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NOTES ON TRANSFERENCE: UNIVERSAL PHENOMENON AND HARDEST PART OF ANALYSIS

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EDITOR'S NOTE

For several reasons, this paper claims a special place in the classical psychoanalytic literature on transference. First, Bird offers a sensitive résumé and evaluation of the difficulties experienced by Freud in the development of this concept. Second, there is in the paper some recognition of the extent to which the analyst participates in the patient's transference neurosis, a recognition which suggests Bird's growing awareness of interactional considerations. This approach affords him a number of additional insights even though his paper stops short of a definitive exploration in this area. Third, the paper attempts to illuminate the transference neurosis, a concept first defined by Freud (1914) and discussed subsequently by a number of analysts.

Fourth, and perhaps most important, Bird quite seriously and carefully investigates the interaction between reality and transference. He is one of the first classical psychoanalysts to discuss in any detail the realistic consequences for the analyst of transference-based expressions from the analysand, including the patient's actual efforts to harm the therapist and the possibility that such destructive endeavors may be a response to countertransference-based, destructive interventions. This particular subject had earlier been studied rather extensively by Searles (1965), but his work has had little apparent influence on the writings of classical analysts; similarly, these important comments by Bird have not as yet gained the recognition they deserve.

As an introduction I would like to make a few general remarks about transference as I see it. Transference, in my view, is a very special mental quality that has never been satisfactorily explained. I am not satisfied, for instance, either with what has been written about it or with its use in analysis. To me, our knowledge seems slight, and our use limited. This view, admittedly extreme, is possible only because transference is such a very remarkable phenomenon, with a great and largely undeveloped potential. I am particularly taken with the as yet unexplored idea that transference is a universal mental function which may well be the basis of all human relationships. I even suspect it of being one of the mind's main agencies for giving birth to new ideas, and new life to old ones. In these several respects, transference would seem to me to assume characteristics of a major ego function.

I tend to go along with those who consider transference unique as it occurs in the analytic situation, and with those who hold that the analysis and

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resolution of a transference neurosis is the only avenue to the farthest reaches of the mind. It is also my belief that transference, in one form or another, is always present, active, and significant in the analytic situation. From this it should follow that rarely is there a need to give up on the transference or to doubt that everything that goes on in analysis has a transference meaning. I would also be inclined to agree with those, perhaps few in number, who harbor the idea that analysts themselves regularly develop transference reactions to their patients, including periods of transference neurosis, and that these transference reactions play an essential role in the analytic process.

Finally, I want to point out that this paper is not a comprehensive study of transference. Nor is it a review paper, for, with the exception of a few references to some of Freud's writings, there is little or no mention of what has been written on the subject by others. As to how transference works, it seems likely there are more questions than answers. Therefore, I hope it will be understood that what I say is for question-raising, and anything sounding like an answer should be especially questioned.

Some Views on Freud and Transference

As a prefatory remark about Freud and transference, the observation can be offered that Freud wrote only briefly about transference and did so, in the main, before 1917. Another observation which can rarely be made about Freud's works, and which everyone may not agree with, is that, with one or two exceptions, what he did write on transference did not reach the high level of analytical thought which has come to be regarded as standard for him. Some indication of what his contributions consist of is given by the editors of the Standard Edition, who list them in several places. One of the longer lists, in a footnote on page 431 of Volume 16, includes six references: "Studies on Hysteria" with Breuer (1895), the Dora paper (1905), "The Dynamics of Transference" (1912), "Observations on Transference-Love" (1915), the chapter on transference in the Introductory Lectures (1917), and "Analysis Terminable and Interminable" (1937). Although the editors in no sense suggest that these six papers include everything Freud wrote on the subject, it does seem evident that, considering the essential importance of transference to analysis, he wrote little. Moreover, the three papers in which transference is the specific theme, "The Dynamics of Transference," "Transference-Love," and the transference chapter in the Introductory Lectures, come across as perhaps his least significant contributions.

Freud's first direct mention of transference occurs in "Studies on Hysteria" (1895). His first significant reference to it, however, did not appear until five years later when, in a letter to Fliess on April 16, 1900, he said (Freud, 1887–1902) he was "beginning to see that the apparent endlessness of the treatment is something of an inherent feature and is connected with the transference" (p. 317). In a footnote to this letter the editors state that, "This is the first insight into the role of transference in psycho-analytic therapy."

Despite these early references, it seems correct to say that yet another five years was to go by before the phenomenon of transference was actually introduced. Even then the introduction was far from prominent, for it was tacked on like an afterthought as a four-page portion of a postscript to what was perhaps Freud's most fascinating case history to date, the case of Dora (1905, pp. 116–120).

Using data from Dora's three-month-long, unexpectedly terminated analysis, and especially from her dramatic transference reactions which had taken him quite unawares, Freud now gave to transference its first distinct psychological entity and for the first time indicated its essential role in the analytic process. His account, although in general more than adequate—in fact elegant and remarkably "finished"—was brief, almost laconic, and perhaps not an entirely worthy introduction to such a truly great discovery. What was uniquely great was his recognizing the usefulness of transference. In his analysis of Dora he had noted not only that transference feelings existed and were powerful, but, much to his dismay, he had realized what a serious, perhaps even insurmountable, obstacle they could be. Then, in what seems like a creative leap, Freud made the almost unbelievable discovery that transference was in fact the key to analysis, that by properly taking the patient's transference into account, an entirely new, essential, and immensely effective heuristic and therapeutic force was added to the analytic method.

The impact on analysis of this startling discovery was actually much greater and much more significant than most people seem to appreciate. Although the role of transference as the *sine qua non* of analysis was and is widely accepted, and was so stated by Freud from the first, it has almost never been acclaimed for having brought about an entire change in the nature of analysis. The introduction of free association to analysis, a much lesser

change, received and still receives much more recognition.

One of the reasons for the relatively unheralded entry of transference into analysis may have been the circumstances of its discovery. Although Freud's new ideas were recorded as if they arose as a sudden inspiration during the Dora analysis, they may in fact have developed somewhat later. In the paper's prefatory remarks, for instance, Freud (p. 13) said he had not discussed transference with Dora at all, and in the postscript (p. 119), he said he had been unaware of her transference feelings. Also pointing to a later discovery date is the extraordinary delay in the paper's publication. According to the editors' Note (p. 4), the paper had been completed and accepted for publication by late January 1901, but this date was then actually set back more than four and a half years until October 1905. The editors add: "We have no information as to how it happened that Freud . . . deferred publication." In my opinion, his reason may have been that only during those four and a half years, as a consequence of his own self-analysis, did he come to an understanding of the significance of the transference. Only then may it have been possible for him to turn again to the Dora case, to apply to it what he had learned in himself, to write his beautiful essay as part of the postscript, and at last to release the paper for publication.

Freud's self-analysis has been considered from many angles, but not significantly, as far as I know, from the standpoint of transference. Opponents of the idea that there is such a thing as definitive self-analysis, some of whom say it is impossible, generally object on the grounds that without an analyst there can be no transference neurosis. Freud clearly demonstrated, I think, the situation that may be necessary to fill this need: self-analysis may require at least a half-way satisfactory transference object. In Freud's case, the main transference object at this time seems to have been Fliess, who filled the role rather well. As with any analyst, his "real" impact on Freud was slight. He was essentially a neutral figure, relatively anonymous and physically separate. All of this, plus Fliess's own reciprocal transference reactions, made it possible for Freud to endow Fliess with whatever qualities and whatever feelings were essential to the development of Freud's transference, and it should be added, his transference neurosis. In the end, of course, the transference was in part resolved. Freud's eventual awakening to the realization of the presence within him of such strange and powerful psychological forces must have come as a stupendous disillusionment, directed not only toward Fliess but toward himself, and yet his subsequent working out of some of these transference attachments must have been both an intellectual triumph and an immensely healing and releasing process.

