RETURN OF THE PRODIGAL DAUGHTER:

HISTORIOGRAPHY AND THE RELATIONSHIP

BETWEEN GESTALT PSYCHOLOGY AND GESTALT THERAPY

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Abstract

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This dissertation investigates a problem in the history of American psychology regarding the relationship between Gestalt psychology, a school of thought within German academic experimental psychology, and Gestalt therapy, an existential method of psychotherapy associated with humanistic psychology in the United States. Mary Henle (1978), a Gestalt psychologist and historian of American psychology, maintains that the growth of Gestalt therapy resulted in a widespread misunderstanding of Gestalt psychology that contributed to the demise of Gestalt psychology. Henle's view has been generally accepted by historians of American psychology ever since; however, some Gestalt therapists have continued to argue that Gestalt therapy is a direct descendant of Gestalt psychology, based on both technical and philosophical principles. Both sides tend to found their case on the writings of Frederick "Fritz" Perls (1893-1970), but have ignored the potential influence of Laura Perls (1905-1990), who was originally a Gestalt psychologist in the German experimental tradition.

The investigation used the historiographic method developed by Taylor (2000) based on the historical research techniques derived from the comparative study of

religions, and applied to archival investigation in the history of psychology and psychiatry. Laura Perls's 1932 experimental doctoral dissertation on color perception at the University of Frankfurt is the primary archival document that was located, translated, and analyzed. The analysis of Perls's dissertation documents her work under Adhémar Gelb (1887-1935) at Frankfurt corrects and clarifies Henle's argument. This leads to a revised historical view of Gestalt therapy as an intellectual descendant of Gestalt psychology, and suggests to historians of psychology that their current focus on psychology is too narrow.

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I. INTRODUCTION

Purpose

The purpose of this dissertation is to investigate a problem in the history of American academic psychology that has remained unsettled for more than thirty years. This problem is whether or not there is a direct and legitimate connection between Gestalt psychology and Gestalt therapy.

Gestalt psychology was a school of German experimental science prominent in Berlin and Frankfurt in the 1920s that aimed at establishing a holistic view of psychological and scientific phenomena. Gestalt psychology was founded by Max Wertheimer (1880-1943) during his time working in Carl Stumpf's (1848-1936) laboratory in 1905, and then later in his collaboration with Wolfgang Köhler (1887-1967), Kurt Koffka (1886-1941), and Adhémar Gelb (1887-1935) (Ash, 1995). Because it had two centers of activity between 1912 and 1933, it is referred to here as the Berlin-Frankfurt school of Gestalt Psychology. The school was dispersed when the Nazis expelled its members from Germany in the early 1930s. Its members relocated to the United States, but were unable to establish a presence in American academic psychology, and, as a result, Gestalt psychology eventually faded as an independent school of thought within several decades.

Gestalt therapy is a method of psychotherapeutics initially developed by Fritz Perls, (1893-1970) and his wife, Laura Perls (1905-1990), and later elaborated by Paul Goodman (1911-1972). It is an integrated approach that eclectically combines a variety of principles drawn from Gestalt psychology, Freudian psychoanalysis, existential philosophy, holism, Taoism, Zen Buddhism, and other sources (Barlow,

1983). It made its formal appearance with the publication of Gestalt Therapy: Excitement and Growth in the Human Personality (Perls, Hefferline, & Goodman, 1951), became prominent in the human potential movement of the 1960s, remains an integral part of humanistic psychology, but now resides mainly on the fringe of contemporary mainstream psychology.

Arguments in the psychological literature regarding the legitimacy of Gestalt therapy and its connection between Gestalt psychology and Gestalt therapy have focused solely on speculation, and on comparing similarities and dissimilarities. Because these discussions have yielded no definitive resolution, the problem was investigated using the historiographic and archival method. It will be shown that the historiographic investigation of the problem documents a historical connection between Gestalt therapy and Gestalt psychology.

Statement of the Problem

The problem is ultimately rooted in the question whether or not the use of the term "Gestalt" in the name of Gestalt therapy is valid. The term, Gestalt therapy, first came into use in 1951. Fritz Perls, who began his career as a neurologist turned Freudian psychoanalyst, worked briefly in 1926 under Kurt Goldstein (1878-1965) in Frankfurt. There, he met his wife to be, Laura Perls (nee Posner), a student of Gestalt psychology under Gelb, at a seminar co-conducted by Gelb and Goldstein. Laura Perls completed her doctorate in 1932 under Gelb and, soon after, she and Fritz were forced to leave Germany when the Nazis came to power. They eventually settled in South Africa, where they established a psychoanalytic institute. Fritz Perls by then

was becoming increasingly disenchanted with Freud and began blending psychoanalysis with other ideas. His first book was a revision of Freudian theory written while in South Africa (Perls, 1947/1969). He called his new approach "concentration therapy." After the war, Perls left South Africa for New York. He arrived with a manuscript for a second book on his new approach. In New York, Fritz Perls enlisted the editorial services of Paul Goodman, a poet, author, social critic, and devotee of Reichian therapy. Perls also enlisted Ralph F. Hefferline (1910-1974), then an instructor, but later a professor at Columbia University, to provide results of awareness exercises given to undergraduate students, which Perls intended would be incorporated into the book.

At first, the co-authors called their approach, "concentration therapy," although they agreed that the name was insufficient. They then considered "existential therapy," but discarded that as well because, according to Laura Perls, the term might be too closely associated in the popular mind with Jean-Paul Sartre (1905-1980) and nihilism (Wysong & Rosenfeld, 1982). Hefferline wanted to name it "Integrative Therapy," but this was also rejected (Shepard, 1975). Fritz Perls and Goodman then decided upon "Gestalt therapy," although Laura Perls was against it because she felt it infringed on Gestalt psychology and might lead to professional criticism (Wysong & Rosenfeld, 1982).

Given that the authors were attempting to establish a new psychotherapy in direct competition with psychoanalysis, then the dominant form of psychotherapy in the United States, they sought endorsement from established authorities. One of the persons they contacted was Wolfgang Köhler, who possessed an international

reputation, and was then teaching at Swarthmore College. Köhler rejected their advances. Perls and Goodman sent Köhler the proof sheets of Gestalt Therapy (1951) and Köhler wrote back that Gestalt therapy appeared harmless, but rather cheap in its use of the Gestalt name. He questioned them as to why they would criticize Gestalt psychology in the text, but then also want to use its name. He then refused to enter into a correspondence with Goodman on the topic (Stoehr, 1994).

There seems little more at this point in the relationship between Gestalt therapy and Gestalt psychology than some failed attempts at gaining the intellectual approval of an established authority that resulted in professional resentment. The situation changes, however, as Gestalt therapy moves from being an obscure, eclectic upstart in the 1950s to an internationally known psychotherapy with a central place in the heart of the human potential movement of the 1960s.

In his autobiography, Fritz Perls praises the Gestalt concept, but maintains a definite distance from Gestalt psychology. For example, he remembers Gelb as being "a rather colorless person," and that Laura Perls took her doctorate under him in "Farben constancy," but he has little positive to say about the Gestalt psychologists (Perls, 1969/1992, unpaginated).

By the early 1970s, the name of Gestalt therapy began entering the professional, academic, and popular literature, as students of the humanistic movement returned to mainstream employment in psychology and counseling. The remaining Gestalt psychologists—primarily Rudolf Arnheim (1928-1997), a German émigré who studied Gestalt psychology in Frankfurt in the 1920s, and Mary Henle, a student and later a colleague of Köhler's, as well as a historian of psychology—

responded strongly that there was no connection whatsoever between Gestalt therapy and Gestalt psychology. (These objections will be explored in more detail in the Literature Review section.)

Henle served as the primary spokesperson for the remaining Gestalt psychologists until her retirement. She wrote what has since been considered by American historians of psychology as being the definitive critique negating any connection between the published writings of Fritz Perls and those of Gestalt psychology (Henle, 1978). Given that no dissenting articles have appeared in the historical literature, it is assumed that Henle's article has remained relatively unquestioned, and largely accepted by American historians of psychology, until Ullman (1997) brought attention to the presence of Laura Perls.

Gestalt therapists, on the other hand, have argued for a direct and close connection to both Gestalt therapy and Gestalt psychology. This is because Fritz Perls adapted several important principles from Gestalt psychology that are central to the theory and practice of Gestalt therapy. Establishing a connection to Gestalt psychology serves to legitimize Gestalt therapy by placing it in a line of intellectual descent leading back to the Aristotelian philosopher and founder of the act school of psychology, Franz Brentano (1838-1917). Without a legitimate connection to bolster its theory and lineage, Gestalt therapy may be misconstrued by historians as being little more than a poor copy of Gestalt psychology; an intellectual poseur.

Given this situation, a historical investigation into the relationship between Gestalt psychology and Gestalt therapy offers a valuable contribution to the history of American psychology in several ways. The first, and most obvious, is a pedantic one

for an antiquarian; namely, because this issue has not been investigated further, it remains unresolved in the history of American academic psychology. Second, historical verification of a connection between the two serves to place Gestalt therapy not only in a distinct intellectual lineage, but also a place in the history of psychology. It would show a connection between the German experimental psychology and American humanistic-existential psychology. Consequently, it would serve to correct the erroneous view that Gestalt therapy purloined the good name of Gestalt psychology (Henle, 1978), and allegedly contributed to the demise of the latter (Henle, 1980).

Defining the Aspects of the Problem

The historical problem of the relationship between Gestalt psychology and Gestalt therapy has several aspects. These aspects must be addressed to provide the necessary context against which to contemplate the implications and significance of the archival research and analysis in this dissertation. These aspects are: (a) Fritz Perls's appropriation of Gestalt principles, (b) the relationship between Fritz and Laura Perls, (c) the role of Laura Perls in the development of Gestalt therapy, and (d) Laura Perls's formal study of Gestalt psychology.

Fritz Perls's Appropriation of Gestalt Principles

Perls admits in his autobiography that he was not entirely familiar with the work of the Berlin-Frankfurt School at the time of his employment in Goldstein's neurological clinic in Frankfurt in the late 1920s. He states that:

> My relation with the gestalt psychologists was a peculiar one. I admired a lot of their work, especially the early work of Kurt Lewin. I could not go along when they became logical positivists. I have not read any of their textbooks, only some papers of Lewin, Wertheimer and Köhler. Most important for me was the idea of the unfinished situation, the incomplete gestalt. The academic Gestaltists of course never accepted me. I certainly was not a pure Gestaltist (Perls, 1969/1992, unpaginated).

Perls is saying two things in the above passage. The first is that he was only superficially familiar with Gestalt psychology, and mainly interested in the Zeigarnik effect.[1] More so, he is apparently criticizing the Gestalt psychologists for their adherence to scientific method, although he wrongly classifies them as being logical positivists. Either way, his opinion is clearly pejorative and anti-scientific. He also appears not to have been able to establish a collegial relationship with the Gestalt psychologists. For example, he describes Gelb as being good teacher, but a "colorless man" (Perls, 1969/1992, unpaginated).

Besides appropriating the Zeigarnik effect for psychotherapeutic application, Perls also states that he also took the figure-background principle and the Gestalt concept as well.[2] Perls maintains that he took the principle of here-and-now

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[1] See Zeigarnik (1927). The Zeigarnik effect is the tendency for incomplete tasks to persist in memory. This study was published in Frankfurt the year after Perls met his wife-to-be, Laura.

[2] A problem is seen in studying Perls's autobiography regarding his statements about the Gestalt psychologists. Here he criticizes the Gestalt psychologists for believing that the whole is more than the sum of its parts. This is clearly a misattribution on Perls's part. The actual view of the Berlin-Frankfurt school was the whole determines the nature of its parts, an entirely different meaning altogether. The saying itself originates with Aristotle when he wrote the whole is prior to the parts. More generally, however, the saying is often mistakenly rendered as the whole is greater than the sum of its parts; an entirely different concept altogether. By mistakenly attributing the incorrect saying to the Berlin-

awareness from existential philosophy and Zen Buddhism, but he may also have been inspired by Lewin's (1936) action-oriented research method used in his topological psychology.

It can be concluded from Perls's statements that he took three basic Gestalt principles, and later the formal name of Gestalt itself, from the Berlin-Frankfurt School of Gestalt psychology. Three principles are actually a very small amount taken compared to the fact that Gestalt psychology possessed 114 laws and principles of its own (Helson, 1933). Given that Fritz Perls himself admits no direct connection with Gestalt psychology, and only superficial familiarity with its content, but that Gestalt therapy is based on several key principles from Gestalt psychology, a second question arises: how did Fritz Perls gain his insights into certain Gestalt principles which found their application in Gestalt therapy? Assuming that Fritz Perls spoke truthfully in his autobiography that he was never a Gestaltist, and given what is biographically known about him, the immediate and most likely sources exposing him to Gestalt principles were: (a) his brief work experience under Goldstein; (b) his attendance at the Gelb-Goldstein seminar in 1926; and (c) his relationship with his wife, Laura Perls.

Frankfurt school allowed its critics to accuse it of philosophically supporting totalitarianism (Luchins, 1975). This was an inaccurate, but somewhat understandable accusation, as the Gestalt concept had been appropriated by the Nazis in their philosophy and propaganda. Felix Krueger (1874-1948) of the Ganzheitspsychologie school in Leipzig, a competitor to the Berlin-Frankfurt group, claimed that the idea originated with his predecessor, Wilhelm Wundt, and that the Berlin-Frankfurt school had plagiarized it (Harrington, 1996).

The initial answers to the first two questions are that Perls's relationship to the work of Goldstein, like his relationship to Gestalt psychology, was based on appropriating just one or two basic ideas, but never comprehending the whole to any great depth. Wheeler (1991), in his critique of the development of the theoretical model used in Gestalt therapy, what he calls the Perls-Goodman model, argues that Perls's understanding of Goldstein's theory and research was severely limited. For example, according to Wheeler's analysis, Perls simplified the deeper implications of Goldstein's organism-as-a-whole concept derived from his and Gelb's studies of brain-damaged war veterans down to a purely mind-body problem rather than a problem of the person in relationship to the world. Wheeler states that:

> Despite reference to Goldstein in connection with the "whole functioning" of the organism, and despite his own work as a lab assistant to Goldstein in the twenties, Perls does not seem to have appreciated fully that when Goldstein speaks of a "whole-organismic" approach... he is referring not just to the psychosomatic whole, but to the whole configuration of the subject's needs and goals in relation to the environment and to each other—to the dynamic organization of behavior, not merely its somatic aspect or expression. Nor does Perls seem at all aware, here or later, of the crucial work of Lewin in understanding motivation in terms of the "whole field"—including Lewin's emphasis on the "here and now," and the "demand quality" of unfinished situations, both of which are topics in Perls's 1947 book, and both of which were to be principal themes of his demonstration sessions in the fifties and sixties (Wheeler, 1991, p. 45).

Perls had a distinct penchant for avoiding in-depth study of complex topics, preferring instead to take key ideas, simplify them for immediate practical application, and often rely on others who were more intellectually familiar with these topics to explain the details. (A brief discussion with evidence illustrating this idiosyncrasy of Perls's will be given further below.) The historical evidence strongly

suggests that his wife, Laura, provided much of his understanding about Gestalt principles and their application.

The Relationship Between Fritz and Laura Perls

Laura Perls's original intention was to study law at Frankfurt, even though there were few women in the legal profession at that time. Laura was drawn to the legal field from a growing social and political consciousness and envisioned herself working with juveniles in the German court system (Bernard, 1986; Humphrey, 1986).

As Laura reports, she dropped her study of economics and law against the wishes of her father. She switched to psychology after attending classes taught by Gelb because she was impressed by Gelb's literate and engaging style of lecturing. She was to be even further influenced by Kurt Goldstein and Max Wertheimer and his associates, Wolfgang Köhler (1887-1967) and Kurt Koffka (1885-1941). Laura also studied under Martin Buber (1878-1965), and Paul Tillich (1886-1965) (L. Perls, 1992).

Laura first saw Fritz Perls while attending a seminar conducted by Gelb and Goldstein in 1926 and was later introduced to him by a mutual friend[3] (Gaines, 1979).

> In the fall of 1926 I was a student at Frankfurt University. My Professor Adhémar Gelb and Kurt Goldstein were giving a joint seminar on the research they were doing in gestalt psychology, which was then a new field. I was bored. As I turned my attention from the speakers I saw this man sitting there whom I had never seen before. I

[3] Fritz Perls recalls that the mutual friend who introduced him to Laura was Fred Omadfasel, a colleague who worked with him at Goldstein's neurological institute, and was a fellow student of Laura's at the University of Frankfurt (F. Perls, 1969/1991, unpaginated).

> didn't know who he was. I had the feeling: "There he is!" A month later, through a friend who was also an assistant of Goldstein's, I met this man. Of course, it was Fritz. He had had his M.D. and was working with Goldstein in the Institute for Brain-injured Veterans from the First World War. Goldstein was working with the whole organism, not just parts of the human being. He was very avant-garde at that time. Gestalt psychology had then been largely concerned with sensory perception, and not as yet with the personality. In working with brain lesions they discovered that when a particular organ is damaged or missing, the whole person changes and reorganizes through this loss. Fritz was thirty-three when I met him and I was twenty-one. I was very young, naive, inexperienced... yah, he was very impressive! (Gaines, 1979, p. 7).

Fritz, at the time of his meeting with Laura, had recently completed his medical degree in neurology at the University of Berlin and was working in Frankfurt at Goldstein's neurological clinic for brain-damaged veterans. He had served in the German Army during World War I as a medical orderly in the 36th Pioneer Battalion, a poison gas attack group. He completed his medical studies in neurology after World War I. At the time of his meeting with Laura in the Gelb-Goldstein seminar, Fritz was interested in theater (a continuation of his adolescent studies with Max Reinhardt [1873-1943], a famous theater director), and with the bohemian life in Berlin. He was also at this time seeking ways to resolve his traumatic war experiences by applying the philosophy of creative indifference of the German expressionistic author, Salomo Friedlaender (1871-1946), combined with Freudian psychoanalysis (Shepard, 1975). These two elements would later become essential to Gestalt therapy.

Fritz claimed that it was Laura who pursued him into marriage, and that the Posner family never cared for him as they "... looked upon [me] as an outcast who dared to intrude into the well-to-do Posner family" (F. Perls, 1969/1992, unpaginated). Laura married Fritz Perls in 1930, and soon had their first child,

Renate. Laura then became involved in psychoanalysis through her relationship with Fritz, although she had been in analysis with an Adlerian psychoanalyst when she was sixteen. She took up an interest in psychoanalysis because the people around her were studying it. She read Freud's early works on dreams and everyday psychopathology. It appears that she picked up psychoanalysis mainly in the interest in preserving the equilibrium in her relationship with Fritz (Humphrey, 1986). As Laura neared the completion of her graduate studies, she became a training analysand, but found the transition somewhat difficult. Laura comments on this double education:

> I was a Gestalt psychologist before I got into psychoanalysis. Fritz was an analyst before he got into Gestalt psychology. Sometimes it set up an insoluble conflict. I sometimes said I felt like Pavlov's double-conditioned dog who fell asleep in the middle of the experiment (Stern, 1992, p. 21).

Laura first entered psychoanalysis with Clara Happel (1889-1945) in Frankfurt for about six months until Happel moved to Hamburg. Laura then continued with Karl Landauer (1887-1945). According to Laura, Landauer was the most prominent psychoanalyst practicing in the area and was associated with Sandor Ferenczi (1873-1933) and Georg Groddeck (1886-1934) (Wysong & Rosenfeld, 1982). Laura also studied under Frieda Fromm-Reichmann[4] (1889-1957) at this time as well (Bernard, 1986; Humphrey, 1986), and completed her training under the supervision of Otto Fenichel (1897-1946) after she and Fritz moved to Berlin (Bernard, 1986).

Perceiving that the German political situation was moving toward greater racial persecution and political fascism, Laura and Fritz emigrated from Germany to Holland after the Nazi government came to power. While living as refugees in

[4] See also Hornstein (2000).

Holland, they were contacted by Ernest Jones (1879-1958), a Welsh psychoanalyst and later Freud's biographer, to establish a psychoanalytic institute in South Africa (Gaines, 1979; F. Perls, 1969/1992; Shepard, 1975; Wysong & Rosenfeld, 1982). They accepted Jones's offer, and for the next fourteen years lived in security and comfort in that country.[5]

Both Laura and Fritz established psychoanalytic practices and, in time, reached a level of affluence that afforded them a home, swimming pool, tennis court, and servants. In the late 1930s, as the major powers drifted toward World War II, Laura published an article in Johannesburg entitled, "How to educate children for peace" (L. Perls, 1939/1992). Laura's South African period is marked by two other events. First, she had their second child, Stephen. Second, both she and Fritz began slowly drifting further away from orthodox Freudianism through Fritz's use of Friedlaender's philosophy, her own perspective in Gestalt psychology, and her past training in dance movement and body awareness (L. Perls, 1992; Wysong & Rosenfeld, 1982).

The drift appears to have begun while they were still in Berlin, when Laura assembled some observations on her experience of the nursing and weaning her first child. This anecdotal research centered on the problem of the child making the transition from the sucking to the biting stage of oral behavior (Bernard, 1986). Laura

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[5] Laura's and Fritz's emigration from Germany to Holland and then to South Africa saved them from the Holocaust. The Nazis later killed Laura's sister, who remained in Holland, and many of Fritz's family who remained in Germany were also killed. These were important experiences for both Laura and Fritz as they influenced their views on life in general, and taught them importance of taking risks (Perls, 1969/1992; Serlin, September 9, 1997, personal communication).

discussed her experiences with Fritz and they both became increasingly interested in the oral stage of development and the "oral instinct," as they termed it. Fritz later expanded these notes, according to Laura, into a lecture entitled "The oral resistances," which he presented at the international psychoanalytic association's 1936 conference in Marienbad, Czechoslovakia. The attendees rejected his presentation because it was too radical a departure from the Freudian doctrine that all resistances are anal in origin (F. Perls, 1969/1992). Disappointed and resentful, but not discouraged, Fritz returned to South Africa and began expanding the lecture into what would become the chapter on "Mental Metabolism" in Ego, hunger and aggression (1947/1969) (L. Perls, 1992). Laura was instrumental in the development of this early work, as she ghostwrote two of its chapters. Laura states:

> Then in South Africa in 1934, we both had a practice and started the book Ego, Hunger and Aggression... . I was in on everything in the beginning, and we discussed everything together. However, I left most of what I produced to Fritz. Actually, Ego, Hunger and Aggression took off on some research I had done on the feeding and weaning of infants and the transition from the sucking to the biting stage. That was then extended into what became the chapters on mental metabolism in the book. I wrote a couple of chapters myself—"The Dummy Complex" and the insomnia chapter (Bernard, 1986, p. 369).

Overall, the book itself was stylistically disjointed, and theoretically weak, although filled with many keen therapeutic insights and observations (Wheeler, 1991). Ego, Hunger and Aggression (F. Perls, 1947/1969) is significant, however, because it details the essential shift of the Perls's application of psychoanalysis from a Freudian perspective to an approach based on a holistic, Gestalt-oriented view of the human as organism-within-environment. The book introduced the practical application of using the client's awareness based on gestalt figure-formation in the present moment as a

diagnostic and psychotherapeutic tool. Fritz and Laura called this use of awareness "concentration therapy" in Ego, Hunger and Aggression (F. Perls, 1947/1969). Concentration was most likely a term Fritz borrowed from the work of Wilhelm Reich (1897-1957), since Reich used the same term and Fritz had been in therapy with Reich while still living in Berlin.[6] Thus, it is seen that the early development of what was to become Gestalt therapy came about through a chain of synthesizing Gestalt psychology with Freudian and Reichian psychoanalytic ideas, existential philosophy, and Friedlaender's philosophy of creative indifference.

Laura did not feel comfortable in South Africa, but used her discomfort to fuel her professional development and personal growth. She states that:

> In South Africa, I felt trapped by the tight provincial atmosphere, the diminutive number of congenial people, the tense threatening political situation. But with no way out, I gradually came to realize that even if I had to stay within my one single room, I had walls of books, a grand piano and the whole classical piano literature, that even three life-times would not be enough to work through it all. This profoundly changed my life. In my practice I started to use face to face dialogue and body awareness; I wrote stories and poems. And Fritz and I started working on what became Ego, Hunger and Aggression. It was just the social and professional isolation and confinement that forced us to focus our interest on our own resources and mobilized our own creative potential, which had been mostly dormant within the limitations of psychoanalysis (L. Perls, 1985, p. 13-14).

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[6] Fritz Perls was in psychoanalysis for one year with Reich, who deeply influenced both Perls and Paul Goodman (F. Perls, 1969/1992; Shepard, 1975; Stoehr, 1994). Even though in Perls, Hefferline and Goodman (1951), the authors go to some length to distinguish their work from Reich's--especially Reich's orgone energy ideas--there can be little doubt that Gestalt therapy is deeply indebted to Reichian ideas, especially Reich's character analytic approach. Works dealing with Reich's studies during the period Fritz knew him include character analysis (Reich, 1945), genitality (Reich, 1980), bio-electrical energy and sexuality (Reich, 1982), and mass psychology (Reich, 1970).

Having spent fourteen years in South Africa, Laura Perls and her family were forced again to emigrate for political reasons. The South African government was becoming more conservative with the departure of J.C. Smuts (1870-1950) as prime minister in 1948. Apartheid, which was already a part of the culture, was becoming more rigidly legalized. This caused profound fears on the part of Fritz and Laura, as it appeared to be a prelude to fascism, and so they immigrated with the assistance and sponsorship of Karen Horney (1885-1952) to New York City in 1946-1947 (Wysong & Rosenfeld, 1982). It was difficult for Laura to adjust to a strange city in a new country without the material security she and Fritz enjoyed in South Africa.

Prior to their leaving South Africa, Fritz had been working on a book manuscript further elaborating their "concentration therapy," and needed an editor to assist him in its completion. He sought out Paul Goodman in New York City because of the Goodman's political writings and interest in the political implications of Reich's body-oriented psychotherapy (Stoehr, 1994). Goodman left Reichian analysis and became a patient, and later a therapist-trainee, under Laura.

It is at this point that the relationship between Fritz and Laura Perls intellectually triangulates, as it were, to include Goodman, and several other New York intellectuals who joined them to form the New York Institute of Gestalt Therapy. The major change in their relationship appears to be that Fritz was losing his dominant position in both the relationship and in the group, a situation which he found intolerable,[7] and so he left Laura and the family. He remained estranged from Laura for the remainder of his life, but the two never divorced.

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[7] Further examples of Perls's need for social dominance can be found in Gaines (1978).

