

## Phenomenology, experiments and the autonomy of Psychology: The earlier work of Johannes Linschoten

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### INFORMACIÓN ART.

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### ABSTRACT

Johannes Linschoten was a member of the phenomenologically oriented so-called Utrecht School. He published his Ph.D. Thesis in 1956. In this voluminous work, published in German, he discussed the (then) current theories of binocular spatial perception, reported 130 experiments on the subject, and argued for his own dynamic theory. I discuss some important aspects of this earlier work, the development of his view on the role of phenomenology and experiments in psychology, and the way he used his earlier studies to argue for psychology's autonomy.

### Fenomenología, experimentos y la autonomía de la Psicología: la obra temprana de Johannes Linschoten

### RESUMEN

Johannes Linschoten fue miembro de la así llamada y fenomenológicamente orientada Escuela de Utrecht. Publicó su tesis doctoral en 1956. En esta voluminosa obra, escrita en alemán, comentó las (entonces) teorías contemporáneas de la percepción espacial binocular, informó de 130 experimentos sobre el tema y propuso su propia teoría dinámica. Se comentan algunos aspectos importantes de su obra temprana, el desarrollo de su visión sobre el papel de la fenomenología y de la experimentación en Psicología y el modo en que usó sus estudios tempranos para argumentar la autonomía de la Psicología.

### Introduction

In his underestimated book *The neglect of experiment*, Franklin (1986, p. 165) observes that in science “very little attention has been paid to the question how we come to believe rationally in an experimental result.” An intriguing question, indeed, but in some

fields of psychology another intriguing question is the opposite one: how we come (or came) to—rationally—not believe in an experimental result. Or—depending on one's viewpoint—maybe: how we came to irrationally not believe in an experimental result? In this article I discuss the earlier work of Johannes Linschoten. Although he pleaded for a phenomenological basis for psychology—if not a

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phenomenological psychology—he took experimenting very seriously and conducted more than 130 in his earlier career. Using experiments he argued that psychology was an autonomous science and that phenomenology had to be at the root of all sciences. How could he? For many phenomenologists are critical about using experiments in psychology and related sciences, if not reject them explicitly and strongly. But why?

In 1874 Brentano was one of the first to introduce an approach later called phenomenology. He was critical of an experimental psychology (or “genetic psychology”, as he called it) if it claimed knowledge that went beyond psychophysical and physiological questions (Brentano, 1874/1973; Feest, 2012). A student of Brentano, Edmund Husserl, later became a famous philosopher who developed these ideas further. I quote Ash:

In his 1911 essay, ‘Philosophy as Rigorous Science,’ for example, Edmund Husserl accused the ‘experimental fanatics,’ as he called them, of confusing their ‘cult of the facts’ with a genuine analysis of consciousness. The essential qualities of consciousness, for example its intentional or directed character, are ‘in principle *different from the realities of nature.*’ They must therefore be studied with methods and described in terms different from those of the natural sciences.

According to Husserl, asking how often subjects make, for example, a judgment that object *a* is the same as object *b* under given conditions cannot tell us anything about the act of judgment as such. Only an ‘essential’ or phenomenological analysis of consciousness can make philosophical sense of ‘the gigantic experimental work of our times, the plenitude of empirical facts and in some cases very interesting laws that have been gathered. . . . Then we will again be able to admit that psychology stands in close, even the closest relation to philosophy - which we can in no way admit with regard to present-day psychology.’ (Ash, 1995, p. 44, my italics, rvh )

And in 1913 Edmund Husserl was one of the six “leading philosophers in Germany” to circulate “a petition against appointing any more experimentalists to philosophy chairs” (Ash, 1995, p. 47) as an answer to the controversial appointment of Erich Rudolf Jaensch at Marburg University, an experimentalist that had been advised by the natural scientists to take the chair. Later Husserl often criticized experimental psychology (e.g. Husserl, 1936), or at least some forms of it (see Feest, 2012 for many related examples and references to Husserl).

Another more recent rejection of experiments as a source of knowledge are Kenneth Gergen’s social constructionist views. Although he is not a “true phenomenologist”, his views are related to phenomenology. Gergen rejects experiments and mental tests as “foundations of objective knowledge” or as “standards of correspondence, predictive utility, empirical fact, resistance to falsification [of theories] or other such criteria” (Gergen, 1988, p. 3). Like Brentano and Husserl he argues against the use of experiments in departments of psychology other than psychophysics. But whereas Brentano and Husserl still had a very individualistic and personalized view of knowledge and of the way to gain it, Gergen realized through his implicit phenomenological approach to psychology’s ways of

working, that it was a social enterprise. His social constructionism has emphasized the *socially* constructed nature of psychological theory. Experiments isolate participants in order to get at what the experimenters call the functions individuals use. But how can the resulting isolated situation of the experiment help us understand the way human beings, as social beings, operate in their natural, that is social, environments?

Some years earlier Amedeo Giorgi critically analyzed why, how and when experiments are used, or—perhaps better—should not be used in a human science. He, for instance, suggested psychology is still in its pre-paradigmatic stage: there is no united theory, there is no relevance for everyday practice, and psychologists conform slavishly to methods that were successful in the natural sciences, especially experimental methods (e.g. Giorgi, 1965, 1966, 1970, 1975). I will only concentrate on the phenomenological criticism of the use of experiments in psychology. I refer especially to Giorgi because already in the nineteen fifties he was an observer of the Utrecht School, particularly of Johannes Linschoten. He visited Linschoten in Utrecht twice (Giorgi, 1999; Köster, 1999) and translated Linschoten’s book on William James (Linschoten, 1968).

### Experiments and psychology

Many psychologists today consider experimenting the hallmark of physics, of the real science. Even the psychologists that are critical of experimental approaches think so. Concerning the alleged privilege of using experiments in physics it is important to point out a few things. One is that although in physics many solutions to theoretical problems can be found by successfully using experimental methods, this doesn’t imply that in other sciences experiments are off limits or not useful. It is strange that if a method is used in one science, i.e. physics, this would imply that it won’t be useful or successful in another discipline because experiments are used in physics. This simply doesn’t follow. Moreover, if experiments were introduced in psychology—or the intellectual activities having to do with explaining and predicting human conduct and human experiences in supposedly controlled situations—they were not, repeat *not*—borrowed from physics, but from physiology and medicine, and co-developed with experimental approaches in biology and agriculture, and even from quality control in production of consumer goods (Danziger, 1985, 1990; G. Gigerenzer, 1987a). The idea of control groups, for instance, will hardly be found in physics. It is typical, however, in agricultural research, and was introduced to psychology by R.A. Fisher in the context of analysis of variance as a sophistication of experimenting (G. Gigerenzer, 1987b; Swijtink, 1987). I hasten to deny that experimenting cannot be improved or that conducted experiments cannot be criticized. Nor do I imply that there is only one ideal type of experiment. To reject experiments without indicating what type of experiments are to be rejected, makes the discussion even more diffusing.

#### Roles of experiments and roles in experiments

There are many ways to deal with experiments, as has been suggested by—among others—Allan Franklin (1986), Ian Hacking (Hacking, 1983, 1984, 1991), Kurt Danziger (Danziger, 1985, 1990) and

more recently Steinle (Steinle, 2002). While Franklin, Hacking and Steinle focus mainly on the epistemological role of experiments using examples from physical science, Danziger concentrated on their role *and the roles participants and experimenters played* in psychological experiments. Especially the analyses of Hacking and Franklin are valuable for psychology as well.

Danziger observed that experiments have gone through a number of significant changes in recent centuries, depending also on their origin in medicine (Charcot, Binet: the Paris model), physiology (Wundt, Helmholtz: the Leipzig model), and anthropometrics (Galton) (Danziger, 1979, 1985, 1990). His analysis focuses on the differing customs and actual practices as well as the diverging knowledge interests in these models. In essence, psychological experiments were more than tests of theories or sources of epistemological supplies for decisions between hypotheses.

Although Danziger's analysis did not *start* from phenomenology as a basic position, his interpretation of the role of experimenting in psychology, nevertheless, implicitly uses a phenomenological procedure to conclude that experiments in psychology are social situations, that they became institutionalized and ritualized as shortcuts to the alleged truth, and even then have to be differentiated according to their function for and relation to theories. In Wundt's so-called "Leipzig model" of experiments the experimental subject was the scientific explorer himself, analyzing results of his own basic mental processes with the aid of the brass instruments and the experimental assistant. The experimental assistant gave the cues (stimuli) and read and recorded meter results. The aim of the experiment was to *explore*, rather than test a theory, and the experimental subject was the experimenter. In the Leipzig community all participants implicitly accepted these roles, as well as the interpretations within Wundt's system. The experiments were also confined to the basic processes of the mind. They did not include, for instance, introspection of thinking, of social relations and interactions, of culture or of emotional experiences (Danziger, 1990). And, as Ernst von Aster (1908) suggested, the experiment was only to accept the description of what the subject introspected ("Beschreibung") as a report. In Wundt's experiments all disclosures ("Kundgaben") of the experimental subject, were utterances that Von Aster suggested were *expressions* of thought, rather than reports of what the experimental subject observed of his own mental processes. Wundt wanted representations of what the experimental subject observed of his (basic) mental processes. In the words of Feest he wanted the report of a front-loaded experience (Feest, 2019, in press).

The *Würzburger Schule* changed the Leipzig style of experimenting using introspection in a frontloading type of phenomenology to a style where the researcher obtained results from a subject that reported their experienced thoughts, appeals, emotions etc. (Bühler, 1907). Gradually the roles of experimental subject and experimenter shifted. The experimental subject became the person questioned by the experimenter. The latter offered a—sometimes complicated—cue phrase (a stimulus, we would now say). The subject had to think out loud, thereby mixing expression and representation of thoughts. But an important difference with the Leipzig model was that the experiment sometimes forced a choice between two options that were to be the outcome of the thought process.

Whereas in Wundt's system the roles could change per session, in the Würzburg system this was less obvious. As Danziger observes (1990, p. 52) the Leipzig community of Wundt's laboratory was its own data source. There was a very good theoretical reason for this. Wundt confined the experimental research to *exploration* of the elementary mental processes. These were processes that in his system were presupposed to be the same for every human being. So it didn't matter who was the experimental subject: the elementary exploratory processes of Wundt were as good as those of his students, and vice versa. The Würzburg model mixed exploration and the test of hypotheses. The Würzburgers, for instance, refuted the theory that thought processes were based on association.

