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A psychoanalysis on the chair and a psychotherapy on the couch. Implications of Gill's redefinition of the differences between psychoanalysis and psychotherapy

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Merton Gill was able to clarify the differences between psychoanalysis and psychotherapy in an unprecedented way, resolving the many incoherences that characterized endless discussions on this topic for decades (for an overview of some of these discussions spanning over two decades, see Wallerstein, 1969, 1989). Gill was not interested in this investigation *per se*, but as part of a major endeavor he pursued in the last part of his life, namely a redefinition of psychoanalysis itself and of one of its central tenets, the analysis of transference. With the intellectual honesty he showed other times in his life, in his theoretical revision he modified his own classical and oft-quoted positions put forth in the '50s (Gill, 1954), and gave psychoanalysis an enlarged and more coherent meaning utilizing only strict "intrinsic" criteria (i.e., the analysis of transference, or, as we will discuss later, we should better say the "analysis of the relationship"). Psychoanalysis, so defined, becomes applicable to a wide range of clinical situations, from individual intensive therapy to the treatment of the most severe diagnoses in diverse psychiatric settings. In a way, he fully implemented what Freud (1933, p. 153) meant when he said that psychoanalysis is not like a pair of spectacles that can be put on and taken off. In fact, "Freud practised only analysis, whatever the circumstances" (Gill, 1984, p. 175; see Lipton, 1977, 1979, 1983).

Gill's coherent radicalization of the identity of psychoanalysis implied the possibility of broadening its "extrinsic" criteria, since these are no longer relevant for the identity of the method (by extrinsic criteria Gill meant what Leo Stone [1954, p. 570] called “formal” factors, i.e., the couch, a high frequency of session per week, etc.). As a consequence, the definition of psychotherapy was restricted almost to the point of its disappearance: according to Gill's (1984) revision, "psychotherapy" proper is a therapeutic practice where the therapist, purposely, decides not to share with the patient any knowledge that he may have about him and of the possible meanings of their relationship, not because of some defenses on his part (in which case there is no reason why these defenses shouldn't be open -- at least theoretically -- to the possibility to be worked through and hopefully overcome). In other words, all that is left for psychotherapy is a method in which one has no interest in the patient learning about himself. Needless to say, it is not so easy to imagine today such a practice and its rationale. Hypnosis could be an example, even if expert dynamic hypnotists, after having modified specific symptoms in particular moments of the treatment, may explain to the patient every detail of the therapeutic process and analyse the repercussions of the suggestive intervention on the transference (incidentally, years ago Gill himself, with Margaret Brenman, studied "hypnoanalysys" [Gill & Brenman, 1959]).

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Before entering into a more detailed discussion of Gill's contributions and analyzing the far-reaching implications of his theoretical revision, I want to present a clinical vignette of a patient I followed some years ago (see Migone, 1991 pp. 54-55, 1995 pp. 88-89). At that time I discussed this clinical material at length with Merton himself. I find this clinical vignette useful because -- amusingly, in a way -- it turns upside down the traditional way of seeing the differences between psychoanalysis and psychotherapy were these differences to be based only on extrinsic criteria (Gill, 1984); at the same time, it allows us to see the risk of many "orthodox" psychoanalyses where the analyst may find himself doing a "manipulative" psychotherapy to the extent that, as Gill showed very well, he allows part of the clinical material to escape from interpretive work if he believes in the universality and “givenness” of the "classical" psychoanalytic situation.

In reading this material, I suggest that the reader attend, rather than to the clinical details, to their theoretical implications, assuming the correctness of my psychodynamic hypothesis. The clinical details of this case should be used as an occasion to examine the theoretical implications of Gill's definition of psychoanalysis.

A 31 year old female patient was in a three-times-a-week therapy *vis-á-vis*. During a difficult phase of the treatment, she began to show various manifestations of what is usually called negative transference (aggressive feelings, fears of not being understood, threats of interrupting treatment, etc.). Among other things, she said that she could not tolerate the eye-to-eye contact with me, complaining of the face-to-face setting of her therapy: she would have preferred the couch which could have allowed her to feel more "contained," "protected," "relaxed," and to express more easily her inner feelings.