It is difficult to parse out the exact influence Laura Perls had on Fritz Perls. Laura Perls observes, as noted earlier above, that her husband needed the assistance of others in the development of his ideas; otherwise he might not have succeeded in his endeavors. She states that:

> Fritz's genius was in his intuitive insights and uncanny hunches, which then would have to be substantiated in more exact elaboration. Fritz very often did not have the patience for this detailed work. He was a generator, not a developer or an organizer. Without the constant support from his friends, and from me, without the constant encouragement and collaboration, Fritz would have never written a line, nor founded anything (L. Perls, 1990, p. 27-28).

The question of how much Gestalt psychology Fritz did understand remains. His own comments indicate that Fritz appeared to have understood relatively little. If so, it follows that the intellectual background for what he borrowed from Gestalt psychology was most likely provided by Laura. As one of Fritz's former students who later came to know Laura speculated:

> I have spent many hours listening to Fritz Perls talking; I talked <u>with</u> him for quite a few. I admired and wondered at his marvelous and sometimes uncanny perceptiveness. But often I felt nagging uneasiness that configurational thinking was not natural to Fritz. In spite of, or perhaps because of, his astute honing in on personality problems of the people he worked with, he appeared to miss out on the larger picture of personal interaction of which he was a part. Certainly my attempts to talk "about" Gestalt psychology with him were frustrating for me. I have not felt this in conversations with Laura; with her I sense easy access to basic ideas underlying both Gestalt psychology and Gestalt therapy. Though she has received little credit for it, perhaps Laura was responsible for integrating Gestaltist thought more than anyone realized (Rayne, 1980, p. 80; original emphasis).

Likewise, Richard Kitzler (personal communication, January 2, 2002), an early patient and later colleague of Fritz Perls in New York City beginning in the late 1940s states:

> The historical connection, this little niche thing from Laura, is that she was the intellectual. He was not. He was very impatient and wouldn't read and would boast about it. He mentions Lewin in Ego, Hunger & Aggression in connection with the repetition compulsion, and it's a shrewd couple of sentences, but that was his connection and he probably got it through Laura. I think the general atmosphere [between them] was what Laura provided as well as the entrée to Gestalt psychology. Fritz spoke only of Goldstein. Laura had at her fingertips the whole of Gestalt psychology. It was just being done then. I suppose that through her he was able to move through those circles... . I began studying this stuff and I told Fritz that he didn't really know what he was talking about, that there was a lot of experimentation that was done, and I mentioned the group dynamic stuff that Lewin began to be involved with, and he knew nothing of that. He didn't know anything about groups [even though] he did individual therapy in a group context with high drama and all that. She [Laura] was exceptionally well read. She was ideal for Fritz because he had a drive to be original, but how can you be original, when you know everything? When I told him—by the way I'm lying on the couch now [talking to him as a patient when I said this]—I said, "You don't know anything about groups," and he said, "Yah, I don't read. I have no referential knowledge." And he was proud of that. What I'm leading to is that she [Laura] was perfect for him because he could get it all through osmosis. He would take what he needed and nothing more.

Renate Perls (personal communication, January 2, 2002) agrees that Fritz's strength was in his intuitive creativity in seeing how theoretical ideas could be applied in psychotherapy, and then in promoting those applications and himself as well. She describes her parents' relationship:

> First of all, she [Laura] was an exceptionally brilliant young woman. She had also taken eurythmics and was very, very aware of how one should move; needed to move for a healthy body. And kept herself agile until shortly before she died. Also her background was more intellectual than Fritz's. She came from a better background. This was there all of her life. I don't know how much Fritz got, but his mother was interested in theater and he got that from her, as well as his love of good food. His father's library had only to do with the Masons. There was nothing at home for him to read; what could he get from his father? What he got from his father was how to misbehave, not how to behave. So I think she [Laura] probably introduced Fritz to a lot of things that he did not have in his background; particularly that of movement, and even awareness, and definitely of music. Laura was

> the one who discovered the connections between how one chews and how one assimilates other things; how one assimilates food to how one assimilates other things in life which Fritz gave in a paper in Czechoslovakia. So, I think in matters of awareness, physical awareness and connection to movement, to just how the person is functioning, I think Laura had a tremendous influence on him. The influence he had on her was to bring her to the field of psychoanalysis and psychotherapy. And when Laura began to see how interesting it was she didn't want to go into law anymore. She was fascinated with how people interact and what happens.

These statements indicate that Fritz Perls must have appropriated a substantial portion of his intellectual understanding of Gestalt psychology as well as other concepts from his wife, Laura. Their working relationship appears to have been that Fritz was the "idea man" and entrepreneur (in the sense of initiating projects and activities) while Laura was an active generator of ideas as well as his intellectual sounding board; she was in part his Muse, and in part his surrogate intellect.

The Contributions of Laura Perls to Gestalt Therapy

When taking up the study of Gestalt therapy, many students of Gestalt therapy and many historians of American psychology tend to identify Gestalt therapy solely with the name of Fritz Perls. This is a relatively easy mistake to make because Perls promoted Gestalt therapy during the 1960s, published a number of books and articles, and assembled a group of students around him to continue his teachings. He also claims that he solely developed Gestalt therapy, and ignores or minimizes the contributions of Laura Perls and Paul Goodman. Given that Goodman shifted his professional interests from psychotherapy to activist politics in the 1960s, and that Laura Perls published relatively little, Perls's claim to sole ownership has been unchallenged until recently. Shane (1999) examined the role of Laura Perls in the

development of Gestalt therapy and showed that many fundamental features of Gestalt therapy are directly attributable to Laura Perls's work and influence. In terms of the theory of Gestalt therapy: (a) she inspired the initial conception of the orality theory of human aggression; (b) some of her own writings give early elaboration to key ideas in Gestalt therapy, and that these predated the writings of Fritz; (c) she studied Gestalt psychology under Gelb; (d) she was the only one of the three original co-founders of Gestalt therapy to formally study many of the main philosophical sources of Gestalt therapy; and (e) her own academic studies and technical innovations predated those of others who came to Gestalt therapy later in its historical development. In terms of technique, Laura Perls was the source for principles essential to Gestalt therapy such as body awareness and movement, contact, and support.

Laura Perls's Study of Gestalt Psychology

Shane's (1999) study suggests that historians of American psychology have largely overlooked the significant and distinctive influence of Laura Perls as one of the co-founders of Gestalt therapy. What has been neglected is the fact that Laura Perls completed her doctoral work in color contrast perception under Adhémar Gelb and Kurt Wilhelm Meissner (1891-1959), a physicist, at the University of Frankfurt. Past historical researchers were either unaware of, or apparently overlooked, the significance of Laura Perls as a graduate student under Gelb in experimental Gestalt psychology. In addition, no historical researcher has focused on the Gelb-Goldstein seminar as the potential starting point for Gestalt therapy.

Given that the Laura Perls and her graduate work in Gestalt psychology have not been investigated, the purpose of this dissertation is to locate and analyze archival evidence that would provide necessary and sufficient documentation to support a connection between Gestalt psychology and Gestalt therapy. The primary source document proving this connection is Laura Perls's 1932 doctoral dissertation under Gelb. This document has been located, translated into English, and will be analyzed in further detail elsewhere in this dissertation (see Appendix).

II. REVIEW OF THE LITERATURE

General Literature

A review of the general literature in the history of psychology affirms two points: first, the literature denies a connection between Gestalt psychology and Gestalt therapy (or neglects comment on the topic) and second, it makes no mention of the role of Laura Perls in the development of Gestalt therapy.

Geldard (1973) describes Gestalt psychology, but does not mention Gestalt therapy. Likewise, in Adams, et al. (1973), there is no entry for Gestalt therapy.

Arnheim (1984/1994) describes Gestalt psychology, but does not mention Gestalt therapy. Conversely, Glass (1984/1994) describes Gestalt therapy, but lists as one of its origins in the work of Kurt Goldstein. He also discusses certain Gestalt principles, such as the Gestalt concept and the Zeigarnik effect, but does not mention Gestalt psychology.

Kemp (1985), in his description of Gestalt psychology specifically denies any connection between Gestalt psychology and Gestalt therapy, citing Henle (1978). In the same volume, Benner (1985) describes Gestalt therapy and includes Gestalt psychology as one of the major systems from which its roots extend.

Sutherland (1989) describes both Gestalt psychology and Gestalt therapy and specifically denies any relationship. His description has a negative tone and appears almost solely intended to disclaim any connection to Gestalt psychology:

> Gestalt therapy. A type of psychotherapy originated by Frederick Perls. It has no direct connection with Gestalt psychology; it emphasizes the spontaneous expression of feelings, living in the here-and-now and personal growth. Whether given to individuals or to groups, the therapy can be aggressive. There is no evidence that it works (p. 175).

Historical Literature

Misiak & Sexton (1966), in their description of Gestalt psychology do not mention Gestalt therapy, although this is understandable, given that Gestalt therapy was just beginning to come to public recognition in the mid 1960s.

Schultz (1969) gives a detailed historical review of the development of Gestalt psychology, but in the section regarding its contributions to American psychology does not name Gestalt therapy.

Henle (1980) in her survey on the influence of Gestalt psychology in America does not mention Gestalt therapy. She does, however, give reasons for the demise of Gestalt psychology in America. While the full significance of her comments will become clearer further below, she states that:

> Gestalt psychology was very active and influential in Germany in the 1920s and early 1930s, but the first generation of young Gestalt psychologists was wiped out by events in that country. Wertheimer, Kohler, and Koffka, the first three Gestalt psychologists, emigrated to the America. There they were without graduate students, and the intellectual climate in psychology was dominated by behaviorism; thus, a new generation of Gestalt psychologists could not be established, and Gestalt psychology has remained a minority movement (p. 189).

In his historical review of Gestalt psychology, Murray (1983) does not mention Gestalt therapy. Hothersall's (1984) history of psychology discusses Gestalt psychology, but makes no mention of Gestalt therapy. Hilgard (1987) discusses Gestalt psychology and profiles several of its founders, but does not mention Gestalt therapy, even in a section on humanistic psychology. Kendler (1987) gives a detailed analysis of the history of Gestalt psychology and its influence on American psychology, but does not name Gestalt therapy. Benjamin (1988/1997) devotes a chapter to Gestalt psychology in his historical survey, but does not mention Gestalt therapy. Brennan (1991) explores the

history of Gestalt psychology, but does not mention Gestalt therapy. Bolles (1993) reviews the history of Gestalt psychology, including a discussion of its influence on American psychology, but does not mention Gestalt therapy. Morton (1993) devotes a chapter to Gestalt psychology, but only two paragraphs to Gestalt therapy. As a journalist, Morton wrote his history in consultation with leading members of the American Psychological Association and historians of psychology. He pointedly disclaims any connection between Gestalt psychology and Gestalt therapy:

> Quite unlike Roger's method, though sharing its philosophy of human health and self-direction, is the technique developed by Frederick (Fritz) Perls, a psychiatrist. He called it Gestalt therapy, although, as noted earlier, it has little in common with Gestalt psychology (p. 591).

In their historical text, Popplestone & McPherson (1994) directly negate any connection between Gestalt psychology and Gestalt therapy by using Henle (1978) and Arnheim (1974) as sole support:

> During the 1960s and 1970s, Gestalt psychology began to be (mis)identified as the theoretical basis of what is called gestalt therapy. This started when some of the Gestalt ideas were used incorrectly by the advocates of the human potential movement, groups of psychologists who emphasize affect and prefer self-expression to self-control. The literature of what is called gestalt therapy refers, for example, to the emotionally compelling as the foreground and advises that changing habits demands extracting them from the background and reorganizing them. Unfinished tasks, unresolved conflicts, and ungratified wishes are labeled "lack of closure." War is called an incomplete gestalt, and peace is designated as closure. This terminology is the product of unmonitored linguistic associations and is not related in any meaningful way to Gestalt psychology (Henle, 1978). Regrettably, this nomenclature has spread and it is now encountered, without correction, in publications of the American Psychological Association (Arnheim, 1974) (Popplestone & McPherson, 1994, p. 56).

Literature Disconfirming a Relationship Between Gestalt Psychology and Gestalt Therapy

Arnheim (1974), who received his doctorate under Wertheimer in 1928 (Hilgard, 1987), states in a one-paragraph editorial letter that there is no connection whatsoever between Gestalt psychology and Gestalt therapy, and that he can only imagine Wertheimer flying into one of his "magnificent rages" if he were to have heard such a thing (Arnheim, 1974, p. 50).

Michael Wertheimer (1970/1979), the son of Max Wertheimer, asserts in his discussion about the impact of Gestalt psychology in American psychology:

> Incidentally, while Frederick Perls, originator of the "gestalt therapy" that was so popular, especially in the 1960s, did hear one lecture by Wertheimer, and admired him very much, there is essentially no conceptual relationship between Perls' neo-analytic "gestalt psychotherapy" and Gestalt psychology in the sense of the Wertheimer-Kohler-Koffka theory (p. 140).

Henle (1978), in what is the definitive article disclaiming any connection whatsoever between Gestalt psychology and Gestalt therapy, bases her argument on the many dissimilarities between the thematic content found in Fritz Perls's published writings and the writings of Gestalt psychology. Henle is extremely conversant in the history and philosophy of Gestalt psychology. She graduated with a baccalaureate in 1934 from Smith College where she studied under Koffka. She did her graduate study at Bryn Mawr, where she met Harry Helson (1898-1977), a Harvard doctoral graduate, who was one of the first American scholars to examine Gestalt psychology in a series of articles in American Journal of Psychology (Helson, 1925a; 1925b; 1926a; 1926b). There, she also met Donald MacKinnon (1907-1987) as well who studied under Köhler and Kurt Lewin (1890-1947) in Berlin. She then took a teaching

position at Swarthmore College where she worked with Köhler and several other German and American Gestaltists. Henle moved from Swarthmore to the New School for Social Research in New York where she worked with Max Wertheimer. Wertheimer died in 1943, and Solomon Asch (1907-1996) replaced him at the New School, with whom Henle was associated until his death.

Henle originally aired her views in her presidential address to Division 24 (History of Psychology) at the annual meeting of the American Psychological Association in 1975. Her purpose, she said, "is to try to set the historical record straight while the history in question is still in the making" (p. 23). Henle suspects that many psychologists and students of psychology erroneously believe that Gestalt therapy is Gestalt psychology, or that Gestalt therapy is an extension of Gestalt psychology. She intends to correct this misunderstanding. Henle is also writing out of "astonishment" that Perls's biographer, Shepard (1975), baldly asserts that Gestalt psychologists claim Perls as one of their own (Henle, 1978, p. 24).

Henle confines her analysis to Perls's writings from 1969 because, as she notes, he himself viewed his early work as "obsolete" (p. 23). She reviews the main concepts from Perls's writings, and systematically negates any connection on the grounds of dissimilarity. Henle's analysis contains ten points of difference between Gestalt psychology and Gestalt therapy.

The first is that Gestalt psychology is a natural science, while Gestalt therapy stems from the tradition of Geisteswissenschaft. What she means by this is that Fritz Perls and his Gestalt therapy stand in an entirely different intellectual tradition from that of the Gestalt psychologists. Gestalt psychology is within the tradition of

Naturwissenschaft of German science aimed at scientific explanation, while Perls's purpose is aimed at understanding. Another related point made by Henle is that Perls appears to be philosophically inclined toward vitalism, while Gestalt psychology attempted to transcend the mechanism-vitalism dichotomy altogether (p. 25).

The next major difference Henle points out is that Gestalt psychology respects the intellect, while Perls advocates an anti-intellectual position and rejects scientific psychology (p. 25).

Henle names a third difference between the two. Gestalt psychology, she says, poses a mind-body parallelism in its theory of psychophysical isomorphism, while Perls adheres to a mind-body monism (p. 25-26). Psychophysical isomorphism acknowledges the difference between mind and matter, but asserts that molar events in consciousness are reflected in molar events in brain physiology (Köhler, 1924).

Fourth, according to Henle (1978), Gestalt psychologists and Perls understand the term Gestalt differently. She compares a definition of the term given by Köhler (Köhler, 1947, p. 177-178) in which he gives a detailed description of two meanings of the term, and how it has evolved since its original usage in Gestalt psychology. First, it emphasized properties of a phenomenon, then the organization of the phenomena. Perls, on the other hand, tends to use the term ambiguously (Henle, 1978, p. 27).

The figure-ground concept, another principle that Perls borrowed from Gestalt psychology, is also misconstrued, according to Henle. The figure-ground concept, originally proposed by Rubin (1915), refers to the visual perception of an object as

being distinct from its background, and that in certain conditions the perception of the object may be reversed with its ground, thus making the ground figural, and the object the background. Henle argues that Perls simplifies the figure-ground concept in service of his own psychotherapeutic theory and perspective. That is, Perls emphasizes the notion that figure and ground can be reversed, but, according to Henle (1978), this premise is never clearly explained or substantiated by him (p. 27).

Likewise, Henle criticizes Perls's apparently partial understanding of the concept of closure. Closure relates to the tendency for good gestalten to form complete figures, as opposed to partially complete ones, which make them self-sustaining, stable, and aesthetic (Koffka, 1935/1963). Henle dislikes Perls's interchangeable use of "closure" and "unfinished business." The former is a concept developed by Wertheimer, along with many other interrelated ones that Perls ignores, according to her, while the latter is a technique invented by Lewin. Lewin's student, Bluma Zeigarnik (1900-1988), experimentally verified the phenomenon of the incomplete situation, and its relationship to closure (Zeigarnik, 1927). Henle (1978) admits that Perls's application of the concept to psychotherapy may have "vague plausibility," but the problem of its application is far more complex than Perls appears to understand (p. 28). Henle comments further:

> But vague plausibility is not enough for a theory of neurosis or therapy or personality—or of anything. It is necessary to be clear about the specific characteristics of the structure we are calling neurosis or personality, about the nature of the processes involved, and the nature of the closure demanded by that structure. Such questions are never found in the material I am considering, and we are left with a terminology so vague as to defy any specific use (p. 28).

The concept of awareness is central to Perls's theory because it is what organizes experience. Consequently, and given his background as a psychoanalyst, Perls asserts that attention, cathexis, motivation, and interest cause psychic organization. Henle disputes this as being related to Gestalt psychology because part of its theory proposes that cortical interaction based on sensory stimuli is the organizing factor. According to Henle, this makes Perls's theory far more related to the work of G.E. Müller (1850-1934), and the Italian philosopher, Eugenio Rignano (1870-1930), circa 1920, than the Berlin-Frankfurt school of Gestalt psychology[8] (p. 28-29).

Henle also observes that Gestalt therapy and Gestalt psychology are deeply opposed on the issue of ethical relativism. It was believed in Gestalt psychology that truth and value are not arbitrarily determined, but depend largely on what is appropriate to a specific situation. Perls, Henle writes, subscribes to an ethical relativism that is "cynically expressed" in his works (p. 30).

Returning to another aspect of the mechanism-vitalism difference noted earlier, Henle criticizes Perls for promoting a mechanical view of the mind with his use of such ideas that the mind is a machine and that thinking is "computing." Gestalt psychology is philosophically opposed to any machine model of the human mind (p. 30).

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[8] Henle does not support her assertion regarding the possible influence of Müller and Rignano in Perls's thinking. Perls, having trained as a neurologist, would most likely have been acquainted with Müller, but it is unknown if he was familiar with Rignano. For Rignano's view of Gestalt psychology and the relationship between form and sensory perception, see Rignano (1928).

Finally, Henle observes that Perls's understanding of phenomenology is drastically different than that of the Gestalt psychologists. Gestalt psychology used phenomenology as an exploratory method that prepared the way for experimental investigation, while Perls understands phenomenology in very general and vague terms (p. 30). Henle concludes:

> From the material already discussed, it is not difficult to reach a conclusion. What Perls has done has been to take a few terms from Gestalt psychology, stretch their meaning beyond recognition, mix them with notions—often unclear and incompatible—from the depth psychologies, existentialism, and common sense, and he has called the whole mixture gestalt therapy. His work has no substantive relation to scientific Gestalt psychology. To use his own language, Fritz Perls has done "his thing"; whatever it is, it is not Gestalt psychology (p. 31).

Continuing on, Wheeler (1991), himself a Gestalt therapist, critiques the theoretical model of the self found in Gestalt therapy. He does not argue against a historical connection between Gestalt therapy and Gestalt psychology, but just as importantly provides a detailed analysis showing how Perls misunderstood and misapplied various principles appropriated from Gestalt psychology, Lewin, and Goldstein.

Sherrill (1991), a Gestalt therapist, assuming the persona of Wolfgang Köhler, provides a fictional, autobiographical perspective of Köhler's view of the historical development of Gestalt theory. What makes this account significant is the pseudo-Köhler's view of Fritz Perls and Gestalt therapy. Sherrill, in the imaginary voice of Köhler takes Perls to task for misunderstanding various principles of Gestalt psychology and Goldstein, and concludes that Perls's use of Gestalt principles most likely originates in the work of Lewin and Goldstein, but these are completely distinct from the Berlin-Frankfurt school.

Literature Confirming a Relationship Between Gestalt Psychology and Gestalt Therapy

Wallen (1957/1970) acknowledges that Gestalt therapy derives from many different theoretical sources, but it took from Gestalt psychology the principle of gestalt figure formation. Wallen finds it of interest that Gestalt psychology applied gestalt formation only to problems of cognition and perception, rather than also extending it "to organic perceptions, to the perception of one's own feelings, emotions, and bodily sensations" (p. 8). Wallen does not argue for a historical connection between Gestalt psychology and Gestalt therapy, but only that the two are linked in their respective applications of gestalt formation in perception. No mention is made of Laura Perls.

Emerson & Smith (1974) use Wallen's (1957/1970) article as their point of departure to review the history of experimental psychology up to the point of the arrival of Gestalt psychology. Their article primarily summarizes the history of Gestalt psychology, connects Fritz Perls with Lewin, Goldstein, and Gelb,[9] through Fritz Perls's borrowing of the figure-ground principle and the Zeigarnik effect from Gestalt psychology. It does little more than make a basic case for Perls being familiar with Gestalt psychology. No mention is made of Laura Perls.

Smith (1976) restates his 1974 position and focuses on the influences of Lewin, Goldstein, and adds, this time, Andras Angyal (1902-1960), an Austrian psychiatrist who advocated a principle of self-actualization similar to Goldstein's.[10]

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[9] Emerson & Smith (1974) misidentify Adhémar Gelb as "Gebb."

[10] See Angyal (1941).

The only response by a Gestalt therapist to Henle's critique was given by Litt (1978), a former student of both Henle and Perls, and a member of the New York Institute of Gestalt Therapy. His response, which appeared as a letter to a journal editor, states that:

> I have seen the article that Gologor mentions (Henle, in press). It is, I am sorry to say, perhaps the only unscholarly work that Henle has ever essayed. Again, Perls is denigrated, not examined. Now, it is always difficult to criticize one's teacher — especially someone as highly respected as Mary Henle; but as one of the few who studied with both Dr. Henle and Dr. Perls, I feel compelled to answer her captious attack on Fritz Perls. First of all, she neglects the major source of Gestalt therapy; instead she quotes at length from Perls's pathetically whimsical autobiography... . But the personal, tongue-in-cheek witticisms of old Fritz (which she takes seriously) have little to do with Gestalt therapy. Let me make this point plain, for many misunderstand it: Gestalt therapy is *not* simply whatever Fritz Perls was saying at any given time. There is a systematic theoretical framework for this novel approach to psychotherapy... . Perls's use of Gestalt concepts may be roughly compared to the work of Wertheimer, Duncker, Luchins, etc., in problem solving, except that Perls is dealing with problem solving in a complex clinical setting rather than in a laboratory experiment. The Gestalt psychologists largely neglected the important realms of personality, psychopathology, and psychotherapy. Perls entered these areas, following, partly, Lewin and Goldstein. Perls, while not a classical Gestalt psychologist, is in the holistic integrative tradition (Litt, 1978, p. 958).

Litt's response offers a compromise position based on general grounds regarding the connection between Gestalt therapy and Gestalt psychology, but it contains two errors. The first is that the gestalt psychologists did not ignore the problem of personality as both Koffka (1935/1963) and Kurt Lewin (1935) offered a field-based personality model. Unfortunately, this line of work did not progress very far because Koffka and Lewin died too early, and the work was never taken up by their students. The second is that Litt falls prey to arguing for a connection based on theory, rather than history, and makes no mention of Laura Perls.

Barlow (1981) acknowledges Henle's (1978) and Arnheim's (1974) negative position, but maintains that:

> Only by critical analysis of certain quotations and extractions from early writings in the Gestalt literature can a meaningful conclusion be drawn as to the degree to which [Fritz] Perls adopted, developed, and integrated certain notions and concepts from Gestalt psychology (p. 35).

Barlow begins his analysis by reviewing how certain Gestalt therapists have disagreed about the relationship between Gestalt psychology and Gestalt therapy. He reviews the theoretical similarities between the two disciplines in the following areas: (a) use of the term, Gestalt; (b) holism; (c) organism-environment interaction, (d) figure-ground, prägnanz, and closure; (e) equilibrium; (f) attention, awareness, and experience; (g) here-and-now orientation; (h) psychological boundaries and neuroses; (i) mind-body unity; and (j) psychotherapy and integration. Barlow concludes that the arguments of Henle (1978) and Arnheim (1974) are "misleading" because "Gestalt psychology has influenced not only the theoretical foundations of Gestalt therapy, but also the realm of philosophical and practical applications of Gestalt therapy" (Barlow, 1981, p. 54).

Sherrill (1986) observes that the relationship between Gestalt psychology and Gestalt therapy is both ambiguous and troubled. He acknowledges the fact that Gestalt psychologists are sensitive to the errors made by Fritz Perls in his theorizing. He also admits that a fundamental error in Perls's theorizing was the tendency to confuse Gestalt psychology with the systems of Goldstein and Lewin. Furthermore, he notes the difference between Goldstein and Lewin on how organismic variables are perceived.

Sherrill's intent is to "review how Gestalt therapy can ground itself more adequately as a descendant of Gestalt psychology" (p. 54). Sherrill discusses several examples of how Perls misunderstood or misinterpreted Gestalt psychology. He presents certain differences among the Gestalt psychologists themselves, primarily among Wertheimer, Koffka, and Köhler, on the one hand, and Goldstein and Lewin on the other. Sherrill points out the fundamental disagreement between Perls's interpretation of certain principles of Gestalt psychology, and the actual principles as originally understood by their creators. His two main examples supporting this contention are the influence of organismic variables on perception, and the figure-ground principle. Despite the negative tone in his presentation, Sherrill rather cagily comments that, "At the same time that we acknowledge our differences with traditional Gestalt theorists, we can still assert them to be perhaps second-generation spiritual ancestors" (p. 61). Sherrill concludes that, "For now, we can acknowledge our spiritual ancestry in Gestalt Theory, as well as our differences with it" (p. 64).