It was this event, the development, the discovery, and then the resolution within himself of the complexities of the transference neurosis, that constituted the actual center of his self-analysis, and it was this event that was the beginning of analysis as we know it.

In the years following this revolutionary discovery, the central role of transference in analysis gained remarkably wide acceptance, and it has easily held this central position ever since. What the substance of this central position consists of, however, is something of a mystery, for, in my opinion, nothing about analysis is less well known than how individual analysts actually use transference in their day-to-day work with patients. At a guess, because each analyst's concept of transference derives variably but significantly from his own inner experience, transference probably means many different things to different analysts.

In the same individually determined way, even Freud's own pupils must have differed on this issue, not only from him but from each other. Although some of their differences may have been slight, others may have contributed significantly to later analytic developments. A question could be raised, for instance, whether differences in handling the transference which at first were the property of one analyst gradually developed into formal clinical methods used by many, and whether these clinical methods, after having been conceptualized, served as the beginning of various divergent schools of analysis. Such an occurrence, consistent with my belief that analytic ideas do arise in this way, primarily out of transference experiences in the analytic situation, would lead to the question whether the history of the ideological differences among various schools might be found to be more consistently traceable to idiosyncratic differences in what was actually said and done in response to transference reactions than to any other factor. Whatever the case, many differences and divergencies did occur among the early analysts, and all of them, I suspect, had to do in some major way with differences in the handling of the transference.

Strangely, Freud himself seems to have taken little part in influencing this rapid and divergent period of growth. Usually accused of being too dominating in such matters, Freud seems to have done just the opposite during the development of this most critical aspect of analysis, the process itself, and, for reasons unknown, detached himself from it.

What was needed, one might be inclined to say,

was not leadership in the form of domination, but leadership in trying to provide what was lacking, and to me is still lacking, namely, an analytical rationale for transference phenomena. The question must be asked, of course, whether in fact this would have been a good thing at that particular time in psychoanalytic history. Perhaps not. The exercise of closure, which Freud's structuring might have amounted to, although adding to understanding and stability at a certain theoretical level, could at another level, as such closures have often done, have placed many obstacles in the way of further analytical developments. Thus, his leaving the matter of transference wide open, even though it led to confusion and uncertainty, may have been just as well.

In many ways the closest Freud ever came to establishing a formal analytical rationale for transference was his first attempt, in the postscript to the case of hysteria (1905). These few pages are, in my opinion, among the most important of all Freud's writings, outweighing by far the paper to which they are appended. Yet, I suspect, the case of Dora has always been taught as an entity rather than, as I would have it, ancillary to the essay on transference. In that essay Freud was clear: his ideas revealed tremendous insights and promised more to come. Imagine his being able to say at this early time that during analysis no new symptoms are formed, and that, instead, the powers of the neurosis are occupied in creating a new edition of the same disease. Just think of the analytical implications of his saying that this "new edition" consists of a special class of mental structures, for the most part unconscious, having the peculiar characteristic of being able to replace earlier persons with the person of the analyst, and in this fashion applying all components of the original neurosis to the person of the analyst at the present time. Surely as profound a statement as any he ever made.

Then he goes on to say that there is no way to avoid transference, that this "latest creation of the disease must be combated like all the earlier ones" (p. 116), and that, although this is by far the hardest part of analysis, only after the transference has been resolved can a patient arrive at a sense of conviction of the validity of the connections which have been structured during analysis.

He concludes by saying: "In psycho-analysis . . . all the patient's tendencies, including hostile ones, are aroused; they are then turned to account for the purposes of the analysis by being made conscious, and in this way the transference is constantly being destroyed. Transference, which seems ordained to be the greatest obstacle to psycho-analysis, becomes its most powerful ally . . ." (p. 117).

These remarkable observations, written in declarative style, with no hint of vacillation, vagueness, or ambivalence, convey a sense of deep conviction that could arise, one feels, only from Freud's own hard-won inner experience. Nowhere is there a suggestion that transference is a mere technical matter. Far from it. Here, in these few lines, Freud announces that he has come upon a new and exciting kind of mental function, or, as I believe, a new and exciting kind of ego function.

Very quickly, however, Freud's conviction seems to have failed him. Nothing he wrote afterward about transference was at this level, and most of his later references were a retreat from it. For instance, he never did develop the promising idea that the mind constantly creates new editions of the original neurosis and includes in them an ever-changing series of persons. Instead, he tended to become less specific, even referring to transference at times in broad terms as if it were no more than rapport between patient and analyst, or as if it were an interpersonal or psychosocial relationship, concepts which, of course, a great many analysts have since adopted, but which were not part of Freud's original ideas.

Perhaps his most persistent deviation was an on-and-off tendency to regard transference merely as a technical matter, often writing of it as an asset to analysis when positive and a liability when negative.

Significantly, because it indicated that an active struggle was still going on within him, Freud occasionally expressed once again, even though briefly, his earlier insights, particularly his idea that transference is an essential though unexplored part of mental life. An example of this appears in his otherwise quite indifferent account of transference in "An Autobiographical Study" (1925). Transference, he says, "is a universal phenomenon of the human mind . . . and in fact dominates the whole of each person's relations to his human environment" (p. 42). In these few words Freud again made the point, and in declarative fashion, that transference is a mental structure of the greatest magnitude. But he never really followed it up.

Rather extensive evidence of his departure from the original concept and of his continuing struggle with that concept is seen most clearly, I believe, in one of his last and one of his greatest works, "Analysis Terminable and Interminable" (1937). To my narrowly focused eyes, "Analysis Terminable and Interminable" is much more than a courageous, brilliant, and pessimistic appraisal of the difficulties and limitations of analysis. Although transference is little mentioned in the paper, a great deal about it comes

through, some quite directly, some by easy inference. When looked at in this way, two themes stand out: Freud's personal frustrations with the enigmas of transference, and his tacit placing of transference in the very center of success and failure in analysis, both as a therapy and as a developing science. What also comes through, to me, is the perplexing realization of how far Freud had, by now, seemingly moved away from his original concepts. Or had he?

It is utterly perplexing, for instance, in reading his otherwise brilliant discussion of the ending of an analysis, to find that he makes no mention of what he had said so compellingly in this connection 30 years earlier: that for analysis to be effective, there must be a transference neurosis and that this neurosis must be resolved in the analytic situation.

His 1937 discussion of the negative side of transference is equally perplexing. Referring (pp. 221–222) to what is assumed to be Ferenczi's late-developing antagonism and to Ferenczi's rebuke that the negative transference should have been analyzed, Freud explains the situation rather lamely, it seems to me, by saying that even if such negative feelings had been detected in latent form, it was doubtful that the analyst had the power to activate them short of some unfriendly piece of behavior in reality on the analyst's part. Further on (p. 223), he also raises the question whether it is wise to stir up a pathogenic conflict which is not betraying itself. Contrast these views with his 1905 statement: "In psycho-analysis . . . all the patient's tendencies, including hostile ones, are aroused . . ." And in the next sentence, "Transference, which seems ordained to be the greatest obstacle to psycho-analysis, becomes its most powerful ally . . ." (p. 117). Here, it seems to me, Freud is saying that transference is precisely the power which is able to arouse "all the patient's tendencies," even latent ones, even ones which do not betray themselves, and that this arousal is not a matter of being wise or unwise but of being essential.

