On Theoretical Integration in Psychotherapy

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Even though theoretical integration has been fostered in psychotherapy in the past few years, so far no clear argument for it has been offered. In this article we try to put forward such an argument. In addition, we argue that not all attempts to integrate theories are adequate and suggest 3 conditions that any adequate attempt should meet. We also outline a particular integrative approach —*integration by expansion*—which meets these conditions.

Keywords: psychotherapy integration, theoretical integration, integration by expansion, psychological research programs

Psychology is a diverse science—not only because researchers are concerned with explaining phenomena of quite different sorts, but also because they are ready to explain the very same phenomenon in many different ways. They do not even share a same conceptual framework. To use Lakatos' (1970) terminology, they pursue different *scientific research programs*.[1]

Psychoanalysis, behaviorism, cognitive psychology, sociocultural psychology, humanistic psychology, and systemic psychology are some of the research programs that coexist at present in psychology. Exactly how many programs there are is hard to ascertain, partly because the boundaries of a program are often not completely clear. But the fact is that there are a few. The question arises then as to how this programmatic diversity in psychology is to be handled.

A view that was once quite popular is that one should choose the theoretical framework of a unique program and forget about the others. The idea is that in order to explain, say, a certain person's being anxious, psychologists might use either such psychoanalytical concepts as (e.g.) *oral stage* and *castration anxieties* or such systemic concepts as (e.g.) *family loyalties* and

coalition or such cognitivist concepts as (e.g.) *information processing* and *mental scheme,* but by no means they should use both the concept of oral stage and the concept of family loyalties, or both the concept of coalition and the concept of information processing, and so forth. That is, an appropriate explanation should not combine constructs stemming from different research programs. Let us call this view *uniperspectivism*—the view that phenomena must be explained from one and only one theoretical perspective.

We take it that nowadays uniperspectivism is not the main trend in psychology or, more generally, in social science. Yet things were different until recently. Few decades ago, many social scientists thought that programmatic diversity had to be simply overcome. For them, the ideal state for psychology, and actually for any other scientific discipline, was one in which one single research program ends up predominating over others. As Feyerabend (1981) reported in the eighties:

> More than one social scientist has pointed out to me that now at last he had learned how to turn his field into a "science" (. . .) The recipe, according to these people, is to restrict criticism, to reduce the number of comprehensive theories to one, and to create a normal

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[1] Some prefer Kuhn's (1970) expression "paradigm." We use Lakatos' terminology because we think that his account of how science works is a little more accurate than Kuhn's. Still, it seems to us that talking of "paradigm"—or, for that matter, of "theoretical framework," "theory," and the like— would make no major difference to our main points here.

science that has this one theory as its paradigm. Students must be prevented from speculating along different lines and the more restless colleagues must be made to conform and "to do serious work" (p. 132).

Few contemporary psychologists think this way, of course, but many endorse a more moderate version of uniperspectivism. Even though they perfectly accept the existence and even the convenience of a multiplicity of research programs—so they do not advocate "the monomaniac concern with only one single point of view," to use Feyerabend's description (p. 135)—they do object to the integration of the different points of view. What they take to be problematic is not the coexistence but the combined use of psychological explanations originated from different research programs. Call this *soft uniperspectivism*; and call the more radical view (the view that the diversity of programs should be simply overcome) *strong uniperspectivism*.

On the other hand, during the last few years the idea that there is nothing wrong with theoretical integration has increasingly gained support among psychologists (and other social scientists). This has been particularly so in the field of psychotherapy, where several attempts have been produced to combine diverse theoretical frameworks (Norcross, 2005; Safran & Messer, 1997; Wachtel, 2004). Underlying all of these attempts is the view that it is perfectly correct, even desirable, to explain a phenomenon by integrating theoretical constructs which stem from different research programs. Henceforth, we will refer to this view as *theoretical integrationism*.

In this article we argue that there are compelling reasons for preferring theoretical integrationism over (both soft and strong) uniperspectivism in psychotherapy. We also suggest three conditions that any adequate theoretical integration attempt should meet. For, as we will try to show too, not all possible integration attempts are equally admissible. Finally, we present a particular way to integrate theories which satisfies our three conditions.

We believe that our arguments can be extended from psychotherapy to psychology in general and even to other social sciences.[2] This, however, requires more than what can be discussed in a single article. So here our focus will be restricted to explanation in psychotherapy—not only to the explanations put forward by

theorists and researchers, but also to those that psychotherapists have to produce as part of their professional practice.

Preliminary Clarifications

Let us start by clarifying what a scientific research program is (in the sense intended here) and making some important conceptual distinctions. A scientific research program consists of a set of principles which specify how things are to be explained and how they are not. The former Lakatos (1970) called the *positive heuristic* of the program; the latter, the *negative heuristic*. The negative heuristic forbids the researcher to explore hypotheses that are incompatible with the basic hypotheses of the program—its *hard core*. The positive heuristic, on the other hand, prompts the construction of theoretical models in order to explain *anomalies*, that is, phenomena which are out of the explanatory scope of the hard core by itself. Such models work as a "protective belt" of the hard core and, unlike the latter, may be modified or even abandoned after further observations and theoretical work. The hard core, on the other hand, must remain intact. Modifying it would amount to abandoning the program. As Hacking (1983) aptly put it, "the negative heuristic says: Hands off—don't meddle here. The positive heuristic says: Here is a set of problems areas ranked in order of importance [. . .]" (p. 116).

For example, the hypothesis that human action is motivated by unconscious mental processes is part of the hard core of the psychoanalytic research program (see Freud, 1979c/1899). Researchers are prevented from trying any hypothesis that may contradict this hypothesis (the negative heuristic), while at the same time are encouraged to explain, say, the fact that often people are or seem to be quite aware of the reasons they act as they do (the positive heuristic). Different models have thus been advanced in order to deal with this and other anomalies (e.g., Freud, 1979a/1923, 1979b; Klein, 1975; Kohut, 1977). None of these models defies the unconscious nature of human motivation, or its sexual character (another basic hypothesis), even

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[2] In fact, some of the ideas that follow were suggested in a proposal we have advanced, not for psychotherapy in particular, but for psychology and social science in general (Gaete & Gaete, 2014).

though they describe it and explain its connections with behavior in quite different ways. They introduce certain theoretical constructs of their own, but they share those with which the hard core hypotheses are built. Moreover, the former presuppose the latter, because any theoretical claim must assume such hypotheses and the theoretical framework in which they are generated and help to generate.

So much for the concept of a research program. We need now to distinguish between theoretical integration and *technical* integration. The latter consists in the combination of two or more therapeutic techniques that have been developed in the light of different theoretical frameworks (i.e., frameworks produced by different research programs), whether or not an integration of such frameworks is also intended. For example, in treating a person a therapist may use both systematic desensitization and the empty chair, and perhaps also cognitive restructuring and dream interpretation; but this does not necessarily mean that she understands the person's problem from all the theoretical perspectives commonly associated with each of the mentioned techniques. Actually, she may have combined these techniques simply on the basis of her (trained) intuition—a not very popular approach known as *syncretism* (Davison & Lazarus, 1995). A more widely accepted practice is to ground the selection of techniques on their proven clinical efficacy. This approach is sometimes referred to as *technical eclecticism* (Norcross, 2005). In its most sophisticated version, *systematic technical eclecticism*, the therapist aims at the right combination of her own and her client's characteristics (Lazarus & Beutler, 1993). Multimodal therapy (Lazarus, 1997) and prescriptive therapy (Beutler & Harwood, 2000) are good examples of therapy models within this approach.