The *experimental* approach of the Leipzig group clearly referred to its physiological origin. Even the epistemological background correlated with the physiological one in that the investigation was about an alleged universally present feature of basic human processes. The one difference that Wundt introduced was that he presupposed universality of the elementary *mental* processes as well. This novelty had a philosophical background. Descartes and Kant only accepted the accessibility of "higher" thought for thought itself (not for experimenting), nor did they suppose that "elementary" mental processes were experimentally accessible. But while Descartes and Kant opened up *philosophical* investigation of the mind, the Würzburgers opened up higher thought processes for *experimentation*.

Experiments from the fifties and sixties until the replication crisis

So according to Danziger the theoretical focus on mental processes came from the philosophical tradition, the methodological setup had a physiological background, the numerical interpretation of data had its origin in Galton's statistical analyses of members of a "population" in the nineteenth century, the division of roles had a medical, especially psychopathological source in its demonstration type features, and the idea of introducing control subjects can be found in agriculture and perhaps also in the Paris model.

In the twentieth century the idea of what was a true experiment slowly became more precise and restricted. Behaviorism played a role in this, surely, but the introduction of computers and, consequently, of more sophisticated ways of statistical analysis played an important role as well. This has led to the evaporation of some varieties of the function of experiments. For instance the function of experiments for *demonstrating* certain phenomena is no longer taken seriously, or at least is no longer considered to be a "true experiment". *Testing* for mental faculties or personality traits to find out statistical distribution in the population is perhaps considered a serious scientific enterprise but not an experiment. And *introspection* (as representation of self-observed mental processes, or even as expression of thoughts using thinking aloud), no longer is used much in psychology. So what remained is a style of experimenting that suggests a hypothesis is tested, using almost no theoretical background information, focusing extremely on the statistical aspects, and avoiding any clues that some exploration of epistemological or experimental options were tried before conducting the experiment that is reported.

And then came the replication crisis (Borsboom, Mellenbergh, & Van Heerden, 2003; Danziger & Shermer, 1994; Feest, 2016; Flis, 2018; Gigerenzer, 2018; G. Gigerenzer & Marewski, 2014). To me,

part of the explanation of the replication *crisis* seems to be the fact that the extreme focus on experiments as tests of hypothesis without considering that the exploratory function of experiments is useful, and that many experiments *reported* as tests of a hypothesis in order to be published, actually *served nothing but* an exploratory function. That could be one reason to look again at what phenomenology recommended according to Linschoten.

### Phenomenology

Phenomenology clearly was inspired by Descartes and Kant, and to cut the path short via Hegel, Brentano and Husserl introduced it in the human sciences. But whereas Kant made the difference between the phenomenal and noumenal world, the former being the only aspect of reality accessible for science, phenomenology happened to not only ignore, but even negate or deny noumenal worlds, and to restrict the phenomenal world to the aspects of which only individuals as such, that is as persons, had conscious access. “We are the true positivists”, Husserl once famously claimed (Husserl, 1950). This resulted in a style of contributing to science in which the personal experience of the reader, and the persuasion skills of the phenomenologist, if not their authority, are the most important sources of argument. Yes, the personal experience of the recipient of phenomenological analyses as well, because essentially the supposed receptivity of the recipient (the reader) of a phenomenological analysis plays an important role in the construction of the evidence. The idiomatic and rhetoric qualities of the phenomenologist as well as the capacity for observance of the phenomenologist, are crucial. Park, Pomata, and Daston introduced an interesting taxonomy of the ways to consider observation and their interpretations (Michael D. Gordin, in Daston & Lunbeck, 2011, chapter 5). They distinguish three tactics: observation as persuasion, observation as generalization, and observational authority. By the first they mean to observe the item of reference, and then convince people of your observations by indicating the steps and methods involved in the observation process, as far as the same kind of items are concerned. The second is the kind of observations that in their interpretations imply that one can generalize the observations of one kind of items to other kinds of items; for instance from rats to humans as far as the same natural processes are involved. The third tactic refers to an “authority” derived from observing, for instance, insects, thereby building credit for controversial observations of, e.g. human features and properties.

This taxonomy could be used for the observation tactics phenomenologists use. All three apply: phenomenologists claim—partly based of their authority as a true phenomenologist and the persuasive capacity of their language—to be able to transcend from individual observations to general conclusions.

### Interpretation styles of experimenters

In the eighties and nineties of the twentieth century Hacking (1983, especially chapters 9 and 16) has revived much of the interest in experimenting. He focused mainly on examples taken from the

history of physics to demonstrate that experimentation has a life of its own. Experimenting interacts with speculation, calculation, model building, invention and technology, and requires a realist interpretation style of—at least—the theoretical entities they *use*, although they do not have to be realists about the entities they *investigate*. Nor do they have to be realists about *theories*, according to Hacking. It all depends on realism as the belief that science aims at true theories, that is, true ideas about how the world is and how its entities interact. (I will call this “theory-realism”.) Entity-realism is about the entities experimenters use to intervene in the world. They are “tools”, ... “ways of creating phenomena in some other domain of nature” (Hacking, 1983, p. 263). The phenomena created with these “tools” need not necessarily be believed to be real, they are at best hypothetical entities. However, often, after a while, “they no longer are ways of organizing our thoughts or saving the phenomena that have been observed” (Hacking, 1983, p. 263). They become real although not always visible to the naked eye. It all has to do with the time scale of the experimenter. Theory-realism involves the believe that science aims at the truth of theories sometime in the future. Entity-realism has to do with the design and conduct of the experiment next week.

### Linschoten's development: from phenomenological psychology to empirical-analytical psychology

I will now discuss how Johannes Linschoten developed his views from phenomenology as the *ultimate basis* of psychology to phenomenology as *only a starting point* for psychological experimental research. I took the example of Linschoten for several reasons. First reason is that he started his career as a phenomenologist. Only later he seemed to have changed his view and wrote a book arguing for an empirical-analytical approach (Linschoten, 1964). I will discuss this later. Secondly, the latter book caused some uproar among his colleagues (Duijker, 1964; Langeveld, 1964; Piron, 1965; Vlek & De Klerk, 1965-1966) and had the effect that many younger Dutch psychologists considered it later as the most important Dutch book in the psychology literature of the Netherlands (De Ridder, 1992; Derksen, 1999; Köster, Broerse, & Zwaan, 1964; Spinhoven, 1999). Third, already in his phenomenological work he advocated the use of experiments, which illustrates that experiments need not necessarily be seen as the prerogative of the natural sciences, but that it is a natural way of critically developing knowledge in any science, as far as situations and theories suggest. And although many believe he was the only phenomenologist to conduct experiments, this simply is not true. In his early career Buytendijk experimented, although later he let his assistants, Benjamin Kouwer and Linschoten, do the experimental work. Buytendijk also was appointed as a professor and director of the psychological laboratory of Utrecht University where many experiments were conducted. As we will see he even stimulated Linschoten to do experimental work for his thesis. Also, the lab produced some Ph.D. Theses that clearly reflect the experimental *and* phenomenological approach (e.g. Meer, 1958)

Who was Johannes Linschoten?

Johannes (Hans) Linschoten was born in the Netherlands in 1925.



Figure 1: Johannes Linschoten

From his fifth year Hans Linschoten (1925-1964) grew up in the Dutch Indies, in what now is called Indonesia. During the Second World War he was interned in a Japanese prisoner's camp. After his repatriation to the Netherlands with his mother and brothers (his father had died in a camp) he started his studies of psychology at Utrecht University, from 1947 till 1950. It took him little time to finish his studies; nevertheless he obtained a cum laude. In his doctoral thesis (master thesis) he defended the assertion that experiencing movements is not only a matter of

sensation. Experiencing movements is the "phenomenal revelation of movements in a situation" (Linschoten, 1949). Seeing movements not only is a matter of physiological events, but depends for the greater part on a psychological activity.

Already during his studies he had become an assistant to Frederik Buytendijk. After graduating he started his career as an academic in 1950, now as an assistant professor to Buytendijk, the leader of the Utrecht School. The Utrecht School consisted of a group of academics defending a phenomenological psychology (Van Hezewijk, Stam, & Panhuysen, 2001, 2002).

#### Linschoten's master thesis on induced movement

The first indication of the development of his ideas is Linschoten's recovered theoretical master thesis (Linschoten, 1949) and the empirical master thesis which was his report of experiments on space and (induced) movement perception (Linschoten, 1950, 1952). Already in 1950 Linschoten published the article that one could read as critical of "logical analysis" and as a plea for a purely phenomenological psychology. It is a thorough phenomenological analysis of movement phenomena, complete with a phenomenological vocabulary although it is unclear if it argues for a "phenomenological psychology". These earliest works already demonstrated his intelligence and capacity for analysis of theories, as well his skill of experimentation. In 1952 Johannes Linschoten published a paper in German called *Experimentelle Untersuchung der sogenannten induzierten Bewegung* (Linschoten, 1952). Actually, this was his doctoral thesis (not the Ph.D. Thesis) which reported of experiments conducted from December 1948 till February 1949. He investigated the so-called "Dunckersche Bewegung", the induced movements first fully investigated (not discovered) by Duncker (1929). Briefly, the participant is in a totally dark room, looking at two lighted figures, one of which objectively is steady, the other is moving. The effect is that the steady figure appears to move as well, or even that the objectively moving figure appears to be steady while the steady object moves. In what are more or less

Duncker's words, in the dark room the objects relate themselves not one-sided, but in relation to one another.

Later investigations by Erika Oppenheimer (1935), and Wilhelm Krolik (1935) resulted in a list of factors influencing the kind of induced movements, which they called "laws". Increasingly influential were the factor of fixation (to one of the objects), the factor of size difference of the objects (if the smaller object is steady and the larger one is moved, the smaller appears to move), the factors of intensity and experience (if an object is expected to move it will appear to move in relation to an object expected to be steady; e.g. a car versus a filling station, even when a picture of the latter objectively is being moved), and the factor of enclosure (if the smaller object is enclosed by the larger one, the smaller appears to move).