Hurd (1987), in an unpublished doctoral dissertation, argues that the primary theoretical connection between Gestalt psychology and Gestalt therapy lays in the similarities found in the writings of Köhler and Perls. Hurd based his argument on the similarities between Perls and Köhler in the following topic sections: (a) Gestalt therapy compared to association psychology; (b) Gestalt therapy compared to behaviorism; (c) experience of internal versus external reality; (d) holistic versus dualistic approaches to therapy; (e) experience of self; and (f) thinking, emotions, and motivation. Hurd argues that Gestalt therapy and Gestalt psychology are similar in many ways. He states that Gestalt psychology and Gestalt therapy are: opposed to

association psychology and to behaviorism; focus on internal experience; holistic in their orientation; share similar views regarding the self, thinking, emotions, and motivation. Hurd's procedure is to examine each of these topic areas and cite how the respective positions of Perls and Köhler are in agreement with one another. Hurd's summary consists of making the case for a close theoretical connection between Gestalt psychology and Gestalt therapy based on science and values, shared philosophical assumptions, personal style, response to the crisis of science, and values.

Summary

There is a sense of irony experienced when reviewing the literature for and against a connection between Gestalt psychology and Gestalt therapy. One is left with the conclusion after reviewing this literature that there is some theoretical connection between the two, but to what degree remains ambiguous. At a minimum, Perls (1969/1992) admits borrowing the Gestalt concept as well as the principles of figure-background and closure from Gestalt psychology, but beyond that the validity of either position fades quickly because all the principal commentators involved confine themselves to theory and theme, rather than historical evidence.

Hurd's dissertation has several significant flaws, however. Like Henle (1978), Hurd tends to exaggerate his case, but in the opposite direction. That is, Henle focused on the dissimilarities between Fritz Perls and Gestalt psychology, while tending to occasionally exaggerate the importance of weak evidence. Hurd tends to

exaggerate the similarities between Fritz Perls and Köhler, inflates the significance of weak evidence, and does not examine any dissimilarity between the two.

Hurd's apparent ignorance of certain historical events necessarily affects his argument. Specifically, Hurd states that Perls married a Gestalt psychologist, but does not speculate on the possibility that this relationship may have influenced Perls's thinking about Gestalt psychology. Hurd also observes that when Fritz and Laura Perls met in the Gelb-Goldstein seminar that the seminar was covering material on recent research[11] conducted in Berlin, but he does not explore this material for possible clues that might lead to a revision of his thesis.

The point here is that by ignoring historical clues, Hurd ignores other possible connections between Gestalt psychology and Gestalt therapy; connections which may have equal or greater weight than his own assembly of evidence. Hurd could have explored the role of Laura Perls; he could have argued for similarities based on the 114 laws of Gestalt psychology; he could have, with equal validity, argued that Koffka (1935), rather than Köhler, holds all of the theoretical connections; he could have also made an identical argument based solely on the work of Lewin, Gelb and Goldstein, rather than Köhler. But, he does not, and this lack of historical sensitivity weakens the integrity of his argument.

Besides a sense of irony, there is also a feeling of partisanship almost palpable in this literature. For example, Henle (1978) tends to use the worst of Fritz Perls's statements as evidence to support her points. Henle's thesis, even though it is quite accurate in its overall thrust, is faulty on several counts. First, it is lopsided in its sole

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[11] Hurd does not provide a reference for this assertion.

focus on Fritz Perls, and excludes the presence of Laura Perls. Second, Henle, because she tends to use only the most damaging statements made by Fritz Perls--such as controversial comments or snippets of poetry--limits her analysis, thus excluding any historical consideration or investigation. Finally, Henle bases her argument solely on theoretical writings and statements, rather than on historical information. These last two points are particularly ironic because Henle herself was a noted historian of psychology. At present, Henle's article has never been critiqued for its errors. Yet, despite its apparent errors and hostile tone, Henle's argument is well formed, well supported, and very persuasive in its systematic analysis. It is also correct in its conclusion because Fritz Perls was not a direct link to Gestalt psychology. Consequently, it is understandable why Henle's argument has stood as the definitive historical statement accepted by historians of psychology.

All of this, however, naturally forces one to beg the question as to what the real issue was behind Henle's subjective analysis. One may speculate that she was personally offended by Perls and Gestalt therapy, and so aimed more at taking revenge upon an enemy than in objectively analyzing the situation. Henle (1980) argues that Gestalt psychology failed to take root in the United States because of the dominance of behaviorism, the failure of the Gestalt psychologists to obtain and train graduate students, and was the victim of three misunderstandings that the American psychological community had about Gestalt psychology. These misunderstandings are confusing Gestalt psychology with Gestalt therapy, wrongly identifying Gestalt psychology as a form of nativism, and only partially understanding the Gestalt

principle of isomorphism. It is telling that Henle places Fritz Perls and Gestalt therapy at the top of her list of misunderstandings. Concerning this, she writes:

> And it was hardly likely that Gestalt psychology would be understood, even when it was listened to politely or with respect. Misunderstandings may be seen as both cause and effect of the less than overwhelming influence that Gestalt psychology had in America. I would like to discuss a few of these. The interesting thing about these misunderstandings is that, except for the first I will mention (which I have only recently begun to take as something other than a bad joke), they are not new... . The most grotesque current misunderstanding of Gestalt psychology is the notion that it has some relation to Gestalt therapy. I will not discuss this distortion of history and of ideas but will merely state that there is nothing in common between these two developments. The reader is referred to a previous analysis of the problem (Henle, 1978) (Henle, 1980, p. 179-180).

Henle's language reveals her personal feelings: any possible relation between Gestalt psychology and Gestalt therapy is a grotesque joke, and this view does not allow for any other possibility. There is also some evidence to support the contention that Henle, Arnheim, and Asch deeply resented Fritz Perls because of his appropriation of the Gestalt name, and the subsequent confusion in the popular mind regarding the distinction between Gestalt therapy and Gestalt psychology. A letter from Rudolf Arnheim to Mary Henle congratulating her on her 1975 APA address, which was later to be published in 1978, provides some insight into the personal attitudes involved:

> Dear Mary,
> I was thoroughly delighted and amused when I saw the job you did on Perls. The quiet professionalism of your surgical technique gives the perfect relief to the embarrassing chatter of this charlatan. I hope your paper will be published at a prominent place so that we have something to refer to when people ask what is wrong with gestalt therapy and how it differs from the genuine article. That you were will [sic] to subject yourself to a careful reading of the stuff shows true scientific devotion. It must have been an ordeal. In principle it is possible that his clinical procedures work in practice; but my mind revolts against the possibility that such shoddy talk could go with wisdom in the practical handling of people (Arnheim, 1975).

Writers of Gestalt therapy, on the other hand, tend to ignore not only Fritz Perls's own autobiographical comments minimizing his connection to Gestalt psychology (Perls, 1969/1992), but they also ignore the published interviews with Laura Perls in which she recalls her graduate work in Gestalt psychology; both valuable clues for further investigation (Bernard, 1986; Humphrey, 1986; Kudirka, 1992; Rosenblatt, 1992).

The Gestalt therapists also appear to have missed the fact that Köhler himself disavowed any connection between Gestalt psychology and Gestalt therapy. This is particularly poignant, given that Hurd (1987) bases his entire dissertation on similarities between Köhler and Fritz Perls. Writers in Gestalt therapy also tend to ignore that not only Köhler, but also Laura Perls herself disapproved of the use of the term Gestalt in the name of Gestalt therapy (Rosenblatt, 1991; Sherrill, 1986; Stoehr, 1994). Plus, Ralph Hefferline (1910-1974), one of the co-authors of the initial and most famous work on Gestalt therapy (Perls, Hefferline, & Goodman, 1951), denied any connection between the two disciplines beyond the figure-ground principle (Hefferline, 1962). These negative assertions made from within their own original inner circle should, at the very least, give Gestalt therapists pause for more reflection, but, as of this writing, they have not.

In any event, none of the articles reviewed address these details or decisively solve the problem. Thus, the problem of ascertaining whether or not there is a historical connection between Gestalt psychology and Gestalt therapy remains unresolved. The common failing of the articles reviewed is that they all argue from theoretical and thematic similarity or dissimilarity, tend to skew evidence in favor of

their respective positions, and totally eschew an historical approach. The real issue that remains to be addressed, however, is not one of theoretical similarity or dissimilarity, and but of historical descent.

III. METHOD

The Historical Prejudice Against Laura Perls

The apparent historical prejudice in favor of Henle's position, coupled with ignoring the presence of Laura Perls as the historical bridge between Gestalt psychology and Gestalt therapy, leads to deeper questions about the nature of American academic psychology, and the way its history has been written up to this time. The first and most obvious question is: why has the historical role of Laura Perls been ignored by historians, Gestalt therapists, and especially by Gestalt psychologists such as Henle and her associates? There are several possible answers for this oversight.

The first is that Laura Perls left behind a very minute published corpus, and so her reputation did not extend beyond the circles of American clinical and folk psychology.[12] Second, she may have been ignored because Gestalt therapy may be, for the most part, synonymous with Fritz Perls in the minds of many historians of psychology. (This is a distinct possibility, given that the third co-founder of Gestalt therapy, Paul Goodman, also remains relatively unknown to historians of psychology.) Third, Gestalt therapy intentionally positioned itself in the tradition of folk psychology of the fringe, and far away from mainstream academia. It has been the case historically that mainstream academic psychology tends to eschew anything related to folk psychology. Finally, the fact that there was a historical intersection

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[12] While folk psychology has many definitions, it is defined here as a mythic and visionary tradition of using intuitive and spiritual practices for transformation of inner experience in service of character development, mental healing, and self-realization. In American folk psychology, these methods are usually drawn from depth psychology (Taylor, 1999a), or have been corroborated by it.

between Gestalt psychology and Gestalt therapy has been ignored because the received view of the history of American psychology is based on certain assumptions regarding what is relevant for inclusion in its own history. Two points are apparent.

The first is that other historians have made no further inquiry since an established authority in the history of psychology has negated any connection between Gestalt psychology and Gestalt therapy. The case in point here is that Köhler rejected Gestalt therapy when it first came out in 1951, and it is most likely that he shared his feelings with his students and colleagues. At the very least, given that Köhler was the last original Gestalt psychologist remaining, and possessing an international reputation, his opinion must have been quite influential on his students and colleagues. Following him, the second-generation leaders of Gestalt psychology—Solomon Asch, Rudolf Arnheim, and Mary Henle--also strongly rejected it. Max Wertheimer's son, Michael, himself a historian, also rejected it (1970/1979). Therefore, what may be evident here is not a prejudice against Laura Perls, but an antipathy against Gestalt therapy and Fritz Perls on the part of American Gestalt psychologists.

This negative attitude apparently stands even today, as evidenced by a recent casual inquiry made to three leading historians of German psychology: Anne Harrington (personal communication, December 15, 2000) at Harvard University, Michael Wertheimer (personal communication, December 20, 2000), professor emeritus of the University of Colorado at Boulder, and Mitchell Ash (personal communication, December 15, 2000) at the University of Vienna. Each stated that they did not believe there was a connection between Gestalt psychology and Gestalt

therapy. Ash, in particular, commented that he doubted that the topic was a legitimate one for doctoral research. Curiously, in his book on the history of Gestalt psychology, Ash lists Laura Perls by her maiden name (Lore Posner) in an appendix of all the graduate students who took their doctorate under Gelb, but he does not mention that she was to become a co-founder of Gestalt therapy; and this even after he had personally interviewed Laura while researching the book.

It must be strongly stated that what has just been presented is in no way a criticism of Harrington, Wertheimer, and Ash. One is left curious, however, as to why these scholars would so quickly dismiss the possibility of investigating the question further. One possible reason these historians, or others like them, so readily dismiss the notion is that they accept the prevailing historical view based on the original assessment by Henle (1978). This is most likely given that there has appeared no compelling evidence to the contrary until Ullman (1997) as the lone exception.

Historical evidence documenting Laura Perls's membership in the ranks of Gestalt psychology is presented further below will be examined using the historiographic method developed by Eugene Taylor.

The Historiographic Model of Eugene Taylor

The historical research method is the critical examination and analysis of public and private records, archival materials, personal memoirs, interviews of witnesses, government documents, and objects. The historical method is the gathering and examination of evidence that leads to historiography as the analysis of evidence and the writing of history. Historiography is the educated reconstruction of the past

based on such records and objects as well as the study of historical methods (Gottschalk, 1950/1969). Taylor's (2001) historiographic model for conducting historical research is a unique variant in the prevailing methods currently being used in the history of psychology because it (a) derives from the comparative study of religions more than the humanities; (b) blends approaches from comparative religion, the history of humanities, the history of medicine, and the history science; and (c) its goal is to use history to challenge the hegemony of the reductionistic method.

Of particular value is Taylor's theory that psychology is actually composed of not one, but three streams or traditions (Taylor, 1999a, 2000b). His theory is that because there are many definitions of psychology in common currency, there must be more than one history and, therefore, more than one epistemology. According to his model, three different histories of American psychology exist concurrently. These streams are: academic experimental psychology, clinical psychology, and folk psychology. The first stream is the history of psychological research and application based on the scientific ideal and derives from the German experimental tradition. The second stream is that of clinical psychology, which is not derived from experimentation, but rises from the French clinical tradition and is related to mental testing, counseling, psychotherapy, clinical psychiatry, nursing, social work, and pastoral counseling, and generates its own separate literature. The third stream, American folk psychology, is a populist, intuitive tradition of psychospiritual development based on mental healing and character development. Taylor states that:

> The point... is to suggest that everything that we see today does not come from the same source and that what exists in American psychology always has a lineage. It was as if we were to meet a group of people all named Smith, the various members of which came from

> three different families that were not equally represented. It would be easy to confuse any one individual and to presume they all came from the same family, or from the ranks of the most dominant family, if we did not inquire carefully into lineages... . The model of the three streams has a certain pragmatic value in that it helps to dispel a common myth in psychology today that there is only a single history of the discipline... . Psychology exists in a condition of multiple realities, with different lineages and domains of operation (2000b, p. 15).

While the method of historical research is not a generally accepted form of inquiry in psychology, it has an accepted place in the humanities. Two of the most famous examples in the history of psychology that have emphasized the historical method are the works E.G. Boring (1886-1968): A History of Experimental Psychology (Boring, 1929/1950) and his lesser well known, Sensation and Perception in the History of Experimental Psychology (Boring, 1942). In 1954, the Social Science Research Council officially recognized the use of historical methods in the social sciences (Social Science Research Council, 1954). The historical method is usually not discussed in many texts on research methods in psychology, and so remains relatively ignored, except on rare occasions. For example, Borg & Gall (1989), in their book on research methods used in educational psychology, devote a chapter to the application of the historical method.

The problem is whether or not the historical method is scientifically legitimate as compared to the experimental method. This is a natural question because the historical method derives from the humanities, while the experimental method derives from the natural sciences. The historical method, when compared to the experimental method of investigation, has similarities and differences in both technique and philosophy. The major difference is that the historical method is idiographic while the

scientific method is nomothetic (Nagel, 1959). A nomothetic method seeks to establish abstract laws for indefinitely repeatable processes. An idiographic approach aims at understanding the unique and the non-reoccurring (Nagel, 1959, p. 203). An example of an idiographic approach would be Allport's (1942) use of personal documents in psychological science. Even though the aim of the historical method differs from the experimental method, it shares a similar aim, in that it is a systematic, disciplined inquiry aimed at producing knowledge. Consequently, as both the historical method and the experimental are empirical, the historical method may not be a science, but it is necessarily an equally valid source of knowledge in psychology.

The historical method tends to be misunderstood by adherents of experimental research when they do acknowledge it. For example, the method of historical research is categorized by Borg & Gall (1989) as part of the qualitative research tradition. This, however, is most likely a mistaken notion based upon an incomplete understanding of the historical method in the humanities since Herodotus (c. 485-425 BC) and Plutarch (c. 40 BC-120 AD), and its most recent applications by contemporary historians of the comparative history of religion (see, for examples, Barzun, & Graff, 1957/1977; Campbell, 1949; Eliade & Kitagawa, 1959; Wach, 1958). The root of the misunderstanding is that the experimental method and the historical method each derive from different and separate epistemological traditions. Historiography is not simply a qualitative method, as compared to a quantitative one, but is a completely different species altogether.

Collingwood (1946/1972), taking a slightly different position, argues that history is a science, although it is a specialized kind. Collingwood maintains that

history is a scientific variant because it is based on an organized, inferential inquiry of evidence possessing four characteristics: (a) it is scientific because it poses questions to be explored and answered, which distinguishes it from mythic narrative; (b) it is humanistic, in that it investigates human activities at specific moments in time; (c) it is rational because it bases its answers upon tangible evidence; and (d) it is self-revelatory, in that its investigations into human activities yield knowledge about the human condition (Collingwood, 1946/1972, p. 18).

While Collingwood makes very cogent points about the similarity of the historical method to the scientific method and is perhaps a special case of the latter, he neglects the fact that the historical method comes out of an entirely different epistemological tradition. This does not necessarily mean the historical method is any less rigorous or valid than scientific procedure, only that it is different as it stems from an entirely different intellectual and philosophical context. As Taylor (2001) states:

> We wish to point out to the [empirical] methodologists that objectivity in historical and philosophical psychology is different from that of psychology as an aspiring science, the standard of which remains numerical and quantitative. The epistemologies of the humanities and the sciences remain quite different (p. 1).

Traditionally, the historical method of research has been accepted in the history of psychology as a valid method of investigation because, even though its aims differ from the nomothetic mode of inquiry, it is aligned with it by its systematic approach based on theory, hypothesis, and evidence. It belongs to both the social sciences and the humanities. This means that the historian is a social scientist to the degree that he

or she uses a rigorous method based on the systematic analysis of such evidence (Krug, 1967).

The Historiographic Method of Eugene Taylor

Taylor's method is a synthetic approach that combines procedures used in the study of comparative religions with standard methods of archival investigation used in the history of medicine, and the history of science. The result is a tool for the analysis of historical dynamics behind the development of American psychology and psychiatry (see, for examples, Taylor, 1985; Taylor, 1996a; Taylor, 1999a).

In general, Taylor (1988) argues that methods drawn from the study of comparative religions offer three practical dimensions for the study of the history of psychology. The first dimension is training in the historical attitude. The historical attitude is both a set of skills and a distinct historical perspective based upon the authentication and critical analysis of historical documents. Historical analysis includes hypothesis formation, testing, and verification, and yields conclusions that must withstand the scrutiny of, and consensual validation by, a critical community of scholars. Taylor observes that psychologists have typically been weak in historical method and analysis because, being grounded in the statistical method, they have tended to view events ahistorically.

Second, researchers in comparative religions are particularly skilled in other language, which gives them an unusual sensitivity to issues of meaning, cultural context, and translation. Research psychologists, particularly in the United States, on the other hand, tend to be fluent only in their native language (usually English) and,

until recently, were required to master only French or German. This makes them relatively isolated from developments in other cultures (Taylor, 1988).

Third, researchers using the methods of comparative religions have been extremely sensitive to the presence of researcher bias, especially when studying other cultures. That is, the researcher must remain constantly vigilant about bias from his or her cultural experience, context, and values when examining historical subjects in other cultures, or other historical eras. Conversely, psychological researchers in our own culture tend to overemphasize method at the expense of the subject being investigated and cannot sufficiently account for personal bias, although this is often recommended and attempted. The historical result in psychology has been an over-intellectualization of the subject being studied, a pseudo, hyper-objectification of psychology's subject matter, and an unconscious bias for a particularly Germanic view of science (Taylor, 1988).

Taylor's version of the archival method is both the science and art of locating, studying, and interpreting primary documents in the pursuit of reconstructing, corroborating, and understanding a historical subject. As a scientific method, it is based on factual evidence derived from materials culled from archival repositories, private holdings, and public information. The rigor of Taylor's archival method comes from the study of concrete evidence (i.e., original documents) combined with a systematic examination of those documents and related sources guided by hypothesis formulation and testing. On the other hand, it is an art, in that the interpretation and understanding of a given subject often requires an unusual depth of empathetic modeling of the subject and his or her historical situation to arrive successfully at an

analysis with a strong degree of confidence. While its procedures generally consist of the discovery, identification, acquisition, and verification of original documents combined with research into the background of the subject, as it arises in part from religious studies, the method permits a wider acknowledgement of human experiences other than the pathological and the normal than those found in the history of science (Taylor, 1999b). Examples would be the comparative study of different definitions of psychology in the same culture, the analysis of altered states of consciousness for the purpose of healing as well as self-realization, and conceptions of personality and consciousness across cultures related to spiritual awakening and transformative mystical states. The historical relation of Gestalt psychology to Gestalt therapy would fall under this purview because from the historiographic view both Gestalt psychology and Gestalt therapy must be seen in the context of the whole culture in which they flourished.

Consequently, Taylor's historiographic approach is a hybrid that systematically applies the historical method derived from the humanities and the comparative study of religion to the history of psychology. The key to the method, however, is the location and analysis of archival documents that serve as evidence for hypothesis testing and argument. The essential archival document verifying a historical connection between Gestalt psychology and Gestalt therapy is the actual doctoral dissertation completed by Laura Perls in 1932. This document was located in France in a private collection of personal papers of Laura Perls.

IV. ANALYSIS OF THE PRIMARY ARCHIVAL DOCUMENT

Background

Laura Perls was awarded a Doctorate in Psychology from the University of Frankfurt in 1932 under her maiden name of Lore Posner. Her dissertation was entitled: "The phenomena of simultaneous contrast and the perception of field illumination" (Posner, 1932; see Appendix). Adhémar Gelb, with Karl Wilhelm Meissner being the other committee member, supervised her research.

Meissner was born in Reutlingen, Germany. He graduated from the University of Munich, taught at the University of Zurich from 1916 to 1925, and then took a position at the University of Frankfurt from 1925 to 1937. He was originally an astronomer and who later turned to physics. His investigations into color wavelengths positively determined the presence of sulfur in the sun and he came to be considered as being the father of modern spectroscopy. Meissner left Germany for the United States in 1937 and worked at the Worcester Polytechnic Institute for two years, and then at Purdue University. He died while crossing the Atlantic on a return trip to Germany after the end of World War II (Layfayette Journal Courier, 1959). Gelb's role in the history of color research in Gestalt psychology will be detailed further below.

Description of Gestalt Psychology

The German term, Gestalt, is nearly impossible to translate adequately into English.[13] Some synonyms usually given to describe it are "form," "structure,"

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[13] American psychology tended to substitute the word "configuration" for Gestalt beginning in the 1920s upon the suggestion of Titchener. Titchener's student, Helson (1898-1977), wrote his

"whole," "unity," or "configuration." It typically refers to an object composed of parts but, by its very nature of being a unified form, the object as a whole is different from the sum of its parts. To attempt to analyze a Gestalt by separating it into its components parts necessarily destroys its wholeness, its Gestalt quality. An example would be a line drawing of a square. The image is composed of four lines at right angles; but it is more than just four connected lines. It is a form that encloses a space thus forming a distinctive figure against a background (Brennan, 1991). It stands out as a square; to separate the lines would destroy the square's squareness.

Gestalt psychology came into being in response to the problem arising from the experimental psychology of that time that combined psychophysics and associationism. This combination failed to make substantial scientific progress because its underlying atomistic epistemology--based on the examination of discrete sensation--was flawed. The Gestalt psychology of Wertheimer and his associates arose as both a criticism of the epistemology of associationism, and its application to experimental design. Gestalt psychology argued early on that associationistic psychology was defective on two counts:

> First, the appearance of any part of the (stimulus) display may be changed significantly by changes elsewhere in the stimulus display. Second, any normal scene has apparent properties (e.g., depth, shape) that go beyond the apparent properties of points or small homogenous patches (e.g., color, direction, and extension). In general, the phenomena that appear depend on the configuration of all (or some sizable subset) of the stimulus pattern, rather than on local values of the individual points of stimulation, will be called the phenomena of perceptual organization (Hochberg, 1974, p. 179-180).

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dissertation on Gestalt psychology. It was then published as a four-part article series (Helson, 1925a; 1925b; 1926a; 1926b), and later as a book.

Hochberg is saying that stimuli operate as interactive units rather than as discrete independent units, and that the nature of the whole phenomenon encompassing the various stimuli affects the individual stimuli. This is the essence of Gestalt theory and the essential premise of Gestalt psychology; the whole determines the nature and function of the parts. Wertheimer (1924/1944) gave his version of Gestalt theory in a famous lecture given before the Kant Society in Berlin:

> Briefly characterized, one might say that the paramount presupposition [of European epistemology and science] was to go back to particles, to revert to piecemeal single relations existing between such individual particles or elements, to analyze and synthesize by combining the elements and particles into larger complexes. Gestalt theory believes it has discovered a decisive aspect in recognizing the existence of phenomena and contexts of a different—of a formally different—nature. And this is not merely in the humanities. The basic theses of gestalt theory might be formulated thus: there are contexts in which what is happening in the whole cannot be deduced from the characteristics of the separate pieces, but conversely, what happens to a part of the whole is, in clear-cut cases, determined by the laws of the inner structure of its whole (p. 84).

What Wertheimer is getting at is that Gestalt theory reverses the scientific view held up until that time. Köhler (1944), in his obituary for Wertheimer, asserts that William James (1842-1910) and other psychological thinkers of the late nineteenth century had serious misgivings as to the validity of psychology's forced migration into experimental physiology. They suspected that the analytical epistemology upon which experimental psychology was based was questionable, and most likely destructive to psychology as a whole, but they did not have an alternative with which to replace it. It was Wertheimer who discovered the alternative according to Köhler. European science had been based on a reductionistic method; a bottom-up view, as it were. Wertheimer opposed this with a holistic, top-down perspective. Wertheimer

was not just suggesting a change in scientific method, but actually proposed an entirely different epistemology altogether (Garber, 1964).

Besides proposing a holistic approach to investigating phenomena in place of the atomistic approach, the Berlin-Frankfurt school also maintained that: (a) a phenomenon could be explained, entirely or, for the most part, by a single mechanism; (b) that atomistic, associationistic theory was invalid; (c) that further knowledge could only be gained by changing the underlying philosophy of the scientific method; and (d) that perceptual phenomena were reflected isomorphically in the brain's electrical fields (Hochberg, 1974).

The primary mission of the research conducted by the Berlin-Frankfurt school in the 1920s, according to Koffka (1935), revolved around answering an essential question: why do things look the way they do? Koffka devoted a large portion of his book in telling the story as to how the Gestalt psychologists investigated this question. It will be seen that Laura Perls's own experimental investigations into color contrasts made a small contribution to answering this question.

Experimental Investigation of Color Perception by Gestalt Psychology

Members of the Berlin-Frankfurt school investigated the phenomenon of color in terms of several theoretical principles drawn from Gestalt theory. These principles were Gestalt, prägnanz, and the figure-ground phenomenon. The term, Gestalt, refers to the primacy of the whole over its parts. Prägnanz is the tendency of gestalten to organize into the best form possible given prevailing conditions. The Danish experimental phenomenologist, Edgar Rubin (1886-1951), discovered the figure-

ground phenomenon in 1915.[14] This is the principle that objects of perception are perceived as separate from the backgrounds against which they occur; a figure is more thing-like as it is organized by a contour boundary that distinguishes it from its background (Petermann, 1932; Rubin, 1915).