Although approaches like these are integrative at the technical level, they do not integrate at the theoretical level and, therefore, our main line of argument is not directly concerned with them. However, we will say something on technical integration when we discuss why theory integration is desirable. The point for the time being is that in favoring technical integration one does not necessarily endorse theoretical integrationism. Actually, some proposals on the integration of therapeutic techniques may even yield or presup-

pose (soft) uniperspectivism. For example, Lazarus' integrative approach "conceives of the therapist working within a particular theoretical framework [. . .] but sometimes using techniques from other orientations deemed effective without subscribing to the theories that spawned them" (Davison & Lazarus, 1995, p. 106). Lazarus himself endorses a social learning approach (within the cognitive tradition), in such a way that "the efficacy of any technique from free association to behavioral shaping will be accounted for in social learning theory terms." This allows him, or so he thinks, to "operate within a coherent theoretical framework while being open to possibly effective techniques that have developed within different conceptual frameworks" (p. 107). We hope to show in the course of this article that staying within a unique theoretical framework is not the best option in psychotherapy, even if one is open to techniques the theoretical origins of which are not in one's own favored framework.

Theoretical integration also differs from the integration of such things as clinical strategies (Norcross, 2005), change principles (Goldfried, 1980), and therapeutic tasks (Cooper & McLeod, 2010). Inspired by Frank and Frank (1991), these variations of the *common factors* approach to therapy seek to "determine the core ingredients that different therapies share in common, with the eventual goal of creating more parsimonious and efficacious treatments based on those commonalities" (Norcross, 2005, p. 9). In so far as the kind of integration underlying this approach does not work by combining theories or theoretical constructs from different research programs, we do not count it as integrative at the theoretical level either.

A third relevant distinction we want to underlie from the beginning is the distinction between the different sorts of phenomena meriting explanation in the context of psychotherapy:

1. **Disorders.** The question explanations are meant to answer in this case is why people present certain symptoms or problems or dysfunctions (the list of words that each therapist is ready to insert here can differ significantly). For example, why a person develops a phobia, or why a teenager forms distorted ideas about her aspect and

decides not to eat any more, or why a couple cannot achieve sexual satisfaction, and so forth.

2. **Therapeutic outcomes.** The question in this case is why clients get well, change, solve their problems (as a result of therapeutic processes). To say that a person got well by having an insight into her motivations is an example of this sort of explanation; another is to say that a certain problem was solved thanks to the therapist's empathy.

3. **Therapeutic processes.** The question here is why clients respond as they do in therapy. Scheflen (1978) offered a number of alternative explanations of a client's smile. Other explanations that are meant to answer this question abound in what has been called *change process research* in psychotherapy (Elliot, 2010)>.

Theoretical integration can then be attempted to get answers to at least three types of questions. Naturally, they are interwoven questions. The explanation of a given problem is often (part of) an explanation of how to solve it; and explaining therapeutic processes is often (perhaps always) subsidiary to explaining therapeutic outcomes (after all, the latter are the result of the former). Actually, there is arguably a primacy of outcome explanation in the psychotherapeutic theoretical endeavor, in the sense that all explanations in psychotherapy seem to aim ultimately at explaining how people get well. At any rate, the existence of these three different sorts of explanations must be kept in mind (for multiple reasons, one of which will be mentioned later).

Last, but not least, a distinction must be made between theoretical integration itself and the different ways in which it can be attempted. Unfortunately, thus far participants in the discussion over integration in psychotherapy have not been as careful to distinguish between these different attempts to integrate at the theoretical level as they have been to distinguish between the different kinds of technical integration (syncretism, technical eclecticism, and the like). Davison and Lazarus (1995), for example, describe theoretical integration as the blending of two theories, in such a manner that "concepts from Therapy A (. . .) become incorporated into the theory underlying Therapy B" (p. 106). This,

however, is only *one way* in which someone may try to integrate different theories. An alternative way would be to assimilate concepts from both A and B into a third, more inclusive conceptual framework. Or, in the light of an approach we will further describe later on, one can use two or more research programs without incorporating or assimilating their theoretical constructs to each other and to any other framework whatsoever. Theoretical integration occurs whenever a certain phenomenon is explained by using concepts originated from different programs, whether or not this is done by merging such concepts together.

So when Davison and Lazarus talk of theoretical integration they seem to have in mind only those integration attempts that work by blending theories (e.g., Ryle, Poynton, & Brockman's, 1990; Wachtel, 1977). Here we use the expression in its more generic sense. In that sense, in so far as a psychological explanation feeds on more than one theoretical framework, it qualifies as a case of theoretical integration.

Theoretical integrationism, then, is not to be conflated with the view that theoretical blending is in order. You are a theoretical integrationist if you favor *some* way to integrate theories, not only the assimilation or reduction of theoretical concepts. Actually, we think—and will argue—that attempts in this vein violate a crucial condition that theoretical integration should always meet. But we will elaborate on this later. Before we want to make clear why we think that theoretical integrationism is better than uniperspectivism in psychotherapy. After all, the current popularity of the former approach does not mean by itself that it should be preferred over the latter.

Is There Any Good Reason for Being Uniperspectivist?

What considerations make uniperspectivism tempting? One feeds on the (correct) idea that every theoretical construct presupposes the hard core of the research program to which it belongs. Thus, for instance, the (psychoanalytic) concept of oral stage presupposes the hard core of the psychoanalytic program just as the (behaviorist) concept of reinforcement presupposes

the hard core of behaviorism.[3] But the psycho-analytic hard core hypothesis that human action is motivated by unconscious mental processes seems or may seem to be at odds with the hypothesis that human behavior is to be explained by virtue of environmental factors, which is plausibly part of the behaviorist hard core (O'Donohue & Kitchener, 1999; Zuriff, 1985). If they are, in fact, incompatible hypotheses, it would be logically incorrect to explain something by using both the concept of oral stage and the concept of reinforcement, since to do so would entail a commitment to two mutually excluding hypotheses. More generally, no two theories presupposing incompatible programs can be integrated into a psychological explanation. Uniperspectivism is then a way to ensure coherence in clinical psychology (and science in general)—or so it may appear. Call this the *coherence argument* for uniperspectivism.

Now even though it is beyond question that one cannot, in logic, explain something by using concepts that presuppose incompatible programs, our view is that few psychological programs are in fact incompatible. True, advocates of the different research programs have traditionally argued for both the viability and the importance of such programs partly by attacking other ones. But their attacks have been less (logically) justified than they have taken them to be. Consider, for instance, the widespread belief that there is some sort of insurmountable abyss between the hard core of psychoanalysis and the hard core of behaviorism. No doubt, both the methodological prescriptions and the theoretical models stemming from the hypothesis that behavior is to be explained by virtue of unconscious mental states diverge dramatically from the methodological prescriptions and the theoretical models stemming from the hypothesis that behavior is to be explained by virtue of environmental factors. But the two hypotheses are by no means inconsistent with each other. For there is no contradiction in believing that behavior is influenced both by mental states and by environmental factors. Nor is there any incompatibility between other psychoanalytic and behaviorist basic hypotheses—or, for that matter, between these and the hard cores of several other psychological programs. After all, the hypotheses so far considered do not say that unconscious processes or environmental factors

are the *only* variable that can explain behavior—and few sensible theorists have ever thought that way. True, the strategy of attacking other programs in order to defend their own theoretical preferences may have misrepresented early psychoanalysts and behaviorists as being closer to uniperspectivism than they really were or wanted to be. Plus, the dissemination of their ideas was not always the most faithful (see, e.g., O'Donohue & Kitchener, 1999). The belief in an insurmountable antagonism between the psychoanalytic and the behaviorist worldviews is due more to these contingencies than to any real logical problem in explaining behavior by virtue of the unconscious as well as the environmental keys that, we all know, are relevant to its occurrence.