Linschoten objected to their list. He criticized the use of "lawlike" and "law". When the phenomena are reported by a majority of participants of the experiments, this implies that a minority did *not* act or perceive according to the alleged law. Therefore, it cannot be called a law. Moreover he criticizes the authors for experimenting without first thoroughly, that is, phenomenologically analyzing the induced perceptions. He observes that when there are some small changes in the nature of the movements, or in the interpretation given to the participants in advance of the perception to be induced, there are less participants experiencing the movement. Therefore the "law" is not a law, it is only a statistical regularity, a rule. Consequently, Linschoten observes, the factors from the list are interpreted as substituting a "play of factors" within each participant, whereby every participant may have played a different game and therefore a different outcome.

This resulted in three problems Linschoten aimed to confront in his doctorate thesis:

1. Are the factors that Duncker, Oppenheimer and Krolik found, the really deciding factors for the distribution of induced movements?
2. Is the use of statistical processing of the experimental results the only legitimate method to obtain explaining hypotheses?
3. To what extent is the phenomenological analysis important or necessary for the explanation of the phenomena? (Linschoten, 1952, p. 42).

Linschoten used a dark room to present the participants with a number of figures that were the only ones visible. He asked them to observe figures like Figure 2 (in which the arrow indicates the direction of the objectively moved figure). The dimensions of the figures used were given in a separate list (p. 43). In phase 1, Series I all moving figures moved to the right (Figure 2); in Series II to the left (Figure 3)

In the second phase Linschoten changed the set-up from a black screen to a white screen, using the same figures, and asked them to fixate one of the figures. He additionally used the figures of Figure 4 in which he asked his participants to not fixate one of the figures.

In the published paper Linschoten exactly described his procedure, the participants, the order of the presentation of figures, the dimensions of the projected figures, the velocity of movement of the projected figure, etc. For those who appreciate the usefulness of replication, all information is present. He obtained 400 answers from 50 participants of the experiments. 32,25 % of the answers indicated that the "objective" movement was the same as the "phenomenal"

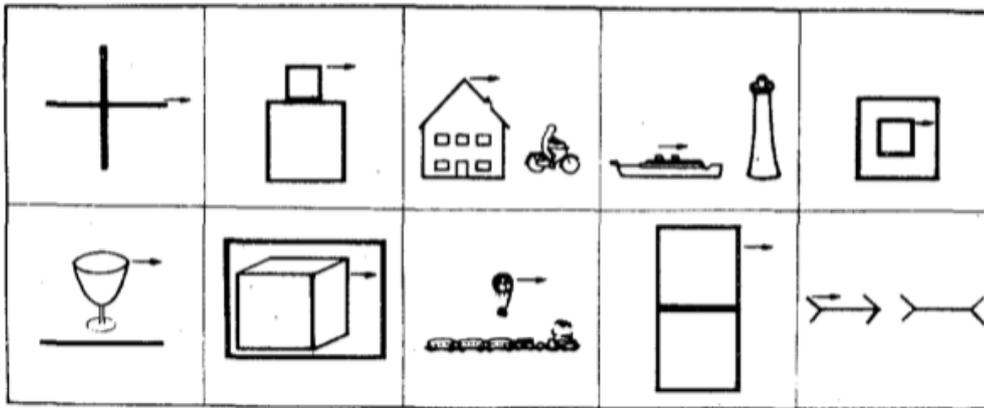


Abb. 3. Versuchsserie I. Der Pfeil zeigt die objektive Bewegungsrichtung der in Serie objektiv bewegten Figur an. In Serie B wurde die andere Figur bewegt, in entgegengesetzter Richtung. Die Figuren der Konfigurationen 4 und 8 sind der KROLIKSCHE Arbeit entnommen.

Figure 2: Series 1 of Linschoten's master thesis on "induced movement".

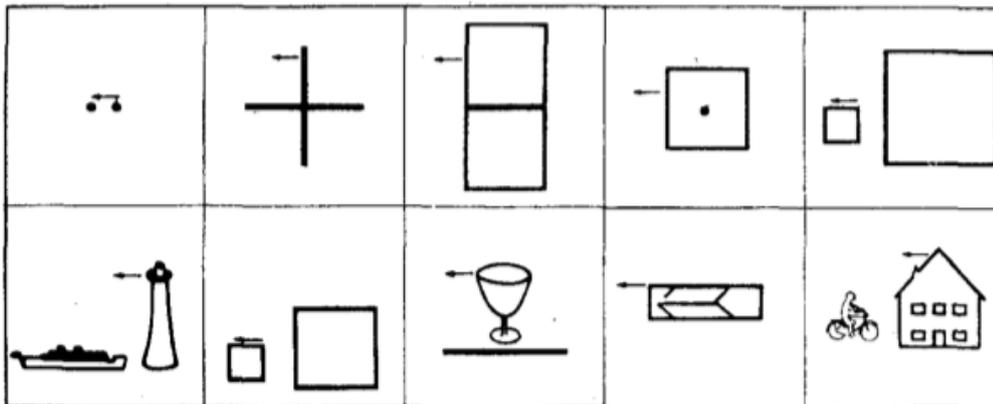


Abb. 4. Versuchsserie II. Der Pfeil zeigt die objektive Bewegungsrichtung der objektiv bewegten Figur an.

Figure 3: Series 2 of Linschoten's master thesis on "induced movement".

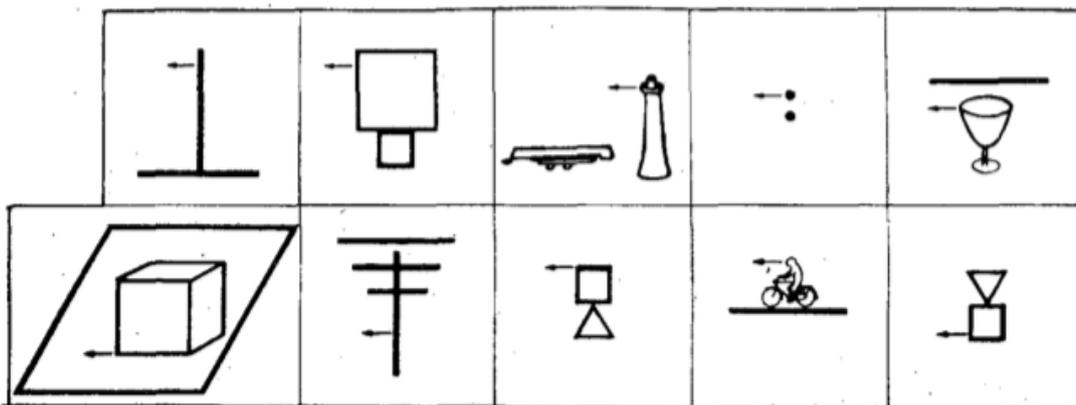


Abb. 5. Versuchsserie III. Der Pfeil zeigt die objektive Bewegungsrichtung der objektiv bewegten Figur an.

Figure 4: Series 3 of Linschoten's master thesis on "induced movement".

movement; 44,75% reported a “phenomenal” movement that did not correspond with the “objectively” moved figure, and 21,50% reported that both objects moved phenomenally. 1,50% reported no movement at all. According to Linschoten this made it clear it is impossible that the objective movement *determines* the phenomenal movement. He implies that it makes no sense to look for the cause of induced movements in the objective movements of figures. In other words, it is a psychological effect, not an optical or physiological effect. This implies that first and foremost a *phenomenological analysis* must establish what phenomenon is to be explained. In other words, Duncker, Oppenheim and Krolik only explained why 32 % of the participants saw the movement, and ignored—as it were—the remainder 65%.

Linschoten's Ph.D. Thesis on binocular depth perception

Already in 1951, very soon after the start of his academic career, Linschoten presented Buytendijk with his plans for a Ph.D. thesis (dissertation). He first suggested a phenomenological analysis, allegedly of “the street”, but Buytendijk urged him to write a thesis in experimental psychology. Linschoten accepted, but told his wife: “If he wants an experimental thesis he will get one that will keep him busy”. So at the 25<sup>th</sup> of May, 1956 Hans defended his dissertation on binocular space perception. He surprised Buytendijk and many a colleague with a work of 573 pages plus a booklet of 226 figures, written in German, containing an introductory part, an experimental part and a theoretical part (Linschoten, 1956). The introduction summarized the dominant theory on binocular space perception: Hering's Theory of Identity, and its follow-up versions. The experimental part of his thesis reported of 130 experiments. In the experimental part Linschoten not only criticized Hering's theory but also candidates from other, more general theories of perception (Wheatstone, Helmholtz, Gestalt theory). He discussed the Panum-effect in his analysis and experiments, described the experience of attraction of image points of the two retinal images as an important part of a phenomenological analysis of the structure of depth perception, and criticized the role of eye movements in depth perception as had been suggested earlier. In the theoretical part he analyzed the role binocular depth perception plays for the organism in the localization of significant objects situated in the depth of their “structured spatiality” (*Räumlichkeit*). He presented his “dynamic theory of binocular depth perception”. In the conclusion he gave his answer to the question why human beings have two eyes.

Although at first hand one can view his dissertation as an excellent combination of an experimental *and* theoretical approach to solve a complex problem, and as the opposite of the phenomenological approach one would expect from this author, I argue for a different point of view. Contrary to what most phenomenologist or anti-phenomenologists or non-phenomenologists or agnostics suggest (Dehue, 1995; Giorgi, 1965, 1966)<sup>1</sup>, the phenomenological approach

of the so-called Utrecht School of the fifties in the Netherlands did not exclude experimental work at all. Linschoten's aim with the experiments in his dissertation was to argue that in any account of binocular depth perception a *psychological* explanation is inevitable: psychology is an autonomous discipline. It demonstrably involves explanatory problems that can only be solved by presupposing a *psychologically* active organism. His experiments help him to demonstrate this assertion, because any reader of his thesis can use the accompanying booklet with the figures he used to check what one experiences phenomenologically—or phenomenally. Epistemologically speaking, however, his experiments can best be seen as exploratory experiments (Steinle, 2002, see below for more details),

Background

Close reading of his—by present-day standards—voluminous Ph.D. thesis makes it clear how already in the nineteen fifties of the twentieth century, Linschoten had a clear view on the relation of phenomenology to psychology, and to other sciences. Although you rarely see this in phenomenologists, his thesis shows Linschoten also as a sophisticated experimentalist. Perhaps because of the volume of his thesis, and because of the language in which it was published—German—in virtually all discussions of Linschoten and his impact on psychology, the dissertation was almost entirely ignored<sup>2</sup>—and still is—but in studying the development of his thought his thesis (1956) is crucial. Although he was only in his mid-twenties, he has a mature view of psychology and demonstrates his knowledge of the psychological literature. It also demonstrates his involvement with the relationship between phenomenology and psychology. It is a repeated theme in his writing.

