



Gestalt psychology, frontloading phenomenology, and psychophysics

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Abstract

In his 1935 book *Principles of Gestalt Psychology*, Kurt Koffka stated that empirical research in perceptual psychology should begin with “a phenomenological analysis,” which in turn would put constraints on the “true theory.” In this paper, I take this statement as a point of departure to investigate in what sense Gestalt psychologists practiced a phenomenological analysis and how they saw it related to theory construction. I will contextualize the perceptual research in Gestalt psychology vis-a-vis Husserlian phenomenology on the one hand and mainstream psychophysics on the other, and I will argue that Gestalt psychologists practiced a form of “frontloading” phenomenology: Instead of requiring experimental subjects to engage in experiential reflections, such reflections were—in a sense—already engrained in the experimental designs used by researchers. This type of phenomenology was decidedly anti-“introspectionist” and as such was compatible with some of Husserl’s basic commitments, while at the same time bearing a surprising resemblance with the methods employed by psychophysicists like E. Boring and S.S. Stevens. This latter point will prompt me to explore what the difference between Gestalt-psychology and psychophysics amounted to. My analysis will reveal some disagreements and misunderstandings, especially with regard to the notions of isomorphism and introspection.

Keywords Gestalt psychology · Psychophysics · Experimental phenomenology · Introspection · History of psychology

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1 Introduction

In his 1935 book, *Principles of Gestalt Psychology*, Kurt Koffka famously stated that the central question of perceptual research is “[w]hy ... things look as they do” (Koffka 1935, p. 75). This question is, *prima facie*, an explanatory question. However, one might suspect that it presupposes a *descriptive* question, namely, “how do things look?” Differently put, it would seem that the task of explaining something needs to be preceded by a delineation and description of the explanandum. Accordingly, Koffka, in chapter 1 of the book, highlights the question of “what are phenomenal characteristics of our perceptual experiences?” In general terms, of course, his answer (like that of other Gestalt psychologists) was that our perceptual experience is typically *structured*, it has a “gestalt.”

In this article, I will scrutinize both, the descriptive and the explanatory project of Gestalt psychological research, as exemplified in Koffka’s writings. I will start by taking a closer look at the methods Gestalt psychologists used in their descriptive task. Koffka himself gives an intriguing hint here, contrasting a “German” and an “American” style of investigation and stating that the Americans often find the Germans too speculative, whereas the Germans find the Americans too superficial. He ultimately sides with “the Germans,” whose method he refers as the “phenomenological method.” Of this method he says that it gives priority to “[a] good description of a phenomenon,” thereby putting constraints on “definite features a true theory must possess.” So, Koffka seems to be arguing here that other approaches to perception have hastily rushed to specific explanatory posits, prior to providing a proper (“phenomenological”) analysis/description of the explanandum. Moreover, he is suggesting that such a phenomenological analysis is a necessary prerequisite to better theorizing. These claims raise a number of questions, of which I highlight two here. First, what exactly does Koffka mean by “phenomenological method,” and how is this method related—both historically and systematically—to the philosophical movement associated with names like Husserl and other phenomenologists? Second, what are we to make of the claim that a phenomenological analysis can prevent us from making questionable theoretical posits? What precisely is the notion of “true theory” assumed by Koffka here, and how can the development of such a theory be constrained by phenomenological analysis?

The questions just raised are partly of a historical and partly of a systematic nature. Accordingly, I am interested both in Koffka’s views in their historical context as well as in the issue of what, according to him, constitutes a phenomenological method in perceptual research. In systematic terms, moreover, I hope to be able to use this historical material to shed light on issues that have arisen in the more recent debates about introspection. I will distinguish between two notions of “phenomenological method”: One in which *experimental subjects* are prompted to engage in phenomenological analysis and one in which *researchers* conduct (or draw on) phenomenological analyses that inform their experimental designs. The latter approach will be referred to as “frontloading phenomenology”. I will argue that even though these two notions of phenomenological method are not mutually exclusive, Gestalt psychologists primarily used frontloading phenomenology. I will show that their approach, while phenomenological in the sense just outlined, was located in the psychophysical tradition, which

places great value on strict experimental control. Conversely, I will suggest that there is a sense in which Koffka's psychophysicist contemporaries (Edwin Boring and S.S. Stevens) can also be said to have employed frontloading phenomenology. I will conclude with a discussion of the question of what the disagreements between Gestalt psychologists and the psychophysicist Edwin Boring consisted in.

2 Gestalt psychology and phenomenology. A quick historical overview

Let me begin by clarifying that when I use the term "Gestalt Psychology" I have in mind the Berlin/Frankfurt school that emerged in the early teens of the twentieth century and was prominently expressed in Wertheimer's 1912 publication, based on his famous experimental study of the phi-phenomenon. (see Ash 1995 for a comprehensive historical account of this movement). Thus, the analysis presented in this paper only applies to this particular branch of Gestalt psychology.