Even the alleged incongruity between behaviorism and the cognitivist basic hypothesis that human behavior is mediated by cognitive processes is fictitious, as the work of E. Tolman should have made clear several years ago (see, e.g., Innis, 1999) and the cognitive–behavioral approach to psychotherapy continues to show. There is no contradiction, but common sense, in the view that both environmental variables and mental life are influential upon behavior. In this vein, Wachtel, Kruck, and Mckinney (2005) have pointed out that "both the reality of the impact of the situation or context on our behavior, and the reality of our capacity to choose and alter the situations we encounter, must be taken into account if our theories and practices are to capture the full complexity of human behavior. Neither is more basic or correct" (p. 178).

But is it not the case that psychoanalysis, cognitive psychology, and behaviorism are committed to irreconcilable *epistemological* outlooks? And should not the epistemological aspect of a program be considered as part of its hard core? Nowadays it seems to be widely accepted that psychoanalysis has a rather "descriptive" and interpretive stance focused on understanding and meaning rather than on

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[3] We include the last two parentheses because one might use the terms "oral stage" and "reinforcement" to express concepts other than those generated in psychoanalysis and behaviorism, respectively. For example, sometimes when people talk of reinforcement they mean, roughly, "something that makes a person feel good." But this is a highly mentalist notion of reinforcement and, to that extent, not the behaviorist classic notion.

causal explanation and other features typically associated with the natural sciences, whereas both behaviorism and cognitive psychology has been mainly concerned with the latter; and despite this common feature between the behaviorist and the cognitive epistemological approaches, the behaviorist one appears to avoid the positing of unobserved processes which cognitive theorists tend to find scientifically indispensable.

Even if these epistemological differences should be located at the hard core of the programs, however, it is still not clear that they become mutually exclusive because of this. For one thing, it seems to us that one can, without contradiction, be concerned with interpretations and meanings as well as with causal processes. Nobody has ever *shown* that this is not logically possible—and the trend in psychology seems to be in the other direction (for attempts in this integrative line, see Brinkmann, 2011; Harré, 1997). Furthermore, after the work of Tolman it is no longer obvious that the behaviorist program does in fact proscribe the generation of unobserved, theoretical entities (which by the way is nowadays widely acknowledged to be a normal scientific resource). As we see it, as long as behaviorists keep their basic assumption about the explanatory role of the observable events that precede and result from people's actions, they can perfectly acknowledge the existence of unobserved processes that are also relevant to such actions and their explanation.

Of course, at less basic levels, outside the hard core of the programs, inconsistencies between them abound. Many of the theoretical models prompted by the psychoanalytic, the cognitivist, and the behaviorist positive heuristic do clash. But because—as pointed out in the introduction—it is only the hard core of a program that cannot be abandoned without abandoning the program, these more superficial clashes by no means yield insurmountable abysses. Actually, clashes can also be found between theoretical models of a *same* program. Compare, for instance, some of Freud's psychoanalytic hypotheses with some of Klein's; or some of Watson's or Skinner's behaviorist hypotheses with some of Tolman's; or some of Ellis' or Beck's cognitive hypotheses with some of Guidano's. But just as these differences at the surface of the programs make no harm to the unity of the programs, so they pose no obstacle to the

integration of theoretical frameworks stemming from such programs.

Let us illustrate our main point with a case. A man develops a phobia for trains after witnessing a train accident. We ask a behaviorist psychologist to explain this and she tells us, say, that the man has generalized the anxiety provoked by the train incident to any other context characterized by the presence of the stimulus train (see Skinner, 1969). From the point of view of a cognitivist psychologist, however, the phobia may be understood as the result of an incorrect way of processing the information related to the accident (see Opazo, Andreani, & Alliende, 1983). Still another way of explaining the phobia may be suggested by a psychoanalyst who points to a defensive construction the man undertakes in the light of intrapsychic conflicts which have to do with castration anxieties (see Gomberoff & Jiménez, 1982). In principle, none of these three different explanations is at odds with each other. Indeed, it is perfectly possible to articulate them without contradiction in a longer explanation by saying, for example, that the accident caused the man to undergo strong castration anxieties he then tried to avoid by generalizing the anxiety to any other situation characterized by the presence of trains, which can be seen as incorrect cognitive processing. This longer explanation may or may not be right, but what is relevant for the present discussion is that it is not self-contradictory. Hence, the acceptance of any of the shorter explanations of which it is composed cannot logically entail the rejection of any of the others. Even if there are some behaviorist, psychoanalytic, and cognitive theoretical models which clash with each other, at least some of them do not. Hence, there can be no incompatibility between the hard cores of the research programs to which these models belong.

Some may still feel that there is something fishy about this. After all, theoretical psychologists have long talked against theoretical frameworks other than theirs. How could they be so wrong? Well, perhaps they did it for rhetorical reasons. But more importantly, to point out that a given Program A can solve certain issues that a Program B cannot is not necessarily to disclose an incompatibility between the two programs. On the other hand, we by no means want to deny the undeniable, namely that some psychological programs are in fact incompatible;

and that in those cases theoretical integration is logically impossible. Later on we will come back to this (in specifying some constraints we believe must be placed on theoretical integration). What is important for the time being is to stress that there are also many psychological research programs that do not really oppose each other. So even if there are some scenarios in which an attempt to integrate different theoretical frameworks would entail contradiction, there are also some other scenarios in which it would not. Consequently, the coherence argument does not provide a good reason for being uniperspectivist.

It is worth adding here that the idea that things can be correctly appreciated from different perspectives is little more than common sense. We all readily accept it in everyday life and that seems to be the right thing to do. It would be odd not to accept, for instance, that a person's reluctance to sell her house can be seen as an expression of her tendency to delay important decisions *and* her loving that house very much *and* her belief that she has not been offered a fair price. All of these (and other) explanations of the person's reluctance would normally be easily accepted—even if, admittedly, whereas some are more inclined to see people's reluctance to do things as due to personality traits, others are more inclined to see it as reflecting some emotion or some belief or something else. As put by Blackburn (2006) in his assessment of *perspectivism* (the philosophical doctrine that there is no absolute standpoint from which one can see the world but rather a diversity of perspectives): "You and I see the world from literally different points of view, but on the face of it we can each be right about what we see. We are in different places, that is all" (p. 87).