Studies in color perception conducted in Germany prior to the arrival of the Gestalt psychologists of the Berlin-Frankfurt school tended to focus solely on the effects of brightness changes in the overall field on color hues. This changed, however, when Gelb & Granit (1923) took a different approach to the problem and investigated, based on the implications of Gestalt theory, whether or not primary hues of colors were influenced by overall brightness changes when the hues were seen as part of a closed figure against a background. Gelb & Granit's (1923) interpretation was that the figure--or inner field--becomes a ground for the perceiver and, consequently, there is greater resistance to perceiving it. That is, the eye, when it is focused on a figure against a ground, resists making the figure into a new background, and so a higher light intensity is needed to overcome the resistance (Hartmann, 1935).

Gestalt psychologists also experimented on the problem of color contrasts. Color constancy is the phenomenal characteristic of a color to remain stable with the change in illumination. The problem of color contrasts is an important area to understand because it is was the specialized focus of Gelb, and Laura Perls's own experimental research.

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[14] Ruben is officially credited with the figure-ground discovery although previous psychologists had recognized the phenomena, such as James (1890) and Jastrow (1900).

The state of color perception research in the Berlin-Frankfurt school in the 1920s appears to have been a series of experiments aimed at testing and validating certain principles of Gestalt theory as applied to basic problems of color perception, including the problems of color contrast and color constancy. The researcher who apparently published the most on color perception was Gelb. Gelb was already conducting research by the late 1910s, and doing so in conjunction with Kurt Goldstein in their studies of visual and perceptual disturbances of brain-damaged veterans. Some of the more prominent features of Gelb's work will now be examined.

Adhémar Maximillian Maurice Gelb (1887-1936)

Gelb, whose name means, "yellow," received his doctorate under Carl Stumpf at the University of Berlin in 1910. He then taught first in Berlin and then later, in 1912, at the Academy of Social Sciences at the University of Frankfurt am Main. He then worked with Kurt Goldstein at the latter's Frankfurt Hospital for Brain Damaged Veterans, and with Wertheimer at the University of Frankfurt.

Through his work with Goldstein, Gelb published papers on the effects of brain lesions on perception, speech, and color vision. He also conducted investigations into figure-ground and color contrasts. These studies were conducted in 1923 in partnership with Ragnar Granit (1900-1991). In conjunction with Wertheimer, Gelb presented experimental findings on space and time perception to the Society for Experimental Psychology in 1914 that supported Wertheimer's Gestalt laws. Gelb also adapted Wertheimer's tachistocope experiments as a diagnostic tool to augment standard tests of attention, concentration, and memory given to patients at

Goldstein's clinic (Ash, 1995). Gelb will most likely be remembered for his discovery of the Gelb effect; a relatively minor phenomenon that has been of intermittent interest to psychophysicists since its discovery in 1929. Koffka (1935/1963) describes the experiment conducted by Gelb that yields the visual effect:

> Somewhat simplified, it is like this: in a dark room a perfectly homogenous black disk is rotated; this disk, and nothing else, is strongly illuminated by a projection lantern. Under these conditions the disk looks white and the room black. Then the experimenter holds a small piece of white paper close to and in front of the rotating disk so that it falls within the cone of light. At the same moment the disk alters its appearance, and looks black (p. 245-246).

Koffka attributes the Gelb effect to the presence of field variables, and their interrelationships.

A good portion of Gelb's research, however, dealt with color constancy of which his discovery of the Gelb effect is one prominent finding. Color constancy is the tendency for the color of objects to appear stable despite changes in lighting conditions. Gelb sought to differentiate the Gestalt view of color constancy from other theories existent at the time. One of his primary targets was the theory of color constancy proposed by Ewald Hering (1834-1918). Hering's first experimental studies dealt with the perception of visual space, and were followed by studies in color. His theory is considered by historians to be "nativist" in that he believed that there were inherent abilities in the human organism that allowed it to function. Hering believed that each point on the retina of the eye possessed three local signs for the perception of height, left-right discrimination, and depth (Boring, 1929/1950; Hering, 1964). Hering's theory opposed those of Thomas Young (1773-1829) and Hermann

von Helmholtz (1821-1894) who argued that the number of physiologically-based, color percepts must be equal to the phenomenologically observed colors.

Hering's method is based on verbal reports of subjects in the process of perceiving colors. He argued for two pairs of four color oppositions: yellow-green and red-green based on the fact that his observers, when asked to identify unique colors, always responded with the basic four: blue, green, yellow, and red (Graham, 1959). Hering's theory is now known as an opponent theory because he believed that light increased the amount of some chemical substance in the eye's receptors. He theorized that the eye was capable of only two kinds of changes to adapt to increases and decreases in light, and that these changes were directly opposed to one another (Wasserman, 1978).

Hering's theory of color contrasts states that they are an induction effect operating on single points on the human retina. From the Gestalt point of view, this is an atomistic conception. The inductions that come about at given local points combine into a total effect, and are perceived as a particular hue. This total effect, however, results "...from the superposition of the single induction values which enter the contrast-field from the various points of the surrounding parts" (Petermann, 1932, p. 173).

Gelb attacked Hering's theory of color constancy in 1929 in an examination of David Katz's (1884-1953) experiments in color contrast (Gelb, 1929/1938). By doing so, Gelb attempted to correct both theories by aligning them with Gestalt theory. Gelb's (1929/1938) study is interesting on several counts. First, it is a systematic examination and summary of articles and books written up to that time, and so may

be construed as being a state-of-the-art summary of Gelb's own knowledge and position at that time. Second, it was written in 1929 while Laura Perls was Gelb's doctoral student. As will be seen further below, some of the experiments Gelb conducted correspond quite closely to Perls's own experiments.

Gelb's way of examining the theories of Hering and Katz in this article is also noteworthy. His style is to first describe some of Katz's experiments, change those experiments to conform to his own parameters, and discuss the theoretical significance of the results. This is fairly standard experimental procedure, but the methods of data collection used in the experiments conducted by Hering, Katz, and Gelb all depend on direct observation and self-reporting of perceived visual experience. This was a standard method used by the Berlin-Frankfurt school which they called experiential observation, although it is also known as phenomenology, within a framework of experimental design (see Boring, 1953, Henle, 1979, and Koffka, 1924). This will be an important point to remember further below when Perls's experimental work is presented, as she used the same method (Posner, 1932).

Examination of the Primary Archival Document

Purpose

Perls (Posner, 1932) opens her dissertation with the idea that the observation and examination of simultaneous light and color contrasts has been performed by various methods (although she does not describe the methods), and that the two "clearest and most expressive" phenomena are those of colored shadows, and the

florescence effect (p. 1).[15] "Lively"--meaning vividly noticeable--contrast effects are typically produced using Hering's hole method[16] (p. 1). Perls notes that this method works especially well using a nuance apparatus. (See Figure 1.) A nuance apparatus is simply a vertical, rectangular box. The front of the box has a door spanning its length. The top of the box has a hole through which one can peer downward. In the lower half of the box is a mirrored shelf that can pivot so as to adjust the angle of reflection of what is in front of the box upward to the view hole. In other words, it is a reflective viewing box that serves as a reduction screen. Perls makes the point that by using either of these methods the color white can be altered from white to almost black depending on the intensity of illumination.[17]

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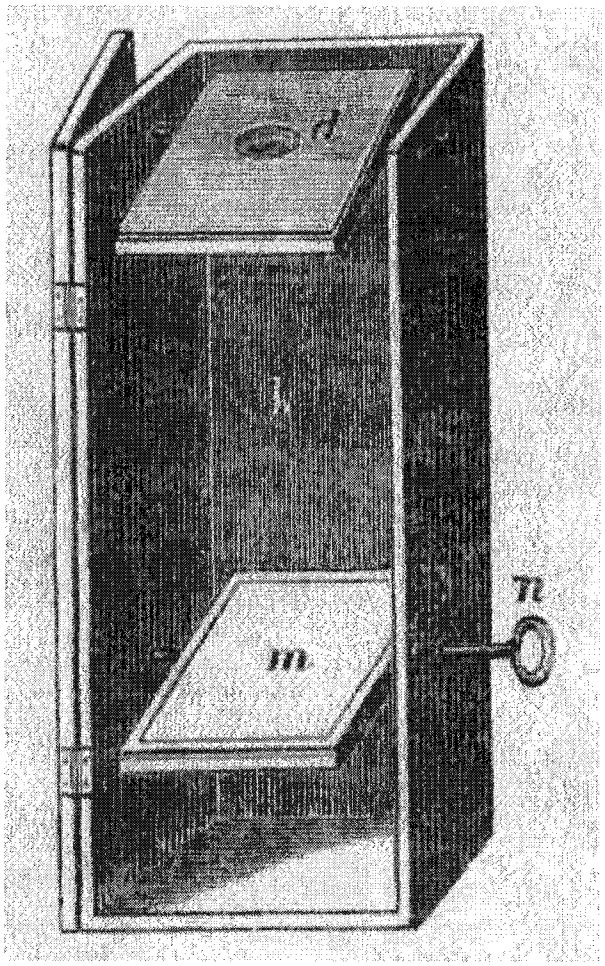
[15] Helmholtz (1924) distinguishes between phosphorescence and fluorescence; the former is when an object is self-luminous after overall illumination is discontinued; the latter is self-luminosity of an object only while it is illuminated as found in such materials as acid sulphate of quinine, uranium glass, horse-chestnut bark extract, or amber (p. 53). Perls neither explains the flor-contrast effect in any detail, nor does she use any fluorescent substances in her investigations. It must be understood that Perls is not working with actual fluorescence, but fluorescence as termed by Evans (1974). An object becomes fluorescent "when the brilliance of the stimulus exceeds that of its surrounding or comparison stimuli" (Evans, 1974, p. 97). In other words, it is the tendency for an object to appear to glow because its degree of illumination is far greater than that of its background. Perls calls this "flor contrast" and devotes a set of experiments to its investigation.

[16] The hole method is the use of a reduction screen to alter the visual perception of colors by eliminating their surrounding background. Gelb used this technique in several of his studies.

[17] Katz (1935/1970) distinguishes surface colors from film colors. Surface colors are those that are thought to be invariable. Film colors do not have any fixed quality of color. My interpretation of the difference is that surface colors are perceived as being more "solid," while film colors appear more

Figure 1. Nuance Apparatus. From The Karl Brown University of Michigan Papers, Box M53, Folder #4, Archives of the History of American Psychology, University of Akron, Ohio.



There are two possible explanations for this phenomenon: the central process explanation offered by Helmholtz and the peripheral process theory by Hering. Perls notes that most researchers concern themselves less with which process explains

"ethereal." Surface colors can be perceptually converted into film colors, and vice versa, by viewing them through the aperture of a reduction screen.

constancy when working with neutral colors (i.e., white, black, or gray), as they believe the key to the riddle in this case is the phenomenon of contrast. This belief in constancy being fundamental to the contrast problem is held regardless of the method of investigation of neutral colors; using Hering's hole method with a nuance machine or using a reduction screen.

Perls moves her discussion into the area of color constancy by relating that Oswald Kroh[18] (1887-1955) argues that when in the case of perceiving colors that contrast perceptions are not about contrast per se, but about color constancy (Kroh, 1921).

According to Perls, both Katz and Hering ask the same question regarding color: how come changes in light and limited changes in color do not affect the perception of color?

Perls states that Katz understands the term constant (or constancy) differently than contrast, and that both of these differ with Kroh's understanding of the same terms. Even though Perls does not elaborate on this difference, she wants to know whose view is correct. To begin working toward an answer, Perls comments on an experimental question posed by Katz: is only the strength and quality of the retinal stimulus important or does the relatively independent, inherent, color of the external field also helps to create contrast? She describes Katz's experiment to answer this

[18] Kroh was a research associate of G.E. Müller, and later the first professor of psychology at the Free University of Berlin in (Haupt, 1995).

question, and that Katz concluded that the contrast effect is a function of the strength and quality of retinal excitement.[19]

Perls then recounts an experiment by Kravkov & Paulsen-Baschmakova (1929) who also concluded that the actual or inherent colors do not contribute to contrast. Kravkov & Paulsen-Baschmakova conducted 57 trials with 16 observers who viewed two gray discs against two green backgrounds. After the subjects individually agreed that the gray discs were of equal color values, one green background was made brighter, and the subjects were asked to judge the equality of the gray disc tones. Ninety-three percent of the subjects could not distinguish any change in gray tone.

Perls goes on to point out that Kroh (1921) found through experimental investigation that colored illumination is always more influential than a colored external field of the same retinal value. Kroh concluded from his experiments, which Perls describes in her text, that the internal field--the color appearing through a hole aperture within the external field of an umbrella--is more strongly influenced by the surrounding colored lighting as opposed to the actual color of the external field of the "same retinal effect." Kroh found that the quality of the color contrast of the internal field was affected more by the field illumination than the external field being an adjacent surface color. The question is, of course, whether the overall room lighting or the adjacent external field of the color being viewed causes change in color. Kroh

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[19] While Perls does not define the term, "retinal excitement," it is presumed here to be the degree of nervous stimulation of the retina and optic nerve caused by a given amount of light.

attributes the color perception to the nature of color constancy rather than contrast. This conclusion differs from that of Kravkov & Paulsen-Baschmakova (1929).

Perls turns to an article by Gelb that criticized the work on color constancy by Kroh and E.R. Jaensch[20] (1883-1940) as a way of clarifying the discrepancy. Gelb's opinion, according to Perls, was that their explanations of color constancy were made in "a totally reckless manner" (Posner, 1932, p. 10). Essentially, Gelb criticized Kroh's designs and concluded that they were actually contrast experiments rather than constancy experiments. That is, Gelb's judgment is that Kroh "overlooked the radical difference between the outer experimental conditions under which, on the one hand, one had colored shadows and, on the other hand, appearances of color constancy manifest" (Posner, 1932, p.10). The crux of the problem plaguing Kroh's experiments, Perls observes, is that Kroh's method is:

> ...based on a method of especially lively contrast appearances which were mentioned already by Hering. Just the same, as in Hering's experiment, the observer is also in a color illuminated room in Kroh's experiment, from which a hole color is observed which undergoes a contrast from its surrounding colored illumination. If Kroh found that an internal field was influenced stronger through the colored lighting of its surrounding than through a colored external field of same retinal effect, then that means that Kroh was not dealing with the phenomena of constancy of color, but rather with the necessity to differentiate within the phenomena of contrast (Posner, 1932, p. 10).

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[20] Jaensch, although he is mentioned, is not referenced by Perls, nor are Gelb's criticisms given. Jaensch was an experimental psychologist at the University of Marburg who studied color perception, eidetic memory, and adolescent psychology. After the rise of Hitler, he published articles about how to indoctrinate youth into Nazi society, and conducted studies to validate Nazi racial ideology (see Boder, 1946; Jaensch, 1939).

Gelb's solution to this problem, according to Perls, is procedural. Gelb asserts that one should first visually examine colored shadows in which a contrasting external field is exposed to colored lighting, and then compare that visual observation to contrast effects produced by real (pigmented) colors of the external and internal contrast fields viewed as surface colors under equal lighting conditions.

Perls then cites the work of Helmut Bocksch[21] (1927) who agrees with the air-light hypothesis[22] of Karl Bühler (1879-1964), while maintaining that hole colors are not independent of the overall lighting.[23] Bocksch, according to Perls, has long known that the same hole color will appear totally different depending on the illumination of the hole umbrella. Bocksch, however, does not recognize this phenomenon as contrast because it is influenced through the overall lighting of the room. Gelb's hypothesis is that, in this case, contrast was not solely determined by the strength and type of retinal excitement, but more so by the given illumination of the viewing room. Consequently, Gelb assigned the task to Perls to experimentally determine if, and to what degree, a room's illumination, and its arrangement contribute to contrast.

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[21] Bocksch was an original member of the Vienna Psychological Institute, and then a research assistant under Karl Bühler (1879-1964) at the Technische Hochschule in Dresden.

[22] Perls does not explain the air-light hypothesis, but is referring to the idea that illumination in empty space is caused by the reflection of particles in the air (Katz, 1935/1970).

[23] Bocksch (1927) conducted a series of color constancy experiments based on Hering's original 1905 experiments using a photometer. Bocksch's findings showed (a) constancy exists only to a small degree; (b) color perception is a function of the presence of physical light and the laws of peripheral contrast; and (c) homogenous lighting virtually eliminates color constancy.

Experimental Design

To investigate Gelb's hypothesis, Perls conducted six sets of experiments in which she examined figure-background contrast effects in the following ways: (a) in neutral light conditions; (b) in color contrast conditions; (c) using Hering's nuance apparatus; (d) in a two-compartment room (i.e., a room divided by a partition); (e) in colored shadow conditions; and (f) in florescent light conditions.

Perls's design is very similar to the design of Gelb's experiment described earlier. That is, she used two umbrellas--either black and white or of a particular color or white--and lit by a particular color. The umbrellas had a small opening in their centers. The umbrellas were termed the external fields. Behind the umbrellas were placed disks--either black and white or colored--so that the color of the disks appeared through the umbrella apertures. These openings were called the internal fields. She then varied lighting conditions as well as the external/internal contrast colors, and had observers make a series of visual reports of the perceived differences in contrast from various distances of observation. Perls quantified the verbal reports from most of her experiments, and used these figures to support the conclusions she drew from the phenomenological reports given by the subjects. Perls used 14 subjects and names them with their quantitative measurements in various tables: Blug, Bodlée, Cohen, Galli, Goldmeier, Kleint, Nahm, Oppenheimer, Rosenbaum, Schwemmler, Siemsen, Sinemus, Usener, and Wingenbach.[24]

[24] Several of the test subjects were fellow students of Laura Perls. Schwemmler, Nahm, Siemsen, Oppenheimer, and Goldmeier also received their doctorates under Gelb (Ash, 1995).

First Set of Experiments: Experiment of Light Contrast

The first experiment measured the perceived difference in contrast of a white, shadowed external field in comparison to an objectively and uniformly lit object without shadows. The physical design consisted of two black-and-white rotating disks. Each disk was situated within a closed box each lit by a 100-watt bulb. A door on the long side of each box allowed for internal access. Above the light in each box, and in the front of each box, was an opening through which the rotating disks could be observed. At a distance of 20 centimeters (7.87 inches) in front of each box were two umbrellas. On the left side was a black umbrella and, on the right, was a white one. Each umbrella had a hole in its center. A screen standing perpendicular to the observer's position separated the umbrellas from one another. A window allowed daylight to illuminate one side. Perls varied the subject's distance of observation of the umbrellas from less than 1.5 meters (4.92 feet) to seven and nine meters (between 22.9 and 29.5 feet). She also varied the illumination of the room, but kept the illumination on the umbrellas the same. The observers reported differences in contrast perceptions with the varying distances.

Second Set of Experiments: Experiments With Color Contrast (Neutral Internal Field and Colored External Field)

Perls next conducted a series of experiments similar to those in the first set, but substituted colored umbrellas--red and green--for the black and white ones under varying lighting of the internal fields with colored light. She collected observations at four different distances while varying the lighting conditions. In one variation, she

used a green umbrella (apparently lit by neutral or white light) and a white umbrella lit by a green light. At the greatest distance of observation--assumed here to be nine meters (29.5 feet)--the external fields appeared green to the observers. That is, visually it appeared as being green in neutral lighting. As distances of observation were moved closer to the umbrellas, observers noticed a greater change in the red-green lighting on the right while at the same time the external field on the right became increasingly whiter. The inherent color of the external field became stronger and turned into a superficial white-red-green color. Perls noted that if such a contrast depended solely on the "retinal lighting conditions," then the internal field would have kept its appearance regardless of distance of observation. She found instead that the internal field changed immensely and, as distance of observation was reduced, the more she had to increase the intensity of the illumination colors (red-green) while reducing white light to "balance the internal field on the left side subjectively" (Posner, 1932, p. 20).

The results showed that the internal field on the color-lit side changed toward the direction of its opposing color with the advance of the observer. The internal field became greener or redder with the red or green illumination of the external field. Perls concluded from this experiment that contrast is not a function of retinal stimulation. These results are basically the same as those obtained in the previous experiment.

Third Set of Experiments: Experiments with the Nuance Machine

Perls wanted to complement the experimental results found in the previous sets of experiments with a new series of contrast experiments of a slightly more complicated design. She conducted these experiments in two phases. In the first phase, she simultaneously compared two gray, internal fields. She placed two disks, each with a black and a white sector, on the floor of a Hering nuance machine. The open sides of the nuance box were situated toward a window. Inside the box, and above the disks, she horizontally suspended a paper umbrella. The underside of this umbrella was black while the topside was half-black and half-white. Two holes were punched out of the umbrella along the division line so that an observer, with his or her head situated on a headrest, was able to fix their gaze on the internal field and report whether the internal field in the black external field or the internal field in the white external field was lighter or darker. Both external fields were illuminated equally with neutral lighting. She called this design the "A-setting." Three sets of observations were made at high, medium, and low lighting levels with the observer focusing on a neutral gray surface in between observation reports.

In the second phase, the black-colored external field was replaced by a white one, but turned away from the light source to such a degree that its retinal effect was equivalent to that of the black external fields. In this design, she used two white umbrellas that were half as large as the previous ones. One umbrella--a black-and-white one from the A-setting--was moved away from the window light until it was in a lower lighting level. The disk settings and lighting levels were then varied and observations made. Perls called this design variation the "B-setting."

Observations of both the A- and B-settings yielded a difference. The external fields of the B-settings appeared differently than the external fields of the A-settings. The difference was that the white umbrella that was turned away from the window in the B-setting appeared whiter than the black-colored external field of the A-setting. Perls concluded that these results confirmed her conclusions drawn from her previous sets of experiments.

Fourth Set of Experiments: The "Two-Room" Experiment

Perls conducted two more sets of experiments within a large, room-sized box divided in half by a wall, thus making two rooms: Room 1 and Room 2. A viewing hole was cut out in one wall with a door in the center of the dividing wall. Within this door was a semi-transparent umbrella. The front of the umbrella was lit by a light in Room 1, and from the back by a light in Room 2. The first experiment examined the color-free, external and internal fields. The observer was instructed not to stare at the internal field, but to casually take in an overall impression of any change in contrast. Changes in contrast were noted with changes in lighting levels.

The second experiment studied a colored external field and neutral internal field. The design of the second half of the experiment was the same as the first except that Perls changed the illumination with colored filters. When the umbrella was lit from the front, the observers reported that it appeared to have a rough granular texture in a space filled with intense red light. When lit from the back, the observers reported that the umbrella surface became a flat, rich red.

Fifth Set of Experiments: Experiments with Colored Shadows

In this experiment, Perls suspended a round cardboard disk in such a manner that a red light, together with a nearby and relatively colorless light source, cast a shadow on a white wall. This resulted in five observations: (a) the green contrast shadow appeared proportionately weaker depending on observational distance; (b) the closer an observer was to the wall, the visual impression increased that the white background of the wall was lit by multiple colors, although in reality it was not; (c) at 50 centimeters (19.6 inches) from the wall, the observer felt literally drawn into the red light, and the contrast shadow was perceived as being dark green; (d) the projection wall appeared whiter with decreasing distance of observation; and (e) the increase in contrast change occurred more under the red light than the neutral white one.

Perls then gradually added red to the green shadow color until it was completely neutralized. The amount of red needed to neutralize the green contrast color served as the measure of the magnitude of the contrast effect. In close-up observations, the shadow color appeared as a more intense or more saturated green as compared to observations made at a greater distance.

Sixth Set of Experiments: Flor-Contrast

Perls notes that researchers who seek to explain "lively" color contrasts often refer to experiments using flor contrast as well as experiments with colored shadows. Perls cites Helmholtz's position that intense colors will appear in conditions of low lighting levels of the external field. Experiments by Hering, however, disprove this

hypothesis, while experiments by von Kries[25] tend to point to factors related to peripheral processes affecting perception; i.e., sensory perception and cognitive judgment. Perls is in partial agreement with von Kries, but disagrees with him about the peripheral factors affecting perception. She states that the series of experiments conducted by her thus far offer another explanation, and one that can be proven using flor contrast.

These experiments were conducted in two parts using a rotating disk under daylight conditions. In the first part, and by means of a three-disk system, a gray ring was produced on a red background, and the subjects had to incrementally adjust the gray ring to eliminate its green contrast color. Red was added to the ring to the point at which the green contrast color disappeared. These compensation adjustments were made at four different observational distances: one meter (3.2 feet), two meters (6.5 feet), three meters (9.8 feet), and four meters (13.1 feet). In the second phase, a similar set of experiments was conducted. However, in this case, a disk of white silk paper covering the entire rotating disk was placed over the three-disk combination, and simultaneously rotated with the three-disk array.

The subjects reported that flor-contrast effects changed with increasing distances of observations, yet there were variations found among the subjects. To attempt to explain this, Perls cites contrast experiments conducted by Theodora

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[25] Von Kries was a physiologist who worked briefly with Helmholtz, and then with the noted physiologist, Carl F.W. Ludwig, in Leipzig. Von Kries is best known for his duplicity theory which states that the retinal rods are for night vision, and the retinal cones for daytime vision (Boring, 1929/1950).

Haack using an epicotister.[26] Observations made while using the epicotister, and then gradually reducing its viewing aperture, first increased and then decreased the intensity of the image and this was analogous to the variations using flor-paper. Perls also describes an experiment by Fuchs in which he mixes two colors of light and obtains a similar effect. Perls concludes that the flor-contrast effect occupies a phenomenologically intermediate position between pigment contrast and illumination contrast.

Summary

Perls drew a series of conclusions from her experiments. Her primary conclusion was that the strength and type of retinal excitement was not the only factor in determining contrast, but that overall lighting conditions of the field were of greater importance.

Assessment: Results of Perls's Color Contrast Experiments

Perls drew a series of conclusions from her experiments. Her primary conclusion was that the strength and type of retinal excitement was not the only factor in determining contrast, but that overall lighting conditions of the field were of greater importance. This conclusion supported Gelb's position that the field effect of overall illumination is the most influential factor affecting the visual perception of color contrast.

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[26] An epicotister is "a rotating disc with open and closed sectors of adjustable angular width interposed between an observer and a visible object" (English & English, 1958, p. 183).

Perls's work is very much a reflection of color perception research that was being done at that time, and gives some insight into how these research problems were being conceived and investigated.

The research topic itself was directly assigned by Gelb and is a continuation of his own research interests. This is not to say, however, that Perls's work is a mere affirmation of her mentor's interest. It is more precisely an experimental confirmation of the Gestalt view that field conditions affect sensory perception. This field view was pioneered by Lewin and adapted by Koffka and Köhler. In other words, Perls's experiments in a small way confirm that the nature of the visual whole determines the perception of the parts (contrasts).