The following information is relevant to a fuller understanding of this material. The patient's parents, who never married, had stopped seeing each other right before the patient's birth; the father, who had strongly tried to convince the mother to have an abortion, refused to give the child legal recognition. Because of a series of arguments that were never fully clarified nor well understood by the patient, the mother's contacts with the father stopped completely. The father, a physician, came from a much higher social class than the mother, and was living in the same town. At times, they would meet in the street, and always they would pretend not to know each other. The patient's feelings were characterized by strong aggression, mixed with fear, affection, and feelings of inferiority and shame caused by the ambivalent fantasy that her father, for some reason, had done "the right thing" in abandoning her and in behaving in such a cruel way. Obviously, the patient could have taken the initiative to go and talk with the father, but she had always been unable to do so out of fear of exploding with anger towards him. Furthermore, if she had approached the father she could have betrayed her loyalty to the mother, with whom she was still living and to whom she was strongly attached. The mother, for her own reasons, had always been unable to work through her problems with the father, and out of pride she was still hoping that he would be the one to make the first step in approaching her. Needless to say, this situation was having the effect of an unresolved mourning process for the patient, having contributed, for example, to the creation of a strong inhibition regarding having a family of her own and having a child (despite the many opportunities she had had in her life), little assertiveness in her career, a strong inferiority complex, feelings of being a "second class citizen", of not deserving the rights of everybody else, and so on.

This pattern was also well expressed in the transference in the form of a devaluation of me, and actually of herself too: by not using the couch I was not doing a real "psychoanalysis" but a "psychotherapy", and I was not a member of the "official" psychoanalytic association that would have been the only one that could have "recognized" or "legitimated" her (the patient was a therapist herself). But I was also well aware that in her ambivalence she had chosen just me as a therapist because she knew very well, that, in a way, I was an ally to her: in her eyes I was not "recognised" (as an analyst I was born, so to speak "out of wedlock").

It is possible that behind her emotional difficulty with eye-to-eye contact with me was also her difficulty in experiencing and expressing all the painful and ambivalent feelings towards her father (love, hate, etc. -- it may not be irrelevant to note the fact, often mentioned by the patient herself, that, like her father, I was a physician, coming from an higher social class, beside being a male). Her request for the couch could have meant also a repetition of the lack of relationship with the father, and if I had agreed to introduce this change, maybe unconsciously I could have

> given her the image of a father who does not really want to deal with all the power of the very emotions she wanted to avoid: we can make the hypothesis that in this case I could have been trapped by the patient's transference. That is, she could have unconsciously succeeded in evoking in me her transference's "role responsiveness" (Sandler, 1976).
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> But I do not want to discuss further these hypotheses, because, whether they are correct or not, they are easily understandable by any expert therapist, and are not the subject of this discussion. As I said before, for the sake of the theoretical discussion I invite the reader to assume the correctness of the above mentioned psychodynamic hypotheses. What I want to discuss here is the role of the elements of the setting in the inner logic of psychoanalytic interpretation. Fortunately, in this case the interpretive working through, centered upon the unconscious meaning of her request of the couch, succeeded in overcoming the patient's difficulties and in helping her to remember and work through a series of memories and painful feelings, both about herself and the father, so that the therapy proceeded in a positive way. But let's assume that my interpretive efforts had not succeeded, i.e., that the patient had felt unable to tolerate the face-to-face interaction with me and that she had threatened to interrupt treatment if I did not accept her request to use the couch: what would have been the right thing to do at that point? I ask this question here only as a theoretical exercise, and assuming, as I said, the correctness of my analysis on the genesis of the patient's resistance.
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> Because of the patient's "ego weakness," I might have introduced what Eissler (1953) called a "parameter" in this "psychoanalysis on the chair," temporarily performing a "psychotherapy on the couch" in order to continue the analytic work, up to the point when it became possible to eliminate this parameter (in this case, the couch) and to return to the "basic model technique" of this analysis (i.e., the use of the chair). Thus, paradoxically, in this case psychoanalysis would have been on the chair, and psychotherapy on the couch, which had become necessary because the interpretive work on the patient's difficulties in looking at me in the face had not been sufficient to overcome her resistance.