Before leaving the coherence argument, let us stress that some programs do contain irreconcilable hard cores and, therefore, their theoretical frameworks simply cannot be integrated. We should mention, too, that even among the supporters of any given program there can be—and usually are—different interpretations about what its hard core really amounts to. So perhaps we should accept that there is more than one version of each program. Take, for instance, humanistic psychology. A chief, hard core assumption of this program is that people are free to make their choices (Bugental, 1964). This,

however, can mean two quite different things depending on whether freedom is construed as compatible or incompatible with determinism (roughly, the view that everything in the universe is causally determined).[4] If it is compatible, then humanistic theoretical constructs can be combined with those stemming from deterministic programs; if not, theoretical integration becomes impossible. So we may have two versions of the humanistic program: a deterministic and a nondeterministic version, the former being and the latter not being compatible with, say, a deterministic version of psychoanalysis (for it seems to us that also psychoanalysis can exhibit a determinist and a nondeterminist version). It is hard to estimate the number of versions each program has, but it may change over time and is given by the number of different interpretations the adherents of the program make of its hard core—which is why the number is both impermanent and hard to estimate.[5]

Another argument uniperspectivists may be tempted by goes like this. A relatively accepted idea in the philosophy of science is that research programs must compete among themselves so as to allow scientists to prefer one to another. It is because of this that there is scientific progress. As Lakatos (1970) put it: "The history of science has been and should be a history of competing research programs (. . .): the sooner

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[4] For more on these two different views about freedom, known as compatibilism and incompatibilism, see Kane, 2005.

[5] Consider the case of behaviorism. A central feature of the behaviorist program is its utmost respect for empiricism. Some behaviorists—especially the more traditionalist, in line with Watson and Skinner—would take this to mean, among other things, that one of the basic assumptions of their program is that an explanation has no place in it for nonobservational terms and, therefore, mental terms should be avoided. Other behaviorists, however, may have a different (perhaps more updated) interpretation either of empiricism or of the nature of mental terms and side with Tolman in that such terms are helpful even for the behaviorist. These and other interpretational issues are part of the reason there are so many versions of behaviorism, as behaviorists themselves acknowledge it (see e.g., O'Donohue & Kitchener, 1999). Toulmin and Leary (1982) pointed out that "the orthodoxy that controlled American psychology from the 1930s until (approximately) the mid-1950s was not Watsonian but 'neobehaviorist;' and there were not one, but several forms of this orthodoxy . . . It was under the aegis of these revisionist versions of behaviorism that the cult of empiricism achieved its most complete formulation and its practical dominion over American psychology" (p. 602).

competition starts, the better for progress" (p. 155; see also Feyerabend, 1981). In psychology, this idea has been linked to pluralism, the view that "all theories are necessarily limited and that the best way of approaching the truth is through the ongoing confrontation of multiple, competing theories with data and with each other" (Safran & Messer, 1997, p. 144). But if we are allowed to explain a certain phenomenon by appealing to theoretical frameworks of different programs, as the integrationist wants, rather than competition we get complementariness. It might then seem that at least because of this we must go along with (soft) uniperspectivism, for only that way can we make room for programmatic competition and scientific progress. Call this the *competition argument* for uniperspectivism.

We agree that without competition it is not possible to properly assess any scientific research program (for an elaboration of this point, see Feyerabend, 1981). But we need this competition to obtain not among *any* two programs but among those that happen to be incompatible—those whose basic hypotheses are mutually exclusive. For only then can the rejection of one or more programs follow from the acceptance of another. Take the rejection, in physics, of the geocentric program as a consequence of the acceptance of the heliocentric program. This happened only because it was not possible to endorse the hard cores of both programs without contradiction. Otherwise, had both programs been compatible, it would have been ludicrous to reject the geocentric program due to the acceptance of the heliocentric one. Competition leads to scientific progress only when it obtains between programs whose hard cores are mutually exclusive.

But many (versions of) psychological research programs are not like this, as we have already pointed out. Thus, given that, for example, the hypothesis that behavior is influenced by mental processes and the hypothesis that behavior is influenced by environmental factors do not exclude each other, in principle the acceptance of behaviorism, or of a certain version of it, does not really entail the rejection of psychoanalysis and cognitivism. Hence, integrating psychoanalytic, cognitivist, and behaviorist theoretical constructs is not an obstacle for scientific progress. More generally, the fact that without competition we have no way to choose

between programs does not mean that all programs must compete with each other but only that incompatible programs must. But because at least some psychological programs are perfectly compatible, the most the competition argument can do is to restrict theoretical integration to explanations involving theoretical constructs originated from compatible programs—and this seems a quite reasonable restriction, to which we will come back when we discuss the adequacy conditions for theoretical integration.

A third line of reasoning that could lead psychotherapists to endorse uniperspectivism starts from the conflation of theoretical integrationism and theoretical blending. For the latter is certainly not a viable way to go, for reasons we will give later on. So it might appear that one has no option but to choose a unique theoretical framework. Call this the *antiblending argument* for uniperspectivism.

To reject theoretical integrationism because of this, however, would be a non sequitur. As made clear in the previous section, the fact that one cannot possibly translate theoretical terms of one research program into theoretical terms of another is one thing; the assumption that this is the *only* way to integrate theory is quite another. Even if theoretical blending is problematic—as we agree that it is—this is no grounds for objecting to theoretical integration in general.

So neither the coherence argument nor the competition argument nor the antiblending argument succeeds in justifying uniperspectivism—and we are not aware of any other plausible argument that had been, or may be, advanced for it. That is to say, to our knowledge there is *no* good reason for being uniperspectivist. Plus, we think there are very good reasons for endorsing theoretical integrationism, which we try to show next. If we are right about this, then it is not clear how uniperspectivism might continue to be a tenable option in psychotherapy.

Why is Theory Integration Desirable?

What is the purpose of science? On this we side with Quine (1992): "One major purpose is understanding. Another is control and modification of the environment" (p. 2). Actually, we find it hard to think that someone would deny

something like this. Science may be thought of as having more purposes as central as understanding and control (construed as intervention power), but if someone did not accept these two we would take her to have a rather peculiar way of understanding the scientific enterprise—if not to have simply misunderstood it. True, sometimes one is not as much interested in control as one is in understanding (especially in the social sciences); and sometimes the only reason one cares about understanding is that by understanding a certain phenomenon one can gain control over it or over some aspects of it or over some other phenomena related to it (especially in the natural sciences). But given that the very concept of understanding seems linked to the concept of mastering a technique or knowing how to do certain things (McGinn, 1997; Ryle, 1963; Wittgenstein, 1974), it does not appear that one can understand a phenomenon without gaining some degree of control over something. Plus, an "explanation" that gives us control but no understanding whatsoever does not really seem to *explain* anything—surely not in a scientific way.

Understanding and control are then two of the most important goals in science and, therefore, two of the most important criteria for assessing scientific explanations. In this vein, these two principles appear to hold:

1. The more understanding a scientific explanation gives us of a phenomenon, the more preferable the explanation.
2. The more control a scientific explanation of a phenomenon provides, the more preferable the explanation.

Principles 1 and 2 can in fact be combined in a quite plausible maxim for scientific explanation, something in the line of, "Prefer those explanations that increase both your understanding and your intervention power." Naturally, there may be other considerations that are relevant for choosing one explanation over others. Our point simply is that understanding and control are two very important (perhaps the most important) aims of scientific explanations.

Now suppose all of the following psychological explanations are advanced by different psychologists in order to explain why a certain teenager has the self-destructive tendency to engage in cutting behavior:

E1. By engaging in cutting she diminishes her emotional suffering, which reinforces her behavior (see, e.g., Linehan, 1993).
E2. Every time she engages in cutting her parents feel guilty and stop worrying about certain issues that threaten family homeostasis, which in turn favors her behavior (see, e.g., Bergman, 1986).
E3. She engages in cutting because of what this has come to signify to her in the process of developing a particular pattern of attachment with her caregivers (see, e.g., Arciero & Bondolfi, 2009).