Perls used a design format in standard use in Gestalt psychology at that time consisting of a combination of using an experimental method combining quantitative measurements and the qualitative method of subjective visual perception. This qualitative method derives from the phenomenological tradition of Brentano and Stumpf, and was termed by Koffka as experiential observation (Koffka, 1916/1925, 1924).

Perls's work verifies her membership in the school of Gestalt psychology at the University of Frankfurt through her experimental studies and dissertation. It shows how Perls, as a graduate student in psychology, was able to specialize in the quantitative and qualitative investigation of color contrast phenomena. Perls's study under Gelb, and her doctoral dissertation are consequently two substantial pieces of evidence supporting the contention that there is a historical connection between Gestalt psychology and Gestalt therapy.

V. DISCUSSION

Initial Findings and Implications

This historical examination of the experimental studies in color perception conducted by Laura Perls at the University of Frankfurt between 1927 and 1932 has achieved several results of historical significance.

The first result is that it provides documented verification of the work of Laura Perls and that she was a formal member of the Berlin-Frankfurt school under Adhémar Gelb. Consequently, it disproves Henle's (1978) position that there is no link between Gestalt psychology and Gestalt therapy. This is not to say that Henle's analysis of Fritz Perls's use of Gestalt principles is inaccurate, but only that her critical focus was too narrow. Likewise, it proves that advocates of Gestalt therapy have been incorrect in their arguments by focusing almost exclusively on role of Fritz Perls in the development of Gestalt therapy. Based on the historical evidence given here, the resolution of the argument of Gestalt psychology versus Gestalt therapy is a compromise position. Gestalt therapy is historically related to Gestalt psychology as an illegitimate child is related to a family; the child may be of mixed descent and of questionable character (i.e., Fritz Perls), but is still related by ties of blood (i.e., Laura Perls).

Second, the results of this dissertation serve to distinguish Perls's experimental studies within the historical context of the Berlin-Frankfurt school as well as their place within German experimental psychology circa 1930.

Third, the findings indicate that there is a need for a revised historical view of the place of Gestalt therapy as an intellectual descendant of Gestalt psychology, and its relationship to the history of American psychology.

Fourth, and further, this necessarily means that there is a connection between German scientific psychology and American humanistic psychology, and that that current historical view will need revision.

Lastly, the results hold deeper implications about the nature of how historical investigation into American psychology has been conducted. That is, the denial on the part of historians of a possible connection been German academic psychology and humanistic psychology within American folk psychology suggests the presence of an inherent flaw in the epistemological structure of American psychology. The epistemological foundation rests upon reductionism, and is maintained at the expense of recognizing alternative forms of inquiry or alternative epistemological models. It also rests upon the received view of American psychology originally given in the 1920s by Titchener and Boring. This contention deserves more detailed attention because it holds the most profound implications for the history of American psychology. Its significance is greater than in merely resolving a minor historical argument.

The History of Psychology: Some Fundamental Problems in Epistemology

One final question to be addressed is that not just Henle (1978), herself an eminent historian, but other historians as well, have neglected to investigate the history of Gestalt therapy and its relationship to Gestalt psychology. This is a

provocative and complex question. Why have American historians for more than thirty years automatically doubted any linkage, and not bothered to investigate further? The problem is more than just that Henle (1978) was an influential scholar. Deeper reflection on this question yields further conundrums in the history of American psychology of far greater magnitude than the problem of Laura Perls. Consider the following views and attitudes prevalent in the history of psychology.

(a) An overemphasis on the significance of Wundt's experimental psychology at the expense of ignoring his philosophy of science that was essentially inductivist, and stressed explanatory motives in human psychology (Danziger, 1980).

(b) The rejection of the unconscious, as well as rejection of the humanistic and existential dimensions of experience, and the possibility that there might be multiple epistemologies underlying human reality (Taylor, 1998).

(c) An general devaluation of the history of American psychology prior to 1879.

(d) The rejection and exclusion of the psychology of religion from American psychology (Beit-Hallahmi, 1974).

(e) A general misunderstanding within American psychology of the work and philosophy of William James (Taylor, 1996a, 1998).

(f) An historical neglect and/or only partial understanding of the work of Carl Jung (Taylor, 1996b).

(g) The chopping up of various theorists' overall works, such as Wundt, James, and Lewin, because only certain parts are considered scientific.

(h) A misinterpretation of the history of the clinical psychology as arising from Wundtian experimental psychology rather than from its actual roots in French neuro-psychiatry (Taylor, 2000a).

(i) A traditional reliance on secondary literature in the teaching of the history of American psychology (Cicciarelli, 1998).

All of these things may be overlooked or summarily dismissed when considered individually, but when taken as a whole, they give one pause about the nature of American psychology, how it perceives itself, and how it has been conducted in the past century. These observations are symptomatic of greater structural and philosophical problems present in the history of the history of the discipline. It is toward these problems that this discussion now turns.

The idea that something is awry in the history of psychology is not a new one. A small group of historians in recent decades has become increasingly sensitive to the presence of these structural problems, and has been offering critical suggestions for their correction.

Young (1966) evaluated the status of scholarship in the history of the behavioral sciences, and was highly critical. He had particularly hard words for historians of psychology for emphasizing a "history of problems of current interest," and for writing history "backwards from the viewpoint of the modern textbook" (p. 18). He pointed out three major faults in the history of psychology: its self-imposed limitations by focusing on great men, great ideas, and great dates. These limitations can be traced back to the influence of Boring (1929/1950).

Wettersten (1975) argues that one major defect in the historiography of psychology is that many historians tend to obscure the contradictory nature of psychology because psychology is itself a group of competing schools organized around differing theories and perspectives. He argues that historians of psychology misportray psychology as a steady evolutionary development, but, as it is not a steady stream, it must obscure its inherent contradictions by using five techniques of historical analysis: (a) vague, uncritical praise for psychological theories; (b) praise for the value of fact gathering rather than examining the significance of individual facts; (c) uncritical praise for the application of method; (d) praise for the development of techniques rather than critical examination of the efficacy of those techniques; and (e) the discussion of individual careers (Wettersten, 1975).

Part of the historiographic obfuscation noted by Wettersten (1975) is understandable because the story of psychology, to be true to its own epistemology and scientific image, must necessarily interpret events in ways that are consistent with its philosophical structure. This necessarily means that psychology, because it seeks to be scientific, must avoid all that which is perceived as not being scientific. Leahey (1980/1997) observes that scientific psychology has had two opponents that it must reject and conquer. These are dualism and folk psychology. As science is based on the naturalistic model, it must reject what it would classify as being "supernatural," such as phenomena that cannot be explained by physics, like subjective experiences of nonordinary consciousness. This rejection is evident in the history of psychology

when it separated itself as the "new" experimental science from the old psychology of moral and mental philosophy.[27]

O'Donnell (1979) argued that Boring wrote his histories because he was defensive about the rise of applied psychology in the 1920s, and wanted to make experimental psychology more significant and relevant in comparison. He asserts, "for Boring, history was not merely a matter of describing the past but of altering the future" (p. 289).

Kelly (1981) criticized Boring's versions of the history of psychology because he wrote them from a specific historiographic perspective; a perspective based on a political, administrative, and methodological agenda.

Gruenwald (1984) criticized the traditional approach to historiography of psychology for its idealistic view, its presentism, and general irrelevance. He proposed a three-level model of historical analysis to replace the traditional approach. Gruenwald's model incorporates the conceptual, the sociopsychological, and the institutional levels of the history of psychology.

Danziger (1984) proposed that a critical history of psychology be developed to correct the traditional approach to the history of psychology. He suggested that a critical history be based on traditional sources while taking into consideration the

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[27] Space does not permit a full examination of experimental psychology's rejection and condemnation of non-scientific, folk psychology, but its presence is easily discerned. See Jastrow's (1900) early critique of "occult" psychologies such as theosophy, spiritualism, alchemy, Christian Science, psychical phenomena, mental telegraphy (telepathy), and hypnosis, and compare it to a recent introductory psychology text by Wade & Tavris (2000) that opens with a criticism of what the authors call "psychobabble," a pejorative name for folk psychology.

assumptions and commitments of historians along with a conscious attempt to unearth current biases on the part of historians. Danziger (1994) then asserted that the history of psychology has been accorded the status of a purely pedagogical activity, rather than as a source of relevant contributions to psychology. This is also symptomatic of the favor given to reductionistic epistemology of the physical sciences rather than alternative methods offered by the human sciences. Consequently, the field of psychology has organized internal consensus regarding its development through the use of the history of psychology. Any history that would be critical of the established view of the historical development of psychology would pose a threat to the community of psychologists. Danziger argues that a critical psychology is now slowly emerging thanks to contributions made by feminist scholarship and through the international diversification of psychology that have led to a general disenchantment with the established view of science.

Furomoto (1989) argued that history of psychology courses and accompanying texts present a celebratory, but decontextualized narrative about great men and great ideas from ancient times to the present, and mostly from a Western cultural perspective. In place of this traditional telling of history, Furomoto proposes a "new history" that would make "a history that is more contextual, more critical, more archival, more inclusive, and more pastminded" (p. 30). She describes her vision of the new history:

> I see at least five noteworthy aspects: the new history tends to be critical rather than ceremonial, contextual rather than simply the history of ideas, and more inclusive, going beyond the study of "great men." The new history utilizes primary sources and archival documents rather than relying on secondary sources, which can lead to the passing down to anecdotes and myths from one

> generation of textbook writers to the next. And finally, the new history tries to get inside the thought of a period to see issues as they appeared at the time, instead of looking for antecedents of current ideas or writing history backwards from the present content of the field (Furomoto, 1989, p. 18).

Equally troubling is that the structural and philosophical flaws behind the conduct of historians of psychology seriously affect the way the history of psychology is taught in American schools of psychology. Cicciarelli (1998) surveyed the goals, structure, and content of History and Systems courses at APA-accredited graduate programs in clinical psychology. He found that 41.5% of the 65 syllabi analyzed that textbooks were the only required reading and that in 61.5% there were no requirements for students to read primary source materials. Cicciarelli concludes the history of psychology courses examined relied "primarily on a decontextualized presentation of 'great men' and intellectual achievements of psychology" (p. 260).

Epistemological Evolution of American Psychology

Early in the development of scientific psychology anything that was not based on the experimental method was vilified as being, what E.W. Scripture (1864-1945) called armchair psychology. Armchair psychology is a pejorative term for pre-scientific psychology and folk psychology. Leahey (1980/1997) defines armchair psychology as the interpretation of psychological events from the everyday perspective of personal beliefs and desires. What makes folk psychology unacceptable to scientific psychology is its appeal to teleology. That is, many of the explanations of folk psychology are based on the idea that a future state determines present behavior. The scientific model, on the other hand, postulates that causes must always precede effects. The problem with this epistemological inflexibility is that it

excises a vast area of human experience, including the validity of alternate epistemologies, from its historical discourse. The case in point here was the reflexive denial of the possibility that a form of German academic psychology could be historically related to a humanistic psychotherapy within the stream of American folk psychology, as in the case of Laura Perls and Gestalt therapy.

The explanation given here is that the reductionistic epistemology of psychology, along with its received view of history originally formulated by E.G. Boring and his successors, has created a kind of unconscious self-censorship in historical analyses. (Boring's role will be discussed further below.) Consequently, this has created a tendency within the history of American psychology to exclude historical events that do not fit its worldview.

Space does not permit a full analysis of the development of the epistemology underlying American psychology, but a brief survey will suffice to illustrate the nature of the received view by historians. This discussion will confine itself to the time period roughly between 1879 and 1935. While there are other models showing the epistemological development of psychology,[28] a simple three-stage model is offered: (a) making psychology a science, (b) the displacement of Wundtian and Titchnerian introspective experimental methods with the Watsonian method of behavioristic observation and Galtonian experimental testing, and (c) making the scientific method of investigation the preeminent quality of psychology. What this

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[28] Robinson (1981) offers a very good model showing the epistemological development in four phases based on the scientific philosophies of Carl Hempel, Karl Popper, Thomas Kuhn, and the neo-positivists. See also Taylor (1995) for an equally informative and thought provoking perspective.

three-stage model demonstrates is the progressive reinforcement (or narrowing) of what constitutes psychology based exclusively on experimental method.

Wundtian Psychology

The first phase of the epistemological development of a narrower psychology begins with the work of Wilhelm Wundt, who sought to distinguish a new psychology based on the experimental laboratory method from the older psychology that based on philosophical speculation. At this point in time, the work of Wundt and his students, especially his many American students, was to ground the new psychology on the experimental method drawn from psychophysics. Wundt himself thought that only the lowest and simplest psychological phenomena were best investigated by the experimental method, while more complex mental phenomena were investigated best using anthropological methods (Klein, 1970). This point was overlooked, intentionally or unintentionally, by Wundt's American successors, especially by two former students: the British émigré, E.B. Titchener (1867-1927),[29] and James McKeen Cattell (1860-1944).

Wundt's influence in the early stages of the formation of an American scientific psychology cannot be understated. A list of his students reads like a Who's Who from the turn of the century: G. Stanley Hall, J.M. Cattell, H.K. Wolfe, E.A. Pace, E.W. Scripture, F. Angell, E.B. Titchener, Lightner Witmer, H.C. Warren, H. Gale, G.T.W. Patrick, G.M. Stratton, C.H. Judd, and G.A. Tawney (Boring, 1950).

Watsonian Behaviorism

The second phase of the epistemological development of psychology begins roughly in 1913 with Watson's famous article announcing the birth of behaviorism (Watson, 1913). Between 1913 and 1920, the American version of Wundtian psychology had changed by excluding many of its structural features--e.g., introspection as a method--in favor of pure observation based on a stimulus-response model taken from animal psychology. American academic experimental psychologists eventually rejected Wundt's psychology on purely experimental grounds. Klein (1970) comments that:

> Often their [American experimentalists] opposition to his [Wundt's] teachings was the consequence of the extension and modification of laboratory procedures that had been introduced in his laboratory. In other words, they based their opposition upon their experimental findings. Their recourse to chronoscopes, tachistoscopes, plethysmographs, galvanometers, and other instruments had been commonplace as a result of what Wundt had initiated at Leipzig. In large measure they were thus indebted to Wundt for the idea of an experimental attack on psychological problems (p. 874).

What is seen in this second phase is a change in the strength of the belief that only that which is based on the scientific method constitutes genuine psychology to an even more extreme standard of rigor. Another change is the aggressive exclusion of consciousness as a valid subject of study because it cannot be subjected to rigorous experimental investigation. Behaviorism was to become the dominant school of psychology up until the 1950s, thus further anchoring psychology in an

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[29] This is not to ignore the fact that Titchener remained committed to the introspective analysis of consciousness as a preferred experimental method at this time, but this detail is negated when

uncompromising epistemology of reductionistic empiricism, physicalism, and determinism.

Operationalism, Quantification, and the Hypothetico-Deductive Method

The third phase comes from more deeply grounding the method of experimental inquiry upon operationalism, quantification, and the hypothetico-deductive method, first articulated by Clark Hull (1884-1952) at Yale University. Drawing from Nobel Prize winner Percy Bridgman's (1882-1961) (1923) conceptualization of operationism in physics, S.S. Stevens (1935a, 1935b) and E.G. Boring (1933) were both instrumental in first recognizing the value of operationalism for scientific psychology, and worked to promulgate its use within the academic community, although distorting Bridgman's original meaning (Watson, 1965). This phase was also influenced by the introduction of logical positivism from the Vienna school. The foremost exponent of logical positivism in experimental psychology at the time was Sigmund Koch (1917-1996).[30] As Robert I. Watson (1909-1980), an intellectual follower of Boring and a historian of psychology, observed 30 years later:

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introspection was later dropped from experimental psychology for being unreliably subjective.

[30] Positivism originated in the critical empiricism of Ernst Mach (1838-1916) and Richard Avenarius (1843-1896), and was first formally explicated by Karl Pearson in The Grammar of Science (1892). The Vienna school of logical positivism consisting of Rudolph Carnap (1891-1970), Hans Reichenbach (1891-1953), Herbert Feigl (1902-1988), and Ludwig Wittegenstein (1891-1951) (Robinson, 1981) then further developed this philosophy. The members of the Vienna Circle saw themselves as followers of Wittgenstein, and were particularly influential in the 1930s. Koch was a student of Herbert Feigl (1902-1988) at the University of Iowa, and received an M.A. in the history

> The present fervent attachment to the hypothetico-deductive prescription helps to account for American psychology following through from one research study to the next to the relative exclusion of the development of new theories. Deductive elaboration of already existing hypotheses is its métier (Watson, 1965, p. 135).

Watson (1965) also observes that the fundamental epistemology of psychology, while it remains the dominant philosophy under American psychology, was not without its philosophical opponents:

> It has been seen that national trends in modern American psychology follow certain dominant prescriptions. Determinism, naturalism, physicalism and monism, although very much operative, are judged to incite relatively little opposition. Functionalism, operationalism, quantification, hypothetico-deductivism, environmentalism, and nomotheticism are likewise dominant, but there are counter-prescriptions that tend to oppose them (Watson, 1965, p. 137).

These counter-prescriptions, as Watson calls them, are Gestalt psychology, psychoanalysis, existentialism, and phenomenology. It is safe to say, now writing almost forty years after Watson, that these alternative epistemologies have failed to counter-balance the scientistic epistemology of American psychology, and have been exiled to the margins of contemporary discourse. Consequently, the epistemological foundations of psychology completed by the 1930s have remained virtually unchallenged since its inception 100 years ago. The one counter-prescription that Watson neglected to mention was James's radical empiricism, but while this may be a viable alternative, it too has had very little, if any, influence as James intended it (see

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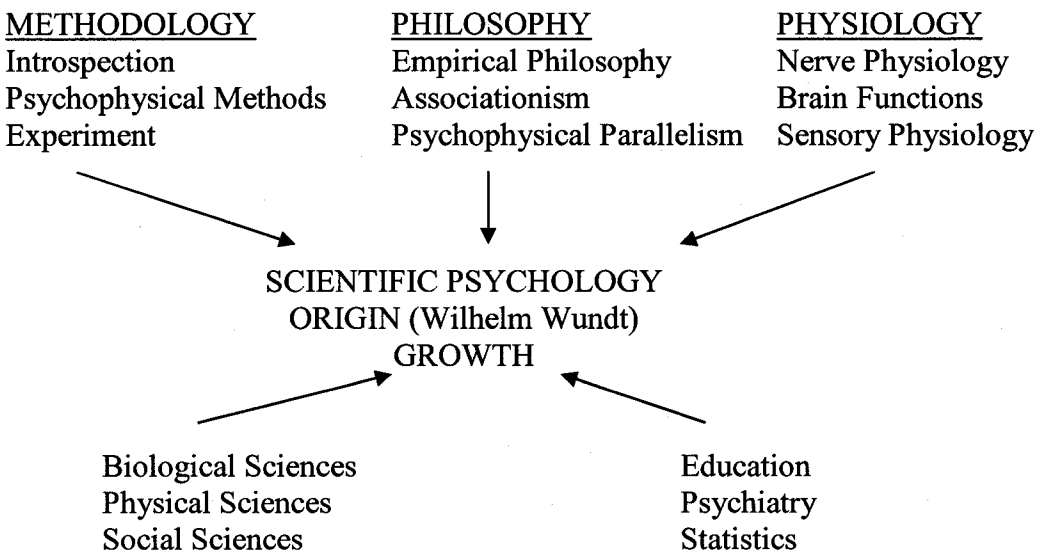
and philosophy of science in 1939 before earning his Ph.D. at Duke University in animal (rodent) motivation.

Taylor, 1995; also Holt, 1914, 1915).[31] Misiak & Sexton (1966) show that psychology has a distinct genealogy based almost exclusively on empirical, physicalistic methods (see Figure 2).

Figure 2: The Genealogy of Scientific Psychology and the Main Sources of Influence on the Growth of Psychology (Misiak & Sexton, 1966, p. 3.)



Organization and Professionalization of Scientific Psychology

The founders of the new psychology in America were William James, G. Stanley Hall, Edward Bradford Titchener, and James McKeen Cattell, among others. Hall was a passionate organizer and spokesperson for the new experimental psychology that founded laboratories at John Hopkins and Clark Universities, and trained a large number of second-generation psychologists. American psychology at the turn of the century had three major goals to achieve in order to establish itself as a

[31] This point is especially poignant given the assumption that most psychologists have ignored this portion of James's work because it is not scientific, and so has no relation to psychology.

scientific profession. These were to establish American psychology as an academic specialty apart from philosophy, as a profession through a professional association, and as a scientific enterprise through the founding of scientific journals (Camfield, 1973).

The founders of American academic psychology were extremely concerned that the new profession be accepted as a science. Interestingly, much of the profession's collective motivation stemmed from internal disagreement as to how exactly define their new field, its preferred phenomena of investigation, and its means of experimental inquiry. This anxiety over professional identity in how the profession would be perceived by the other sciences was a large part of the motivation on the parts of the founders to quickly establish the profession. As Camfield (1973) notes, a survey of the first 25 years of the Psychological Review and Psychological Bulletin reveals a persistent debate over theory and methods up to 1917. This internal dissension within the profession in its formative years contributed to the launch of behaviorism because "if psychology was to become a science, and hence to command respect, he [John B. Watson] went on to argue, it had to emulate the established physical sciences through the precise and objective study of behavior" (p. 73).

The project of establishing American psychology as a scientific profession was quite successful, for the most part, as it aligned academic psychology more closely with the physical sciences, although another portion of psychology scattered elsewhere, primarily as depth psychology. With its success in making incursions into the academic and professional fields, scientific psychology also gained further social

acceptance and professional respect by entering other fields such as education, medicine, criminology, business, industry, and advertising.

What is seen as the main driving force behind the professionalization of psychology was a fervent desire on the part of early scientific psychologists to establish their profession as a science on a par with physics or chemistry. Consequently, for a person to become a psychologist, he or she had to adhere to the scientific model while eschewing other nonscientific models, such as philosophy. Underlying this drive for professional organization and recognition is scientific epistemology; indeed, the early organizational efforts of the founding psychologists can be thought of as being the social manifestation of this epistemology.

The History of Psychology: The View Received From E.G. Boring

The essential outlines of the history of American psychology were written in the late 1920s and early 1930s by several influential psychologists of the day: Brett (1912/1921), Boring (1929/1950, 1942), Murphy (1949), Flugel (1933), and Pillsbury (1929) (Wettersten, 1975). The one history that came to dominate over all of these and forming the basis for the received view of the history of American psychology is "Boring's 1929 masterwork" (Brozek & Evans, 1977). Given this, a sketch of Boring and his development, especially those who influenced him, and whom he in turn influenced is needed.

Boring, at the time of his death, had an international reputation. Besides being an internationally recognized professor of psychology, he was the author of 176 papers, 202 editorials, 45 book reviews, and many other items, including his

influential histories of psychology, and various textbooks on psychology. He had been editor of the American Journal of Psychology (appointed to the position by Titchener), the founding editor of Contemporary Psychology, and a member of the editorial board of the Journal of the History of the Behavioral Sciences. Boring was very influential within American psychology as he held a succession of professorships, administrative positions, committee chairmanships, and presidencies of various psychological associations. His introductory course in psychology was made into 38 one-half-hour, videotaped lectures by a Boston educational television station in 1960. At the time of his death in 1968, the popular press dubbed him, "Mr. Psychology" (Jaynes, 1969).

Boring graduated from Cornell University with a master's degree in engineering in 1908, and worked briefly for the Bethlehem Steel Company in Pennsylvania. During this time, he also taught science and physical geography in the Moravian parochial school system. He returned to Cornell in 1909 to study physical geography with the intention of becoming a teacher. During the course of his study, he took a laboratory course in experimental psychology with Madison Bentley. Bentley encouraged Boring to pursue psychology. His early research projects included the study of the visual contrast effects perceived by planaria (a small flatworm), maze learning in rats and humans, and learning in schizophrenics. He then undertook study under Titchener in 1912 (Boring 1961). Titchener assigned him the thesis topic of studying visceral sensitivity using a stomach-tube technique (Jaynes, 1969). Titchener was a powerful, lifelong influence on Boring. Titchener, according to Jaynes (1969), determined Boring's writing style, his vision of psychology, and the

nature of his collegial relationships. Following Titchener, Boring placed inordinate emphasis on the German tradition of experimental psychology.

Boring's major imprint on the history of psychology, however, was in his historical works (Boring, 1929/1950, 1942). Boring (1929/1950) was dedicated to Titchener and, significantly, the only picture in this text is that of Wundt. The 1942 book on the history of the experimental investigation of sensation and perception is dedicated to Helmholtz. The 1929/1950 text became the standard history and required reading for psychology students for many decades. The text itself, however, besides being a brilliantly detailed exposition, is in the main a reflection of Titchener's influence. It had, as Jaynes (1969) observed, the Titchnerian prejudice of excluding philosophy, biology, and general culture from the history of psychology, thus making the history of psychology exclusively a history of experimental psychology. Further, Boring held to the philosophy of physicalism throughout his life, and presented his position in The Physical Dimensions of Consciousness (Boring, 1933). Boring attempts to reconcile the problem of the dualism caused by the problem of consciousness in this particular text. His solution was not to ignore consciousness, as the Watsonian behaviorists had done, but to make consciousness compatible with monism. Boring's solution was a prototype for the operational model he later promoted (Friedman, 1967). While Boring called this work immature, and while Jaynes (1969) believed that Boring himself never was truly comfortable with a physicalistic monism, the overall position and its philosophical implications were clear enough, and Boring's perspective remained unchanged throughout the rest of his career. By 1942, Boring was a confirmed physicalist and monist, and would later

write that operational logic demonstrates that consciousness is an inferred construct (Friedman, 1967).

Jaynes (1969) gives an overall assessment of Boring's role in the history of American psychology:

> Edwin G. Boring, unlike the present writer, was a single spirited man, and that single spirit, around which he organized a furiously active life, was experimental psychology. He helped fashion it out of its nineteenth century heritage, helped fill in that structure with various theoretical and experimental work through a half century of working his eighty-hour week, and told others where to build other parts of the edifice in his role as chairman of the Harvard department and his many editorships. Knowing and being known by everyone, he moved through that structure with the ease of a foreman who realized he was not the architect, but who felt sincerely that without his integrity that building would not be safe for posterity (p. 111).

Given that Boring's histories became the dominant version of the history of psychology, the historians who came after him used him as both mentor and model. One of the most influential historians of this second generation was Robert I. Watson at the University of New Hampshire. Watson was one of the founders of the Journal of the History of the Behavioral Sciences, and a founding member of Cheiron: The International Society for the History of the Behavioral and Social Sciences. Watson was greatly influenced by Boring, as he himself admits:

> During the sixties Edwin G. Boring had an important influence on me. My relationship is epitomized in the dedication of The Great Psychologists (1963)—"To E.G.B. my teacher, under whom I never studied." His erudition, his helpfulness, even his narrowness expressed in the grand manner, were important to me (Watson, 1977, p. 15).