In his Ph.D. thesis he argued that phenomenology both shows that our experiences cannot be reduced to physiological properties or physical (i.e. optical) laws, and that it is necessary to find psychological explanations of our experience of things in depth, 3D. Moreover, he shows that the experimental method complements phenomenology. He even uses experiments *as a phenomenological tool* to investigate what psychology must investigate and explain, by choosing as the subject of his thesis the function of an organ that is often thought to be at odds with the phenomenological approach. Or, that even if it is whole *persons*—and not *mental functions*—what psychology is to be concerned with, there are things persons have in common that can be seen as their “mental standard equipment”, and that can be investigated as such. I will come back to this a little later. The interesting thing about Linschoten's focus on depth perception is that the subject seems to belong to psychophysics or to what often is called experimental psychology—what in The Netherlands is called *functieleer* after what Carl Stumpf called *Funktionslehre*. Depth perception was already studied in the nineteenth century by scientists with roots in physiology (Wilhelm Wundt, Hermann von Helmholtz), medicine (Ewald Hering), or physics (Helmholtz, Gustav Fechner, Ernst Mach), focusing on questions raised in philosophy and epistemology by philosophers like René Descartes, John Locke, David Hume, George Berkeley, and Immanuel Kant. Later, for example the

1 Of course many authors discussing experiments and phenomenology pay lip-service to the idea that experiments can be useful in certain situations. However, these confessions seldom take more space than two sentences in their analyses. Only Herbert Spielberg (1965) discusses the relation of phenomenology and phenomenologists to experiments, and points out how important Carl Stumpf's “experimental phenomenology” was for the phenomenological movement.

2 I found only one review (Heinemann, 1957)

Gestalt psychologists continued the study of depth perception (Koffka, 1930; Köhler, 1933; Köhler & Emery, 1947). Linschoten considered himself a discussion partner of these giants. In a self-confident way, using sophisticated experiments he refuted their ideas, or modified and improved them.

### The thesis

In the 36 page introduction to his thesis, Linschoten formulates the general problem of depth perception. Traditionally, most authors attacked the problem with either an optical or a physiological approach. In his preliminary remarks he points out how he will approach the problem. He also emphasizes the importance of perceiving depth for its role in dealing with the environment of persons. Especially handling things at arm's length presupposes a very keen sight of things in depth. There is no way to thread a needle, to pour a drink, or to hit a ball when your depth perception fails.

He next introduces his main antagonists. The most prominent ones are Johannes Müller (1801-1858) and Ewald Hering, (1834-1918) who had suggested a nativist, and sensory physiological theory of binocular depth perception. Next are the Gestalt psychologists and Helmholtz (1821-1894) who explicitly defended—what they considered being—a psychological theory of depth perception.

If Linschoten had known Lakatos (1970, 1971) he would have called their maneuvers “theoretically ad hoc”, solutions out of the order of the programmatic line of their theories. Linschoten, however, did not know Lakatos' work, and considered himself the protagonist of the autonomy of psychology (1956, p. 8) who unmasks the nativists/physiologists as covertly using psychological arguments to “cover up” for some unexplained results in their physiological theory.

Perceiving depth, according to Linschoten, is to *experience* depth. He meant this to be understood in a *psychological* sense. Should not “the retinal image, as a stage in the neurophysiological events, ... better remain unobserved”.<sup>3</sup> Although he acknowledges that optical and physiological properties play a role in the formation of the two retinal images, he observes there is only one image that we *experience*. Psychologically, we should study depth *vision* as the *perception* of depth with one eye that has two members (“zweigliedrig”).

And so he does in his experimental studies. Although the role of the brain in perception has still to be explored, neurological knowledge can never *replace* psychological knowledge, he argues. In any other approach to perceiving depth, one *presupposes psychology*. So, neither the optical theory that analyzes the properties of the stimulus or stimulus situation, nor the psychophysical theory that analyzes the proportions of physical to experienced properties, nor the structure of retinal relations (cf. Hering or Müller), will fully explain what happens when a human being perceives the pencil on their desk. Theories like these have their own value. But the most fundamental approach is the psychological/phenomenological one. It lies at the base of the questions about the physiological, optical or psychophysical mechanisms that may be suggested to explain binocular depth perception. Explaining

depth *perception* is, primarily, a matter of using a phenomenological method, which is the task of psychology, if not of epistemology. Before explaining depth perception one has to analyze the structure of depth, the structure of things seen in depth, the structure of what it is for an object to have an orientation and of what it is for moving things to have a direction, etc. One has to acknowledge that—according to Linschoten—height, depth and width are dimensions of a localization system that typically presupposes a perceiving subject—be it a human being or even a non-human organism. Moreover, the reason for being as good as we normally are in seeing depth, is to be able to handle things, which presupposes the intentionality of perception as Brentano already argued. Phenomenology acknowledges all that, and offers explanations, according to Linschoten.

His variety of psychological phenomenology resembles the Husserlian perspective—although most of Husserl's work was still unknown to Linschoten, if published at all at the time.<sup>4</sup> Husserl had argued that the laws of logic and mathematics are grounded in the experience of the essences of things and relations between things. Later Husserl claimed autonomy for logic and mathematics. Physics is about things *as experienced*, and mathematics about relations between things as experienced (Husserl, 1891, 1900-1901). Linschoten claims something similar: phenomenology is descriptive of experience even *before* science can start to find explanations of what is experienced. Because phenomenology provides in the precise description of what it is that we experience, and what will have to be explained. So he sometimes seems to give psychology the same role as phenomenology (or v.v.). It appears to be equally relevant for the experience of depth, as for the experience of the relations between human beings in personal encounters. The latter was the central claim of the alleged Utrecht School of which Linschoten was then supposedly a member (Langeveld, 1957; Van Hezewijk et al., 2002). To analyze the structure of social relations and the structure of the way a person encounters his or her “natural” world, are of equal importance. His aim was to analyze the structure of the experience of relations between “natural things” perceived, that is to analyze the “seen things” or—as he calls them in his thesis “*die Sehdinge*”—in the one and unified “seen field” or “*Sehfeld*”, which is the space as visually experienced. Although anatomically there are two retinal fields and consequently two optical fields that constitute it, depth and things seen in depth with an orientation, as well as things moving in a direction, are characteristic of the *Sehfeld*, the *one seen field*. The experienced space is relative to a point somewhere between the physical eyes of a person, an imaginary third eye that perceives in three dimensions whereas our real eyes are technically speaking only capable to register two dimensions. Yet experiencing the third dimension—depth—is not an unconscious inference a posteriori the stimulation of the two retinas, made possible thanks to the innate capacity to infer depth from the “two-eyedness”. Depth is not *sui generis*, it is *originally given and irreducible* (p. 30) and thus the working ground for phenomenology.

This is the general underlying approach that allowed Linschoten to write phenomenologically oriented papers while conducting research on fundamental perceptual experience. Actually, only on two or three

<sup>3</sup> “... ob nicht vielmehr auch das Netzhautbild, als Stadium im neurophysiologischen Geschehen, ausser betracht bleiben soll...” (1956, p. 8).

<sup>4</sup> There is only one reference in the thesis to Husserl's *Logische Untersuchungen*, on page 418, where in a footnote he claims that it is to be regretted that Gestalt theory has neglected Husserl's phenomenological investigations.

locations in the thesis he makes this explicit, such as on page 12-14, where he states that

The space-likeness (or spatiality) of things is a phenomenal quality that is established in an act of perception. That is why psychology has, among other ones, the task to investigate the immediately given spatiality. ... First through a preparatory descriptive analysis of the essential properties of spatiality as such. ... Second, it is psychology's task to analyze the continuity of spatial phenomena with certain act structures. ... Third, it has the task to give a structural analysis of the space phenomenon as an empirically encountered fact (my translation, Linschoten, 1956, pp. 12-14)<sup>5</sup>

The claim for an autonomous psychology is definitely anti-Cartesian, non-dualistic. Only *after* the experience of depth, the dualism of the explanation of seeing things *in* depth by referring to the internal and external worlds comes into existence. And it is only after this experience that the logic and physics, or the physiology and mathematics of spatial perception and spatial relations become possible.

So his claim is anti-reductionistic and anti-mechanistic, and seems to contrast with his later work, *Idols* (Linschoten, 1964). However, we think it is only a superficial reading of both his thesis on binocular depth perception and of *Idols* that would support this claim. The message from his thesis of 1956 is only a little different from that in 1964, while its means of investigation, experiments, are what he explicitly pleaded for only in 1964. What is different is the scope and domain of the subject, and the explicitness of the message. In the Ph.D. thesis of 1956 Linschoten explicitly has the intention to give a phenomenological analysis of the structure, that is the a priori's of perceiving depth, using theoretical and experimental analysis very much the way it is done in some domains of modern sciences, such as linguistics. He (still) thinks a phenomenological analysis provides in the foundation for subsequent analyses in as different domains as optical and physiological sciences. In his *Idols* the role of phenomenology has been reduced to providing in the *starting point, not the foundation* for experiment and reduction.<sup>6</sup> So, depth perception is essentially a phenomenological experience. It is not a physical (optical) event, nor is it the result of an "unconscious inference" (Helmholtz' *unbewußter\_Schlüsse*) about a retinal event, let alone a conscious construction. It is not even remotely possible to have a physical, psychophysical, optical or physiological explanation of perceiving depth, without first having had an in-depth analysis of what it is to perceive—that is to *experience*—depth. In fact, in section

6 of his introduction Linschoten his analysis of depth resembles what Husserl intended with the so-called eidetic reduction, and what Brentano implied when he pointed to the intentionality of conscious experience. "Spatiality" (*Räumlichkeit*) lies at the root of depth. For an observer, and for an observer only, space has a left-right dimension, a front-back dimension and an up-down dimension *in the experience of the observer*. Space has a field structure, it provides opportunities for objects to have an orientation *from the perspective of an observer*. It is an observable, but this implies observer.