Each of E1, E2, and E3 may need some elaboration, but we hope it is fairly evident both that they do not exclude each other and that either of them, if correct, would in fact improve our understanding of the teenager's behavior. That is to say, assuming that all E1, E2, and E3 are in fact correct explanations, if I am aware of E1 but not of E2, my understanding of the behavior in question will be poorer than if I am aware of both E1 and E2; and if I am aware of E1and E2 but not of E3, my understanding will be poorer than if I am aware of E1, E2, and E3.

More generally, the higher the number of (correct) explanations one has of a given fact or phenomenon, the better the understanding one has of it. Actually, more often than not we *need* to see things from different perspectives in order for our understanding of them to be minimally satisfactory. Some even think this is the only way to come close to the objectivity theoretical work aspires to: "There is *only* a perspective seeing, *only* a perspective 'knowing'; and the *more* affects we allow to speak about one thing, *more* eyes, different eyes, we can use to observe one thing, the more complete will our '*concept*' of this thing, our '*objectivity*', be" (Nietzsche, 1967, p. 119). In any case, it seems pretty clear that the longer or richer the (correct) story is that one can tell about something, the better one's understanding of it.

Think again of the example of the phobia considered in the previous section. Assuming that in fact (a) the accident caused the man to undergo strong castration anxieties he then tried to avoid by (b) generalizing the anxiety to any other situation characterized by the presence of trains, which is the result of (c) an incorrect or maladaptive cognitive processing, any shorter

story including only one or two of a, b, and c will yield a poorer understanding of the phobia. Of course, you may have reasons to think that some of these three explanations are incorrect. The point is that in so far as they are all correct, being aware of all of them makes up a better understanding than being aware of just some of them.

Principle 1, then, provides one very good reason for preferring theoretical integrationism over uniperspectivism. For by that principle, an explanation consisting of two or more (correct) explanations is to be preferred to either of those explanations separately. Theoretical integration allows us to tell fuller stories about the world and, by doing this, it enhances our understanding of that world. The more perspectives we have from which to explain, the richer our explanation and the better our understanding.

Furthermore, at least because, as suggested above, understanding a phenomenon entails gaining some degree of control over something, an increase in understanding entails an increase in control (even if not always over the same phenomenon one understands). So again, this time by Principle 2, an explanation consisting of two or more (correct) explanations is to be preferred to either of those explanations separately. Indeed, each of E1, E2, and E3 as well as each of a, b, and c is associated with one or more particular therapeutic techniques. Thus, for instance, if we see the teenager's behavior as related not only to the history of conditioning but also to both the preservation of family homeostasis and a certain attachment pattern, we know we can try not only techniques designed to operate upon the variables involved in conditioning (e.g., systematic desensitization) but also techniques developed in the light of family therapy and attachment theory. Analogously, if one accounts for the person's phobia by means of an explanation encompassing a, b, and c, all behavioral, psychodynamic, and cognitive interventions are available. More generally, to explain a certain phenomenon from diverse theoretical perspectives yields a higher capacity to intervene upon it or upon other phenomenon or phenomena related to it. This is a second reason theoretical integrationism is desirable.[6]

There is still a third reason. Let us assume that nobody will defend random technical integration—that there must be *some* rational story underlying any scientifically acceptable attempt to combine different therapeutic techniques. If that story is not provided by an integration of the theoretical frameworks in the light of which the techniques in question were created, what else can provide it?

Based on the current literature on psychotherapy integration, we can think of two plausible options, both of which were briefly described earlier: technical eclecticism and the common factors approach. Neither one of them is free from problems, though. In technical eclecticism, the rationale behind technical integration is given by clinical efficacy (Davison & Lazarus, 1995; Lazarus & Beutler, 1993; Norcross, 2005). A big problem with this is that creativity and especially innovation in psychotherapy are left aside (for an elaboration of this idea, see Davison & Lazarus, 1995). Other problems have been pointed out by Safran and Messer (1997). Regarding the common factors approach, it may well yield very good results whenever the commonalities of the different therapies do, but it neglects the fact that sometimes a particular therapy system does work better than others. The very point of psychotherapy integration is that sometimes certain techniques are more efficacious than others. Put differently, it is not the commonalities but the differences among therapy systems that have prompted integrative approaches.

Plus, both technical eclecticism and the common factors approach may offer a good deal of control without offering too much understanding. Indeed, they do not provide us with a rational story of *why* a certain integrative intervention happens to be successful. Even if they can be appealed to in order to rationally justify the integration of two or more different techniques, they shed no light on the reason such a technical integration works.

In contrast, none of these issues can be raised against an intervention grounded on theory integration. Even more, theory is the central

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[6] Needless to say, in practice not every phenomenon is to be intervened by using *all* of the techniques available for dealing with it. Different theoretical frameworks offer different paths for solving psychological problems, each of which must be taken or not, depending on such things as feasibility, therapeutic expertise, pertinence, etc. The point is that to consider diverse explanatory perspectives is to open up more possibilities for change, and that this may increase the likelihood of therapeutic success (see APA, 2006).

source of justification in any scientifically oriented discipline—even more central than evidence, given the fact that without a theory nothing can really count as evidence (Feyerabend, 1993; Hoefer & Rosenberg, 1994; Longino, 1990; Quine, 1992). Hence, theory and, by Principles 1 and 2, theory integration in particular seems one of the best ways to both justify and explain a scientifically oriented therapeutic intervention.

Theoretical integration is then desirable for at least three very good reasons: it enhances our understanding of the problems we have to deal with in psychotherapy; it increases our ability to deal with such problems; and it offers the most suitable way to integrate elements at the technical level, at least under scientific standards. Therefore, considering also that no good reason for endorsing uniperspectivism has ever been given, theoretical integrationism seems the right way to face the diversity of programs in psychotherapy (and, *mutatis mutandis*, in social science in general).

But what if theory itself showed not to be that important after all? Recent research might lead some to think this to be the case. Take, for instance, Duncan & Miller's (2000) declaration that "given that theories only account for 15% of outcome variance, theoretical integration efforts focus on the weakest link in the chain of factors accounting for change" (p. 170). Are statements like this justified? Not quite. That the use of this or that particular theory is less predictive of change than other factors says nothing about the predictive power of the *combined* use of two or more theories. Even if the 15% invoked by Miller accurately represents the difference it makes to use one single theory rather than another, it says nothing on the difference between being and not being guided by theory. If the effectiveness of theoretical integration is to be called into question on empirical basis, research must be cited not on the effectiveness of particular theories but on the effectiveness of (adequate) theoretical integration. But to our knowledge this kind of research is nonexistent.

Furthermore, change is just one thing that psychotherapists are interested in accounting for. As we pointed out earlier, their explanations may also be directed toward disorders and therapeutic processes. So even if theoretical integration were not that relevant for explaining therapeutic outcomes (which we seriously doubt), it cannot be despised until it is shown that it is not very relevant, either, to explain disorders and therapeutic processes.

Conditions of Adequacy for Theoretical Integration

There are, of course, ways and ways to try to integrate theories, as we have already said. In this section we suggest (and defend) three conditions that, if we are right, any *adequate* attempt to explain something by integrating different theoretical frameworks should meet.

The Conceptual Conservation Condition

It is widely accepted in the philosophy of science that scientific theories are conceptually or semantically *incommensurable*, in the sense that it is impossible to translate theoretical terms coming from one research program into theoretical terms coming from another (see, e.g., Oberheim & Hoyningen-Huene, 2013). This does not mean that one cannot compare two or more theories (semantic incommensurability does not entail other sorts of incommensurability), but only that in order to fully grasp theoretical notions rooted in the theoretical framework of a given research program, one has to stay within that framework (even though, as theoretical integrationism permits, one can be within many frameworks).