It can be seen that the received view of the history of psychology was, for the most part in the first half of the twentieth century, written according to the materialistic epistemology of scientific psychology of Boring and his adherents. Since

Boring, the history of psychology has been predominantly the history of experimental psychology using the framework of great men and great ideas. While there might seem to be many exceptions to this contention, it must be understood that all of the histories written since Boring remain implied histories of physiological psychology, psychophysics, behaviorism, and cognition.

Why did Boring's view of history win out over the other histories existent at the time? Capshew's (1999) answer is that as psychology began to move outside of the academic boundary, it needed to reaffirm its "fundamental faith in experimental research" while "providing a scientific pedigree that was both impressive and plausible, to insiders and outsiders alike" (p. 25-26).

Because American experimental psychology founded itself upon a physicalistic epistemology, this philosophical view has necessarily affected how psychologists write the history of psychology, and causes subsequent problems in terms of perspective and analysis. Collingwood (1972), a noted British historian, argues that the main flaw of American psychology was to have originally identified itself with the model and method of the physical sciences. Psychology, according to Collingwood, would have been better to have modeled itself on history because, as Robinson comments:

> A science concerned with the determinants of human conduct, with the character of human perception and emotion, will find in the annals of civilization a laboratory more varied and rich than any we could hope to re-create. This part of Collingwood's instruction is unimpeachable, and it is this part that urges us to study history if we are to study psychology (Robinson, 1981, p. 7).

Conclusion

The resistance of historians to considering a closer relation between Gestalt psychology and Gestalt therapy points to the presence of a received view of the history of psychology. This is the established view of the history of psychology as historians within the framework of American academic experimental psychology have written it. This received view determines the validity of certain historical topics over others in the discourse of American psychology.

The point has been to outline a position showing that psychology, in its drive to be scientific, adhered to a particular epistemology, and that the first historians of psychology bequeathed a received view of the history of psychology based upon that epistemological perspective. The consequence of this is that and the original design of the founders of American psychology made it into what they considered to be a professional, scientific enterprise. Many contemporary historians of psychology are now re-examining historical events with new eyes, and the field is changing. Wettersten's (1975) criticisms have been mentioned, while Ash, in another example, comments that:

> Recent research has undermined traditional conceptions of psychology's history in two important ways. Where standard textbooks often told the story of a continuous upward climb from the depths of philosophical speculation to the heights of cumulative experimentation, newer work shows that psychology's intellectual development in this century was based in part on a rapprochement with certain kinds of philosophy, in the United States with logical positivism and operationism. On the other hand, perhaps for pedagogical reasons, standard texts have treated psychology's development largely as a history of ideas, isolated from the discipline's social and cultural contexts. Recent work has brought to light new material about the impact of social and cultural change on psychological thought and practice. In the process it has shown that psychology, like other disciplines, is not only a set of ideas and

> methods, but also a set of institutions having definable connections with the societies in which they are located—a point of particular significance in the age of "big science" (Ash, 1987, p. 3).

While Ash's comments are laudable, the problem is that the epistemological structure of American academic psychology remains unchanged. The main weakness, of course, is that the history of psychology as an almost exclusive history of experimental psychology suffers from a kind of tunnel vision, as it excludes large areas that are related to its historical development, but have been judged unacceptable topics in its discourse. The solution would be to broaden the definition of psychology beyond the bounds of the purely experimental. Taylor (1995, 1999a, 2000b) has undertaken work along these lines.

Given that American psychology is based on an underlying reductionistic epistemology, and that the history of American psychology has been told from that perspective, and that this established history denies the validity of any other historical stream of psychology, the denial of a connection between Gestalt psychology and Gestalt therapy by mainstream historians of psychology becomes more understandable. Resistance to the idea that Gestalt therapy is a child of Gestalt psychology is just a miniscule symptom of the overall epistemological defect of the history of American psychology. Given the adamantine structure of reductionistic epistemology, American psychology must automatically negate any connection between the two because, if such a connection were affirmed, then this would mean that the dominant stream of American academic psychology was related to the stream of American folk psychology. The question is not ultimately about how Gestalt psychology and Gestalt therapy are connected, but more significantly that the two are

unquestionably connected, and that the leaders of the history of mainstream American psychology have unreasonably denied this connection. The verification of such a reality must always be judged unacceptable, given the epistemological beliefs that American academic psychologists hold about themselves which, in turn, arise from and are consistent with, their received historical view. This perspective, however, may be amenable to change as new evidence is uncovered and examined. Ironically, the failure of Gestalt psychology to be accepted by American psychology is not so much that it was confused by Fritz Perls and other factors, but more that it promoted an alternative and completely unacceptable epistemology. The Gestalt epistemology of Wertheimer and his associates did not have a prayer when Watsonian behaviorism was at its zenith in American psychology. Given the results and implications of the present research, it is equally doubtful that Gestalt psychology would be able to survive if it were introduced today, and for the same epistemological reasons.

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APPENDIX

Posner, L. (1932). Die Ersheinungen des simultanen Kontrastes und der Eindruck der Feldbeleuchtung, [The phenomenon of simultaneous contrast and the impression of the field of illumination]. Unpublished doctoral dissertation. University of Frankfurt am Main, Frankfurt, Germany.

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Johann Wolfgang Goethe-University
Frankfurt a.M.

Produced by

Lore Perls nee Posner
of Pforzheim

Advisor: Prof. Dr. A. Gelb
Co-Advisor: Prof. Dr. K.W. Meissner

Day of the Oral Examination: 18 July 1932

Introduction and Statement of the Problem

For the observation and examination of the simultaneous light and color contrasts, very different methods and procedures are proposed.[1] Depending on the method used, the contrast phenomena are variously distinctive and lively. Among the clearest cut and most revealing are the "colored shadows." Also, the so-called "florescence contrast" is very powerful; definitely much more penetrating than, for example, the contrast of the usual pigment paper as can be produced with a color wheel. To achieve a very lively contrast phenomenon, the "hole method" by von Hering works especially well using the "nuance apparatus."[2] With the nuance apparatus, one hole color appears almost white, then almost black, depending on the intensity of light with which the hole umbrella is illuminated.

No matter whether one posits regarding the controversy of the origin and the nature of contrasts more peripheral explanatory principles as being the solution (e.g., Hering) or whether one posits more central, explanatory principles (e.g., Helmholtz), with reference to one issue one may have agree, at least until fairly recently: it always has to do with "contrast" no matter whether one uses, with references to internal field and external field pigment paper with the same objective lighting; or, if in order to produce internal fields, one uses hole colors, and thereby exposes the external fields to a quantitative or qualitatively different lighting.

More recently, Kroh[3] has stated the view on the basis of specific experimental results, that with specific color phenomenon, which until now were considered the most

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[1] A collection of various experimental principles and procedures by C. von Hess in Albderhaldens' Handbook of Biological Work Methods, p. 202ff.

[2] Outlines of a Theory of the Light Sense, p. 54 and 121.

[3] Journal of Sensory Physiology, 52, 1921.

lively contrast perceptions produced, actually are not so much about the phenomenon of contrast, but rather about the phenomenon of the so-called "color constancy." Other authors essentially have agreed with this position.[4]

The term, color constancy, is understood as being all those appearances first experimentally studied by D. Katz, with the exception of specific experiments by Hering, 1911, in his book, The Manifestations of Colors and the Extent to Which They are Influenced by Individual Experience."[5] All of these presentations deal basically with the question: how is it that all of these detailed changes in the amount of light, and within certain parameters, the changes in lighting color do not make a noticeable difference on our everyday perception of color; the way we recognize color?

In all of these manifestations, there is concern with the substantial question of the relationship between "color" and "illumination" and, in this context, particularly how it is that significant changes in the intensity of illumination also lead to (within certain boundaries) without exerting any influence on our everyday perception of color—as regards our recognition of primary colors.

An example should clarify this. A white paper lying in a neutral light can be made light weaker in two ways. First, one can replace it with a paper of a lesser albedo value that will reflect much less light. In this case, the substitute paper appears dark gray or black. Second, the same white paper can be made to appear darker when one removes it

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[4] G.E. Muller: On the color sensation: Psychological investigations, Journal of Psychology, 17, 1930, p. 88.

[5] Journal of Psychology, 7. Meanwhile in 1930, a revised edition of D. Katz's work on color was published under the title, The World of Color. In this book, Katz agrees substantially in many points with the views given by A. Gelb in Color constancy in visual objects, (Handbook of Normative and Pathological Physiology, edited by A. Bethe, et al., Vol. XII, I, Hallie, p. 594ff.

from the light source, i.e., the window, and far enough way so that the lighting is reduced to the same amount as in the first example. Although the white paper in both cases is objectively exposed to the same amount of reduced light, one can observe a phenomenal difference. The white paper that received less light by moving it away from the window usually does not appear as "dark gray" or "black," but "white." Of course, a "white" that shows a weak illumination and a "white" of "low concentration" (Katz). One also expresses this phenomena as: the paper has a "real color" white, and this white as a real color stays that way, inclusive of the fact that with lowering the illumination it becomes just as light-weak as the exchange of the white paper with the dark gray or black paper in the original lighting. As in this example, the same way we see the objects that surround us regardless of the observed illumination in which objects stand.

Katz introduced a very different explanation for the colors "constant" than for the color "contrast." For a long time, the opinion was that there are two different groups of manifestation that would require different principles of explanation.

There is a discrepancy between this interpretation and the one previously mentioned by Kroh that needs an explanation. Who of the two is right?

Katz questioned contrast by asking is only the strength and quality of the stimulation of the retina important or if the relatively independent "inherent color" of the external field also played a role in creating contrast. To answer this question, he did an experiment illustrated in Figure 1.[6]

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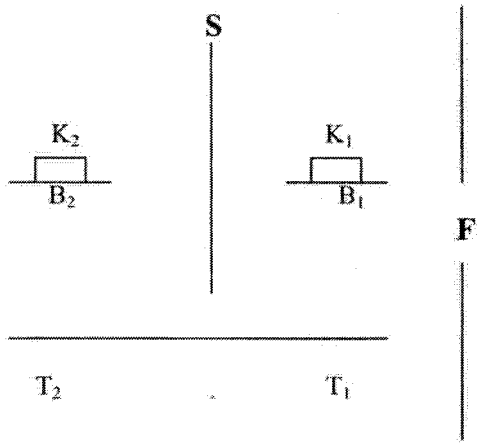
[6] The World of Color, p. 393.

Figure 1



K_1 and K_2 are two spheres; each respectively mounted with disks B_1 and B_2. B_1, a black disk, and its surroundings are illuminated from the daylight that floods through the window "F." B_2, a white disk, and its surroundings are illuminated through an umbrella "S." B_1 and B_2 are exposed to the same amount of measured light, but they appear different at a relatively short distance. B_1 appears "black" in normal daylight. B_2 appears "white" in a room with reduced light. T_1 and T_2 are two white or gray boards which, in front of the evenly gray and, therefore, against the same contrast-induced background "h," appears light. If the background "h" is removed, then B_1 and B_2 serve as backgrounds, and appear as contrast-inducing fields. It showed that the two boards, even with the contrast they were subjected to from the two disks, remained light at the same time disk B_2 appeared lighter (whiter) than disk B_1."

Katz concluded from this experiment "under the same figurative conditions that the contrast effect is only influenced by the strength and quality of the retinal excitement."[7]

[7] D. Katz, The World of Color, p. 393.

Kravkov and Paulsen-Baschmakova represent the same thesis in that they examined the contrast simulating reaction of a green-pigmented background (illuminated with a 300 watt lamp) in comparison with one of the same retinal value, but a white background and illuminated with a green light.[8] Looking at the drawings, the background with the colored light seemed to be much further back than the green pigmented one. The internal fields consisted of gray fields that were similar to Katz (see Figure 1) and were quite a bit in front of the background—especially the one lit by the green light—but in one row.

Kravkov and Paulsen-Baschmakova discovered that the white, green-lit backgrounds in 93 percent of all cases were recognized as their "inherent color." But appeared lighter and not as deep in comparison to the green-pigmented background lit with the lamp. Nevertheless, it showed that the light appearance of the background did not influence the contrast of the internal field. The authors concluded that the "inherent colors," which are relatively independent of the lighting, do not contribute to contrast.

Kroh came to the conclusion in his work, "About Color Contrast and Color Transformation," that with an internal field... colored illumination is always a stronger influence than a colored external field of the same retinal value. This strong influence of a colored light was examined by using a room with artificial light in which a field itself was only lit with normal daylight. We will show this process in Figure 2. (Jaensch also worked on this as well.[9])

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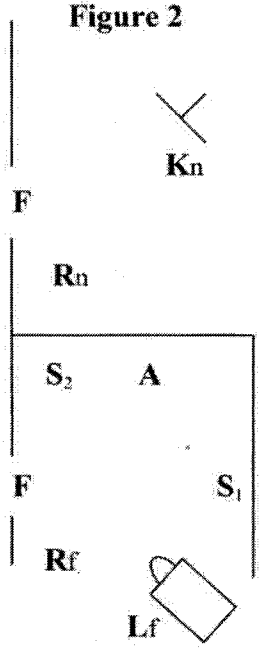
[8] Kravkov, S.W., and Paulsen-Baschmakova, W.A., Concerning the contrast effect of transformed colors. Psychological Research, 12, 1929, p. 88.

[9] Journal of Sensory Physiology, 52, (1921), p.165ff.

Figure 2



A room (R_f) is lit with a colored light source (L_f) and is separated with white paper umbrellas (S_1 and S_2) from another room that is lit only with normal daylight. An observer (R_f) sees a small opening (A) (1.5 centimeters to 2 centimeters) in the umbrella (S_2) and the color of the beam which comes from the gray, regular lit disk (K_n).

What is Kroh talking about when he states that an internal field is always influenced more strongly by the colored lighting of its surroundings as compared to a colored external field of the same retinal effect? What he says is, if one takes the white umbrella that is lit with colored light (S_2) (Figure 2), color it with the appropriate pigments, and expose it to daylight in such a way that it has the same retinal effect as (S_2) with colored light, one would find a lesser coloration of the internal field (A) compared to the color of the internal field.

Compared with the results of Katz and Kravkov and Paulsen-Baschmakova, this conclusion must come as a surprise. Let us look more closely at the phenomenal

conditions of Kroh's experiments. If the observer is in the color lit room (R_f), the white umbrella (S_2) (Figure 2) will, more or less, appear in its "inherent color," i.e., "white." With Kroh, this white as the inherent color is also not contrast creating, otherwise Kroh would find a darker internal field with his lighting in comparison to the pigmented external field lit by daylight, and of the same retinal effect. This is not very noticeable and compares with the results of (S_4), (Katz). However, Kroh says something totally different. He states that the internal field in (A) will have a stronger color change if the color of the external field is directed to the opposite color of this lighting than in the direction of the opposing color of a same retinal effect of a colored pigmented external field with daylight. The impression of the lighting as such is what is important. In fact, the colored lighting in Kroh's experiment is immediately noticed: Room (R_f) appears filed with colored light. Does this colored external field, or rather this colored illumination of the room, appear to cause a stronger contrast than a pigmented external field of the same retinal effect light with daylight? Kroh does not think so because he attributes the results not to the area of contrasts, but to the constancy of color.

Kroh believes that the change in color of hole (A) is basically a phenomenon of the constancy of color; just like the color lit umbrella (S_2) would be seen in its own color, or a "transformed" white, so would the hole color in (A) under the influence of its surrounding colored light also be "transformed."

This opinion did not contribute to the clarification of the facts and, therefore, did not gain much agreement. Let us look at the critique by A. Gelb who showed that Kroh

and Jaensch explained their experiments regarding constancy of color in a totally reckless manner.[10]

A. Gelb pointed out that the conditions under which Kroh's experiments were done were in principle those which were used in contrast experiments. The same as in those experiments, the experiments of Kroh's external and internal field are also changed independently; whereas in all experiments regarding the constancy of color, the pigment would be illuminated differently, but also the pigment of the illumination. These radical differences of the physical experimental conditions make it impossible to align Kroh's experiments with color constancy. Additionally, Gelb noted that the color of the viewed objects remains independent of the quantitative change of lighting; that the viewed objects would keep their original color in spite of the change in lighting. The colored field of the cutout (A) (see Figure 2) is a hole color that follows whatever change in lighting; the same as with disk (K_n) and also umbrella (S_2). Therefore, one cannot speak of an inherent color of this field having used hole colors to eliminate so-called color constancy. Hole colors are not subjects to the laws of color constancy. Additionally, Kroh himself explained that his experiments were nothing more than "shadow experiments"—meaning colored shadows—"with measurable variations of accessible conditions." Surprisingly, Kroh is of the opinion that the colored shadow came about by itself through the cooperation, and in accordance with theoretical principles that rule the phenomenon of color constancy. As Gelb noted, here he again overlooked the radical difference between the outer experimental conditions under which, on one hand, one had colored shadows, and, on the other hand, appearances of color constancy manifest.

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[10] ibid, p. 662. Herein is found reference as well to other opponents. D. Katz (The World of Color, p. 400ff), entirely agreed here with the critique given by A. Gelb.

In reality, the experimental order of Kroh (Figure 2) is based on a method of especially lively contrast appearances that were already mentioned by Hering.[11] Just the same as in Hering's experiment; the observer is also in a color-illuminated room in Kroh's experiment from which a hole color is observed which undergoes a contrast from its surrounding colored illumination.

If Kroh found that an internal field was influenced more strongly through the colored lighting of its surrounding than through a colored external field of same retinal effect, then that means that Kroh was not dealing with the phenomenon of color constancy, but rather with the necessity to differentiate within the phenomenon of contrast.

Gelb says that one needs to examine colored shadow and such phenomena of contrast in which a contrast-induced external field is exposed to a noticeable colored lighting. This must then be compared with such contrast experiments in which the pigment colors of the external and internal field are used as surface colors, and are viewed under the same illumination. Katz also pointed out that the phenomena of contrast "which appears with colored illumination demands special observation." For example, experiments of the contrast effect with colored shadows could originate from the colored illumination in that they strengthened impression of contrast on the consciousness. The white background on which the colored shadows rest appears obviously in colored lighting.

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[11] Hering, E., Pflügers Archive, 42, (1888).

Finally, Bocksch[12] agrees with the so-called air-light hypothesis of Bühler[13] in that hole colors "are not independent of the lighting." He means by this that he has long known that the same hole color will appear totally different depending on the illumination of the hole umbrella (Hering). But, Bocksch also does not want to recognize this phenomenon as contrast because it is influenced through the "lighting of the room" which appears before the hole umbrella. Therefore, he excluded all experiments in which the external field had any type of lighting because they did not belong in the field of contrast. We do not need to go into any discussion about Bocksch's work as Katz gives a detailed critique.[14]

From unpublished experiments, Professor A. Gelb realized that strength and type of retinal excitement was not all that determined contrast. What was important to influence contrast of a field was the impression that a particular field—this way or that—neutral or colored—was standing in an illuminated room.

Professor Gelb assigned me the task to examine if, and to what degree, the evidence of a particular room's illumination, and its arrangement contributed to contrast. In order to decide that, experimental controls had to be met where contrast constellations would be compared, where the excitement of the retina had the same controls, and, in regards to the visibility of the room's illumination, the field of vision was always different.

§ 1. Experiments in Light Contrast

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[12] Bocksch, H. Duplicity theory and color constancy, Journal of Psychology, 102, (1927), p. 385.

[13] Bühler, K., Handbook of Psychology, I, Part I. The Appearance of Color. Jena, 1922.

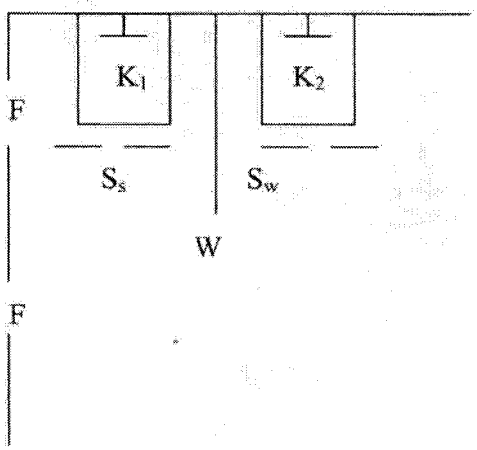
[14] Psychological Research, 11 (1928), p. 147ff; further in The World of Color, p. 457 ff and 449ff.

What follows is the examination of the contrast effect of a white, shadowed external field in comparison to an objective (retinal) uniform lighting without shadows or black external fields.

K_1 and K_2 (Figure 3) are rotating disks with black and white sections. Each disk is in a black, totally closed box (25 centimeters deep) where the light source (L), a 100-watt opaque light bulb, illuminates each box equally. On the long side of each box there is a door through which one can comfortably regulate the relationship of the various sections. Above the light source (L) on the front side of the box there is an opening through which the black-white mix focused on K_1 and K_2 is visible.

Twenty centimeters in front of the boxes (see Figure 3), left, there is a black umbrella with a hole (S_s), and on the right there is a white umbrella with a hole (S_w). Both are separated with a large screen (W) that reduces the illumination for S_w and its surroundings. S_s and its surroundings are illuminated by the daylight that comes through the window (F).

Figure 3



S_w and S_s are illuminated equally. The color appears to be the same when observed through a reduced aperture. Through free observation, as long as the distance is not too far, they are very different. In a room with reduced illumination, S_w appears "white" and, in "darkness," S_s appears "black" in a room with stronger lighting. The visibility of the lighting differences, and the difference in appearance of the two umbrellas, becomes even closer the observer moves to the groupings; up to 1.5 meters. In distances less than 1.5 meters, it becomes very difficult to have an overview of the situation. With incremental backing away, the grouping loses its clarity of lighting and organization. At large distances (e.g., seven to nine meters), the groupings become invisible to the observer. Then it is no longer possible to observe "everything in the field of vision of different lighting" (Gelb); i.e., one no longer has the impression that there is another light source to the right of the shadow-making umbrella (W) as of the left. At a distance of nine meters, the lighting appears the same—everything that is visible left and right of the wall (W) lies in the subjectively combined light of the room in which the observer is himself—and now umbrella S_w and Ss appear the same. S_w appears in the same "inherent color;" i.e., the same black as Ss.[15]

With this organization the following is observed: if one applies the same black-white mix on K_1 and K_2, for example, each 210° white + 150° S, they then appear as internal fields for umbrellas S_w and S_s, which appear different when viewed from different distances even with the same objective lighting.

Our question is: how do the objectively identical internal fields behave under the influence of the objectively identical external fields S_w and S_s if the same grouping is

[15] On the role of the distance of observation, D. Katz, ibid, p. 200 and 201, and A. Gelb, ibid, p. 623ff.

viewed from different distances? Would the contrast of the internal field depend only on the retinal stimulating conditions? If so, then the internal fields would always have the same contrast, and, therefore, would always appear the same regardless of the distance from which they are observed. Theoretically, that is the first possibility.

Let us assume that we first observe from such a far distance that (a) S_w in relationship to the "inherent color" looks just as black as S_s, and (b) S_w and S_s are in subjectively uniform lighting. When we then approach the grouping, then what phenomenal changes happen to S_w and its surroundings? At the approach, one can see the surface of S_w in its "inherent color" more clearly; it becomes whiter (the so-called "transformation") and, at the same time, the space around S_w becomes darker. The changes in contrast as the grouping is approached could also depend on the increasing "whiting" of the surface of S_w (the question of D. Katz in Section 3), in which case the internal field of S_w should become darker as the grouping is approached. Or, the changing in contrast could be influenced with the darkening around S_w. In this case, the internal field of S_w should lighten when approached. Which of these possibilities is happening in our grouping?

In order to answer this question, we observed the grouping from four different distances: nine meters, six meters, three meters, and 1.5 meters. To keep the retina of the external and internal fields constant, considering the different distances, we chose for the distance of nine meters S_w and S_s an opening of 50 x 65 centimeters and an aperture opening of 36 millimeters for the internal field. For the distances of six meters and three meters, the external field size was 33.4 x 43.4 centimeters and 12 millimeters. A corresponding reduction of the external and internal field for the distance of 1.5 meters was not possible otherwise the internal fields would have become too small. Therefore,

we had to use the same external and internal fields as we used for the three meter distance. The observation made at the shortest distance (1.5 meters); the aperture of the external and internal field was larger than those of the previous experiments. We will return to this point again later.

For the four distances, we used three different lightings for the internal field: the size of the white sector came to K_1 210° and 50°; the rest of the black-sector consisted of black cloth.

At the beginning of each observation, the internal field of S_w had the same objective lighting as the internal field S_s; the white sector of K_2 was at the beginning of the each observation also 210° white, 130° white, and 50° white. Now the observer successively compared the internal field of S_w with the constant internal field of S_s. (A simultaneous comparison was not possible since the physical distances between internal field S_s and S_w were too large.) An exact amount of time for an observation was not required. However, the observers were asked to keep their viewing to 2-3 seconds, and give their judgment: "Right (i.e., the internal field of S_w) lighter," or "Right – darker," or "Don't know," or "The same." What was being judged was always the internal field of Sw. After making their judgments, the white-sector of K_2 would be varied with the barrier method until the left and right internal fields appeared subjectively the same.

The quantitative results of these experiments are shown in Table 1. Under the column "Total Value for Internal Field S_w," the white sectors are shown in degrees that were set at K_2—in the internal field of S_w—so that a subjective constancy of internal field S_d was achieved. The values are averages of three to four individual settings. The next column on the right consists of middle variations. Next to that is the quotient that was

achieved by dividing the values of the white-sector of the constant internal field with the values of the white-sector to result in S_s.