As I said earlier, I find this clinical vignette interesting because it clearly exemplifies the implications of Gill's (1982, 1983, 1984, 1991, 1994 Ch. 5) redefinition of psychoanalysis. In Gill's view, psychoanalysis is simply a continuous attempt on the part of the therapist (and hopefully also of the patient unless he is temporarily incapacitated by his defenses) at exploring and analyzing the various meanings of the relationship. I say "analysis of the relationship" and not "analysis of transference" (even if this latter term is used interchangeably) because Gill gave a different meaning to the traditional concept of transference. Due to his adoption of a "relativistic", "perspectivistic" or "socio-constructivistic" paradigm (Hoffman, 1991, 1998) for the analytic situation, the analyst has no right to define what is transference (what is distorted from a supposedly non-distorted or realistic relationship), since "distortions" are part and parcel of any ways of perceiving reality. Actually, we should not use such terms as distortions or manipulations in the first place (that's way I used the quotation marks), since it might imply that we believe in the opposite, i.e., that it is possible to eliminate distortions and perceive an uncontaminated or "true" reality.

In analysis there are only various types of realities, various ways of perceiving and feeling, and these can be discussed freely until a consensus can be reached on a shared reality that seems to us more "realistic". Although our perception is subject to the constraints of reality, "we cannot say what the reality really is" (Gill, 1994, p. 2; see also Freud, 1940, p. 196). There is no single definitive or ultimate way of understanding reality, since analysis is an endless process (in this sense Gill is close to the hermeneutic positions, but only in this sense, because he disagreed in other respects -- I will not discuss here Gill's views on hermeneutics). The perception that the analyst (or, for that matter, the patient) has at any one time, T_1, is conceived as *the reality*, and if for some reason later, at time T_2, the perspective changes, he may label as *transference* the perception he had at T_1, while in turn also the perception at T_2 could eventually be seen as transference once later interpreted; and so on. In this Socratic, hermeneutic dialogue, the therapist has no right to think he is more knowledgeable than the patient: he is simply trying to do his job, and the patient hopefully profits from the therapist's help. However, if the patient believes that the therapist, simply because of his role as therapist, must necessarily know the "truth" better than himself, this could be

immediately a matter for analysis and might possibly be seen as a transferential reaction (similarly, the analyst's belief that he knew more than the patient could imply a countertransferential reaction). The fact that one of the two asks for the other's help and pays him a fee (we should not forget, however, that also some therapists "need" their patients and depend on them) is an asymmetry that marks the entire meaning of the interaction, with obvious suggestive implications. This asymmetry can be discussed by both partners, and in theory, when it is sufficiently analysed and resolved, the therapy may end. Indeed, the patient's request for treatment itself, when appropriately analyzed, could be seen as his "symptom," and in such a case this should be interpreted and worked through.

In a traditional way of conceiving the ground rules of analysis -- as for example in Eissler's (1953) classical concept of "basic model technique" -- the couch, a high weekly frequency of the sessions, and other aspects of the therapeutic situation (such as a form of silence, anonymity, neutrality, and so on) were seen as instrumental to the emergence of the transference. To the extent that we accept these traditional ground rules as characterizing "psychoanalysis" (and also, we might say, to the extent that these or other rules are officially adopted as technical standards by international psychoanalytic organizations), it is assumed that they bear universal meanings and that they are relevant to the identity of psychoanalysis. Gill was clearly implying that this very assumption is in itself, so to speak, antipsychoanalytic. No wonder that Gill, although much respected for his theoretical contributions, was never the best candidate for a career in the psychoanalytic establishment. If we believe that some elements of the analytic setting (e.g., the couch, or the weekly frequency) have always a given meaning for the patient, we block the interpretive process; we cannot proceed further in the understanding of the patient's inner reality. Some classical analysts, for example, see the couch as an instrument for inducing an openness towards the investigation of the patient's unconscious: this meaning, however right or wrong it may be, is taken as an *a priori* (in Codignola's [1977] terms, it belongs to the realm of the "true" and not of the "false", i.e., it becomes uninterpretable [see Migone, 1987]).

The clinical vignette shown earlier is an example of how the couch could have a very different meaning. The same applies of course to other elements of the setting, for example to the frequency of sessions: a patient might feel unbearably invaded by frequent sessions (see Gill, 1994, p. 66), and in this case only a lesser frequency could establish an analytic situation. As Gill often used to say, some patients fear the couch, while others cannot resist jumping defensively onto it and avoid eye contact with the analyst: obviously it is great mistake to interpret only the former behavior as a resistance.