Other evidence of his strange and at least partial removal of transference from analysis appears where he says: ". . . we can only achieve our therapeutic purpose by increasing the power of analysis to come to the assistance of the ego. Hypnotic influence seemed to be an excellent instrument . . . but the reasons for our having to abandon it are well known. No substitute for hypnosis has yet been found" (1937, p. 230). As I read it, this statement seems to be a paradox. What about transference? Is not transference this very power, the power Freud now says we have not yet found? Indeed, what better definition of transference could there be than to say, using Freud's words, that when properly taken into account, transference increases, in the most exquisite way, "the power of analysis to come to the assistance of the ego"? Is this not precisely what transference does? Is this not what Freud had earlier said its function was?

Again, toward the end of the paper (p. 247), in an otherwise masterful discussion of difficulties contributed by the individuality of the analyst, he fails almost completely to direct these difficulties to their most obvious source, the countertransference.

This fluid, inconstant, and ever-shifting state of Freud's views on transference may be explained, I believe, by the fact that for so much of his life he was himself deeply engaged in transference situations with many different persons. It should not be forgotten that Freud's discoveries were made primarily on himself. His primary sources were his own transference experiences. This, I suspect, was the principal executive agent of Freud's genius: his great capacity to become deeply involved in and to resolve myriad transference feelings, and then to derive from such experiences the basic principles governing them. One has to wonder, of course, whether this creative process was in any way unique with Freud. Perhaps not. Perhaps all great discoveries, or at least all "creative leaps," are made, via the transference, within the discoverer's own person. Perhaps all monumental breachings of the confines of the known depend not only upon the basic givens of genius but upon a capacity for greatly heightened cathexis of certain ego apparatuses, a development which, in turn, may require the kind of power generated by the ego only in a transference situation.

In this connection I would like to mention Isaac Newton, whose revolutionary discoveries were so far-reaching and so immense as to place him among the greatest geniuses of all time. My sketchy knowledge suggests that the circumstances of Newton's staggering creative breakthroughs might be profitably studied from the standpoint of transference and of transference's possible role in hypercathecting Newton's tremendously rich and expanded inner resources. The circumstances I refer to were unusual. In his third and fourth years at Cambridge as a bright but not remarkable student, he worked with and was encouraged by a gifted mathematician who was one of the few who recognized Newton as being something special. In 1665 the Great Plague forced the University to close for 18 months, and the students were dispersed. Newton went to his mother's house in the small village of his birth and he stayed there almost the entire time, completely cut off from all colleagues and practically isolated from

the world. There, according to Andrade (1954), at the age of 23 and 24, alone with his mother and his thoughts, "the young Newton mastered the basic laws of mechanics; convinced himself that they applied to heavenly as well as to earthly bodies and discovered the fundamental law of gravitational attraction: invented the methods of the infinitesimal calculus: and was well on his way to his great optical discoveries" (p. 50). Other developments in Newton's long life might also be studied from the point of view of transference and creative productivity. Of particular interest are the intense and often stormy relations with his colleagues and the great impact these changing friendships and enmities may have had on his creativity.

In the case of Freud, the perplexing attitudes he took toward transference, his vacillations, contradictions, and omissions, his great insight and his apparent obtuseness, may all have reflected changes and phases of what was then going on in him with respect to the level and quality of his transference attachments to people, and his attempts to resolve and understand those attachments. In this respect, it might be scientifically rewarding to study Freud's personal data, particularly his letters, for evidence of transference reactions in his relations with various persons, and, taking the study a step further, for evidence of causal connections between the content or nature of these relationships and the particular analytic developments he was working on at the time.

Although the constant activity of Freud's great transference capacity was essential to his genius, it may also have been the very thing that prevented him from giving to transference itself the highly cathected and creative attention he gave, with such success, to many other subjects; and, because he did not, transference never attained a cohesive and stable analytic entity.

TRANSFERENCE: THE HARDEST PART OF ANALYSIS

Without being entirely aware of doing so, most of us have tended to follow and to extend Freud's somewhat meandering transference path. And, like Freud, we have moved steadily away from his original concepts.

How far we may have moved is uncertain, but a milestone of sorts, indicating how far we may have gone by 1952, is recorded in Orr's paper "Transference and Countertransference: A Historical Survey" (1954). Orr sums it up this way: "Most, if not all, recent psychoanalytic articles concerned with technique agree that handling of the transference continues to be the *sine qua non* of the treatment." But things were changing. "Increasingly," Orr says, ". . . 'handling' is taken to mean 'manipulation' in one form or another, and with the intensity of the transference or the depth of the therapeutic regression the points at issue." And although Orr could say that "The development, interpretation and resolution of the transference neurosis in the analytic relationship is still the hallmark of psychoanalysis for perhaps a majority of analysts today," he added the qualification that "for a considerable minority this is by no means the case, or at least not without considerable attenuation and modification" (p. 646).

By 1952, therefore, it seems possible that a great many analysts may have already given up on rigorous concepts of the transference neurosis and on a rigorous handling of it. The extent of this giving up, I think, is not surprising. Freud himself seems to have anticipated it even from the beginning, for in his 1905 paper, on page 116, he says: "This [the transference] happens . . . to be by far the hardest part of the whole task." Then he adds this most remarkable sentence: "It is easy to learn how to interpret dreams, to extract from the patient's associations his unconscious thoughts and memories and to practice similar explanatory arts . . ." This short statement, I believe, was intended to be a warning: the transference, Freud implies, is so hard to work with that we will be tempted to attenuate, modify, or even omit it. But if we do this, the warning goes on, analysis will be reduced to an explanatory art.[1]

The general sense of this warning seems clear, but Freud's stated reason why transference is so hard to work with scarcely matches the seriousness of the warning. "Transference," he says, ". . . has to be detected almost without assistance and with only the slightest clues . . ." Is this all there is to it? Or is Freud's warning in response to yet another reason? Is he saying, as I think likely, how very hard it is *on the analyst* to work effectively with the transference neurosis? We forget sometimes that a neurosis is based upon conflict and that what is specific about a transference neurosis is the active involvement of the analyst in the central crunch of this conflict. The wear and tear of this abrasive experience can be considerable and must surely be one of the major reasons some analysts pull away from the transference

1. Ten years later in "Transference-Love" (1915) Freud again makes the same point: ". . . the only really serious difficulties he [the analyst] has to meet lie in the management of the transference" (p. 159).

neurosis and away from analysis itself. Yet if analysis is to proceed successfully, if a transference neurosis is to develop and be analyzed, the analyst cannot pull away, cannot merely sit back, observe, interpret, and "practise similar explanatory arts." In addition, via the influence of the analytic situation, the patient must be enabled to include the analyst in his neurosis, or, as it were, to share his neurosis with the analyst. Only in this way, it seems, can the patient effectively reawaken the early stages of his neurosis, only in this way can its latent parts and forces be rendered sufficiently identifiable and functional to be available for analysis.