Not all phenomena—all things as experienced—are "only" optical or physiological events. The "spatiality" and the "orientationality" of things cannot be reduced to optical or physiological properties. One sees *nothing*—no space—when there is not a *thing* to be seen; however, a "thinglike" or a "spacelike" thing will be seen *somewhere* in the optical field (Linschoten, 1956, p. 21), as a thing. So one should understand spatiality not so much as an observable but as a "phenomenal" ("*anschauliche*") quality. The structure and forms of this quality are analyzable in terms of the conditions of the optical field they depend on. One can predict when an empty space is perceived, for instance when the so-called 'glass experience'<sup>7</sup> will occur, or when one will experience fog, or when the volume of physical bodies will be perceived. All of these presuppose the irreducible "*Urphänomen*" of spatiality.

## The experiments

Although the phenomenological verbalizations he used were accepted, and even usual among the members of the Utrecht School, Linschoten was not satisfied with them. So after discussing the work on binocular depth perception by Johannes Müller, and the modifications by Ewald Hering, Wheatstone, Fechner, Panum, Volkmann, Helmholtz and others, Linschoten presents the design and results of his 130 experiments. In the *experimental* division there are five chapters discussing and reporting of experiments on

- on seeing double and the experience of corresponding retinal points
- the Panum effect<sup>8</sup>
- the limits of the attraction between an occupied and an unoccupied visual field
- the formative moment ("*Gestaltungsmoment*") in the spatial organization of the aggregated image ("*Sammelbild*")
- the relations between eye movements and depth perception.

5 "Die Räumlichkeit der Dinge ist eine phänomenale Qualität, die durch einen Wahrnehmungsakt erfasst wird. Daher hat die Psychologie unter anderem die Aufgabe, diese unmittelbar gegebene Räumlichkeit näher ou erforschen. (...) Erstens durch eine vorbereitende deskriptive Untersuchung nach den wesentlichen Merkmalen der erscheinenden Räumlichkeit als solcher. (...) Zweitens hat die Psychologie zur Aufgabe, den Zusammenhang von den räumlichen Erscheinungsformen mit bestimmten Aktstrukturen zu untersuchen. (...) Drittens hat [sie] die Aufgabe einer Strukturanalyse des Raumphänomens als eines empirisch vorgefundenen Tatbestandes" (Linschoten, 1956, p. 12-14).

6 Please note: "reduction", not reductionism. Linschoten explicitly rejected reductionism, but saw reduction as an important source of knowledge. "We learn much from reduction, we learn nothing from reductionism", he asserted in one of his lectures.

7 "Glass experience" refers to the phenomenon of being able to see through, but not walk through, glass. It is opposed to the phenomenon of, for example, walking through fog one cannot see through.

8 The Panum Effect is the effect when looking through a stereoscope (or focusing beyond the stereoscopic picture) at a single straight line a on the left and two straight lines b' and c' on the right, the experience will be of two lines a and c' (in which b' apparently has joined a).

### KAP. IV.

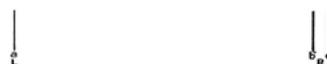


Abb. 54 PANUMSCHER Grundversuch.

Linschoten tests and retests all the elements of Hering's theory of depth perception and its modifications, using replications of Hering's experiments as well as new test figures of his own design. Interestingly, he explores binocular depth perception by systematically varying the parameters of the experiments, conducted earlier by Hering, Müller, Wheatstone, Von Helmholtz. The parameters they did not investigate were, according to Linschoten, probably ignored because of their disregard of the psychological aspect. That is also why he called his study a "*Strukturanalyse*", a structural analysis of depth perception. And why, in his experimental procedures, Linschoten used a basic rule still used in psychophysics and such disciplines as linguistics. Sometimes it is relatively unimportant to include more than a few participants in an experiment. The most important reason to invite a larger number of participants is to diminish the effect of individual differences and of individuals intentionally influencing results. This would explain why sometimes  $n = 1$  or  $n \leq 5$  is acceptable, although sometimes Linschoten used 40 participants as well. Power is not an issue when there are good arguments to believe personal idiosyncrasies cannot influence the outcome of the experiment so that the outcome will be the same with every normal subject. It also follows the tradition of early German experimentation in psychology (e.g., Wundt), in which an experiment was a demonstration of an effect, or which had an exploratory aim, instead of the test of a hypothesis. Of course it depends on theory to establish when humans will not differ in the way they react to sensory input, but the processing of visual stimuli in the periphery of the perceptual system meets these requirements. The reasonableness of this assumption, however, always depends on one's prior theory of the processes involved. That is what Linschoten considered the task of phenomenology as well: to clear the ground from idiosyncrasy, or—as the phenomenologists would have it—to transcend subjectivity. Linschoten does it by experimenting. So in his thesis Linschoten seldom mentions the number of persons that participated in his experiments.<sup>9</sup> However, the readers can check the results of the experimental setup themselves. In this sense, his experiments also function as demonstrations. The stimuli he used in his thesis are illustrated with hundreds of figures, printed in the accompanying 60 pages booklet. In his laboratory experiments he used the stereoscope, but even without a stereoscope it is often possible to verify the outcome with the naked eye. As in classical psychophysics, the experiments are continued until one achieves the desired effect and using the illustrations, any reader can verify the phenomenon for themselves. But it is important to realize what the phrase "until one reaches the desired effect" means. It is not to prove or disprove a theory, so much as it is to explore and analyze, qualitatively and experimentally, in depth, the fundamental ("structural") features of seeing things in space. The desired effect is "to secure a qualitative specificity (*Angemessenheit*) of descriptions and observations." (p. 35).

9 E.P. Köster, one of Linschoten's students and assistants in the early fifties, reports that he assisted Linschoten in experiments 121 and 122. In these experiments he worked with 10-12 participants—students that were not familiar with the theory at stake (E.P. Köster in an e-mail communication with me, dated 19-10-2006).

It is also important to remember here that Linschoten claimed an autonomous domain for psychology apart from the physiological and sociological domains, in an epistemological sense, not to be confused with university politics. Linschoten claimed phenomenological accessibility for the results of psychological research without any necessity to refer to theories of other domains, while theoretically, he had already demonstrated that Hering implicitly presupposes psychological (or at least subjective) concepts in his alleged physiological explanation of seeing in depth. For instance, Hering introduces "height" and "width", direction and orientation of seen things but they already presuppose e.g. a direction *for an observer*.

Already in the first experiment he discussed in his study, he shows that one must irreducibly involve psychological explanations in understanding phenomena as showed below.



Figure 5: From the addendum of Linschoten's Ph.D. thesis, p. 6

Looking at the picture in Figure 5 through a stereoscope, or focusing a point infinitely beyond the picture plane in the middle, one experiences the "floating" of both pictures together to one perceived in the middle, in depth: the resulting picture is no longer skewed. Also, the lines a-b and c-d have joined and resulted in a vertical line, tilted towards the observer.

The "corresponding points" here indicated as *h* and *i*, (meaning pointing at them, they are not meant to be real points) will unify into the resulting plane and will not be experienced as before or behind the plane. We hardly "see" them. *h* will be somewhere on the upper left quadrant of the resulting plane, *i* will be on the upper right part.

If we take the points as points as such (not as indications of something in or on the planes) the disparate points will not result in experiencing one point on the plane but as a point in the space before or after the plane, that is 'in depth'. Hering already criticized Helmholtz for not recognizing that. Hering was right, according to Linschoten. But he was wrong in *not* concluding that this implies a *psychological* explanation. It also shows that this is not a matter of *points*, whether corresponding or disparate or whatever, but of *planes* and *contours* of items that one can see them as 3D things in depth. That is why Linschoten, already after the third page of the experimental part of his thesis (Linschoten, 1956, p. 83), concludes that it is *functional* points and contours, versus *functionless* points in the figure, that are at stake in the explanation of seeing things in depth. And if they are functional, they must be functional *for* a subject (an organism, a person). This justifies his claim that the perception of depth is first and foremost a psychological phenomenon.



Figure 6: from the addendum of Linschoten's Ph.D. thesis, p. 7

Staring “through” a picture like Figure 6 (Abbildung 19 in his addendum) reveals, according to all participants of the experiment, that lines will unify in the following way: (a,a’), (b, d’), (d,b’) and (c,c’). The outside lines unify, the thick lines join, and the thin lines join. The thin lines skew such that the top of it (like d does in the left picture), tilts outside the image plane toward the observer, with the tip-over point exactly at the crossings of the line. (Exchanging locations of both semi pictures leads to a change: the line tilts “away” from the observer.) He used this picture to refute the hypothesis that seeing the lines in depth results from rolling our eyes in opposite directions as Enjalran (1917) claimed. The skew of the lines would need our eyes to roll not only in opposite directions in the upper half of the figure but also in ‘the opposite of the opposite direction’ at the lower half of the figure.

In other experiments he unambiguously demonstrates that Hering’s theory is incorrect. Linschoten used Figure 7 (Abb. 34) to show this. He designed the figure himself and considered this his basic figure, wondering why nobody had found it earlier (p. 98)<sup>10</sup>.



Figure 7: From the addendum of Linschoten's Ph.D. thesis, p. 10

The original theory by Johannes Müller suggested that there would be identical (or, within limits, disparate) corresponding retinal points of the left figure with retinal points of the right figure. Both monocular figures would lead to the perception of one binocular figure. The question is: what would be the resulting figure? Apart from other theoretical problems the original physiological (retinal) theory would predict the same figure as Hering’s last modification of the theory. The last modification of Hering suggested that the virtual lines from the virtual binocular (third) eye to the optical points in the figures would result in a compiled figure (*Sammelbild*) with one rectangle (resulting of i and i’) and three other lines: c, (a,a’) and b, as is shown in Figure 8.

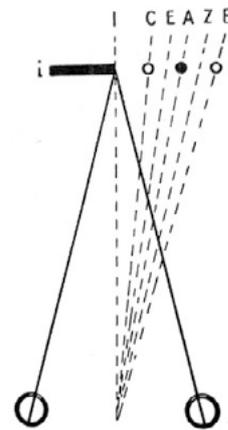


Figure 8: From the addendum of Linschoten's Ph.D. thesis, p. 10; compare Figure 7

b and c’ were supposed to result from a double Panum-effect. According to Hering’s theory it *would* have led to what is presented in Figure 9.