Gill's (1984, pp. 168-169) critique of Macalpine's (1950) concept of "infantile setting" (i.e., a setting made of some stable and secure elements that may induce a form or regression and/or transference) is particularly acute: this setting is supposed to induce infantile strivings to be analyzed later. But -- Gill argues -- if what we are looking for is a "pure" transference uncontaminated by current reality, why in the first place do we need to provoke it or create it by a suggestive or manipulative attitude aimed at inducing an "infantile" reaction? This is not a transference, then, but an appropriate regressive reaction to an "infantile setting". It is an iatrogenic symptom. This form of psychoanalysis actually is, *pace* the "orthodox" analyst, a manipulative psychotherapy. In a way, it is conceptually similar to hypnosis.

Macalpine's conception, in my opinion, refers to the widespread and commonsensical idea that, in order to better see and analyze the transference, it could be useful to "maximize" it with an appropriate setting or attitude on the part of the analyst. Gill did not specifically oppose attempts at "maximizing" the transference, he simply implied that it is a mistake to believe that any given attempt at "maximization" (e.g., the classical ground rules, or, for that matter, also a "non classical" setting) favors the emergence of "the" transference (as a pure repetition of the past). Every therapist's behavior evokes a response, because it is "interpreted" in light of the patient's transference. This has always been well understood also by Weiss, Sampson, and their colleagues of the *San Francisco Psychotherapy Research Group* (Weiss, Sampson & the Mount Zion Psychotherapy Research Group, 1986; Weiss, 1993), who have often argued -- supported by research data -- that the therapeutic environment is never neutral: it is always interpreted by the

patient in light of his pathogenic beliefs (i.e., of his transference), so that in some cases even an "unorthodox" analytic attitude could be very well suited for identifying and changing a specific pathogenic belief.

May one still now ask: what is the appropriate setting to evoke the "real" transference, what are the ideal ground rules for analytic treatment? There is no way to evoke a "pure" or "genuine" transference, if one means by that a set of patient's reactions that are uninfluenced by the therapist's cues or behaviors. Every patient's behavior includes a mixture of his responses to the therapist's current attitude and material from the past. The only thing we can do is to be aware of that, and accept that we always influence the patient without being apologetic for that -- actually, taking it as a welcome opportunity. One of our aims, for example, is to observe possible rigidities in the transferential patterns that we evoke, and investigate why other, different responses are not preferred by the patient. This kind of questioning is highly personal**;** it varies from analyst to analyst, and this is one of the reasons why every analysis is different. It is only by being aware of our influence on the patient that we can begin to analyse it, and to try to understand the peculiar or idiosyncratic ways with which the patient responds. The question, then, is not "Why the patient *distorts*," but "Why he distorts or reacts in one way and not in another possible way that we may think of."

I will never forget a definition Gill gave of "analytic neutrality." It is the best definition I ever heard. Once Gill provocatively said that yes, he did believe in neutrality, but his personal definition of a neutral analyst was the following: an analyst is really neutral when he strongly believes that he can never be neutral, i.e., when he is aware that he knows only minimally the infinite ways in which he influences the patient. Only in this way does the analyst have some chance, so to speak, to begin to be somehow "neutral."