Accomplishing this is not easy. By the time a patient comes to analysis, his neurosis has moved a long way from where it began. Not only will it have gone through many changes and phases but, in all likelihood, it will have established itself as a rather fixed, walled-off, and independent institution. As a consequence, the drives and defenses originally involved in creating the neurosis may now act mainly within the confines of this neurotic institution and may no longer respond readily to extraneurotic influences. The only force powerful enough to bring the constituents of this encapsulated structure back into the main stream of the patient's mental functioning seems to be the transference neurosis. Bringing this about, calling as it does for the active inclusion of the analyst in the patient's neurosis, is probably, as perhaps Freud meant, the hardest part of analysis; but, as he also may have meant, it is what analysis is all about, it is what the analytic situation is set up to do, and it is why definitive analytic work leans so heavily upon the analyst's skilled fortitude.

Admittedly, many potential dangers attend the analyst's becoming involved in the patient's neurosis. The commonest would seem to be the analyst's unawareness of his own reciprocal transference reactions. A more subtle danger threatens when the analyst, although understanding his own transference, gains his insights so exclusively from this inner source that he pays little or no attention to the possible inapplicability of these insights to the patient's current transference developments. Although these and other problems with the analyst's transference involvement are obviously serious, the alternatives are not particularly inviting, for I have yet been unable to find evidence that a "safe" analysis, in which such dangers do not arise, has much chance of reaching the patient where he needs to be reached.

In view of how hard the whole thing is, can it be too speculative to believe that Freud's 1905 prediction may have come true, that, as an act of self-defense, handling of the transference has been steadily attenuated until analysis has finally become, in a great many hands at least, an explanatory art?

Transference and Transference Neurosis

Although things may not have gone quite this far, I do believe they have reached a point where most analysts nowadays work only with transference feelings. They either ignore the transference neurosis or believe, as anyone has a right to, that there are no significant differences between a transference neurosis and other transference reactions, that transference is simply transference. For myself, I believe just the opposite: there are differences, and they are significant. And I feel sure that if we could only learn more, a great deal more, about both transference and transference neurosis, life would be easier for the analyst and analysis would be better for the patient.

For me, the transference neurosis is essential to the analytic situation. Not the whole of it by any means, or even the most of it, but essential. Sharing a place with the transference neurosis are at least two other kinds of relationships: one based on ordinary transference feelings and the other on reality considerations—those of a patient to his doctor. These three share the time, as it were. All are important, all overlap, but each is specific. Each comes and goes, appearing and disappearing in response to a seemingly endless number of influences. The easiest relationship to maintain and to work with, and the one most generally used in analysis, is characterized by the patient's almost constant attribution of transference feelings to the person of the analyst. The most difficult relationship to establish and work with, the one most easily lost hold of, the one that is essential if definitive analytic work is to be done, is the transference neurosis. The one most likely to interfere with the others, and often the hardest to exclude, is the reality relationship.

In my view, as I have said, a transference neurosis differs fundamentally from those transference feelings which a patient experiences and expresses during much of the analytic time. When I think of transference, I think of feelings, of reactions, and of a repetition of past events; but when I think of transference neurosis, I think literally of a neurosis. A transference neurosis is merely a new edition of the patient's original neurosis, but with me in it. This new edition is created, for reasons I wish I knew

more about and in ways that are quite perplexing, by the patient's shifting certain elements *of his neurosis* onto me. In this way he replaces *in his neurosis* mental representations of a past person, say his father, with mental representations of me. Although this maneuver would make it seem that the patient now regards me as his father, the actual situation is somewhat different. Because the maneuver is basically intrapsychic and deals with specific elements of his neurosis, I come to represent, not his father, but an aspect of his neurosis which, although contributed to by early, primarily oedipal experiences with his father, is now an intrapsychic structure of its own.

As I see it, this is quite different from what happens in a simple transference reaction. In a transference reaction, the patient displaces certain cathexes from early memories of his father to me, as if in the present. This is transference in its universal sense; it is the means of displacing feelings and attachments from one object to another, and of repeating the past in the present. In this process the two separate identities—the father and I—are merged, but the patient's own identity and my identity remain clear and separate. This is not the case with a transference neurosis. There the patient includes me somehow in the structure, or part structure, of his neurosis. As a result of this process, the identity difference between him and me is lost, and for the moment and for the particular area affected by the transference neurosis, I come to represent *the patient himself.* More specifically, I come to represent some complex of the patient's neurosis or some element of his ego, superego, drives, defenses, etc., which has become part of his neurosis. I do not, however, represent as such actual persons from the past, except in the form in which they have been incorporated into the patient's neurotic organization.

May I present an example of what I mean?

For the first two years of a young man's analysis, he became increasingly affected by one of his most crippling characteristics: an inability to get things done. Although generally stiff, rigid, and inhibited, there was more than this to his inability to act. Faced with a situation in which he should take specific action, he would balk and withhold such action in a procrastinating, stubborn, helpless, and often harmful way. Historically, throughout the patient's childhood, this characteristic led his mother into endless nagging at him to get things done, and when nagging did no good, in her frustration she wound up doing them for him. It was not surprising, I think, that in analysis I came to play the same role and that eventually my interpretations came to be regarded either as nagging or as my doing his work for him. Although the patient easily recognized the similarity of this to what had gone on in childhood, disappointingly he gained nothing analytically useful from it.

One reason he did not, which took me quite a while to discover, was that the *act* of interpretation itself had become deeply involved in the transference. With this change, the *content* lost its importance, and instead he reacted to almost everything I said, interpretation or not, as if I were nagging him or doing something for him. But there was more to it than this. Upon realizing that such a shift had taken place, I became much less interpretive, in fact much more quiet all around. Surprisingly, the patient responded to my substantial quietness as if it did not exist. He went right on talking about one situation after the other in which he had failed to act, and went right on feeling that I was nagging and acting for him, although now I rarely even commented on what he reported.

This peculiar behavior, I suspected, indicated that still another shift had occurred. This was no longer a simple transference reaction, and I no longer represented a mother-object. This was a transference neurosis. In it his representation of me, now internalized, stood for certain elements of his neurosis, particularly, it seemed, elements of his ego and superego. In effect, the conflict was now remarkably self-contained; he was now nagging himself and doing things for himself. Upon noting this shift, I did my best to explain it to the patient and to speculate on what was revealed by it. What seemed most apparent was that in this way he was revealing a significant capacity to take over his own affairs and to be effective in getting things done, and that indeed the very strength of this drive might be a central factor causing his ego in his neurosis to react against it.

The patient responded to this formulation with a sense of its aptness. He began to appreciate the internal, personal, and conflicting nature of his neurosis and to accept some responsibility himself for his troublesome behavior. He also recalled periods of time when he had in fact been active and aggressive and had had no difficulty getting things done.

Following these inner discoveries, but only then, we were able to explore with meaning some of the origins of his problems as they concerned his relations with his mother.

In this particular instance, interpreting the transference neurosis in this specific way made a significant difference, a difference which effectively made this phase of the patient's analysis more than "an explanatory art." Very often, of course, this difference may not matter. The target, after all, is

immense, and in whatever form an interpretation is made, if it is aimed generally in the right direction, it may have an impact. But when the difference does matter, as it commonly does, it may matter very much.