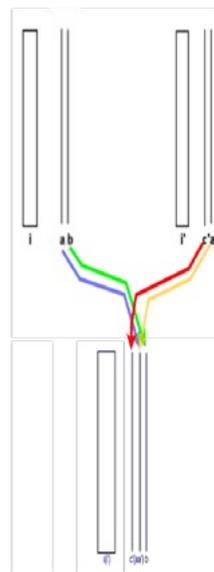


Figure 2ii: Hering's prediction

Figure 9: Hering’s predicted changes represented by the colored lines

That is, when using a stereoscope, or focusing on an imaginary point in space beyond the figure, the effect will be a virtual movement of the right and the left parts of the pictures towards each other. Hering’s theory predicted that rectangles i and i’ fuse into one rectangle and that *three* lines will show: c’, <a,a’<sup>11</sup> and b. However, all forty participants reported, that <i, i’> have fused and that the other lines have fused in two lines: <a, c’> and <b, a’> on the right of the rectangle (as in Figure 10).

10 “Sie [Abbildung 34] is so einfach, dass man nicht versteht, dass sie nicht eher konstruiert wurde!”

11 <a,a’> means that lines a and a’ (or objects a and a’) are seen as one line.

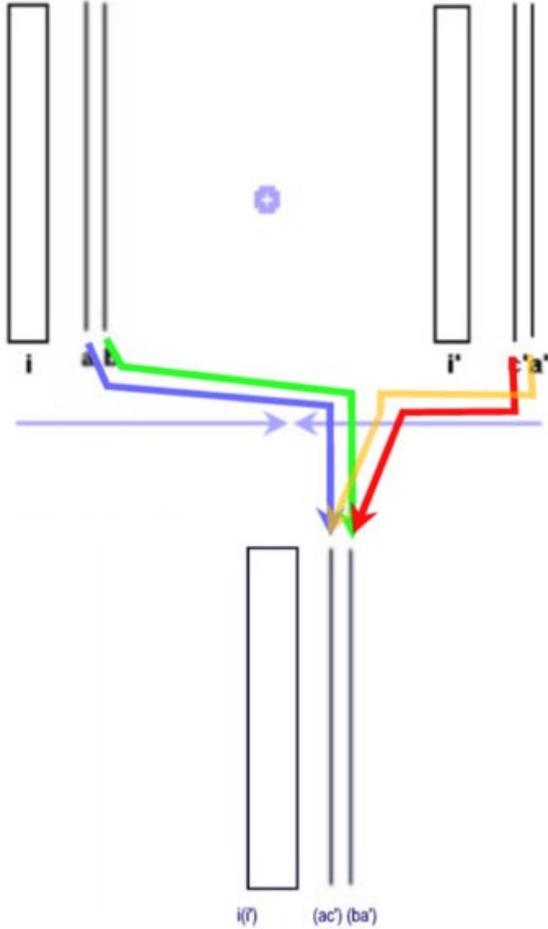


Figure 10: What you really will see when presented in a stereoscope with Figure 7

No participant reported a separate line b. Note that on the right of the rectangle the space is larger for the left rectangle (i) than it is for (i'); cf Figure 11 for an illustration.

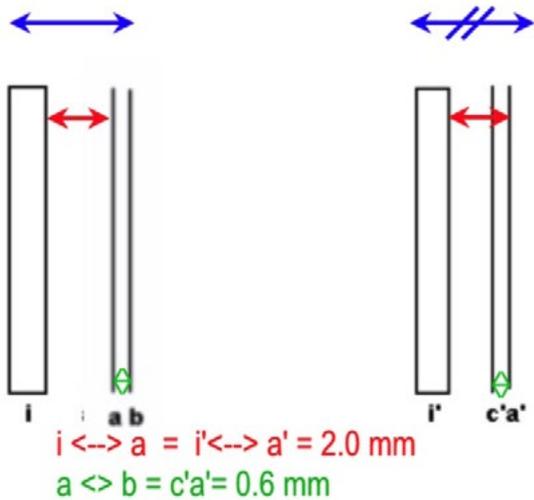


Figure 11: Note added to Figure 4; see text

This implied, according to Linschoten, that neither the physiological law of correspondence is correct, nor the law that states that objects on disparate positions can only fuse when the locations that correspond with each other are only slightly disparate.

**Other experiments and observations**

From this and other complementary experiments Linschoten concluded that Hering's theory was insufficient on both theoretical and experimental grounds. Neither the properties of the corresponding or disparate points can explain the phenomena of depth perception, nor the virtual lines drawn from the virtual foveal to the seen spots (or the projected spots). Instead, one must find the explanation in the *psychological Gestalt* qualities of the figures as a whole. His proposition summarizes this as follows: "in the (one) experienced image the (two) seen pictures that represent the left eye and right eye versions of one gestalt fuse independently of whether they are pictured as corresponding or as disparate points" (1956, p. 124).

Linschoten concluded that psychological judgments like these are autonomous, irreducible, and that they are *phenomenal* judgments that are very real, at least in their consequences. *Gestalt qualities* are irreducible *psychological* phenomena. Therefore some phenomena cannot be reduced to physiological or optical qualities. This is not to say that one should believe the "naive" participant's reports under any circumstances, even if (or especially not as he would advocate in his later work *Idols of the psychologist*) if he or she is a psychologist. He claims that only persons that are experienced in optics and have analyzed the phenomena to be explained in their phenomenal properties, can draw conclusions about some workings of perception. Phenomenology, enlightened by knowledge of nature, helps the psychologist explain perceptual phenomena.

In his thesis a hundred and some experiments are discussed in detail. They explore and demonstrate the qualitative properties of binocular depth perception, such as boundary conditions of the attraction between the monocular images, the minimal dispersion necessary for depth perception, the relation between the degree of dispersion and perceived depth, and so on. Linschoten also develops his own dynamic theory of depth perception. After experimentally testing the contribution of Gestalt theory, he shows it to fail in explaining binocular depth perception and stereovision. First, although Gestalt *theory* denies the 'point-for-point' approach of binocular depth perception (the foundation of Hering's theory), it is still based on the comparison of two monocular, 2D, Gestalt configurations. "It is as if the elementary processes still have the same configurational properties as the phenomenal Gestalts".(Linschoten, 1956, p. 315).<sup>12</sup>

Second, Gestalt theory supposes that there is a configurational explanation for the attraction between the disparate elements of the two retinal Gestalts, instead of a dynamic explanation, the one that Linschoten advocates. Gestalt theory explains the fusion of images as an *effect* of the configurational properties like the Gestalt laws of proximity or resemblance. However, Linschoten shows experimentally

12 "Man tut als hätten die Elementarvorgänge noch die gleichen konfigurationsellen Eigenschaften wie die phenomenal Gestalten."

that the tendency to fuse is dynamic, that the dynamic tendency is more fundamental than the configurational one, and that it sometimes even goes against the resemblance or proximity laws of the Gestalt theorists. “The tendency to organize the total image with a minimum of conflicting experiences” is more fundamental (1956, p. 318)<sup>13</sup>. Correspondence and dispersion are *not* geometrical projective proportions that *cause* fusion of monocular images, but are dynamical *results* (p. 389) of the *attraction* between the elements of the monocular images that emanates from the binocular, 3D image. It is as if the one binocular image *with* and *in* depth forces the monocular images to fuse, or to be attracted to each other, according to the laws of the imaged 3D object in its own spatial field. The attraction is *psychological*.<sup>14</sup>

### Manuscript “Het experiment” (the experiment)

The experiments on binocular depth perception were meant to phenomenologically analyze and demonstrate his *psychological* theory of depth perception, and—moreover—to support the abstract argument of his thesis, namely the claim that psychology is an autonomous science. *In casu*, depth perception could very well do *without* optical or physiological hypotheses. Linschoten supposed the experiments played a fundamental role in his argument. He still adhered to phenomenology as fundamental for psychology, if not science as a whole. But experiments were one kind of instruments used and to be used in psychology, even if psychology eventually was supposed to be a phenomenological psychology (Linschoten, 1959, 1961, 1968). Important and perhaps peculiar, however, is what Linschoten claimed was the role of the experiments in the manuscript in Dutch that probably was the draft for a paper to be presented in German. (Linschoten, 1955a, 1955b). As already mentioned, Linschoten argued that “it is impossible to think of any human phenomena in another way than as situated; and thus it is impossible to think of any situation in another way than from its perspective center; which establishes for psychology the primacy of the person.” (Linschoten, 1955b, pp. 31–32). So if psychology is to be an objective science it should take the person as its starting point, not “consciousness”, psyche, organism, or behavior. If the non-phenomenological psychologists presuppose the person, but ignore the effect of this presupposition, they are guilty of a *petitio principii*. For instance, the question always is for whom there are these ideas or—for instance— these sensations; or whose are

the characteristics and properties or features, other than a person’s. According to Linschoten phenomenological psychology acknowledges this presupposition, which is a step forward. Therefore, if we want to explain the person from certain properties of—say—the mind, and later we need to explain the mind by referring to a person, we have a circular argument. Unless we accept and respect—as Linschoten claims we should—that with the person the mind is supposed and vice versa. So the question to be asked and answered is: What is the phenomenon of movement, or depth? To answer that question is the most important role of experiments. “Experimental and phenomenological analysis implicate each other, they cannot replace each other” (Linschoten, 1955b, p. 27).

Results obtained in situation A cannot be claimed to be valid for situation B, unless the structural identity of A and B has been [phenomenologically] established.... This doesn’t imply that experiments should be rejected or disproved (invalidated). The claim that experimental research is useless or worthless because the experimental situation is “unnatural”, should be repudiated. ... For ultimately, the experimental situation is a human situation, and thus in a sense “natural”; and also important. (Linschoten, 1955b, pp. 29–30)

We also find this approach in, for instance, Danziger’s *Constructing the subject* (1990). Danziger analyses the experimental situation from the perspective of the persons acting as participants—including the experimenter. They play roles in a situation in which they already know what is expected from them, what is at stake, etc. For Danziger, an experiment is not primarily about cognitive dissonance or perceiving depth or a bystander effect. It is a *social* situation where *persons* act certain acts. And the ways these acts are played have changed during the one and a half centuries of psychology’s history. To enhance Danziger’s analysis, this is not in the least the case because the consumption by the consumers of the acts has changed and thereby influenced what happened in experiments. The statisticians and programmers of statistical programs, the APA, the publishers, the editors, the reviewers, the readers, the “quoters”, the newspaper journalists, the administrators, the supporting foundations and the research councils all have been involved in the production of “the experiment”.