That's why a "classical analysis" could be also a perfect analysis, insofar as the analyst is aware of his "infantile" contribution to the transference (if this is the case). There is no difference, in principle, between a classical and a non-classical analysis: both have manipulative aspects. Also what we could call an "adult setting" exerts a suggestion to behave in an adult way. The question, to be subjected to analysis itself, is why should we induce an adult or an infantile transferential reaction to our patient. As we know, Gill (and, to some respect, also Leo Stone, 1961, p. 15) questioned the need to induce further regression beside the regression that is already part of the patient's psychopathology. However, in my opinion the important factor is the way we deal with the rationale for our using a given rule, the way we explain it to the patient, and especially the way we explain it to ourselves, our self-analytic ability in understanding why we chose a given setting: for self-defensive reasons or to help the patient better. One problem is that we cannot know, especially at the beginning of an analysis, what is the "best setting" for a patient. Actually, it would be antipsychoanalytic to know it; it would mean to have an *a priori* idea of the "ideal transference". It is only during the unfolding of an analysis that we may construct some meanings of the setting for both partners and the possible reasons why we choose one ground rule or another for ourselves or for that particular patient. Probably the best explanation for the use of a classical setting is simply admitting to the patient that this is what we learned to do it from our teachers, analysts and supervisors; this is the way we are used to, or simply that's the way we like it and we feel comfortable with. But we should be open to discussing and analysing it with the patient, in order to discover further meanings or other possible unconscious and possibly countertransferential explanations. Freud (who, in this respect, was never a "Freudian") gave us an admirable example -- not only of honesty but also of theoretical consistency -- when he justified his use of the couch simply by saying that he could not bear to be gazed at for eight hours a day (Freud, 1913). Other therapists may like to look patients in the eyes, both for defensive and non defensive reasons. Hoffman (1994, p. 200), Gill's closest associate, once honestly admitted, to one of his supervisory cases who was questioning the frequency of four sessions a week, that this was what he had to do in order to graduate from psychoanalytic school -- a hardly psychoanalytic reason, some might say, but an explanation consistent with theory.

In what follows I will trace a critical history of terminological issues regarding the difference between psychoanalysis and psychotherapy from the point of view of the relationship between theory and technique. My aim is to complement Gill's (1984) theoretical revision and his discussion of terminology, which reveals important theoretical problems.

Terminological issues as a reflection of the consistency of the relationship between theory and technique: an historical review

Terminological inconsistencies may express a lack of clarity concerning the relationship between theory and technique. One of the most common terms in our field, i.e., "psychoanalytic psychotherapy", is a pivotal example. Since Freud (1915), right or wrong, said that severe pathologies were not treatable with psychoanalysis because the patients were not able to develop a transference, gradually the term "psychoanalytic psychotherapy" began to be used for those treatments that were somehow derived from psychoanalysis and that were employed for these more severe cases. This, however, is only one meaning of the term: in fact, many therapists today treat "classical neurotic" patients -- and not psychotics -- with what they call "psychoanalytic psychotherapy," by which they usually mean a modification of the traditional analytic technique: without the couch, lower weekly frequency, a supposedly more supportive and less interpretive attitude, etc. In this case it seems that the rationale for the modification of the classical technique is not justified by external factors such as severe pathology, but is based on an "internal" reason (i.e., internal or external to the theory). Undoubtedly there is a need for discussion and clarification on these issues, and Gill's theoretical research went just in this direction.

In order to improve our understanding of the rationale of the term "psychoanalytic psychotherapy", it is useful to go back to the original meaning of the term "psychoanalysis". It is well known that this term meant to Freud (1923a) three things: a research method, a therapeutic technique, and a psychological theory. But we should not forget -- and this is an important point -- that, according to Freud, psychoanalysis rested *simultaneously* on these three levels, i.e., they could not be separated from each other. Consistent, then, with Freud's conception, if psychoanalysis is a general theory that can be applied in various clinical situations, we could legitimately talk about "brief psychoanalysis", "psychoanalysis without the couch", "group psychoanalysis", "psychoanalysis once a week", "psychoanalysis of the psychoses", and so on. But if we fragment the triad that Freud postulated, and if by psychoanalysis we mean only a *technique* (i.e., the technique that has been formalized in the classical tradition characterized by four or five sessions per week, the couch, interpretation as privileged intervention, etc.), we are forced to call "psychoanalytic *psychotherapy*" a technique that, although maintaining psychoanalysis as a general theory, implies some modification such as once a week sessions or the face-to-face setting. One may object that the confusion stems from the fact that Freud (1923a) in his oft-quoted "triadic" definition said that psychoanalysis is also a *techinque*, but we should not forget that eight years earlier he had already espoused explicitly an enlarged definition of technique, tied only to intrinsic criteria: anyone who recognizes transference and resitance is a psychoanalyst -- he said -- even he comes to conclusions other than his own (Freud, 1914).

A complication comes from the fact that, according to a certain classical conception (the one expressed, among others, by Macapline, 1950), there is a strict link between analytic method and analytic setting, one influencing the other. Some formal aspects of the setting (the couch, frequent sessions, some constancy of the environment, etc.) are just those that characterize an "infantile setting," which produces regressive phenomena such as the "transference neurosis." They are considered indispensable to the analytic process because they are the very phenomena that are supposed to be interpreted and worked through during the course of analysis.