It is also true, of course, that the transference neurosis is not always available to work with. Being an on-and-off thing, as I believe it to be, there may be long periods when it is not in evidence. This means that the bulk of the bread-and-butter work of analysis is carried on largely in a transference relationship that is broader and less specific than a transference neurosis. Interpretations and constructions based on material evoked by these day-to-day transference reactions enable the patient's neurosis to unfold, and his character structure to come into clearer view. When the process goes further, as it may, the infantile neurosis may be retrieved from limbo and some of its vicissitudes may be traced. Doing this much is a great deal, but, much as it may seem, it will not reach all the way to the center of the patient.

This can happen, in my experience, only if the persistent and effective handling of the daily transference reactions, along with everything else it does, sets the stage for the appearance of episodes of transference neurosis. These may be short or long, clamorous or silent, but, in whatever way they appear, they will provide an opportunity to carry analysis the further step that does promise to reach the patient as nothing else can. It is this further step, however, which, because it is the hardest part of analysis, may never be taken.

Adding to whatever else makes this further step hard, are difficulties caused by transference itself: transference and transference neurosis are both subject to such serious limitations, interferences, and distortions that they may be very slow to develop, or they develop in such ways that long periods of analysis must go by before they reach a useful and workable state. Some of these interferences are iatrogenic, some seem to be a specific feature of the kind of disorder affecting the patient, and some may be inherent limitations in the phenomenon of transference itself. What I propose to do for the remainder of the paper is to comment on some of these interferences and limitations.

The Impact of Reality on Transference

"Reality" is a difficult word to use to everyone's satisfaction or even to one's own satisfaction. In this instance I use it rather arbitrarily to designate the direct, here-and-now impact of the analyst upon the patient. Reality, in this sense, contrasts with the impact the analyst has through his representation in the patient's fantasy life, neurosis, and transference. Since both kinds of impact seem always to coexist and since the former—the analyst's real impact—may be the worst enemy of the transference, the matter of their differentiation is possibly the most challenging aspect of analysis.

The analytic situation, which is set up to shut out ordinary reality intrusions, cannot and possibly should not exclude them all. In the beginning months, for instance, reality inevitably has the upper hand. The analyst, the office, the procedure, are all overwhelmingly real. Everything is strange, frightening and exciting, gratifying and frustrating. Until the patient can test it and orient himself to it, the impact of this reality is usually so great that even an ordinary useful transference relationship cannot be expected to develop.

Perhaps the most confusing aspect of this beginning period is the frequent appearance in it of what I regard as a false transference relationship. With great intensity and clarity, the patient may reveal, through transference-like references about the analyst, some of the deepest secrets not only of his neurosis but of its genesis. This pseudotransference, too good to be true, is almost sure to be nothing more than the patient's attempt to deal with the new situation: as completely as he can, he goes through, in respect to the person of the analyst, the entire spectrum of his various patterns of behavior. If, as it is easy to do, the analyst overlooks the likelihood that the patient's relationship with him at this time is real and that almost everything said about it is best related to this reality, analysis may get off to a very bad start. And if, as is even easier to do, the analyst interprets the genetic meanings of the openly exposed material, a good transference relationship may be seriously delayed and a workable transference neurosis may never appear. Even after initial reality has had time to fade, reality may continue to intrude in ways that are very hard to detect and that are very troublesome.

One of the most serious problems of analysis is the very substantial help which the patient receives directly from the analyst and the analytic situation. For many a patient, the analyst in the analytic situation is in fact the most stable, reasonable, wise, and understanding person he has ever met, and the setting in which they meet may actually be the most honest, open, direct, and regular relationship he has ever experienced. Added to this is the considerable

helpfulness to him of being able to clarify his life story, confess his guilt, express his ambitions, and explore his confusions. Further real help comes from the learning-about-life accruing from the analyst's skilled questions, observations, and interpretations. Taken altogether, the total *real* value to the patient of the analytic situation can easily be immense. The trouble with this kind of help is that if it goes on and on, it may have such a real, direct, and continuing impact upon the patient that he can never get deeply enough involved in transference situations to allow him to resolve or even to become acquainted with his most crippling internal difficulties. The trouble in a sense is that the direct nonanalytical helpfulness of the analytic situation is far too good! The trouble also is that we as analysts apparently cannot resist the seductiveness of being directly helpful, and this, when combined with the compelling assumption that helpfulness is bound to be good, permits us to credit patient improvement to "analysis" when more properly it should often be recognized as being the result of the patient's using us, and the analytic situation, as model, preceptor, and supporter in dealing practically with his immediate problems.

Gross examples of this kind of reality-caused problem are common: a neurotically inept medical student who was able to stay in school for four years and graduate only because of the literal day-by-day support he took from visits to his analyst; a man with an unstable hold on his business whose analysis became little more than a source of real support needed to keep his business intact; and a woman whose analysis was almost completely absorbed in using it to keep a teetering marriage from collapsing. In none of these patients did any significant transference relationship develop. Instead, they clung to their actual dependence upon the analyst and the analytic situation. Because this problem so often goes unrecognized, and because even when recognized it is not sufficiently dealt with, this kind of usefulness may be one of the major reasons why analysis fails.

Perhaps I should mention one more difficult-to-handle intrusion of reality into the analysis. This is the definitive and final interruption of the transference neurosis caused by the reality of termination. Here, in a sense, the situation is reversed and the intrusion is analytically desirable, since ideally the impact of the reality of impending and certain termination is used to facilitate the resolution of the transference. As with the resolution of earlier episodes of transference neurosis, this final one is brought about principally by the analyst's interpretations and reconstructions. As these take effect, the transference neurosis and, hopefully, along with it the original neurosis is resolved. This final resolution, however, which is much more comprehensive, is usually very difficult and may not come about at all without the help of the reality of termination. Accordingly, any attenuation of the ending, such as tapering off or casual or tentative stopping, should be expected to stand in the way of an effective resolution of the transference. Yet, it seems to me, this is what most commonly happens to an ending, and because of this a great many patients may lose the potentially great benefit of a thorough resolution and are forever after left suspended in the net of unresolved transference.

Yet, slurring over a rigorous termination seems understandable. As difficult as transference neurosis may be on the analyst at other times, this ending period, if rigorously carried out, simply has to be the period of his greatest emotional strain. There can surely be no more likely time for an analyst to surrender his analytic position and, responding to his own transference, become personally involved with his patient than during the process of separating from a long and self-restrained relationship. Accordingly, it may be better to slur over the ending lightly than to mishandle it in an attempt to be rigorous.

Some Special Transference Difficulties in the Case of Negative, Destructive Tendencies

Various other difficulties with transference, both in its development and in its analysis, occur, as we all know, in respect to the nature of different forms of illness, e.g., acting out, psychosis, character disorders, etc.[2] But rather than discuss particular situations such as these, I would like to consider a different kind of difficulty, one which I think casts a very dark shadow on all transference manifestations and which may therefore be a severely limiting factor in analytic work generally.

This limiting factor, which may be universal, is the apparent inability of transference to reproduce with any verity the full range of man's negative, destructive tendencies. In contrast to libidinal drives, even the mildest and commonest negative ones seem to run into a good deal of trouble finding their way into the transference, ending up at best as

2. In two papers (Bird, 1954 and 1957) I have described some of the transference difficulties met with in a specific, narcissistic form of acting out.

wishes, feelings, and fantasies, while the more robust varieties, those involving literal destructive acts, seem to stand little chance of entering the transference at all.

