institutionalized training, is part of the reason why the integrative/eclectic movement is still regarded by some as a "wishy-washy" orientation. Although efforts to organize training programs have begun to emerge (e.g., Castonguay, 2000; Consoli & Jester, 2006; Norcross & Beutler, 2000; Wolfe, 2000) more needs to be done in order to teach new and experienced therapists what we know about the benefits and limitations of combining different strategies of intervention, and how do to so in the most effective way. The integration movement has also largely ignored calls for accountability via research evidence. This, also, is in contrast with major traditions in psychotherapy. Cognitive-behavior therapies were born with a predisposition toward outcome research. The future of this orientation is safe, if only because most of today's empirically supported treatments are cognitive–behavioral. Although a clear consensus about the value of empirical research has yet to be achieved within the psychodynamic orientation, a rich body of empirical knowledge has been accumulated on the process and impact of this approach. In fact, there is now enough scientific support for psychodynamic constructs to recommend that this orientation be an integral part of every training program based on the boulder model, including behaviorally oriented programs (see Schut & Castonguay, 2001). I also believe that the humanistic movement is likely to regain some of the visibility it lost during the last 15 years, precisely because the new generation of leaders within this approach (e.g., Elliott, Greenberg, and Hill) are actively involved in research—the same way that some of the pioneers of this orientation were half a century ago (e.g., Rogers, 1957).

As noted elsewhere (e.g., Castonguay & Goldfried, 1994), there is an important gap between the conceptual and clinical innovations in the integrative movement and the empirical support that can be claimed for this movement. For sure, some research has begun to be conducted within each of the major trends associated with psychotherapy integration (see Castonguay et al., 2003). I would argue, however, that there is not yet definite empirical evidence to support what most eclectic or integrationist therapists believe to be true, that is, that the combination or integration of interventions derived from various forms of therapy has improved the effectiveness of therapy. The integration movement in general, and SEPI in particular, have been able to flourish under the logical assumption that incorporating the best of different systems can only make things better. However, I am afraid we will eventually lose our credibility if we continue

to rely primarily on theoretical conviction and anecdotal observations to support our claim of superiority over "pure form" treatments.

As for the future of SEPI, I have one piece of advice: Let's be bold again! The creation of SEPI was the result of ambitious efforts by and for people who "think outside the box." In my view, the ultimate outcome of SEPI is the process it has generated. It has opened up boundaries that had been futilely imposed by dogmatic teaching. It has also created a setting where people raised in different intellectual traditions can understand one another, not because they speak the same language but because they agree that, fundamentally, they are talking about the same things. The creation of the SEPI infrastructure also reflected innovative ways of doing and thinking about things. In contrast with other professional organizations, SEPI has no president, no executive officer, no awards, and so forth.

Not surprisingly, SEPI meetings used to be a breath of fresh air. You never knew who would show up and how discussions would turn out. In my eyes, however, SEPI meetings have become traditional. I go there more to meet some old friends (which is good, of course), than to learn something new (which is really sad considering the goal of this organization). As with other organizations, the meetings are taking place regularly, involve many of the same people, who frequently talk about the same topics. I say, let's be bold: Let's meet every two years, so that each meeting is a special occasion for which we reserve our best ideas and finest presentations. Let's invite people we have hardly heard of, coming from completely different perspectives, which are likely to either disagree with the way we think, or to tell us about things we have only a faint idea about. The worst thing that could happen is that some of us will leave the meetings with a sense of confusion. After having spent all these years with that feeling, I can tell you that this would not be that bad!

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