The term "Gestalt" had been coined by Christian v. Ehrenfels (1859–1932) in his 1890 paper, "Über Gestaltqualitäten," where he observed that when presented with a number of perceptual elements (e.g., musical tones), we perceive not just those perceptual elements, but also a structured whole (a chord, a melody, a rhythm), which cannot be reduced to those elements. This is evidenced in the fact that one can experience the same "Gestalt" (e.g., a minor or major chord, or a particular melody) even if none of the elements are identical. The notion of Gestalt was widely discussed in subsequent years (see Feest 2007), but prior to the work of Wertheimer and his colleagues a central premise of Ehrenfels's analysis was rarely questioned, i.e., that Gestalt perception—while not being *reducible to* elementary sensations—was *comprised of* elementary sensations. Taking this assumption seriously, then, the basic question was how Gestalt phenomena might be said to emerge from basic sensations. Wertheimer fundamentally rejected this question, arguing instead that Gestalt perceptions are immediate and basic. In making this claim, Wertheimer gave epistemic priority to what experimental subjects were actually experiencing, pointing out that they don't experience simple sensations prior to having a Gestalt perception. This is famously illustrated by the phi-phenomenon, where subjects report seeing movement when two stimuli are presented in rapid succession. His point was that even though in this experiment two stimuli are presented, the subjects do not experience two elementary sensations prior to experiencing movement.

Now, given Wertheimer's appeal to what one might characterize as a phenomenological description, what was the relationship between his approach and those of phenomenologists at the time, like Edmund Husserl? An important intermediary figure in this regard was Carl Stumpf (1848–1936), who combined a scientific and empirical orientation with a profound knowledge of philosophical problems (Sprung and Sprung 1997, 2003). All three founders of the Berlin/Frankfurt school had studied with Stumpf in the early years of the twentieth century. Stumpf had been hired by the Berlin philosophy department in 1893, at a time when psychology was a fixed part of the philosophical curriculum, but its increasingly empirical and experimental orientation was beginning to be heavily debated. He was also instrumental in expanding the

psychological institute at the University of Berlin, which ultimately (at the beginning of the Weimar Republic) led to its institutional separation from the department of philosophy and made way for an understanding of psychology as a separate discipline. It was also at this point that Wolfgang Köhler took over Stumpf's chair at the Berlin institute of psychology.

Prior to becoming a professor in Berlin in 1893, Stumpf had been a professor in Halle, where, from 1886 to 1887, Edmund Husserl had been his assistant, while finishing up his habilitation, *Über den Begriff der Zahl: Psychologische Analysen* (1887). This work was still strongly influenced by Franz Brentano, with whom Husserl had studied in Vienna a few years previously (Husserl finished his dissertation in Vienna in 1883), though he would later reject Brentano's descriptive psychology as too naturalistic (Feest 2012). In turn, Ehrenfels had also studied with Brentano in Vienna, and Carl Stumpf had also studied with Brentano as a young student (more precisely, in 1866; Baumgartner 2015), back when Brentano was himself finishing his habilitation in Würzburg, though Stumpf soon transferred to Göttingen, where he studied with Hermann Lotze and was also exposed to the psychophysical tradition of Weber and Fechner. Finally, one of Stumpf's major psychological works, his *Tonpsychologie*, placed great emphasis on what we may, again, regard as phenomenological descriptions.

These multiple points of overlap between early experimental psychology and phenomenology (converging in the Berlin/Frankfurt school of Gestalt psychology) are suggestive, but we need a clearer account of what precisely constitutes a phenomenological method. In developing such an account, we should also keep in mind that in some ways the method of finding functional dependencies between stimuli configurations and experiences was also practiced by psychophysics, raising the question of whether Gestalt psychological experiments should be described as psychophysical. Indeed, I will argue below that a psychophysical approach is not mutually exclusive with (a certain kind of) phenomenological analysis.

3 Phenomenological methods: Husserl and the Würzburg school

It will help to begin with a rough understanding of how phenomenology, as a philosophical project, might be said to differ from phenomenological approaches in psychology. In this regard, Husserl himself is a good source to turn to.

Edmund Husserl distanced himself from empirical psychological work at the time in two (interrelated) ways, namely by way of a critique of *naturalism* and *introspection*, respectively. With regard to the first of these issues, Husserl's whole philosophical project was premised on a critique of the natural scientific mode, with its split between the mind and the physical world. He saw this split as deeply embedded in Western philosophy since the rise of early modern science (see, e.g., the analysis he provides in his 1936 *Crisis of the European Sciences*). Husserl, instead, suggested that the split between the subjective and the objective is one that is constructed *within* our phenomenal experience, and that it was therefore here that a proper philosophical analysis should start, attempting to attain, via "phenomenological reduction," knowledge about the essences of ideal objects. For him, the proper philosophical method

was therefore one of methodological solipsism, i.e., a suspension of questions about the world outside our experiences and a detailed analysis of the experience itself. Importantly, he viewed such detailed analyses as contributing to a philosophical, not a natural scientific, endeavor.