The question why this limitation exists is not easy to answer. One suggestion, speculative to be sure, but nonetheless seeming to be worth serious consideration, is that negative, destructive tendencies are derivatives of a "death instinct" and as such are bound, not by ordinary principles of mental functioning, but by whatever principles do govern this elusive concept. Unfortunately, I believe, little study is being devoted to clarifying this important issue. Most analysts seem to have turned their back on the death instinct and on Freud's attempts to explore it. Many of us, with some logic, explain away our disinterest on grounds that the death instinct is a biological and not a psychological concept and therefore is not within our province.

Another somewhat less logical but perhaps more significant reason for our shying away from the death instinct is that analysts seem to shy away from everything touching on violence, destruction, and death. In our developmental theory, for instance, we prefer to regard the concepts of "killing of the parents" and "sexual union with the parents" as more or less antithetical equals, each suffering much the same fate at the instance of the ego's resolution of the Oedipus complex. In this way we are able to gloss over the differences between the two concepts and to avoid facing the apparent fact that, while the ego's oedipal impact does make it possible later on in life for sexual union to be normally carried out with a substitute for the parents, it does not make it possible for killing to be carried out normally at all. That there is no norm for whatever the killing drive consists of is not an insignificant matter. Most of us, of course, try to get around this difficulty by means of the somewhat fuzzy assumption that oedipal events do convert the killing drive into a much nicer one called aggression, which we regard as normal. Our accepting this rather broad assumption makes it easy to ignore the possibility that man's tendency to kill may not be basically changed by oedipal events, and to ignore the likelihood that whatever control the ego does have over violent tendencies is somewhat tenuous. Perhaps the most surprising thing we ignore is the overwhelming evidence of how uncertain the ego's control is, viz., the tremendous outbursts of violence that surround us in our daily life.

Even our analytic language, which leans heavily on euphemisms, seems designed to ignore the reality of destruction. We tend to use words like "negative," "aggressive," and "hostile" in describing patient behavior that may have caused actual damage. Or we speak of angry feelings, murder fantasies, castration wishes, and death wishes in respect to a patient's determined attempt to cause harm. To me, this language always seems at least once removed from what we are actually dealing with, or should be dealing with.

The inappropriateness of our language came home to me one day with a patient who, as we say so nicely, liked to "castrate" men. While listening to her describe some extreme behavior of this kind, I suddenly asked myself the question, What would I call this behavior outside of analysis? The answer was easy. I would call it vicious and destructive. So I told the patient what I had been thinking. She was shocked by these terms, but she admitted that the euphemisms we usually used had made it very easy for her to ignore the literal harm caused by her behavior.

In addition to failing to recognize a patient's violent intentions and actions for what they are, analysts sometimes further obscure the situation by regularly discouraging a patient from allowing his anger to deepen to the stage where its basic violent quality is unmistakable. Some of us sense a patient's "negative feelings" or "hostility" so accurately, and draw his attention to it so quickly, that nothing but superficial use can be made of it. Or when angry accusations do come from the patient, we nip them off too prematurely and may even couch our interpretations in just the right way to clear ourselves of the accusations.

Similarly, when a patient behaves violently in his daily life and reports this to us, we tend to get uneasy, and, although we may not tell him to stop, we may directly warn him of the consequences, or in our interpretations may feel compelled to add a subtle warning or in some way to introduce a suppressive note.

Why, one has to wonder, is this suppression needed? Is it because we all sense the limited extent to which actual destructive tendencies can enter into the transference neurosis, and thus the limited extent of their analyzability? Is this incapacity perhaps what we refer to when we say, as we commonly do, in the case of incompletely analyzed patients, that certain key aspects of their neurosis simply did not arise in the transference?

Was this, I wonder, the particular concept of transference which Freud had gradually come around to and which was responsible, especially in "Analysis Terminable and Interminable," for his becoming so cautious and pessimistic about the mobilization and analysis of negative elements?

Is this why he said of Ferenczi that even if latent negative feelings could have been aroused, it would probably not have been wise to do so? Should we, therefore, if we are to follow the line Freud seems to have taken, consider discarding altogether his 1905 statement, "In psychoanalysis . . . all the patient's tendencies, including hostile ones, are aroused; they are then turned to account for the purposes of the analysis by being made conscious, and in this way the transference is constantly being destroyed" (p. 117)? Or should we, while acknowledging the known and suspected limitations, nevertheless continue to search for evidence of significant negative representation in the transference? And, in doing this, should we perhaps concern ourselves not merely with watered-down versions of violence, such as aggressiveness, negative feelings, hostility, anger, etc., but with harmful actions, particularly actions directed against the analyst?

Tentatively, I would like to suggest what may be a rather common but generally unacknowledged way in which patients attempt to cause the analyst harm, and perhaps succeed at it more often than we think. This is to convert some element of the analytic situation into a weapon to use against the analyst. That a patient does use his analysis to attack and to injure others, especially his family, is well known. That he would try to injure the analyst by the same means should not be surprising. He has to use what is available to him, and the various elements of the analytic situation are about all he has.

Most suited to be used as weapons, I should think, are a patient's resistances. Almost any aspect of analysis can be used as a resistance, and almost any resistance carried a step further can be used as a more or less effective weapon. This further step is usually taken only after analysis is well along, and consists of the patient's clinging so determinedly to some form of behavior that it threatens to engulf and destroy the entire analysis. Although the resulting stalemate is terribly frustrating to the analyst, the patient himself is often unperturbed by it, even when it means that month after month, year after year, he shows no improvement. Typically, the resistance seems more directed against the fact of analysis than against any specific part of it and may strikingly lessen or disappear if the analyst, in despair, announces a termination date.

The best known and most talked-of resistance of this kind is the so-called negative therapeutic reaction. Such reactions, of course, have been written about by many authors, and there is probably little to add to what has been said about them. Except one thing! Rarely have these very serious, very difficult, and very puzzling reactions been regarded as an attack upon the analyst. Yet, in addition to whatever else they may mean, this is precisely what many, or even most, of them may be. Why they are not readily seen in this way is something of a mystery. Every analyst, I suspect, would be willing to regard these reactions as deadly serious and as imposing severe limitations on the outcome of even the best analyses. No one, it seems, is unaware that most analytic patients at some point in their analysis, in varying degrees and in various ways, take an unconscious but implacable stand against analytic advance; that some patients regularly and silently undo each step of progress, and that some even seem absolutely bent on destroying the analysis and with it their chance for various life successes. The self-destructive aims in such behavior are usually obvious, and it may even be obvious that along with this behavior the patient is trying, often unconsciously, to hurt the analyst.

This much seems clear. But it is probably rare for us as analysts to set our euphemisms aside and to suspect these stalemates, these therapeutically negative events, of being not merely hostile fantasies, wishes, or reactions, but very real destructive acts, actual attempts to injure us, the analyst. Is this not indeed probably the only way a patient can envision actually doing us serious harm? By and large, the analyst is immune to a patient's simple slings and arrows; they are chaff which the analyst blows away without being damaged. The patient's coming late, his delayed fee payments, his withholding of material, his carping criticisms, his open anger, his demands, his teasing, his acting out, even his outright quitting, are all, at most, irritating or unpleasant. But this other thing is different. The patient's largely unconscious determination to make the analysis go nowhere, his slow, often silent, and secret undermining of the analyst's every move, is not merely irritating, it hits the analyst in the very center of his functional life, and it may cause harm.