There may be more than one reason for accepting semantic incommensurability, but one that is highly compelling is this. Part of the very *meaning* of any theoretical term is given by the hard core of the program it comes from—in such a way that the term cannot possibly be put to work without being committed to that hard core. Thus, for instance, the psychoanalytic concept of Oedipus complex cannot be expressed by any theoretical term originated from, say, the systemic program. For no systemic term is such that by using it one necessarily endorses the hard core of psychoanalysis; and, therefore, it is *never* the case that a sentence in which the term *Oedipus complex* occurs—whenever it does express the psychoanalytic concept—says exactly the same as a sentence whose theoretical terms are all systemic (or, more generally, a sentence in which no psychoanalytic term occurs). Even if the sentences are close enough to

be coreferential, they do not express the same proposition.

Put differently, there can be no synonymy between theoretical expressions of different programs. To use a term stemming from one program is inevitably to say something at least slightly different than anything that can be said by using a term originated from another program. But then, one condition for proper theoretical integration is that an explanation combining theories rooted in different programs must preserve the conceptual frameworks in which each theory has been originally formulated. This entails that no integration can ever succeed if it involves the transposition of concepts of one theoretical framework into another. To integrate cannot be to translate.

The conceptual conservation condition becomes especially important if we consider that one of the currently most favored ways to try to integrate at the theoretical level is grounded precisely on some kind of conceptual transposition or "theory smushing," to use Norcross' (2005) expression. Indeed, many theoretical integrationists (e.g., Dollard & Miller, 1950; Ryle, 2005; Safran & Segal, 1990; Wachtel, Kruck, & Mckinney, 2005) proceed as if the concepts of one theoretical framework could be imported into another. For instance, Dollard and Miller (1950) tried to grasp the psychoanalytic concept of unconscious from a neobehaviorist framework. For them, "unconscious" means (roughly) unlabeled or unverbalized. But this is quite an inaccurate way of representing the Freudian concept of unconscious, which is logically connected to such other core psychoanalytic notions as, for example, *libido* and *psychosexual development* (see, e.g., Freud, 1979a/1899, 1979b/1923, 1979c/1923) and, therefore, to a series of theses that Dollard and Miller are far from endorsing in speaking of "unconscious." Their conceptual transposition attempt is then doomed to fail, because nobody can, in logic, succeed in using any psychoanalytic concept outside the psychoanalytic framework. Either they leave that framework or stay in it. In neither case are they integrating anything.

What "integrators" like Dollard and Miller often really do is create new theoretical models (in Lakatos' sense). They do not create a new hard core, but they do not put together two different hard cores either. They work at the level of the hard core's protective belt.

Actually, we take Dollard and Miller's proposal to be a coherent and potentially useful theory of personality built from modified elements of the protective belt of a behaviorist hard core. This may well be a case of theoretical progress, but it is not theoretical integration. No really psychoanalytic construct has been incorporated, even though old behaviorism has clearly been modified with the introduction of the (nonpsychoanalytic) notion of unconscious. Perhaps this modification of the behaviorist positive heuristic will eventually lead to deeper changes, perhaps it will not. The point is that none of these scenarios involves an integration of psychoanalysis and behaviorism (at the theoretical level).

On our view, Dollard and Miller's (1950) work is just one example of a relatively popular "integrative" approach commonly referred to as *assimilative integration* (Messer, 1992; Norcross, 2005; Safran & Messer, 1997). This approach prescribes "a firm grounding in one system of psychotherapy but with a willingness to selectively incorporate (assimilate) practices and views from other systems" (Norcross, 2005, p. 10). Unlike straightforward theoretical blending, the assimilation of one theory to another is supposed to provide with a more solid epistemological foothold by keeping at least one theoretical hard core untouched. Instead of treating research programs as eggs to be broken into a blender, they are represented as sponges that can absorb theoretical powers from each other.

Why exactly this would yield an epistemological gain is far from being clear, of course. Indeed, theoretical assimilation comes down either to a logically impossible form of reduction that simply neglects semantic incommensurability or, alternatively, to a successful reduction which yields not the integration of two frameworks but the replacement of one by the other. The latter is the sort of move the uniperspectivist is after; the former fails to meet the conceptual conservation condition. None of these two scenarios seems satisfactory in the light of theoretical integration.

The Coherence Condition

Earlier we saw that theoretical integration cannot be performed between frameworks of

incompatible research programs. As the meaning of theoretical terms is partially or totally given by the hard core of the program to which they belong, in explaining some phenomenon by using one or more of such terms one is inevitably committed to that hard core. But then, the use of terms originated in programs whose basic hypotheses exclude each other would lead to self-contradictory, that is, necessarily false, assertions—and the truth is that necessarily false "explanations" do not really explain anything.

Take, for example, humanistic psychology (in its nondeterminist version) and cognitive psychology (in its determinist version). According to the latter, a person's actions are causally determined by cognitive processes. This is part of the hard core of the program. Hence whenever a cognitive psychologist explains an action by using cognitivist concepts like, say, *information processing* and *mental scheme*, part of what her explanation necessarily entails is that a person's actions are causally determined by cognitive processes. But this is unacceptable from the perspective of (the nondeterministic version of) humanistic psychology, whose hard core tells us that people's actions are not causally determined. Consequently, in order to keep coherence no theoretical construct of this program should be admitted in an explanation including also a theoretical construct rooted in (the deterministic version of) cognitive psychology.

The Validity Condition

Finally, it is fairly obvious that no integrative explanation can be correct if any of the theories it draws on is either false or misapplied. Suppose, for example, that the Freudian theoretical framework were shown to be false of phobic symptoms. Then it seems clear that the explanation of the man's phobia that we offered above by conjoining a, b, and c cannot possibly be right. Or suppose the Freudian framework is not false but one uses it wrongly, for example, by making the wrong inferences from it, or by making the wrong observations, and so forth. Again, no attempt to integrate this misapplication of the theory with something else can possibly work. More generally, any two or more theories that are included into the explanation of a given phenomenon must be both correct and well applied.

Of course, whether or not a certain theory is correct and well applied can be a matter of controversy. How to settle this controversy is beyond the scope of the present work, because the issue is not restricted to theoretical integration in particular but has to do with the more general subject of the correctness of theories and procedures in science. Yet whatever the criteria for the correctness of theories and procedures are, the attempt to integrate theories will be inadequate unless such criteria are met by all of the theories one tries to integrate. Even if the integrative whole is more than the sum of its parts, it will not be right unless each of its parts is.

Perhaps it is worth noting that these three conditions are *logical* requirements. This means that no set of empirical data can do anything to discredit them. For instance, no actual integration attempt that violates one of these conditions can be appealed to as "evidence" that the condition is misplaced. On the very contrary, any attempt to explain something by combining theoretical constructs originated in different research programs must be rejected if it does not respect either of these three conditions. For the logical status of the latter would simply render *nonsensical* any "explanation" of this sort.

Integration by Expansion

We are now in a position to describe the kind of theoretical integration we favor. It involves neither the existence of a privileged conceptual scheme in the light of which all theoretical concepts are to be construed nor the existence of a privileged language to which terms of one or more theories are to be translated (i.e., it respects the conceptual conservation condition). It consists in enlarging the theoretical range of an explanation by means of the coexistence in it of two or more subexplanations based on different theoretical frameworks. In other words, the *explanans* is not restricted to a single story but to a number of stories involving different theoretical commitments.