#### Exploratory experiments

Steinle (2002) distinguishes between *experiments as tests* and *exploratory experiments*:

There was a theory that led to expecting a certain effect; the expectation led to designing and conducting an experiment; and the success of the experiment counted as support for the theory. ... Not much attention has been paid, however, to how [...] different epistemic goals may result in different types of experimental work. ... [Steinle has] labeled [another] type of work “exploratory experimentation”. Far from being a mindless playing around with an apparatus, exploratory experimentation may well be characterized by definite guidelines and epistemic goals, [that is,] the systematic variation of experimental parameters ... to find out which of the various parameters affect the effect in question, and

13 “Die Tendenz das Sammelbild mit einem Minimum von Wettstreiterscheinungen zu organisieren.”

14 I would like thank to one of the anonymous reviewers of an earlier version of this article who pointed at the “extramission theory” of vision that existed in Antiquity. According to it the eye actively emanated a visual power that made it possible to see features like color, form and position of external objects (Aivar Rodríguez & Traviesa García, 2009). Since Kepler, most modern theories of vision are “intromission theories” according to which the eye passively receives information that comes from the object. Aivar and Traviesa suggest that the dominance of the intromission theories has led to serious explanation problems, for instance where the relation of vision to action is concerned, or how actively seeking eye movements have to be explained, or how it is possible to have the many types of constancy (size constancy, color constancy, etc.). In a sense Linschoten’s dynamic theory is an extramission theory. It presupposes an active power of the “one eye with its two members” that organizes the small differences between the monocular images. Further exploration of this idea has to wait for another opportunity.

which of them are essential. Closely connected there is the central goal of formulating empirical regularities about these dependencies and correlations. (Steinle, 2002, pp. 418-419)

According to Steinle this typically results in “if-then”-propositions at the empirical level, and in revising existing concepts and categories and suggesting new ones. Often new experimental paradigms are suggested that help stabilize phenomena and formulate regularities in terms of the new experimental parameters.

Often the “exploring experimenters” have hints from earlier work which of the parameters to vary, and which not. Often, in a later phase, the resulting empirical regularities are used to point at suggested hidden theoretical entities. And, as he emphasizes, the aspect of revising concepts and categories is crucial and contradicts naive empiricist accounts of experimenting in science.

Although Steinle demonstrates his analysis using work in electromagnetism (Dufay, Ampère, Faraday) which clearly supports his distinction, there is much to say in support of the hypothesis that in psychology many examples presented as experiments-as-tests actually are (or were) exploratory experiments or (as a third category) as experiments-as-demonstration, or even (as a fourth category) as experiments-to-produce-new-phenomena.

So the epistemological role the majority of Linschoten's stereoscopy experiments seem to play is an exploratory one. But they sometimes have other consequences as well, such as refuting older theories, and creating new phenomena, as well as demonstrating phenomena, and replicating experiments conducted earlier by other researchers by varying the parameters. In that sense his work was rather new and went beyond what was known.

As for the style in which he experimented, he presented them in the tradition where experimental subjects (“proefpersonen”, “Versuchspersonen”, in Dutch and German, respectively) were asked to report their experiences after having been presented with the stimulus situation in question. This is the style Uljana Feest described as “the ‘phenomenological’ approach ... of the experimental American psychophysics” (Feest, 2019, in press, p. 6) in contrast with the “phenomenological analysis ... in which *researchers* conduct (or draw on) [their own] phenomenological analyses that inform their experimental designs.” (Ibid, p. 2). In other words, Linschoten partially followed the by then developing style of experimentation that later became the only one accepted to be used, the APA approved way. He never reported, however, the number of participants, let alone the results in a statistical form, nor the power, p-value, correlation, or whatever. Nor were there control groups. His reason was clear: his work was to give a “structural analysis” of the phenomenon of depth perception, not to measure the effect of an intervention compared with the non-intervention of another group of persons, randomly assigned or not. In his experiments he did not show much of the “frontloading phenomenology”<sup>15</sup> as Feest (2019, in press) called it inspired perhaps by what William James called “experience” influenced by the “front-door-way of simple habits and associations” (James, 1890, p. 628, vol. 2.).

## The theoretical part of the thesis

In his thesis Linschoten demonstrated he was an outstanding experimenter, though this should not be measured by comparing his work with present day standards. It is the ingenuousness of the figures he used to test theories and to demonstrate and explore the structure of binocular depth perception, not his skill of statistics. He also showed that the experience of (things in) depth cannot be explained completely by reference to optical properties of the stimulus or the physiological properties of the perceiver. In the theoretical part of his thesis he develops his dynamic theory of depth perception. This part has four chapters<sup>16</sup>, in which he discusses and analyzes the implications of experiments, hypotheses, theory and postulates at levels ranging from the philosophical to the mathematical. It is a strong anti-mechanistic and anti-reductionistic argument for a dynamic, *psychological*, explanation of depth perception. Perceiving objects in depth is an active process in which the Gestalt of the *binocularly viewed object* leads the two monocular images to fuse. In that sense it is dynamic. And therefore it seems right to call it—as he did in one section of the theoretical part—the *unity of the double eye* (“*die Einheit des Doppelauges*”, p. 483, p. 494) and a little later the *eye with two members* (“*das zweigliederige Auge*”, p.521)

Second, he considered this to be an argument for an autonomous psychology. Although optical and physiological/anatomical features of perception play an important role, their role is not an exclusive one. So every time the question rises how humans achieve a certain function, the next, more fundamental question should be what is the achievement and what meaning the achievement has for being human. This is ultimately a psychological question, according to Linschoten, although he cannot avoid to formulate it as—in my view—strongly related to a biological function. It strongly points to an evolutionary role of, in this case, depth perception:

Depth perception... puts man in the position, with the largest possible rest of eyes, head, body and arms, and with the largest concentration in the natural range of attention (that is, in the range of largest viewing acuity) to ultimately execute fine finger movements in a three-dimensional working space, where sensorially and motorically the largest precision can be attained. (Linschoten, 1956, p. 531 my translation )

Third, note that, nowhere, Linschoten used phenomenology as a psychological method. He uses phenomenological analyses to check whether one can hold claims for explanations of perception at all; these analyses are a priori of optics, physiology, anatomy, *and* psychology. They are not indigenous to psychology. However, it cannot be insisted that phenomenology provides in the foundation (or justification) of only psychological theories of depth perception. In this respect his thesis must have pointed the way to what in his last book (1964) had been revised to “phenomenology as only the starting point”. So Linschoten showed that phenomenology helps to get the problems

<sup>15</sup> By “frontloading phenomenology” Feest referred to the approach where “researchers conduct phenomenological analyses which inform their experimental designs” (Feest, 2019, in press, p. 3 of the manuscript).

<sup>16</sup> Chapter eight discusses a dynamic theory of depth localization, chapter nine confronts Herings theory and the attraction theory with sense physiological facts, and chapter ten looks at the dynamical explanation of the Gestalt theory of depth perception at the psychophysical level.

right and even helps to suggest where to find answers. But it does not give definite arguments for a specific theory. But then, again, it has an important role in exploring the phenomenon and checking whether a theory possibly has given the answer to the original questions. In this respect his 130 experiments not only explore thoroughly, but can also be seen as a vast number of replications to find out what the parameters are that have to be taken into account when investigating depth perception.

That Linschoten used depth perception as the field on which to focus is—interesting and important though it is—merely a matter of example. His thesis remains a contemporary work of the nineteen fifties. By now, many researchers have added to the field—both psychologically, physiologically, optically and anatomically. His thesis constitutes a central part of his work and approach. We should still consider it as a key work in relation to his other work, particularly when considering the question how he saw the role of phenomenology in psychology. I will briefly discuss this in the next section.

### Conclusion

When writing his thesis, Linschoten still believed psychology was to be a discipline involved in the analysis of experiences, which included, if not was based on a phenomenological analysis. As argued in other contexts (Stam & Van Hezewijk, 2004; Van Hezewijk & Stam, 2008; Van Hezewijk et al., 2001; Van Hezewijk, Stam, & Panhuysen, 2002) he believed only a phenomenological approach could guarantee the autonomy of psychology as a discipline (Linschoten, 1959, 1968). He changed his view later, to end his short life with *apparently* the opposite view (Linschoten, 1964) that psychology should be reductive and experimental<sup>17</sup>. Although he became much more critical of the view that phenomenology was the sole *basis* of psychology, the turn to reduction and experiment was not meant to be exclusive. “The psychologist... leaves the clarification of the images [of the *sensus communis*] as images to phenomenology. He doesn't deny phenomenology has as its task the philosophical-verbal analysis of the life-world: to express and organize experiences as clear as possible in the language in which human experiences are expressed, and to see their own starting point [for a scientific psychology] clear as well. But [the psychologist] accepts the result [of phenomenological analysis] only as the determination of the starting point” (translated from Dutch by RvH, Linschoten, 1964, p. 405). So to a certain degree his Ph.D. thesis (Linschoten, 1956) already illustrates what Linschoten may have meant by this task of phenomenology versus the task of reduction and experiment.

My suggestion has been to look at Linschoten's experimental work on the perception of movement, and the perception of (things in) depth from another perspective. When we distinguish experimental work as either “creating new phenomena”, as “demonstrating

phenomena”, as “exploratory experiments”, or as “experimentation to test”, his work is best characterized in general as “exploratory”. This doesn't mean the other categories do not apply. For indeed some of his experiments on binocular depth perception had the effect, if not the aim, to test earlier theories of, e.g. Helmholtz, Wheatstone, Hering. But this seems to be the result of thoroughly exploring the parameters that were used in experiments with stereoscopic vision.