Peculiarly, although often sensing frustration, many of us do not suspect such resistances of being a personal attack. Perhaps, if we did, we would be in a better position to deal with them. That is to say, when, as I believe happens, resistances are used to attack the analyst, it would seem to follow that, in order to discover the neurotic meaning of these resistances, we must first discover and analyze their current "transference" use. Doing this would seem to begin by confronting the patient with what he is doing. I choose the word "confront" in place of "interpret" for the same reason that I prefer "destructive" and "harmful" to "hostile" and "negative," viz., to

move from the concept of wish to the concept of deed, from hostile feelings to hostile acts. In my experience, resolving this destructive situation depends upon speaking of it directly, even assertively, in terms of action.

The patient's initial reaction to this confrontation depends upon many variables. A common reaction is a verbal attack in return, an attack which, perhaps for the first time, contains an injurious intent that is unmistakable to both patient and analyst. Sometimes the reaction is dramatic. One patient responded by telling me, with some wonderment in his voice, that for several weeks he had been carrying a gun in his car. Whatever the response, it will no doubt be a welcome relief, for the patient as well as for the analyst, from what has probably been a monotonous, many-months-long stalemate.

Significant success, however, can be counted only if the response leads to some rather detailed "chapter and verse" discoveries as to how and why the patient's malicious intent against the analyst was actually developed and carried out. This might include gaining some idea of how much the patient's attack was simply a matter of transference, how much it was caused by the analysis mobilizing his destructive impulses, and, finally, how much it was a retaliation for attacks made on him by the analyst.

Although it is tempting to attribute all occurrences of patient malice to transference, the opposite consideration is not without appeal. Is it possible that the ego's internalization of hostile-aggressive drive elements and their per se inclusion in intrapsychic structure is so limited that in the analytic situation they are represented more as a reality than as a transference fantasy?

With regard to the effect of the analytic process upon the patient's negative posture, it is again tempting to make an assumption, viz., the situation should improve as analysis goes along. It may, however, be just the reverse. The analysis of neurotic libidinal elements may gradually bring about, through a defusion-like process, a freeing of hostile-aggressive elements, which may then be increasingly applied to the analyst and to the analysis itself.

In regard to the third factor, how much the patient's destructive action is a retaliation, there surely must be many points of view. Ideally, it could be said, the analyst should do nothing hostile toward the patient. He should not make hostile remarks, should not phrase his interpretations as attacks, should not be silently hostile, and so on. Perhaps we can all agree on a policy of this sort, even while also agreeing that many of us do not always live up to it. Some of us, at least some of the time, do speak caustically, sarcastically, and accusingly, do put ridicule in our voice, and sullenness in our silence. Personally, I would be inclined to say that I am not too concerned about these overt, individually characteristic hostile acts. What concerns me more about the analyst is something different. To me, the analytic setting, in which the analyst remains constant as an objective, detached, uninvolved interpreter of the patient's productions, is almost sure to bring about a silent but significant built-up of the analyst's own unconscious negative-destructive impulses. As this goes on, the analyst can rarely avoid putting some of these impulses into action, and, like the patient, the analyst, being unable to represent these negative feelings fully in his own transference, will be forced to put them into action and will do so in about the only way available to him: by using elements of the analytic situation as a weapon. What I come to, then, is the proposition that a stalemate in the analysis, an implacable resistance, an unchanging negative therapeutic reaction—anything of this kind should be suspected of consisting of a silent, secret, but actual destructive act engaged in by *both* patient and analyst.

In this respect I would refer again to Freud's comments about Ferenczi in "Analysis Terminable and Interminable," where he implied that the patient's negative feelings for the analyst could have been mobilized only by an unfriendly act on the part of the analyst, the inference being, I believe, that the analyst should not say anything to the patient which might be regarded as unfriendly. My suspicion here is that we tend to lean too far backward on this issue, so far backward that our not confronting the patient becomes in itself not merely an unfriendly act but a destructive one. By not confronting the patient with the actuality of the patient's secret, silent obstruction of the analytic progress, the analyst himself silently introduces even greater obstructions.

I suppose what I am saying is that, to me, analysis, especially as it concerns negative destructive elements, is not merely an intellectual or an emotional experience; rather, it is as well a conflict, a conflict starting out within the patient's neurosis as an intrapsychic event and gradually becoming a conflict within the analytic situation. Only then, only when the analytic situation becomes, in a sense, an adversary situation, should we expect the kind of transference neurosis to develop that can admit to it a representation of destructive impulses strong enough and faithful enough to permit this aspect of the patient's neurosis to be effectively analyzed.

I do not mean by this that analysts should fight with their patients. Nor do I mean that an adversary

situation per se is good. What I do mean is something rather different. I am referring specifically to the patient's intrapsychic neurotic life. In it, expectably, are many destructive elements. These elements, as I think many of us would assume, do not remain or perhaps do not even exist in isolation. They are engaged with other destructive elements, either as protagonist or antagonist or as both, to form an organized intrapsychic conflict. This organized conflict, which might be regarded more accurately as an adversary situation, seems to constitute a unitary neurotic structure and, as such, I believe, seems to stand a chance of finding representation in the transference neurosis. If it does, it should be expected to appear there as an adversary situation between patient and analyst. This is what I mean when I say that perhaps only when the analytic situation becomes an adversary situation should we suspect that a transference neurosis adequate enough to represent destructive impulses has developed.

In order for such a transference neurosis to come about, the analyst, through the analytic process, must somehow enable the patient to extend his intrapsychic conflicts to include the analyst. Whereupon the analyst becomes protagonist and the patient antagonist, or vice versa, in a real conflict within the analysis. In this way, through the patient's attributing one of the two or more adversary positions to the analyst, and through the patient's then being able to espouse more single-mindedly the opposing position, the patient's negative-hostile-destructive forces are likely to achieve a more personal, current, powerful, and real quality, a quality that hopefully makes them amenable to analysis.

In order for this to happen, I am tempted to believe, the analyst's own transference involvement is necessary. For one thing, his own transference may be the factor that enables him to accept an adversary role in the patient's neurosis. For another, it may be that only through the analyst's insight into his own "destructive" transference involvement can he understand and analyze the patient's destructive forces. The first thing he will be able to understand, I should think, is that the patient's literal attacks upon him, the patient's literal attempts to destroy the analyst, probably represent in the transference neurosis the patient's own intrapsychic destructive struggles, the patient's own attempts to destroy certain aspects of himself, and his own equally destructive attempts to preserve himself and instead to destroy others. The analyst, at this point of his understanding, will recognize most clearly that the patient's internal destructive forces are organized as an intrapsychic adversary situation, an organization which, with some success and some failure, and perhaps at great expense, has prevented these destructive forces from completely annihilating either himself or others.

To say that the development and analysis of a transference neurosis of this kind is the hardest part of analysis, seems believable. For it to happen at all, I feel sure, requires major contributions from both analyst and patient. From the analyst it requires great perseverance, and, despite how tangled and acerbic and hopeless the analysis may seem to get, it requires rather strict adherence to the principles of the analytic method. There is nothing the analyst can do to deliberately create an adversary situation. He can only not stand in the patient's way. It is the patient's business to bring his adversary situation into the analysis. This is what is required of him—that he do what, hopefully, the analytic situation permits him naturally to do.

When the transference neurosis does develop, neither patient nor analyst may realize for awhile that it has. What they will realize, very likely, is only that the analysis has been caught up in a stalemate, a negative therapeutic reaction, a strong immovable resistance, or in some other seemingly impossible negative struggle between patient and analyst. Hopefully, this struggle will eventually be recognized as a transference neurosis, as a re-enactment in the transference of various destructive elements of the patient's neurosis, a re-enactment in which unconscious destructive acts of the analyst are likely to be involved.