Table I

		Internal Field in Ss = 50° W.			Internal Field in Ss = 130° W			Internal Field in Ss = 210° W		
Subject	Distance	Total Value for Internal Field SW	Middle Variations	Quotient	Total Value for Internal Field SW	Middle Variations	Quotient	Total Value for Internal Field SW	Middle Variations	Quotient
Sinemus	9 m	50°	0°	1	130°	0°	1	190.3°	13.3°	0.9
	6 m	42.5°	1.5°	0.85	114.6°	12.5°	0.88	153.3°	10.3°	0.73
	3 m	31.6°	0.8°	0.63	98.7°	21.9°	0.75	135.6°	10.6°	0.764
	1.5 m				71.3°	14.3°	0.54	108°	4.1°	0.51
Nahm	9 m	45°	0.6°	0.9	114.5°	8°	0.88	138.8°	29.5°	0.66
	6 m	35.3°	3.4°	0.7	76°	2.3°	0.58	123°	12.5°	0.58
	3 m	29.1°	3.2°	0.6	65.8°	4.8°	0.5	105°	3.6°	0.5
	1.5m							99°	12.6°	0.47
Wingenbach	9 m	50°	0°	1	130°	0°	1	210°	0°	1
	6 m	50°	0°	1	121.6°	0.6°	0.93	163.6°	4°	0773
	3 m	41.5°	3°	0.8	102.3°	0.7°	0.78	139.5°	9.1°	0.66
	1.5m	38.5°	2.5°	0.77	96.5°	1.1°	0.74	113.8°	2.5°	0.54
Kleint	9 m	43.1°	0.8°	0.86	130°	0°	1	191°	13°	0.9
	6 m	35.8°	2.1°	0.7	124.3°	4°	0.95	136.5°	3.1°	0.65
	3 m	28.3°	1.6°	0.56	85.1°	7.6°	0.65	109.1°	2.8°	0.51
	1.50m				66.8°	2.5°	0.51	79.3°	2.6°	0.37

Figure 4

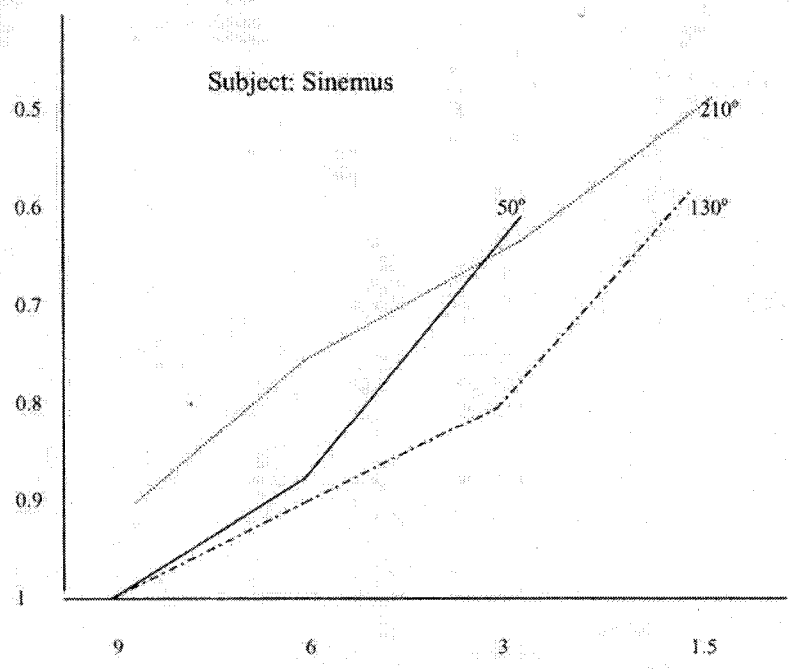


Figure 5

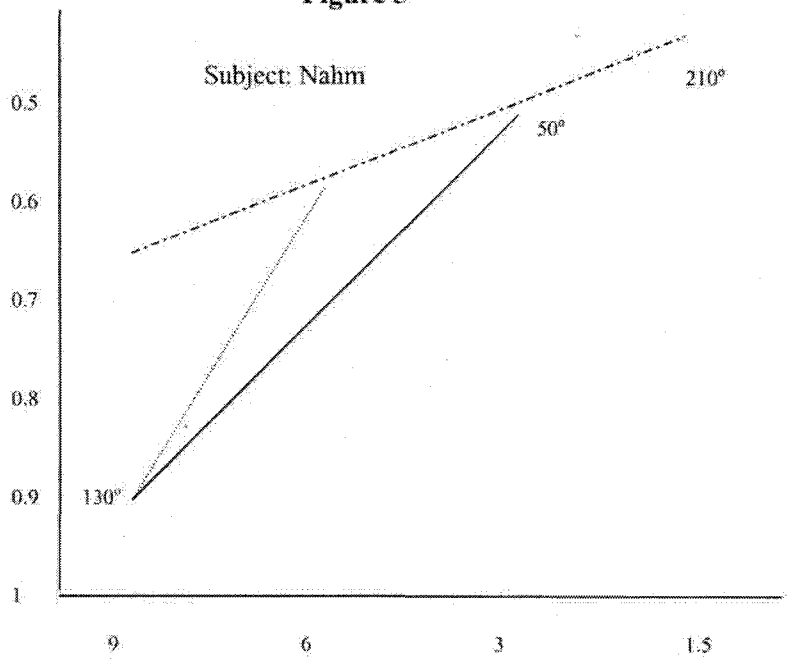


Figure 6

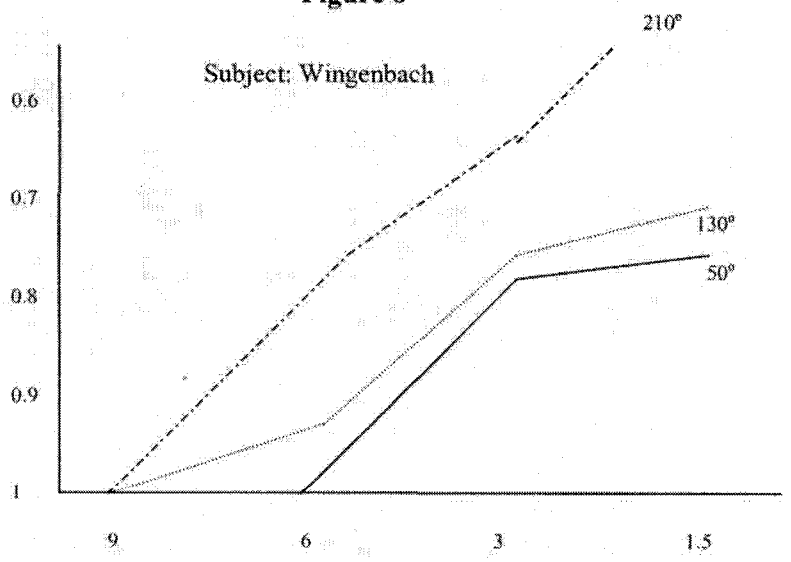
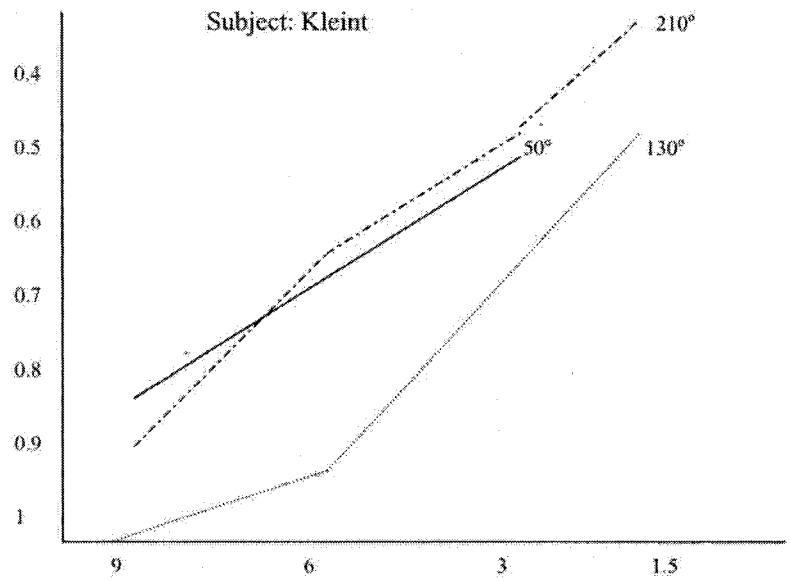


Figure 7



The numbered results show that the results of our contrast experiments are not only dependent upon the strength of the retinal stimulation as the lighting of the room also plays an important role, especially the lighting character of the external field. The so-called "transformation," i.e., the "whitening" of the external field, did not have an influence on contrast and, one would expect that the internal field of Sw would appear darker with smaller distances than with larger ones. In reality, the opposite results occurred; at smaller increments of distance, the internal field of Sw became lighter as compared to larger distances. The increasing lighter contrast of the internal field is the consequence of the increasing darkness of the space around the external field when viewed at smaller distances. That is way, as was said before, when one comes closer to the experimental organization, the effect of the contrast appears as if Sw is darker than Ss. Sw, when viewed as a superficial color, will appear "whiter" at small intervals. At small observation distances, they appear in a "darker field of lighting," in a space that is a

field with "darkness," and from where the lighting contrast of the internal field is decided. One sees at a distance everything as one and the same lighting of the space. And since Sw makes the same, or almost the same impression, one of a black surface color as Ss, that is why one finds the same, or almost the same, contrast left and right when viewed from a distance.

Our results were validated through the following experiment. If at first one observed the greatest distance, and then gradually came closer to the grouping without changing the size of the internal and external field, then it was surprisingly clear how the internal field of Sw became increasingly lighter. This observation could be repeated as often as wanted with the same results and regardless if one used small or large external and internal fields.[16]

Because of this reason, the quantitative results that we achieved during observation at 1.5 meters are indisputable. In these experiments, however, the retinal size of the internal and external field was twice as large as with the experiments that were observed at a distance of 3 meters. But, the way we see it, this change in contrast from 3 meters to 1.5 meters cannot be explained.

§ 2. Experiments with Color Contrast

(Neutral Internal Field, Colored External Field)

The following experiments were conducted in the same order as those in Figure 3 as explained in the previous paragraph. Instead of a black umbrella in daylight (Ss), we

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[16] Lehman, A., "Physical expressions of psychological states: Part II," Leipzig, 1899. Further: Révész, G., "On criteria of grayness," Journal for Sensory Physiology, 43, p. 351, 1909. Further: Katona, G., Implications of the relationship between achromatic and chromatic visual processes, Journal of Sensory Physiology, 53, p. 173, 1923.

now use a colored—red and green—hole umbrella that serves as the external field. On the other side of the wall (W), a white hole umbrella was installed and lighted with colored light—red and green from colored gelatin over a light bulb. Both external fields were grouped in such a way, and with the choice of colored lighting, that the same lighting mix was reflected back to the observer. (Helped with the control of a reduction umbrella.)

The observations were done the same way as before. The same four distances were chosen. However, at each interval, only two objective neutral internal fields were used instead of three as before with the internal field illumination of 50° and 210° white. The sizes of the external and internal fields were as before and held in such a way that the retina stayed constant during the different intervals of observation.

These subjectively identical settings created huge difficulties at times because the internal fields appeared phenomenally different. In the color-pigmented external field (left), the internal field appeared most of the time as a "glued on disk," whereas the internal field on the color-lit white external field (right) appeared more like a "room lit with a hanging lantern," especially in the middle observation intervals. This alone may be responsible for the large individual differences in the results of the numbers. However, the experiments came out the same overall.

Let us make the number the values more understandable by considering the phenomenal arrival through the change from the observation from afar (9 meters), and the steps in between to the close observation (1.5 meters; see Tables II and III). In daylight, if one places on the left a red pigmented external field and, on the right, a white external field that is red illuminated, and if both have the same retinal value, then both fields appear the same from the largest distance of 9 meters. Both show an almost identical

"inherent color" of red that appears like the same neutral room lighting. Here the internal fields also look almost the same. With an internal field lighting of 210° white, both internal fields appear as a light yellow-green. With an internal field light of 50°, both internal fields appear as a dark green with gray overtones. With our grouping, however, a total exactness between internal and external fields did not occur. To achieve that, one would have had to go to a greater distance than 9 meters that our experimental conditions did not allow. That is why the internal field in the red-illuminated white external field often appeared more as an intensive green at the largest intervals of observations.

If one uses green-pigmented external field on the left and a white but green-illuminated external field on the right of the same retinal value, then both external fields appear the same at the greatest distance. That is, a green in neutral lighting with internal fields of 210° white and of 50° white also appeared completely identical in density and color; that is, a light yellow-red or, better said, a gray-yellow-red.

When one changes the observation from being at a distance to one up close, the visible lighting order also changes gradually, and with that, the appearance of the external field on the right changes as well. One now increasingly notices that the space on the right from the wall (W) stands in a special-colored lighting (red, green). At the same time, the right external field becomes whiter. The white "inherent color" of this external field becomes increasingly stronger and becomes a "superficial white-red-green lighting." If the contrast would depend only on the retinal lighting conditions from which the right internal field suffers, then the internal field would keep its appearance when undergoing changes in observations from distant to close up. However, in reality, the appearance of the right internal field changes immensely. The closer one approaches the grouping, the

more one has to add red (green) and reduce the white from the red (green) illuminated side. This will balance the internal field on the left side subjectively.

In Tables IIa and IIb, and also in Tables IIIa and IIIb, the values of the white sector for the internal field in the color-lit external field continually go down while the values of the red (i.e., green) sectors go up as the observation distance decreases.[17] (The large variation [m.V.] is due to the changes of daylight illumination.)

Internal field in the red pigmented with normal light—external field (left) constant = 50° white. To achieve the same appearance, the internal field in the red-illuminated, white external field (right) required the following values for the white and red sector:

Table IIa

Subject	Distance	White Sector	Middle Variations	Red Sector	Middle Variations
Nahm	9 m	54.6°	5.9°	8.1°	9.8°
	6 m	45.2°	7.8°	12.6°	6.8°
	3 m	37.1°	6°	22.7°	4.1°
	1.50 m	35.6°	6.5°	19.3°	9.7°
Sinemus	9 m	45.8°	2.6°	14.7°	1.6°
	6 m	41.4°	3.5°	19.2°	3.1°
	3 m	35.2°	5.9°	28.6°	6.7°
	1.50 m	29.8°	3°	38.6°	4.8°
Usener	9 m	43.7°	2.6°	12.8°	1.5°
	6 m	40.5°	2.2°	17.3°	1.1°
	3 m	28.8°	3.6°	22.8°	1.1°
	1.50 m	29.5°	2.6°	32.4°	4.7°
Bodlée	9 m	60.6°	1.1°	21.1°	0.5°
	6 m	54°	1.3°	31.4°	1°
	3 m	38.4°	0.9°	36.3°	0.7°
	1.50 m	28.7°	0.8°	40.8°	0.9°
Schwenmler	9 m	60.5°	8.3°	23.6°	0.8°
	6 m	48.1°	1.4°	30.9°	1.3°
	3 m	36.6°	2.1°	39.6°	1.2°
	1.50 m	32.8°	0.9°	45.9°	0.9°

[17] The only exception in the values of the white areas is provided by the value given in the transition from 3 meters to 1.5 meters observational distance on the part of the subject, Usener; the only exception in the values of the real areas found in the values at the same change of distance.

Internal field in the red-pigmented, normal light—external field (left) constant = 210° white. To achieve the same appearance, the internal field in the red-illuminated, white external field (right), required the following values for the white and red sector:[18]

Table IIb

Subject	Distance	White Sector	Middle Variations	Red Sector	Middle Variations
Nahm	9 m	214.1°	4.3°	0°	0°
	6 m	206.2°	5.2°	22.7°	7.7°
	3 m	166°	3°	38.2°	14.8°
	1.50 m	131.9°	10.1°	78.2°	7.7°
Sinemus	9 m	194.2°	9.2°	31.6°	3.8°
	6 m	154.6°	9.6°	53.2°	10.8°
	3 m	137.1°	11.3°	74.3°	13.3°
	1.50 m	116.6°	11.1°	103°	14.2°
Usener	9 m	190.8°	11.4°	17.3°	8.7°
	6 m	164.4°	11.4°	50.7°	5.5°
	3 m	136.3°	8.9°	80.6°	15.8°
	1.50 m	111.7°	4°	97.2°	7.1°
Bodlée	9 m	183.7°	7°	16.5°	2.9°
	6 m	163.1°	16°	38.5°	13.8°
	3 m	138.9°	16.7°	67.7°	14.4°
	1.50 m	122.1°	9.1°	87.8°	15.6°
Schwemmler	9 m	179°	11.5°	59.1°	7.8°
	6 m	147.7°	7.5°	75.2°	9.2°
	3 m	129.6°	11.1°	95.2°	7.9°
	1.50 m	116.1°	13.9°	110.3°	10.1°

Internal field in the green-pigmented, normal light—external field (left) constant = 50° white. To achieve the same appearance, the internal field in the green-illuminated, white, external field (right) required the following values for the white and green sector:

[18] The remaining sector was here as in the follow tables also: Ib, IIIa, and IIIb black cloth.

Table IIIa

Subject	Distance	White Sector	Middle Variations	Green Sector	Middle Variations
Sinemus	9 m	50.8°	1.3°	0°	0°
	6 m	42°	1°	15.5°	1.3°
	3 m	34.3°	2.7°	25.8°	3.5°
	1.50 m	30°	1°	35.8°	2.7°
Nahm	9 m	48.8°	0.9°	5.3°	1.8°
	6 m	39.6°	0.6°	15.8°	2°
	3 m	37.1°	0.2°	30.6°	2.2°
	1.50m	34.8°	1.9°	37°	1.6°
Schwemmler	9 m	54.3°	3.6	11°	1.3°
	6 m	47.3°	2.6	17.5°	1.8°
	3 m	39.5°	2	23.8°	2.1°
	1.50 m	34.8s°	4.2	27.3°	2.1°

Internal field in the green-pigmented, normal light—external field (left) constant 210° white. To achieve the same appearance, the internal field in the green-illuminated, white external field (right) required the following values for the white and green sector:

Table IIIb

Subject	Distance	White Sector	Middle Variations	Green Sector	Middle Variations
Sinemus	9 m	210°	0°	0°	0°
	6 m	194.1°	3.4°	16.5°	2.6°
	3 m	171°	4.3°	30.5°	5.3°
	1.50 m	161.8°	3.1°	46.3°	7°
Nahm	9 m	216.1°	4.1°	5.5°	1.8°
	6 m	198.1°	1.8°	18.1°	1.8°
	3 m	186.3°	4.1°	28.3°	2.1°
	1.50 m	182.5°	3.1°	34.8°	5.1°
Schwemmler	9 m	210°	0°	0°	0°
	6 m	188.6°	4.4°	24.1°	2.8°
	3 m	158.8°	3.5°	48.8°	4.8°
	1.50 m	153.3°	3.7°	66.6°	10.8°

The internal field of the color-lighted side changes accordingly with the advance of the observer. That is, it changes into the direction of the opposing color of the special lighting. The internal field becomes greener (or redder) when the external field is

illuminated in green (or red). The contrast does not alone depend on the retinal lighting. The whitening (the "inherent color" becomes clearer) of the external field does not play a role, otherwise the internal field would become darker and darker but for the spectacular coloring present in the room of the right external field. With these color-pigment and colored lighting experiments of the external field, we have the specific results of experiments that we showed earlier above with a black and a shaded, white, external field.

Now the question becomes: Why were the experiments of Katz and Kravkov & Baschmakova—in which the contrast of a pigmented external field in daylight was examined with the contrast of a white with the same retinal value—did not produce the same analog results as we did. This was due to the fact that in the experiments of Katz and Kravkov & Paulsen-Baschmakova, the internal fields did not lie at the same level of the external fields and, therefore, could not be seen in the specially lit room. The internal fields were quite a distance before the specially lit room, and much closer to the observer, so that the conditions did not allow the results that we achieved.

The conclusions drawn from the results of Sections 1 and 2 are now given. The contrast, which a shadowed or color-lit, white, external field exerts on an objective, gray, internal field often can phenomenally appear the same as those that start from an identical retinal, pigmented (black or color pigmented) external field in normal daylight. This similarity in regards to the contrast phenomenon happened in our experiments when, at the observation for a distance left and right of the umbrella (W), two rooms with different special lighting could not be seen. Instead, everything was lit with the same, even room illumination in that the observer stood and it appeared, therefore, the external fields also showed the same "inherent color." When the conditions of observation were produced in

such a way that the lighting of the field would visibly increase, we would have achieved that by decreasing the distance of observation. Then a shadowed or a color lit, white, external field created an increasingly divergent contrast compared to an identical retinal pigmented (black or color pigmented) external field in normal daylight. In this case, the contrast is not influenced from the so-called "transformation" ("whitening" as we call it) of the external field, but from the field of illumination of the room of the external field.

§ 3. Experiments with the Nuance Machine

(Light Contrast)

To complement our present results, the following experiment will consist of two phases. In the first phase, two gray internal fields (hole colors) are successively and simultaneously compared, and are adjusted to the subjective equality. One internal field is in a white-pigmented external field, and the other is in an immediately neighboring, black-pigmented external field. Both external fields are in a neutral, same-strength illumination. We describe these same regulations as "A-setting."

In the second phase of the experiment, the black-pigmented, external field is replaced with a white-pigmented one. It is turned away from the light to such a degree that its retinal effect is the same as that of the previous black-pigmented, external fields. Again, the observers have to notice that subjectively identical focus of the internal fields. We call this the "B-setting."

The A- and B-settings result from completely identical conditions of stimulation. However, the appearance of the external fields with the B-setting is different than that of the A-setting.

Now we will examine why and how the B-setting appears different due to the apparent deviation of appearance of the external fields from the results delivered from the A-setting.

The A-setting was done was follows. On the floor of a nuance machine (Hering),[19] with open sides turned to the window, are two disks each with a black and a white sector (Baryt-white and Cloth-black). Inside of the box, and 65 centimeters above the disks, a paper umbrella is horizontally mounted. The umbrella is black on its underside and its upper side is covered with a half-white and half-black paper. A round, clean-cut hole with a 12 millimeter diameter is punched out on both division lines in a moderate distance from one another; the disks are visible as internal fields through these holes. The top wall of the box is replaced with an open frame over which a headrest is installed so that the eye of the observer is always at the same distance (42 centimeters) from the hole umbrella. The observer fixates on the dividing line between the two holes and must state if the internal field in the black external field looks the same as in the white one or if it appears lighter or darker. The individual observations take only approximately 2 seconds. During the structured pause, the observer looks at a gray umbrella and, during this time, necessary changes are made to the disk sectors of the black external field to make them identical to one another. The disk under the white external field stays the same for one row of experiments. In the first row of experiments the white sector is 210°; in the second it is 130°, and in the third it is 50°.

For the B-setting, the order was changes as follows. Instead of umbrellas that are half-black and half-white, now we use two white umbrellas (black on the underside) that

[19] Hering, E., Outlines of a Theory of the Light Sense, p. 54 and 121.

are only half as big and that can pivot on a horizontal axis. Just as before in the A-setting, a hole (12 millimeters) is punched out at the appropriate space of each umbrella. The umbrella of the A-setting with the white half of the umbrella is now again horizontal. The other one, which now is at the point of the black, external fields, is moved away from the window light to the point where it becomes as low in light as the previous black, external field. (This seeing of the objective identical light exposure results through the help of a reduction umbrella.) Also, the B-setting is conducted in the same manner as the A-setting. The required variations of the disk sectors for the identical settings of the internal fields are done with the disks that are under the umbrella with the reduced light.

The degrees of the white sectors are noted in Table IV which contained the variable disk under the horizontal, black, external field (A-setting), and the other under the retinal, identical white from the light deviated external field (B-setting) to produce the same impression of both internal fields.

The values of Table IV are averages from five settings. One can notice in Table IV that a smaller, white sector was more necessary for the B-setting than the A-setting.[20] The contrast appears on the internal field of a white external field that is diverted from a light source, but of identical retinal effect as a black external field that is pointed toward a light source, as if it is darker than the black pigmented one.

[20] A certain exception is provided the value of subject Bodlée with a brightness of 50 degrees white.

Table IV

	Lower Horizon Situated White Surrounding Field Constant 210° White				Lower Horizon Situated White Surrounding Field Constant 210° White				Lower Horizon Situated White Surrounding Field Constant 210° White			
	A-Setting		B-Setting		A-Setting		B-Setting		A-Setting		B-Setting	
Subject	White Sector	Middle Variations	White Sector	Middle Variations	White Sector	Middle Variations	White Sector	Middle Variations	White Sector	Middle Variations	White Sector	Middle Variations
Bodlée	75.3°	6.2°	64.7°	4.5°	39.1°	5.1°	38.1°	4.1°	10°	0.7°	11°	0.7°
Blug	77.7	9	58.8	7.7	41.8	6.4	32.6	4.2	12.8	3.3	10.5	1
Sinemus	91.8	8.6	74.7	8.6	50.5	4.1	34.1	2.3	17.6	2.6	10.2	1.1
Wingenbach	80	9.5	70.5	8	38.5	3.5	29.7	3.7	12.7	1.2	9.7	0.7
Rosenbaum	51	5.6	23.8	2.8	24.5	3.8	15.5	2.6	4.5	3.6	3.6	0.6

A measure for the contrast results for A- and B-settings, the following A- and B-quotients were obtained by dividing the resulting white sectors of the various disks with the white sector of the constant internal fields.

Table V

	Internal Field Held Constant =210° White		Internal Field Held Constant =130° White		Internal Field Held Constant =50° White	
Subject	A Quotient	B Quotient	A Quotient	B Quotient	A Quotient	B Quotient
Bodlée	0.35	0.3	0.3	0.29	0.2	0.22
Blug	0.37	0.28	0.32	0.25	0.25	0.21
Sinemus	0.43	0.35	0.38	0.26	0.35	0.2
Wingenbach	0.38	0.33	0.28	0.22	0.25	0.19
Rosenbaum	0.24	0.11	0.18	0.11	0.09	0.07

About Phenomenology

The white external field of the B-setting that was turned away from the light appears "whiter" (so called "transformation") than the same retinal, black-pigmented,

contrast field of the A-setting.[21] If, nevertheless, it creates a contrast on the internal field as if it were darker than that of the black-pigmented one, then it proves, as we have shown in our results in Section 1, that (a) the retinal relationship alone was not decisive, and (b) that the white as "inherent color" was contrast inducing. Otherwise, we would have found the opposite result; that is, the "darkness" that was over the light diverted the external field.

So far, there was agreement with our previous findings, but these results, and their clarity, cannot just be added to those of Section 1. The differences between the contrast effect of a white umbrella that is turned away from the light, and a black umbrella that is illuminated with the same retinal values, are clear with the nuance machine. Nevertheless, in spite of the short distance of the observer to the objects, the difference was not nearly as large as between that of the shadowed white and same retinal black umbrella's contrast induction when observed at 1.5 meters or 3 meters distance in the experiments shown in Section 1. The reason for the noticeable difference in results between Section 1 and the nuance machine becomes clear when one closely examines the phenomenal relationship of the experiments of the nuance machine.

The self-shadowing of the white umbrella, when turned away from the light, does not show so much as independent darkness in front of the umbrella in the nuance machine as the shadowing of the white umbrella with wall (W) in the experiment in Section 1. Additionally, the border of the shadow in the nuance experiment, and the border of the umbrella totally overlap whereas with the experiments in Section 1., the

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[21] In this case, it is a question of the perception of "true color" of colored bodies under varying orientations of their surfaces in relation to the light source. Compare to E. Hering, (Outlines of a Theory of the Light Sense, p. 9), D. Katz, The World of Color, p. 150 ff, and A. Gelb (ibid.), p. 617ff.