According to Linschoten, the results indicated that, firstly, the “metaphysics” of stereoscopic vision had to be revised as a purely *psychological* effect or phenomenon, because optical or physiological theories presupposed without saying so that depth vision presupposes a *person* that views things in depth in a situation of “spatiality”. There always is a person for whom the thing is in his or her depth, with width (left-right), height (below, above), and distance (nearby or far away). And there is always a person who has to answer the question “how does it look?”, “where is it from my perspective?”, etc. That is, the phenomenological questions have to be answered before a physicist, optometrist, physiologist, can start measuring their physical, optical or neurophysiological parameters. Actually, according to Linschoten, physiology nor optics were necessary to explain the experience of depth. In Linschoten's words, the experience of space is *a priori*, not the result of unconscious or conscious inference.

The results also indicated, secondly, at a theoretical level that the phenomena of perceiving depth are that the binocular 3D image is responsible for the attraction of the two 2D monocular images, not the other way around. This may seem strange in the modern “intromission” view of stereoscopy, but for a phenomenologist it is essentially what it looks like.

Also, thirdly, on a theoretical level he found that disparities between the edges (boundaries) of the used figures are the essential parameters of seeing 3D figures in depth. Not points, neither known objects are essential.

A few years later

Three years after defending his dissertation, Linschoten published a book on the psychology of William James (Linschoten, 1959), somewhat later translated in German (Linschoten, 1961) and almost ten years later also in English (Linschoten, 1968). In his study on James he defended the *complementarity* of an interpretative psychology—using the perspective of *the intentionality of the person*—and an explanatory psychology—using *description and analysis of the experience of the body in the spatio-temporal domain*. In both perspectives experiments were used, and to be used, according to Linschoten.

Five years later his “*Idols of the psychologist*” was published posthumously (Linschoten, 1964). He finished his book only a few weeks before a third heart attack took his life. It was only published in Dutch. Planned translations never materialized. A pity, because in it Linschoten demanded an experimental psychology, aimed at controlling and explaining behavior with the aid of formal models (Van Hezewijk & Stam, 2008). The demand for formal models was new, but the suggestion to use experiments was not. As Linschoten observed in his *Idols*, as well as his notes on the experiment in phenomenology (Linschoten, 1955a, 1955b) experimenting (in the broad sense of exploring and testing an idea) is, after all, human,

<sup>17</sup> His *Idolen van de psycholoog* (Linschoten, 1964) was published posthumously. A few weeks after he finished this volume he died of a heart attack. *Idolen de psycholoog* became one of the most popular texts among Dutch psychologists, especially those in academe. Unfortunately, plans for a translation into English never materialized although there was a draft for a contract with Duquesne University Press.

all too human. Ever since Eve humans have been experimenting to escape from social and physical as well as natural challenges, and to improve life conditions. So what is unnatural about it? Of course this is a rhetorical question, because there is much unnatural in the way some forms of experimenting have developed, due to, for instance the introduction of analysis of variance in the nineteen thirties (G. Gigerenzer, 1987a). Indeed, experiments can be, and are conducted in circumstances that are more ecologically valid (“are more natural”), by, for instance, ethologists, criminologists, education researchers and political scientists. And after all, eidetic reductions as Husserl proposed, can be seen as thought experiments as well, only by *thinking* of alternative ways to represent an object in order to come to the essence of it.

Gergen's and Giorgi's views, briefly discussed above, seem to be that the natural scientific approach violates our experiences and alienates the essential characteristics of the phenomena. Reduction, abstraction, quantification, experimentation deform the world and our social environment as they naturally appear to us. They are unnatural because they no longer work with the things we as individual persons got used to find natural: cars, stones, trees, bees, etc. But what is natural and what is not natural are presuppositions that can be questioned, and will not be the same for everyone, and certainly not for bees and trees. “Nature” is an idea, like “consciousness”, that is useful in describing the world for daily purpose, but it is not necessarily the only truthful way to do that (Linschoten, 1955a, 1955b).<sup>18</sup>

This argument, which is typical for a phenomenologists' position, prioritizes experience. But it never was Linschoten's position. Exactly because we focus on the conscious experiences of certain phenomena in their appropriate contexts, we could miss quite a few relevant and interesting aspects of them. This applies to lay persons as well as psychologists, even when the latter claim to be observers trained to transcend personal experiences (Linschoten, 1964). Their experience is not only bound by, if not determined by and limited to present circumstances and period, but also shielded from the experience of others. An approach that is open to alternative analyses may very well enhance our experience and provide insights we would never have when we restrict ourselves to phenomenology. A phenomenological text of—say—the fifties (for instance about the experience of “the hidden place in the life of the child”, or of “the hotel room”, or “the psychology of driving a car” (e.g. in Buytendijk, 1968; Langeveld, 1957; Linschoten, 1953). All these views now seem strange to us, just as—indeed—some experiments or other empirical results can be and have been overtaken by better ones. Moreover, these phenomenological writers used language in a special way, but—as, again, Linschoten (1964) observed—it is difficult to translate them to—say—French, Spanish, Swahili; their meanings will be lost or at least transformed. Language used to describe experiences is, after all, a form of reduction just as numbers are the main descriptors of what happened in an experiment (Linschoten, 1964). As he stated in his foreword, he may

have ridiculed some of his colleagues work, but he made fun of his own verbalizations in his earlier work as well.

It is the latter book, *Idols*, that most Dutch psychologists like to refer to as the work of the phenomenologist that converted to empirical-analytical psychology. They think Linschoten did a 180 degrees turn. Almost none will have read the Ph.D. thesis (in German). On the other hand many of the phenomenology oriented American psychologists never read his Ph.D. Thesis, nor his *Idols of the psychologist*. Again a pity, because they may have been surprised. In his *Idols*, Linschoten made fun of the phenomenologists, although he now declared his position as follows: phenomenology only offers a starting point for psychological (experimental and theoretical) work. It offers no method, no justification, no alternative to the psychology he advocated in *Idols*.

So initially he seemed to have thought phenomenology was essential for a non-reductive psychology. In 1964 his view had shifted towards the idea that reduction and experiment are indispensable for a psychology that emancipated from the *sensus communis*<sup>19</sup>, and could and should contribute to science, to the human sciences in particular.

It may be clear that the 180 degrees turn is not really what happened<sup>20</sup>. It is much more complicated than that. For his Ph.D. thesis he conducted many experiments in the style that later became the preferred one, but with an epistemological role that supported some phenomenological conclusions. That is, that it is important to investigate how the *person* experiences depth, using (of course) his or her one eye with its two members, in order to be able to act in nearby space. One would rather say this is a psychology of phenomenal perception, not a phenomenological psychology. In this perspective, today, a psychological theory and experimental approach of the phenomenal experience of depth would not be very different.

On the other hand, some of his “metaphysical” and theoretical conclusions in the thesis were soon refuted. Already three years after his thesis was published, Hubel and Wiesel refuted the idea that physiology was unimportant, with their experiments with the neurons of cats (Hubel & Wiesel, 1959). In 1964 Bela Julesz demonstrated that “random dot stereograms” could evoke the

19 Linschoten introduced the concept of *sensus communis* to indicate “the [implicit, non-scientific] opinions about man and world that are normative, obvious, self-evident and normal for daily life, in its entirety ... [including] beliefs regarding human beings, their essence, their destination and origin, limitations and opportunities, rights and duties, etcetera.” (my translation, Linschoten, 1964, p. 19)

20 It even has been questioned if *Idols* presented his final view. In an interview the present author and Hendrikus Stam had with Amedeo Giorgi the latter asked Linschoten about his changed view (Giorgi, 1999). Linschoten is said to have answered that his next book would be phenomenological again. However, in an article Linschoten published just before his death he stated “[he was] less prepared than some years earlier to defend a radical phenomenological design of psychology. One of the reasons for that is the fruitfulness of reductive models in the positivistic fashion. [For it is] in the positivistic design that psychology recently has been successful” (Linschoten, 1963, p. 113). Moreover, in the documents that were in Linschoten's legacy were many handwritten and typed drafts for parts of books and articles. The only clues for future books Stam and I found were for a history of psychology book, and for a book to be called “Radical Anthropology” by which he meant a phenomenological anthropology, not as a science, but only to provide critical reflection on the world of daily affairs and the presuppositions in psychological theories. The number of written pages for this future book doesn't justify the conclusion that he would return to his earlier vision. It had only the list of chapters planned.

18 I would even contest the claim of a learned anonymous reviewer of an earlier version of this article, that consciousness is unnatural (in the sense of non-physical), so that it is strange that unnatural phenomena like consciousness are studied in a natural (in the sense of borrowed from physics) situation (experiments). Consciousness perhaps is not physical, but it sure is natural if humans are part of nature, and not excluded of being part of the world.

experience of depth as well. So an optical theory could indeed be used to explain depth experience: no edges or boundaries of minimally meaningful objects are necessary (Julesz, 1964, 1971). Moreover, in 1970, Blakemore and Cooper (1970) demonstrated that perceiving depth not is a priori nor innate. They used kittens raised in environments with only vertical cues and observed the effect thereof in later life. Seen from the perspective of Linschoten's claims all of these experiments were crucial (Steinle, 2002) in that they definitely changed the psychological landscape and reinforced the myopic way of experimenting only as if it were tests to support a hypothesis. And still later David Marr published his *Vision* (Marr, 1982). The subtitle of this work is illuminating for the change perception theory went through. The subtitle is *A Computational Investigation into the Human Representation and Processing of Visual Information* which suggests on the one hand a human approach, but on the other hand a rather different vision of what are the relevant psychological processes. The analysis of visual information, for example depth perception, leads to quite a different view of the task of the visual system. And moreover, it seemed to be the basis for a mechanistic approach. Marr's work turned out to be crucial for years to come for both the psychology as well as the artificial intelligence of perception, i.e. the processing of visual information. In Marr's words (a little bit out of context, I confess), it "it open[ed] the way for a rational investigation of the phenomenon rather than the confused cataloguing of its phenomenology." (Marr, 1982, p. 199). Although Marr may have been wrong about the "confused cataloguing" of Linschoten's work, crucial or not, it remains to be seen if either Marr or Linschoten was for the good of psychology. If we could appreciate the value of both phenomenological as well as experimental contributions, in relation to the psychological problems to be solved, and rationally evaluating their results instead of judging with the prejudices of the trades they come from, we might see real growth of knowledge.

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