We are confronted then with two theoretical positions. According to one, a given setting (the classical one) is the only and necessary condition that allows the analytic process to emerge and grow; the development of transference, as such, would not appear in therapies that are different from "classical" psychoanalysis. According to the second position, the transference is not exclusive or unique to psychoanalysis, but a natural phenomenon that appears in various human conditions --

interpersonal relations, love relations, etc. In analysis it is simply "analyzed", i.e., put under our professional attention.

It is obvious that, according to which one of these two options we follow, we can speak about psychoanalytic *psychotherapies* or *psychoanalyses* -- different technical *types of psychoanalysis* (time-limited, with groups, with psychotics, etc.). This latter term means the application of a general theory to different situations according to the field of intervention (institutions, groups, emergencies, etc.), the type of patients (more or less severe diagnoses), and our goals (more or less deep therapeutic changes).

In the history of the psychoanalytic movement there has been no unanimous definition, or position statement, on the term psychoanalysis. On the one hand, as Sandler (1982, p. 44) noted, in some case the best that we could do was to say, tautologically, that "psychoanalysis is what is practiced by psychoanalysts," and, circularly, that "psychoanalysts" are those who practice "psychoanalysis". Often, only members of the *International Psychoanalytic Association* or of other private associations are considered to be psychoanalysts. This use, of course, is not only illegal -- being motivated by monopolistic reasons, as was successfully argued by the recent legal suit of the psychologists against psychoanalytic organizations -- but also dangerous because, by reducing the problem to a matter of institutional affiliation, one puts into serious danger the discipline's scientific status. On the other hand, the common trend within the psychoanalytic movement to separate theory from technique (and hence to fragment the triad of meanings that Freud instead conceived as unitary) unfortunately favors the use of the term psychoanalysis only for a given technique, namely the classical one.

An interesting phenomenon has resulted. While for the founder of psychoanalysis, theory and technique were interdependent in the sense that if progress occurred in the field of theory it was soon reflected in the technique and *vice versa*, later -- because of this more or less implicit trend of defining psychoanalysis mostly as a professional practice codified by a series of behavioral rules -- theory and technique have become progressively split from each other, undermining the scientific status of the discipline. For example, when Freud was convinced of the theory according to which the cure occurred by bringing about the recovery of repressed memories, he did not hesitate to manipulate his patients to help them to remember, to put pressure on their foreheads, to exhort them, to hypnotize them, and so on. Later, when he realized that the memory reached through hypnosis was not curative in itself, but the curative factor was the emotional experience during the difficult and long conscious work aimed at overcoming the resistances to remembering, Freud coherently changed technique, and employed free associations, thus creating psychoanalysis and paving the way to what later, with an enphasis on the analysis of defenses, transformed psychoanalysis into Ego Psychology.

But throughout this century, psychoanalytic theory has undergone various modifications in some of its basic assumptions, up to the point where today it is not only fragmented into multiple schools, but also shaken by an epistemological crisis (suffice to mention here the crisis of the concept of "true interpretation," and what this can mean for the entire theoretical edifice). As a consequence, "classical technique" became more and more a ritualized activity, void of its original meanings, transmitted by the various generations of analysts almost in a sacred way (Cremerius, 1984, 1986, 1987, 1989, 1996; Kernberg, 1986, 1996; Fossi, 1984, 1989; Holt, 1989, 1990). Poignant, in this regard, is Grünbaum's (1984) critique of the technique of hermeneutic analysts as inconsistent. They continue to employ free associations, forgetting that this method was originally developed by Freud in order to *discover* something, to reconstruct a missing link that had been in the patient, and not in order to *construct* a new meaning or to have a dialogue to make up a story. For that purpose a normal dialogue without free associations (and, for that matter, without the couch) would be much more appropriate.

The motivations, then, that pushed the psychoanalytic movement in the direction of identifying psychoanalysis more and more simply with a technique now seem clearer: Given the crisis and the fragmentation of the theory, the natural tendency was to find the "common ground" in the

technique. In addition, the latter seemed more objectifiable, more concrete, and more functional for the pressing needs of a well-organized professional group of the international scope.