Suppose, for instance, that a Dialectical Behavior Therapy (DBT)-informed therapist (Linehan, 1993) succeeds in helping a person overcome his *emotional dysregulation* by combining exposure therapy, social skills training, and unconditional acceptance and validation.

Suppose too that the following assertions are true:

1. The person was able to make progress in regulating her emotions, for example, felt less anxious, after being desensitized (in the behaviorist sense) as a result of having been exposed to a feared interpersonal situation.
2. Part of the reason the person felt less anxious after being exposed to the feared situation was that she was trained in social skills.
3. Another factor that lessened the person's anxiety was her participating in a therapeutic relationship in which the therapist systematically validated her experience and helped her to "make sense" of this fear in the light of her life history.

Each of these explanatory assertions allows us to account for the outcomes of the psychotherapy. So instead of choosing between them, and instead of trying to perform any sort of theoretical reduction, assimilation, or blending with the constructs employed in them, we can take the three of them and combine them in a bigger picture: we can say, simply, that the person got better because she was desensitized, socially more capable (thanks to the training), and more hopeful about being able to live a meaningful life (thanks to the validating, sense-making relationship she was in). This fuller explanation enhances our understanding of the phenomenon at issue, while at the same time respects the conditions of adequacy previously stated, because its three subexplanations are correct (validity condition), do not contradict one another (coherence condition), and involve no attempt to transpose concepts or expressions from one framework to another (conceptual conservation condition).

If this is everything our DBT-informed therapist does in order to explain the results of her intervention, she can be correctly said to have integrated by expansion (and DBT may prove compatible with this kind of theoretical integration). Something similar goes for the two cases, considered earlier, of the self-destructive teenager and the phobia for trains. Provided that E1, E2, and E3, in the former case, and a, b, and c, in the latter, are all logically compatible and valid, we can proceed to combine each set of explanations in order to get a more detailed and complex picture of the teenager's behavior and of the person's phobic reaction, thereby enlarging the theoretical range we use to explain—which in turn yields an increment of our explanatory scope and our understanding.

So when we integrate by expansion we neither create a new theory nor reduce one theory to another. Adequate theoretical integration, on this view, is neither about coining a language (or "metalanguage") nor about replacing one language with another. It is about using the languages and theories we already have in a complementary way. This cooperative work of different research programs makes possible the elaboration of more complete, richer stories about the phenomena we want to explain, which does justice to the complexity of psychological life.

It is worth noting that integration by expansion is not an intellectualist approach. We are not saying, for example, that therapists should go through all of the theories they know every time they have to come up with an explanation. The point is that once they undergo a proper training—which of course would include learning different theories—their theoretical knowledge and its application becomes a sort of second nature. Often experts do not need to consult the theories they so readily and effectively are able to apply.

Nor are we suggesting that theoretical integration entails the herculean task of being proficient in all theories and all methods of intervention. The ability to perform adequate theoretical integration is a key component of clinical expertise, one which certainly requires time for personal cultivation. But even highly competent practitioners need not master every single theoretical framework. If they can comfortably move in a few of them, their explanations will be likely to be comprehensive stories with enormous intervention power.

Perhaps many practitioners often do integrate by expansion, even if they would not explicitly put it like that. In this sense, it is quite possible that our contribution here is less on therapy innovation than on conceptual clarification. Yet we hope this clarification may have an impact on those who tend to weaken the power of both their explanations and their interventions either by endorsing uniperspectivism or by trying inadequate forms of theoretical integration.

Final Remarks

The main objective of this article has been to argue that theoretical integrationism in psychotherapy is preferable to any version of uniperspectivism and to outline three adequacy conditions for integrating theories. We hope it is now clear that (and why) the satisfaction of these three conditions—the conceptual conservation condition, the coherence condition, and the validity condition—is necessary for any attempt to integrate theories to be adequate. Further reflection will tell us if they are sufficient as well. At any rate, psychotherapists and theorists will be in a position to benefit from theoretical integration only if their explanations conform to the three of them.

We have also suggested a form of integration, integration by expansion, which, unlike other attempts to integrate theories, meets the three adequacy conditions and, so far as we can see, presents no epistemological or practical problem. We strongly believe that this kind of integration works in other scientific disciplines too, especially (but not only) in the social sciences.

A subtler objective has been to support the view that theory matters in psychotherapy (Stiles, 2010). Not only because explanations based on (good) theoretical work are the best we have (and the only way to remain scientific), but also because technical integration is desirable and we have every reason to think that theory can illuminate this sort of integration better than anything else.

References

APA Presidential Task Force on Evidence-Based Practice. (2006). Evidence-based practice in psychology. *American Psychologist, 61,* 271–285. http://dx.doi.org/10.1037/0003-066X.61.4.271

Arciero, G., & Bondolfi, G. (2009). *Selfhood, identity and personality styles*. Chichester, UK: Wiley-Blackwell. http://dx.doi.org/10.1002/9780470749357

Bergman, J. S. (1986). *Pescando barracudas*. Buenos Aires, Argentina: Paidós.

Beutler, L. E., & Harwood, T. M. (2000). *Prescriptive psychotherapy: A practical guide to systematic treatment selection*. New York, NY: Oxford University Press.

Blackburn, S. (2006). *Truth*. London, UK: Penguin.

Brinkmann, S. (2011). Towards an expansive hybrid psychology: Integrating theories of the mediated mind. *Integrative Psychological & Behavioral Science, 45,* 1–20. http://dx.doi.org/10.1007/s12124-010-9146-3

Bugental, J. F. (1964). The third force in psychology. *Journal of Humanistic Psychology, 4,* 19–26. http://dx.doi.org/10.1177/002216786400400102

Cooper, M., & McLeod, J. (2010). *Pluralistic counselling and psychotherapy*. London: Sage.

Davison, G. C., & Lazarus, A. A. (1995). The dialectics of science and practice. In S. C. Hayes, R. M. Follette, R. M. Dawes, & K. E. Grady (Eds.), *Scientific standards of psychological practice: Issues and recommendations* (pp. 95–120). Reno, NV: Context Press.

Dollard, J., & Miller, N. E. (1950). *Personality and psychotherapy. An analysis in terms of learning, thinking, and culture*. New York, NY: McGraw-Hill.

Duncan, B., & Miller, S. (2000). The client's theory of change: Consulting the client in the integrative process. *Journal of Psychotherapy Integration, 10,* 169–187. http://dx.doi.org/10.1023/A:1009448200244

Elliot, R. (2010). Psychotherapy change process research: Realizing the promise. *Psychotherapy Research, 20,* 123–135. http://dx.doi.org/10.1080/10503300903470743

Feyerabend, P. (1981). *Problems of empiricism*. Cambridge, UK: Cambridge University Press.

Feyerabend, P. (1993). *Against method*. London, UK: Verso.

Frank, J. D., & Frank, J. B. (1991). *Persuasion and healing* (3rd ed.). Baltimore, MD: Johns Hopkins University Press.