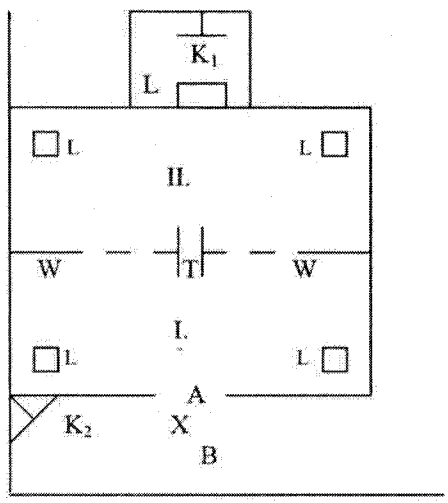
visible special lighting went considerably over the border of the hole umbrella of the external field. Something similar happened here as in the experiment by W. Fuchs.[22] He noticed that color surfaces that lie next to each other were only visible as two different color surfaces if their borderlines did not sit on top of one another.

A second factor is monocular vision is an experimental variable that may have reduced clarity of perception of shading. The binocular visual approach which we used in the trials in Section 1 and shifting the focus of vision in successive comparisons[23] both significantly favored perception of lighting.[24]

§ 4. "Two Room" Experiment

a. Color-Free External and Internal Field

Figure 8



[22] Fuchs, W., Experimental investigations of simultaneous overlapping images from the same angle of light, Journal of Psychology, 91, 1923.

[23] Katz, D.: The World of Color, p. 263ff.

A large box (Figure 8) measuring 2.36 meters high, 2.36 meters long, and 1.54 meters wide is made into two rooms by a wall (W). Through a cutout (A) of the exterior wall of Room 1, an observer (B) can look into Room 1. There is a door (T) in the wall (W) in which a hole umbrella of semi-transparent white paper (50 x 50 centimeters) is installed. The hole in the middle of the umbrella is filled with light as the internal field. The light comes from a lighted black-white disk (K_1). The disk is mounted on a sphere which is itself mounted in a black, enclosed box just like the ones used in the experiments in Section 1 and 2 and illuminated from inside the box. The box stands close behind the hole groupings; a cutout in the back wall of Room 2 exposes a piece of the disk. The semi-transparent hole umbrella in T is now lit at intervals from the front with lights (L) from Room 1--these lights are invisible to the observer—and also from rear with lights from Room 2. The lights are regulated and situated in such a way that the amount of light received by the front and the rear of the semi-transparent umbrella is the same for the eyes of the observer. (The objectively identical settings are achieved with the help of a gray reduction umbrella that is placed in front of cutout A).

In spite of the identical retina lighting, one gets very different impressions depending if the transparent hole umbrella is lit from the front or the rear. The surface of the umbrella appears like a wall that closes a room in which there is "light" when lit from the front. The wall itself appears as a granular surface in this light-filled room. At the same time, the surface of the umbrella appears itself as "very light." This lightness does not mean almost "white-similarity," but the impression that the hole umbrella is strongly "lighted." When illuminated from the back, it is not always this way at all. The umbrella

[24] With regard to these reduction effects, we find great individual differences. The largest reductive effect is found in the subject Bodlée; the smallest with subject Sinemus.

surface itself, which closes the back of Room 1, appears in a darker, dusk-filled room (Room 1) and is, glowing by itself from analog-specific fluorescent fields. It has light from itself and in itself. At the same time, when the umbrella surface is lit from the back, it takes on the characteristics of a surface color; the surface granulation is now no longer visible.

Although the umbrella surface has light when lit from the back, the observers declare it "darker" than when lit from the front. This "darker" does not mean "blacker," only that the lighting appears inferior; much like standing in a room (Room 1) filled with twilight.

The instructions were to report if the internal field in the middle of the semi-transparent hole umbrella changed when the umbrella's light alternated between Room 1 and 2. The observer was not to fixate his or her attention on the internal field, but to casually give in to the overall impression without staring.[25]

Now it showed that the appearance of the internal field changed in spite of identical retinal excitement that the external field evoked in both cases. If the external field—the umbrella surface—is lighted from the front (and from the back), then the internal field becomes darker (lighter). This becoming darker or lighter of the internal field in the umbrella surface must come from the lightness or darkness that is in front of the umbrella surface.

The darkening of the internal field when the external field is lighted from the front, and the lightening when illuminating the umbrella surface from the back, becomes even clearer when viewed with eccentric retina lenses than when viewing the internal

[25] Influences by adaptation fluctuations were out of the question with the fast lighting changes.

field directly. This coincides with the fact that the contrast effect is stronger when viewed indirectly than when viewed directly.

Our experiments cover six different gray internal fields: the white sector of the round disk K1 (compare Figure 8) resulted each time 70°, 100°, 150°, and 330°. Exact quantitative measuring could not be made. Whatever we did was not satisfying. We had to be satisfied with qualitative results in our experiments. We also want to comment that we also always only received the same comments of the same direction. Sometimes, the change in the internal field was very noticeable, other times less noticeable. The biggest change of the internal field happened when the lighting was changed on the disk K1 to 150°, 210°, and 180° white. For some individuals, a clear change of the internal field was noticed at illumination of 70° and 330° white; i.e., at an almost black or almost white internal field.

b. Colored External Field and Neutral Internal Field

The same experiments were conducted with colored lighting of the aperture umbrella. The lamps were covered with red gelatin and were placed closer to the transparent umbrella, which the observer could not see. The lamps were regulated so that both methods of lighting—front and back—produced physically the same amount of light. This time we had three different carpenters' papers that had the following disk values in daylight:

Gray 1 (light)	165° white + 6° blue	+	4° red	Rest
Gray 2 (middle)	67° white + 6.5° blue	+	9° red	Cloth
Gray 3 (dark)	47° white	+	4.5° red	Black

It is not easy to explain the phenomenal conditions that existed when one lighted the umbrella surface alternately from Room 1 and from Room 2. When the umbrella surface was illuminated from Room 1, it appeared as a rough granular surface in a room

filled with intense red light. The surface of the umbrella appeared to some observers as "white," to others as a "red white" bathed in red lighting. When lit from the back, the umbrella surface appeared more as a flat, rich red without any granulation; "like red plate glass" or "like a self-glowing red." In front of the red, flat color was a "darkness" or, at the least, a "deep husk."

The experiment was constructed in such a way that the lighting was alternated from front to back, and so on. With this we wanted to find out if the internal field would stay the same or not. We were not able to do this very satisfactorily, but the size and direction of the change of the internal field, and its results, are clearly noticeable. From the experiments with the larger numbers we can give the following values as examples (see Table VI). The values on Table VI show that the internal field appeared green to all observers when the external field received red light from the front. The results are valid for all three light settings of the internal field.

Table VI

	Screen vs. Front Lit			Screen vs. Back Lit		
1) Subject: **Siemsen:**						
Gray 1[26]	102° Green	68° White	61° Yellow	83° Green	89° White	62° Yellow
" 2	49° "	38° "	15° "	43° "	54° "	14° "
" 3	25° "	12° "	7° "	22° "	17° "	6° "
1) Subject: **Nahm:**						
Gray 1	90° Green	62° White	53° Yellow	53° Green	73° White	52° Yellow
" 2	44° "	32° "	21° "	40° "	45° "	21° "
" 3	15° "	12° "	3° "	11° "	22° "	3° "
1) Subject: **Schwemmler:**						
Gray 1	93° Green	64° White	53° Yellow	76° Green	84° White	52° Yellow
" 2	74° "	24° "	21° "	56° "	38° "	19° "
" 3	30° "	14° "	10° "	29° "	28° "	8° "
1) Subject: **Usener:**						
Gray 1	134° Green	85° White	70° Yellow	100° Green	101° White	90° Yellow
" 2	51° "	49° "	22° "	44° "	51° "	18° "
" 3	18° "	8° "	2° "	10° "	18° "	2° "

[26] Cf. above: the reciprocal effects among three different gray tones, p. 33.

§ 5. Experiments with Colored Shadow

We used a red and colorless light source when experimenting with colored shadow. Each light source threw a shadow on the projection wall. We noticed the following: (a) the green contrast shadow appeared weaker at a distance of 5-6 meters, and (b) intense green when viewed at a distance of 1 meter. The closer an observer got to the projection wall, the stronger the impression became that the white background on which the shadow appeared was in a many-colored room lighting. If the observer came very close to the projection wall (50 centimeters), then he or she saw felt drawn into the red lighting and the shadow appeared as the strongest green contrast.[27] The projection wall appeared much "whiter" when viewed up close than when viewed from afar. The increase of the change in contrast of color happened mostly under the influence of the colorful (red) room lighting, but not with the continually clearer "inherent color" of "white."

We tried to work out a method of compensation similar to the disk method used by Sachs & Pretori for this purpose.[28] Enough of the external field color was to be added to the contrast shadow until the contrast shadow would disappear. In our case, it was important to add enough red to the green contrast shadow until it appeared colorless. After many unsuccessful attempts--most of the qualitatively useable methods did not bring quantitative results--the following steps produced a workable method:

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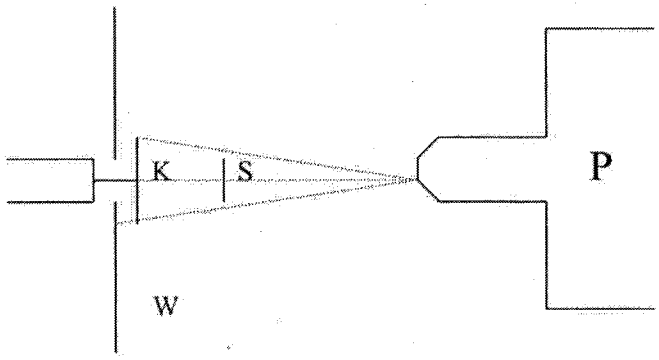
[27] Observational distances of less than 50cm are not included, since at that point the surface variations of the wall begin to overwhelm the contrast effects.

[28] Viz. Sachs & Pretori: Measurement investigations of the colored simultaneous contrast. Pflüger Archive, 60, (1895).

Figure 9



A round cardboard disk S (Figure 9) with a diameter of 12 centimeters was suspended on a very thin string in such a manner that a red light source (projection lamp P with a red gelatin covering before the lens), together with a nearby and relatively colorless light source (a regular Osram light, not noted in Figure 9), cast a shadow on a white wall (W) (75 x 100 centimeters). We then fully covered the white "Kreisel" rotating disk (K) mounted on a Musil-type color variator at about the same level as the wall (W). (The axis of the color variator was aligned through a hole in wall W.) By means of a Kreisel rotating disk K which, at the outset of each observation, appeared as white as the wall with a second sector gradually mixing in the color red--that is, simultaneously adding red to the green shadow color--until this shadow color was completely neutralized. The extent to which the red sector needed to neutralize the green contrast color served as the measure of the magnitude of the contrast effect.

This red sector, however, was not a sufficiently exact measure because the added mixture of red changes the brightness of the contrast shadow as well. We may, however, ignore this change in brightness as it only modifies to a minor extent the magnitude, but not the direction of the change in contrast.

The red illumination of the surrounding field was not very intensive, but yet it was strong enough to assure that the following could be observed at close proximity: the contrast shadow clearly was of green color. The observation was made at distance(s) of 5 meters and 1 meter. The experimenter mixed into the green contrast shadow on the rotating disk the color red and the subject said, "Stop," when she observed that the shadow appeared purely gray without any intermixing of any color. The additional mixing-in of red, however, did not take place on a continuing, gradual basis, but occurred in stages as after-images were formed during the process of longer periods of observation. The subject was requested to conduct her observation during a very brief time span (2-3 seconds) and to provide a judgment to the effect of "Green," "Still green," "Just a little green," etc. In the intervening pauses, the red sector was increased. Thus, the subject came to see at various points only the altered stimulus, but not the actual change as such.

Table VII

Subject	Observation Distance: 5 m		Observation Distance: 1 m	
	Red Sector	Middle Variations	Red Sector	Middle Variations
Cohen	177.6°	6°	216.4°	12°
Usener	170°	7.4°	218.4°	9°
Oppenheimer	122.9°	8.6°	170.2°	4.2°
Galli	154	7.3	171.3	7.5°

According to Table VII, one can readily note that at a distance of observation of 1 meter, a significantly greater red sector was required to compensate for the green shadow closer by all subjects than at an observation distance of 5 meters. Thus, in close-up

observation, the shadow color appeared as a more intensive or more saturated green than in more distant observation.

Thus, our measurement experiments clearly confirm that which we had stated at the outset of this section as hypothesized, qualitative propositions.

§ 6. Experiments with Flor-Contrast

To demonstrate the presence of lively color contrast phenomena, the so-called Flor-experiment, in addition to the shadow experiment, is often cited. The fact that the lively character of the colored shadows comes about substantially under the conditions of the joint effects of perception of multi-colored illumination was demonstrated above.

In conventional explanations, one frequently refers to the observation that granules and unevenness in the paper(s) becomes invisible during Flor-contrast. In spite of this, this alone does not seem to be decisive as in the case of the ordinary rotating disk in which one does not perceive any unevenness. Yet, in ordinary disk experiments, not even a proximately strong contrast effect is found in comparison to that which is found in Flor-contrast.

Helmholtz assumed that particularly intensive contrast colors would appear under conditions of low intensity of the surrounding color. He specifically cited as proof the Flor-contrast and the colored shadows.[29] Hering,[30] in work contrary to that of Helmholtz, proved that this was not the case. The intensity of the contrast colors increases, as Hering claimed, with the intensity of the induced colors. The fading of the colors thus cannot be regarded as the direct cause of the strong contrast effect in the case of the Flor. Therefore,

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[29] Helmholtz, H., Physical Optics, II, p. 234, 1911. (Published by Nagel).

[30] Hering, E. An apparatus for color mixing for diagnosis of color blindness, for the purpose of the examination of the contrast phenomena, Pflügers Archive, 42, 1888.

von Kries[31] has urged that, in the explanation of the Flor-contrast, attention should be given to the so-called "peripheral factors."

We agree with von Kries to the extent that in the explanation of the Flor-contrast, one cannot pay attention exclusively to such factors as physical intensity of the surrounding color, the disappearing quality of the surface of the paper, etc. On the other hand, we cannot agree with von Kries insofar as he includes judgment factors under the heading of peripheral factors, particularly as these relate to the dualistic separation of "sensory reception" (Empfindung) and judgment (Wahrnehmung) taken from Helmholtz.[32]

We believe that by means of our experiments so far, we also have established a new viewpoint for the explanation of the Flor-contrast. This we wish to present by means of the comparative contrast experiments with and without Flor.

The experiments were conducted by means of the rotating disk (Kreisel) in daylight in two phases.

In the first phase, and by means of a three-disk system, an objectively gray ring (90° white) was produced on a red background, and the subjects were requested, in the manner of Sachs and Pretori (ibid.) to provide compensation adjustments on the gray ring to eliminate its green contrast color. Red was to be added to the ring to the point at which the green contrast color would disappear. These compensation procedures took place at four different observational distances: 1 meter, 2 meters, 3 meters, and 4 meters.

In the second phase, a comparable set of experiments was conducted. However, in this instance, a disk of white silk paper (Flor), that covered the entire rotating disk, was

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[31] Von Kries, General Physiology of the Senses, p. 276/77ff. Leipzig, 1923.

[32] Compare to Gelb, A. (ibid), p. 607ff.

placed over the three-disk combination, and rotated as well with the disk. A solid disk of silk paper of the same size as the largest disk was fastened to the axis of the rotating system.

Table VII presents the average values for five positions as were required to compensate for the green contrast color in the ring in experiments with and without Flor.

Table VIII

Subject	Distance	With Flor		Without Flor	
		Red Sector	Middle Variations	Red Sector	Middle Variations
Siemsen	1 m	28.3^0	1.6^0	195^0	33.3^0
	2 m	44.3	3.7	139.6	15.1
	3 m	59.6	3.1	152	21.3
	4 m	84	2	155.3	24.7
Usener	1 m	31.6	1.8	153.3	3.8
	2 m	45.6	2.8	133.3	3.8
	3 m	63.6	2.8	144.3	14.4
	4 m	76.3	2.9	158.6	2.4
Goldmeier	1 m	76.6	20.8	113.3	6.9
	2 m	106.6	9.5	138.6	47.1
	3 m	128.3	5.5	167.3	31.5
	4 m	144.6	2.8	189	47.3
Schwemmler	1 m	73.6	14.8	139	6.6
	2 m	99.6	12.4	134	2.6
	3 m	136.6	24.4	156	4
	4 m	170.6	28.8	187.3	23.2

First of all, one readily notices according to Table VIII that the Flor-contrast is much stronger at smaller distances of observation. To compensate for the green contrast color, one requires significantly more red than in experiments without Flor. As one observes more closely how the contrast changes, on one hand without Flor, and, on the other with Flor with increasing distance of observation, one finds the following. In experiments without Flor, the counter-active coloring change of the ring increases with

distance of observation; that is to say, with a reduction of the angle of observation as the distance of observation increases. Thus, one observes that the necessary red sector required for significant grows with increasing distance of observation.[33] In experiments with Flor, such simple regularity or lawfulness cannot be claimed.

Figure 10

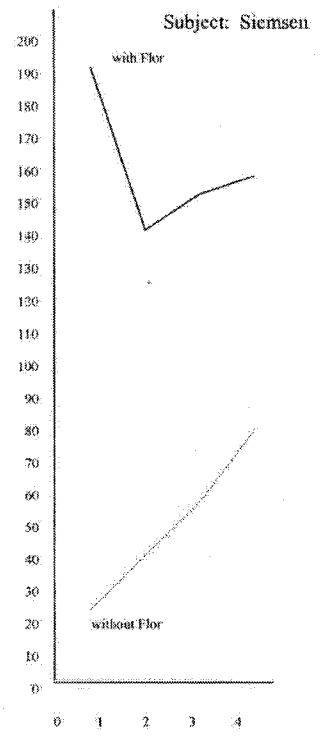
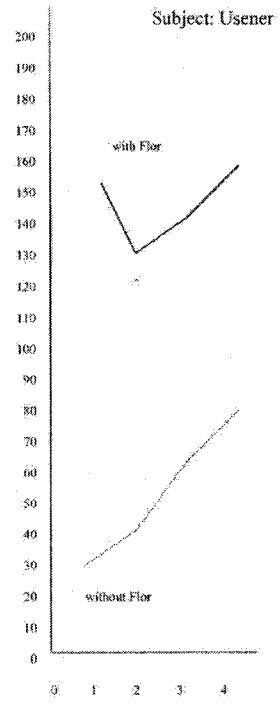


Figure 11



The subjects--Siemsen, Usener, and Schwemmler--note that the Flor contrast increases in transition between a distance of 1-2 meters, particularly in the experiments with Siemsen and Usener where it increases further. In the experiments with Goldmeier as the subject, Flor contrast increases with increasing distances of observation. It appears similar as without Flor. One notices most distinctly this difference in behavior of contrast with or without Flor at various distances of observation. These are necessary within the

[33] This result is consistent with experimental results previously obtained by Révész (ibid). Katona (ibid) p. 173 conducted analogous measurement experiments and found analogous results.

curves if the vertical observation is measured in meters and imposed on the ordinate in degrees of the red sector, and that all of this is necessary for the compensatory conditions.

Figure 12

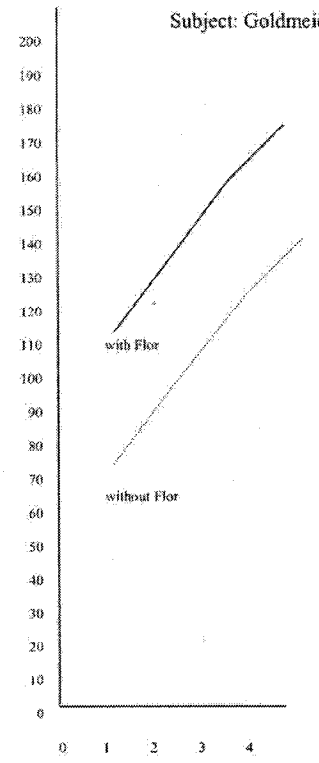
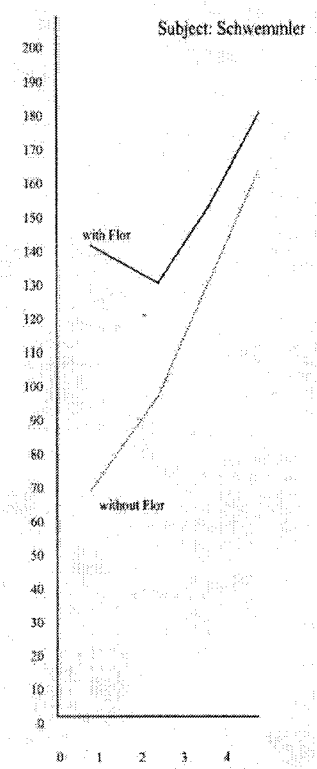


Figure 13



This rather noteworthy behavior of Flor contrast can only be understood if one takes into account the phenomenal conditions associated with the various distances of observations. The way in which the Kriesel wheel appears in Flor is described as follows. At closer distance of one meter, one has the impression that a bright veil or fog clearly separates itself in front of the disk. This phenomenon, which was already explained by Helmholtz, points to a certain analogy of the impression of a reddish, special illumination of the disk. With increasing distances of observation of 1.5 to 2 meters the disk and the Flor are no longer perceived as being separate. The impression of a quasi-special illumination still appears, but it is no longer as compelling as before. Accordingly, in most cases, the contrasts beyond those found by Goldmeier with greater distances of

observations—3-4 meters—Flor and disk no longer appear separated. Rather, one has the impression of a pigment color as in normal illumination of the room one and as one would otherwise note in a rotating disk. Now one should expect the complete disappearance of the separation between disk and Flor. With this disappearance of the separation, one has the impression under these conditions that a quasi-illumination is present, and thus the Flor contrast is further diminished. However, now the opposite is the case. A different set of regularities or lawfulnesses appear following the simultaneous contrast with increasing distance of observation. Compare this to the experiments without Flor. This lawfulness cuts across and overcompensates the prior lawfulness that we had observed where we have noted that the contrast becomes smaller and smaller to the extent to which a special illumination is observed. In the set of experiments with Flor, these results still maintained both sets of regularities. Only in the experiment of Goldmeier does that lawfulness consistently appear in which contrast grows with increasing distances of observation. The extent to which the increase in contrast with increases of distance of observation is to be interpreted (experiment without Flor). This must remain to be answered by future experiments. We would guess that the major question in this connection relates to the extent to which the disks are observed at greater distance and to which a less compact, substantive character appears so that this involves a softer structure, which favors the appearance of contrast.

In connection with the Flor experiments we point to the work of Theodora Haack.[34] Haack describes on page 124 contrast experiments at various intensities of illumination. This variation in intensity of illumination appears through an epicotister

[34] Haack, T.: Contrast and transformation. Journal of Psychology, 112, 1929.

placed before the eye. The results of these experiments are formulated as follows. The epicotister tends to have a similar impact as Flor paper in which, with a particular thickness of paper, a contrast is clearly shown. This relates to the experiments on the circumstances that the color contrast is more distinct with the use of the epicotister than without. The distinctiveness of the phenomenon with reduction of size of opening, first increases and then again decreases with the use of the epicotister, and this is analogous to the use of Flor paper. Here the contrast with the epicotister provides a very justified parallel with Flor contrast. The observations by Haack with the epicotister confirm and support our understanding that in the case of Flor contrast there is a clear impression derived of a quite different illumination that serves to increase the extent of contrast. A further support for our explanation of Flor contrast is provided by the experiments of Fuchs.[35] Fuchs makes use of an experimental design in which a projector apparatus projects a red rectangle onto a screen and upon which, in turn, a white light is directed via a mirror upon the red square. The white light appears generally as a veil. The experiments thereby provide the following observations. If one has before us the white veil without interruption, and in addition to that, the rose colored image, then this relates to the duality of colors that are observed in a common sphere even though in fact there is no duality. It is clearly within a common field that appears a white veil through which one simultaneously sees an object that has taken on a color of the veil. In the obverse, the veil shows at this point the color of the object that is permitted to penetrate. Conversely, the veil at this point shows the colors as they came through. The veil includes the colors that have seeped through the object. These colors glitter through the veil. This appears to be a

[35] Fuchs, W., Experimental investigations of simultaneous overlapping images from the same angle of light, Journal of Psychology, 91, p. 218, (1923).

rather noteworthy boundary condition between seeing two colors as against seeing only one. Accordingly, an observation appears where there is also a boundary condition and the Flor effect between seeing a single color or the colors of a multicolored paper disk. The same illumination also provides the appearance of two comparisons. On one hand, through the Flor effect involving colorful and varied paper disks the color is brighter or more saturated while on the other hand, the Flor paper acts as a Flor veil as under white special illumination in the same way as white special illumination provides the impact of a colored disk. The Flor contrast thus occupies a phenomenological viewpoint as a kind of intermediate position between pigment contrast and contrast resulting from illumination.

Summary

We compared the contrast of an external field to that of an internal field under the influence of a colorless or a colored external field in daylight. The identical internal field under the influence of the conditions of retinal excitement was contrasted with an external field illuminated by identical, white, colorless, or colored light. The first we describe as pigment, and the second as a lighting constellation. We found that the contrast in both constellations was the same in a number of cases. That happened when the experimental conditions were met in such a way that pigment and lighting constellations made the same phenomenal impression; i.e., when the appearance of the external fields in both cases, including the lighting, was identical in the field of vision.

When the experimental conditions were changed so that the field of vision showed clearly separate spaces with special lighting, then the contrast appeared different, although both constellations had the same retinal lighting conditions. This fact shows that the strength and type of retinal excitement is not the only factor in determining contrast.

Our experiments showed that when differences in contrast existed in both constellations being compared, then this difference did not originate from the so-called "transformation." That is, it did not come from being able to see in the light constellation the so-called "inherent color" of the internal field. The changing contrast in the light constellation came with the perception of the room and field illumination as such. The internal field changed strongly in the direction of the opposing color of the visible room illumination than the retinal identical pigment constellation changed in the direction of the opposing color of the pigmented external field.

We gained new views for the explanation of colored shadows from our results. These shadows undoubtedly bring about lively and impressive contrast phenomena because they basically come about through assistance of the colored lighting and the background of the shadow. We also were able to work out new ideas in regards to the explanations of the florescence contrast to which we attributed a connection between pigment and lighting contrast.

Résumé

I was born on August 15, 1905 as the daughter of Manufacturer Rudolf Posner in Pforzheim (Baden). Because of my marriage to Psychiatrist Dr. med. Friedrich Perls in Berlin, I am a Prussian National.

I attended Reuchlin-Gymnasium in Pforzheim and graduated in March of 1923.

From winter semester 1923-1924, until the summer semester of 1925, I studied at the University of Frankfurt/Main. In the winter semester of 1925-1926, I studied Law at the Friedrich-Wilhelm University in Berlin. Since the summer semester of 1926, I studied Psychology and Philosophy at the University of Frankfurt/Main.

I want to acknowledge and thank Prof. Gelb for his guidance in regards to my work.

Lore Perls, nee Posner