This dark and ominous time, when both patient and analyst are about ready to call it quits, is, according to my thesis, perhaps the only kind o transference in which the patient's most deeply destructive impulses may be analyzable. If, as i sometimes possible, the analyst is able to work hi way through this tremendously difficult, anger laden impasse, the most effective, enduring analyt ical progress may be made.

CLOSING REMARKS: NOTES ON TRANSFERENCE AS AN EGO FUNCTION

The foregoing, on one score at least, brings n around to the paper's introduction and impels me close the paper by commenting again on two ideas opened with: the notion that an analyst's tran ference reactions are essential to the analyzi

process, and the notion that transference is an ego function. Boiled down, these two ideas seem but one: if the analyst's transference is essential to the analyzing process, it could hardly be thought of as anything other than an ego function; and, conversely, if transference is an ego function, the analyst's transference would have to be seen as essential to his analyzing activity.

As to the nature of transference, there has never been much popular support for its designation as a regular function of the ego. This turn of affairs is somewhat surprising in view of Freud's early comments, especially in the Dora case (1905), where his description of transference was of a kind that could be reasonably attributed only to the ego. Perhaps failure to make this attribution is a consequence of our rather complete dependence upon transference in conducting clinical analysis. This dependence understandably may have established transference so securely as a technique that the analyst has seldom given himself the opportunity to wonder about its nature as a phenomenon, or about which agency of the mind it works with or belongs to. When these questions do come to mind, however, it is extremely difficult, for me at least, to escape the idea that transference must be regarded as one of the ego's principal structures, a very special, very powerful, and possibly even a very basic ego apparatus. Most remarkable is the closeness of its relationship to the drives. This closeness, amounting almost to an alliance with the drives, may make it possible, although seemingly paradoxical, to think of transference as being the ego's main antirepressive device. Such antirepressive action, so clearly exemplified by the usefulness of transference in analysis, may be seen as the power which in a general sense endows the ego with its crucial capacity to evoke, maintain, and put to use the past-in-the-present. It may also be this antirepressive force that enables transference to activate and expedite other parts of the ego, particularly, it would seem, the ego's conflict-free givens and its differentiating, synthesizing, and creative capacities.

If this is correct, if transference is indeed to be regarded as a significant ego function, a number of inferences are rather obvious. One is that analysis does not "cause" transference. Yet, although not caused by analysis, transference as it occurs in analysis does seem unique. What is unique, however, may not be transference itself but rather the effect upon transference of the unique conditions of the analytic situation. These conditions may affect most strongly such things as the choice of content of transference reactions, the intensity of these reactions, their exclusiveness, and their sharp focus on the person of the analyst. Although, as a result of these conditions, transference developments in analysis may differ from those occurring elsewhere, this does not mean that in analysis transference as a function is any different.

Another rather obvious inference, following from the first, is that transference can never be resolved. The content may be, but not the function. Through analysis, the symptomatic, neurotic, and historical complexes which have been brought into the transference may be resolved, but not the function itself. The function of transference, like other functions of the ego, may be affected by analysis in many ways, but it never goes away.

Still another inference is a general one concerning transference and the analyst. If transference is to be regarded as an ever active ego function, then the analyst's transference goes on all the time too, just like the patient's, and despite what he might wish to think, his transference has not been resolved in his own analysis. Admittedly the impact of the analytic situation upon the analyst is vastly different from what it is upon the patient, but many aspects of that situation do favor development in the analyst of transference reactions involving his patient. This does not mean, however, that it would be correct to believe the analyst should attempt to inhibit his transference function, much less disavow it. Yet what the analyst should do about his own transference is a question that has never been significantly pondered over. Aside from my belief that the analyst's transference is remarkably useful in the process of analyzing and may even be essential for certain aspects of analysis, what can be said?

Would it be wrong, I wonder, to propose that this ego function be dealt with in the same way the analyst deals with his other ego functions? Just as the analyst must consciously regulate his responses to other functions in order to create and sustain the analytic situation, should he not also regulate his responses to his transference activity? This does not mean, I should think, that the analyst must decide either whether or when a transference reaction to his patient exists. Such an attempt is beside the point on at least two counts. For one thing, significant transference reactions are usually not conscious; and, for another, transference activity in some form is always going on.

In view of these considerations, the simplest position for the analyst to take, and the one most likely to be helpful, may be to assume that *all* feelings and reactions of the analyst concerning the patient are *prima-facie* evidence of the analyst's transference.

Under this arrangement every feeling of warmth, pity, sadness, anger, hope, excitement, even interest; every feeling of coldness, indifference, disinterest, boredom, impatience, discouragement; and every absence of feeling, should be assumed to contain significant elements of the analyst's transference as focused on the patient. This would mean, essentially, that everything arising in the analyst about his patient is assumed to be part of the substance of analysis, that nothing represents merely the analyst's "real" reaction to his patient, and that especially when something seems most real it can be counted on to contain important aspects of the analyst's transference.

Were the analyst to take this rather imperative view of his own transference potential, he might be much more likely to remain abreast of the personal, neurotic meanings of the myriad but often subtle reactions and attitudes he develops toward his patient. This in turn might make it possible for him at least to keep his transference out of the patient's way and hopefully to use it to further the analysis.

The final inference I want to draw from all this is perhaps the most promising. This is that transference, if it indeed belongs to the family of ego functions, can be counted on to possess many of this family's characteristics. Thus, presently existing knowledge about the ego should provide many ready-made leads as to the nature of transference. The ego's ways of reality testing, for instance, its responses to internal and external stimuli, its uses of defense mechanisms, may all reveal much about the basic phenomenology of transference. Similarly, much may be surmised about transference's functional vicissitudes by assuming that transference suffers the same general developmental and neurotic deficiencies, distortions, limitations, and fixations to which various other functions of the ego are susceptible. A particularly important study would seem to be the special strengths of transference functioning, especially its way of joining with other agencies to serve and facilitate the individual's idiosyncratic interests and developments. Such a study, for instance, might center on the ego's object relations with reference to the question of whether transference is the ego function mainly responsible for their development.

Viewing transference in this way as an ego function means, of course, relinquishing certain elements of our existing viewpoints. One prominent feature of these existing viewpoints, no matter what form they take, is how hard they are to define or even to elicit. Another is how unquestioning we seem to be about the viewpoints we grew up with, how easily we assume transference to be but a therapeutically helpful given, an isolated psychological event having little to do with other psychological events, and, except in the analytic situation, to be lacking useful purpose. Assigned, without even wondering why, to neither ego nor id, it is usually dropped somewhere in between. Labeled but rarely described, it is most commonly called a projection or a repetition of the past, neither of them labels of great distinction.

Nevertheless, no matter how inadequate the form in which transference presently exists, it is a form that is deeply entrenched and that does not beg for change. Accordingly, wresting transference from its syntonic limbo is not likely to be easy and may be impossible; but doing so, bringing it out into open view where it can be contemplated as a major member of the ego family, is to me an utterly fascinating prospect, one that permits me to see transference not only as the best tool clinical analysis has, but possibly the best tool the ego has. It well may be, as Freud suggested, the basis of all human relationships and, as I have suggested, may be involved in all the ego's differentiating, integrative, and creative capacities. It is these aspects of transference that offer the most exciting questions, and it is with these questions that I wish to close my paper.

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