The gap between theory and technique manifested itself very soon in the history of the psychoanalytic movement. Already in the '20s Freud himself was a "dissident" towards the Institutes of London and Berlin that had become more and more "Freudian" (Cremerius, 1982). In the following decades this situation acquired greater and greater proportions up to the point where it was possible to see, within the technical level, the use of concepts (such as anonymity, abstinence, free associations, etc.) that were detached from the theoretical context in which they had been formulated, as if they could legitimize themselves autonomously. Technique was monstrously raised to the rank of theory**.** There were no more precise theoretical principles, but only behavioral rules that had to be followed within an established setting, as they had been taught by supervisors and training analysts, who in turn had learned them from the previous generation. It is not a coincidence, as Galli (1986, 1988a, 1988b, 1990) often noticed, that gradually some previously strong concepts of the psychoanalytic method (such as insight or interpretation) began to be put aside and became weak, while other concepts, previously peripheral to the method and close to the technical pole (such as setting or therapeutic environment) were raised to the rank of strong concepts.

Historically, this problem grew wider when, more and more, an increasing number of therapists began experimentally altering the psychoanalytic method with patients that were not classical neurotics. As it is well known, one of the pioneers who tried psychoanalysis with previously excluded patients was Sullivan, who worked with psychotics. His usage was not to talk about psychoanalysis but about psychotherapy (if not *tout court* psychiatry -- see for example the titles of all Sullivan's books and of many of his articles). The same occurred for brief therapies, which were called "psychoanalytically oriented" or "short-term dynamic psychotherapies" and not "brief psychoanalyses" (for a detailed critique of the radical theory of short-term dynamic psychotherapy, see Migone, 1985, 1995 Ch. 3). In the same way, for group therapies there was a trend not to talk about "group psychoanalysis": in some anecdotes, when psychoanalytic institutions were confronted with proposals to found associations for group psychoanalysis, they imposed their *veto* on the usage of the name psychoanalysis, but suggested simply group psychotherapy or "group-analysis" (see Galli, 1989, p. 216). We could go on with such examples. Curiously, however, this problem did not arise when psychoanalysis was proposed for children, when the term "child psychoanalysis" was used, and not "child psychotherapy." This seems paradoxical, considering the great differences between the technique for adults and that for children, in which, for example, play technique modifies radically the classical rules. No doubt the authoritative presence of Freud's daughter in this field had some influence. We should, however, consider a sociological hypothesis, that it was a minor danger to the identity of the discipline to keep the term psychoanalysis for patients of a different age range where the competition with other therapies was less important, while for adult therapies the problem of the threat to identity (especially at the market level) was higher.

Parallel to the "widening scope" (Leo Stone, 1954) of psychoanalysis, the studies on Ego Psychology (Hartmann, 1937, 1964; Hartmann, Kris & Loewenstein, 1964) developed, whose beginnings can be traced, even before Anna Freud's (1936) contribution on defenses, to Freud's (1923b) introduction of the structural theory, the tripartite model of ego/id/superego which marked the long way that transformed psychoanalysis from Id Psychology to Ego Psychology (while, according to some contemporary authors, today we might be in the phase of Self Psychology). According to the conception of "Id Psychology" (a term that is incorrect because the concept of id belongs to the structural theory, being inseparable from the concept of ego), the therapeutic work consisted essentially in rendering conscious the unconscious, while with the introduction of the structural model naturally concepts such as ego, defenses, adaptation, and development gained more importance. This new approach necessarily implied a different psychoanalytic technique, in which a modification of the classical technique was legitimized starting from these theoretical changes. One no longer selected patients to suit the technique, but it was the technique that had to be "selected" to

suit the patient, taking then the latter (the patient), not the former (the technique), as the independent variable.

We can say that the literature on the ego, on the defenses, and on the developmental point of view formed the conceptual basis of what later became the debate on "psychoanalytic psychotherapy" and on the differences between psychoanalysis and psychotherapy, i.e., on the possibility of changing the standard technique according to the patient's ego structure. This point is important, if we think that within psychoanalytic schools outside of Ego Psychology, such as the Kleinian school, the standard technique is employed with every patient, even severely disturbed psychotics, and there has never been any room in the Kleinian literature for the debate on psychotherapy. It is for this reason that Gill, in his characteristically provocative way, used to say that clinically he felt close to the Kleinians.