Freud, S. (1979a). La interpretación de los sueños [The interpretation of dreams] (J. Strachey, Trans.). In *Obras Completas* (Vol. IV y V). Buenos Aires, Argentina: Amorrortu. (Original work published 1899)

Freud, S. (1979b). El yo y el Ello [The Ego and the Id] (J. Strachey, Trans.). In *Obras Completas* (Vol. XIX). Buenos Aires, Argentina: Amorrortu. (Original work published 1923)

Freud, S. (1979c). Esquema del psicoanálisis [A short account of psychoanalysis] (J. Strachey, Trans.). In *Obras Completas* (Vol. XXIII). Buenos Aires, Argentina: Amorrortu. (Original work published 1923)

Gaete, A., & Gaete, J. (2014). El Problema de la Integración Teórica en Psicología [The problem of theoretical integration in psychology]. In F. Osorio (Ed.), *Epistemología de las Ciencias Sociales: Ensayos Latinoamericanos* (pp. 153–166). Santiago, Chile: LOM.

Goldfried, M. R. (1980). Toward the delineation of therapeutic change principles. *American Psychologist, 35,* 991–999. http://dx.doi.org/10.1037/0003-066X.35.11.991

Gomberoff, M., & Jiménez, J. P. (1982). *Psiquiatría*. Santiago, Chile: Medcom.

Hacking, I. (1983). *Representing and intervening: Introductory topics in the philosophy of natural science* (Vol. 355). New York, NY: Cambridge University Press.

Harré, R. (1997). Forward to Aristotle: The case for a hybrid ontology. *Journal for the Theory of Social Behaviour, 27,* 173–191. http://dx.doi.org/10.1111/1468-5914.00033

Hoefer, C., & Rosenberg, A. (1994). Empirical equivalence, underdetermination, and systems of the world. *Philosophy of Science, 61,* 592–607. http://dx.doi.org/10.1086/289824

Innis, N. (1999). Edward C. Tolman's purposive behaviorism. In W. O'Donohue & R. Kitchener (Eds.), *Handbook of behaviorism* (pp. 97–119). San Diego, CA: Academic Press.

Kane, R. (2005). *A contemporary introduction to free will*. New York, NY: Oxford University Press.

Klein, M. (1975). *The writings of Melanie Klein* (Vol. 4). London, UK: Hogarth Press.

Kohut, H. (1977). *The restoration of the self*. New York, NY: International Universities Press.

Kuhn, T. (1970). *The structure of scientific revolutions*. Chicago, IL: University of Chicago Press.

Lakatos, I. (1970). Falsification and the methodology of scientific research programmes. In I. Lakatos & A. Musgrave (Eds.) *Criticism and the growth of knowledge: Proceedings of the International Colloquium in the Philosophy of Science* (pp. 91–196). London, UK: Cambridge University Press.

Lazarus, A. A. (1997). *Brief but comprehensive psychotherapy: The multimodal way*. New York, NY: Springer.

Lazarus, A. A., & Beutler, L. E. (1993). On technical eclecticism. *Journal of Counseling & Development, 71,* 381–385. http://dx.doi.org/10.1002/j.1556-6676.1993.tb02652.x

Linehan, M. M. (1993). *Cognitive behavioral treatment of borderline personality disorder*. New York, NY: Guilford Press.

Longino, H. (1990). *Science as social knowledge. Values and objectivity in Scientific Inquiry*. Princeton, NJ: Princeton University Press.

McGinn, M. (1997). *Wittgenstein and the philosophical investigations*. New York, NY: Routledge.

Messer, S. B. (1992). A critical examination of belief structures in integrative and eclectic psychotherapy. In J. C. Norcross & M. R. Goldfried (Eds.), *Handbook of psychotherapy integration* (pp. 130–168). New York, NY: Basic Books.

Nietzsche, F. (1967). *The genealogy of morals*. New York, NY: Random House.

Norcross, J. C. (2005). A primer on psychotherapy integration. In J. C. Norcross & M. R. Goldfried (Eds.), *Handbook of psychotherapy integration* (2nd ed., pp. 3–23). New York, NY: Oxford Uni-

versity Press. http://dx.doi.org/10.1093/med/9780195165791.001.0001

Oberheim, E., & Hoyningen-Huene, P. (2013). *The incommensurability of scientific theories*. Retrieved from http://plato.stanford.edu/archives/spr2013/entries/incommensurability/

O'Donohue, W., & Kitchener, R. (1999). Introduction: The behaviorisms. In W. O'Donohue & R. Kitchener (Eds.), *Handbook of behaviorism* (pp. 1–13). San Diego, CA: Academic Press. http://dx.doi.org/10.1016/B978-012524190-8/50002-4

Opazo, R., Andreani, A., & Alliende, F. (1983). La terapia cognitiva de Beck en la depresión y sus relaciones con la teoría de la autoeficacia de Bandura [Beck's cognitive therapy for depression and its relations to Bandura's self-efficacy theory]. *Terapia Psicológica, 2,* 22–55.

Quine, W. V. (1992). *The pursuit of truth*. Cambridge, MA: Harvard University Press.

Ryle, A. (2005). Cognitive analytic therapy. In J. C. Norcross & M. R. Goldfried (Eds.), *Handbook of psychotherapy integration* (2nd ed., pp. 196–217). New York, NY: Oxford University Press.

Ryle, A., Poynton, A. M., & Brockman, B. J. (1990). *Cognitive-analytic therapy: Active participation in change: A new integration in brief psychotherapy*. Oxford, UK: John Wiley & Sons.

Ryle, G. (1963). *The concept of mind*. Harmondsworth, UK: Penguin.

Safran, J. D., & Messer, S. B. (1997). Psychotherapy integration: A postmodern critique. *Clinical Psychology: Science and Practice, 4,* 140–152. http://dx.doi.org/10.1111/j.1468-2850.1997.tb00106.x

Safran, J. D., & Segal, L. S. (1990). *Interpersonal process in cognitive therapy*. New York, NY: Basic Books.

Scheflen, A. E. (1978). Susan smiled: On explanation in family therapy. *Family Process, 17,* 59–68. http://dx.doi.org/10.1111/j.1545-5300.1978.00059.x

Skinner, B. F. (1969). *Contingencies of reinforcement*. New York: Appleton-Century-Crofts.

Stiles, W. B. (2010). Theory-building case studies as practice-based evidence. In M. Barkham, G. Hardy, & J. Mellor-Clark (Eds.), *Developing and delivering practice-based evidence: A guide for the psychological therapies* (pp. 91–108). Chichester, UK: Wiley-Blackwell. http://dx.doi.org/10.1002/9780470687994.ch4

Toulmin, S., & Leary, D. E. (1982). The cult of empiricism in psychology, and beyond. In S. E. Koch & D. E. Leary (Eds.), *A century of psychology as science* (pp. 594–617). New York, NY: McGraw-Hill.

Wachtel, P. L. (1977). *Psychoanalysis and behavior therapy: Toward an integration*. New York, NY: Basic Books.

Wachtel, P. (2004). Prólogo. La integración en psicoterapia llega a la mayoría de edad [Preface.

Psychotherapy integration reaches maturity]. In H. Fernández-Álvartez & R. Opazo (Eds.), *La integración en psicoterapia* (pp. 9–12). Barcelona, Spain: Paidós.

Wachtel, P., Kruck, J., & McKinney, M. (2005). Cyclical psychodynamics and integrative relational psychotherapy. In J. C. Norcross & M. R. Goldfried (Eds.), *Handbook of psychotherapy integration* (2nd ed., pp. 172–195). New York, NY: Oxford University Press.

Wittgenstein, L. (1974). *Philosophical investigations*. Oxford, UK: Basil Blackwell.

Zuriff, G. (1985). *Behaviorism: A conceptual reconstruction*. New York, NY: Columbia University Press.

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