Conclusions

With his detailed theoretical revision, Gill was able to show that the traditional difference between classical psychoanalysis and psychoanalytic psychotherapy had little meaning. Of course, a once a week therapy can be a different experience from a four times a week therapy, but it is also true that a four times a week therapy of one patient can be very different from a four times a week therapy of another patient, and that a once a week therapy can be different from a four times a week therapy for reasons that are the opposite from what one may believe. The crucial factor is how the patient experiences the frequency of sessions.

The seeds of this conceptualization were already present in the development of Ego Psychology, a school in which Gill participated actively for awhile. When another conceptualization of the psychic apparatus and of the concept of defense was put at the center, naturally the theoretical difference between the two modalities collapsed. In other words, psychoanalysis and psychotherapy, from the theoretical point of view, gradually began to overlap already back in the '30s and '40s, and the discussion about their difference continued for many years probably because of the reasons discussed above. As far as terminology is concerned, Gill (1984, p. 175) tentatively chose to call -- in my opinion reductively -- his revised technique "psychoanalytic therapy," in order to differentiate it from the traditional usage of the term psychoanalysis, which so often, unfortunately, is linked with extrinsic criteria such as four or five weekly sessions and the use of the couch. A definition based only on extrinsic criteria is a poor definition because it doesn't shed light on the essence of the analytic process, which could be present in once a week therapies and, for that matter, could be absent in an "orthodox" psychoanalysis.

As we know, in his latest revision Gill espoused a relational or "socio-constructivistic" paradigm (Hoffman, 1991, 1998) of the analytic situation, and this new perspective is what allowed him to put forth his critique of the theory of technique. Still, a problem seems to me not yet resolved: why was Gill able to come to these theoretical conclusions only so late in his life, i.e., in the late '70s and early '80s? The bulk of the theoretical advances to which Gill alluded had already been made by the analysts of the interpersonal tradition early in the century, even starting in the '20s. His adoption of a relational paradigm added his lucid and detailed theorizing to the pioneering intuitions of Sullivan, Fromm-Reichmann, and other interpersonal analysts. They had anticipated the same ideas and had believed in a total overlapping not only between psychoanalysis and psychotherapy, but also between psychoanalysis and psychiatry itself. Why did Gill, like many others of his colleagues in the course of this century, remained for so many years loyal to the "classical" tradition and unable to see its limits?

Personally, for a long time I did not succeed in finding a convincing answer to this question. It seemed to me reductionistic to resort to such explanations as fear of going against authority, threat to professional identity, or the enormous economic advantages of a profession rigidly characterized by standards such as four sessions per week. Even if these factors may have played a role in some analysts, surely many American psychoanalytic theorists did not make their living solely on their professional practice, and had enough independence of thought not to be exposed to the overt or subtle pressures of organized psychoanalysis. This was true especially for a person like Gill, who,

as noted earlier, was never afraid to expose his ideas and to change them if necessary. Furthermore, both in USA and in Europe, many analysts who at the political level had espoused progressive or radical positions remained conservative at the theoretical level, thus showing some form of cultural submission. Also, personal life circumstances (such Gill's analysis with Samuel Lipton, who we know played an important role) could have had an influence, but somehow they do not seem enough to me. How, then, can we explain such a delay that there has been, and still persists, in wide sectors of the psychoanalytic movement? This delay, incidentally, could be related to the well known isolation of psychoanalysis, all along this century, from the advances of other disciplines, especially philosophy of science and neurobiology. In recent years, psychoanalysis has been trying to fill the gap and to update itself in various areas of the discipline.

Being unable to find a satisfactory answer to this apparently insoluble question, I was finally able to ask Merton directly and discuss this problem with him. The reason why he made his theoretical revision so late -- Merton told me -- probably was what he called the "objectifying" culture, the "one-person psychology" derived from an outdated (and, in my opinion, misunderstood) medical model that so strongly influenced American psychoanalysis, and that made it very difficult for many analysts of his generation to make such a theoretical revision.

Although I find this explanation interesting, I still feel that this enigma is not totally resolved. Maybe one day some historians or sociologists of science will be able to shed more light on this question. In the meantime, I think we could leave this problem without a definitive answer and open to further analysis. This attitude of critical analysis and uncertainty towards the "truth" is probably what Merton would have liked.

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