We turn now to the second way in which Husserl distanced himself from empirical psychology as it was practiced at the time. As a direct consequence of his critique of the natural scientific mode, Husserl was critical of what he saw as scientific psychology's attempts to adopt a natural scientific stance, even if and where psychologists turned their scientific efforts to human experience and employed what might be called "phenomenological" methods. For this reason, he was critical of his former teacher Franz Brentano, whose descriptive psychology was ultimately intended as an empirical project, modeled on the natural sciences (albeit with a distinctive subject matter, namely mental phenomena). Husserl argued that the distinction between inner and outer perception, which was central to nineteenth-century psychology from Fechner to Brentano, and which underwrote the use of so-called introspective methods, was fundamentally mistaken. We can turn our attention to the phenomenology of our experience, but for Husserl this was emphatically not an exercise of introspection or of empirical psychology, because the latter presupposed a distinction between the "outer" phenomena of the physical world and the "inner" phenomena of the conscious mind, declaring the latter to be the proper subject matter of psychology.

Unsurprisingly, therefore, Husserl was critical of those empirical psychologists who appealed to his work when characterizing their (introspective) methods. For example, Bühler (1907), a member of the Würzburg school of thought psychology, argued that it was possible to identify types of thought processes by engaging subjects in complex cognitive tasks and asking them to describe the phenomenology of their thinking. Bühler explicitly named Husserl as an inspiration. However, Husserl (in his 1913 *Ideas*) quite harshly asserted that (supposed) advocates of his method, such as Bühler, had failed to appreciate that he was talking about a priori phenomenological analyses, not about empirical research by means of inner perception (Husserl 1913, p. 159; see also Feest 2012).

But what about the Gestalt psychological research of the Berlin/Frankfurt school? Did it use phenomenological methods *in the same sense* as the researchers of the Würzburg School, or was it more closely aligned with a Husserlian approach? To be sure, members of the Berlin/Frankfurt school conducted empirical, not a priori, analyses. Yet, I will argue in a moment that there was also an important commonality between theirs and the Husserlian methods, insofar as the Gestalt psychologists, like Husserl, had no use for a dichotomy between perceptions of the "inner" and "outer" world. In their experiments, they did not ask subjects to describe the phenomenology of their experience, but instead instructed them to simply report what they were perceiving. Consequently, they rejected the notion of introspection (which is typically construed to be some kind of inward looking). In the following, I will explicate this point by presenting Koffka's argument to the effect that a "phenomenological" method is compatible with a rejection of introspection.

4 Phenomenology, introspection, and psychophysics

We return to Koffka's 1935 book, where (a few pages before the passage cited above), he addresses the relationship between introspection and phenomenology. He defines phenomenology as referring to "as naïve and full description of direct experience as possible" (Koffka 1935, p. 73) and continues by remarking that "[i]n America the word 'introspection' is the only one used for what we mean" (ibid.). However, he critically notes that introspection is tied to a particular type of research program, namely one "that analyzes direct experience into sensations or attributes" (ibid.). Koffka then states that introspection "became unpopular in America," but adds that "in their justified criticism they threw out the baby with the bath, ... tending to leave out phenomenology altogether" (73).

Prima facie, Koffka seems to agree with those who abandoned introspection as a method, but charges that they went too far. Who is he talking about here? First and foremost, Koffka is presumably referring to the decline of the "structuralist" research program that had been pursued by Edward Titchener, which had been closely tied to an introspective method in an effort to experimentally identify "elements" of the conscious mind. As various scholars have pointed out, the decline of introspection was due, in part, to a decline of the structuralist research program, not of introspection per se (Beenfeldt 2013). In this vein, we might take Koffka to be saying that even though the project of identifying elements of the conscious mind had declined, there was a certain sense in which attentiveness to conscious experience was still not only viable, but also epistemically fruitful and necessary.

I would like to add a more specific historical context, however, namely that of psychophysical research of the 1930s. During that time, some scientists were still committed to the project of identifying basic features of consciousness, though (as I will explain below), they referred to those features as "dimensions" rather than "elements." Specifically, Titchener's student Edwin Boring, and Boring's student Stanley Smith Stevens pursued this kind of research and they explicitly saw themselves as situated in the lineage of the psychophysical tradition of Titchener (see Feest 2005, for more details). However, they tried to avoid the suspect language of phenomenal experience and introspection altogether. In this vein, S. S. Stevens attempted to formulate a very parsimonious understanding of "experience" as *discriminative reaction* (Stevens 1935a, p. 521) hoping to connect basic dimensions of experience closely to tightly controlled experimental stimuli and reactions to those stimuli. In the light of this, I suggest that the targets of Koffka's criticism were Boring and Stevens, whom he regarded critically on the grounds that they tried to eliminate the language of subjective experience, while purportedly studying "dimensions" of experience.

This juxtaposition of Koffka's "phenomenological" approach with that of experimental American psychophysics at the time cannot be the whole story, however, since, on closer inspection, the differences between the perceptual psychologies of Gestalt psychologists and psychophysicists such as S. S. Stevens and Edwin Boring were not as big as one might suppose at first. To understand what Gestalt psychologists like Koffka took to be distinctive of their phenomenological approach, we need a more fine-grained understanding of why they distanced themselves from psychophysics. I will begin by highlighting two points of prima facie similarity between the Gestalt psycho-

logical research of Koffka and his colleagues on the one hand and the psychophysical research of Boring and Stevens on the other.

First, experimental research in Gestalt psychological investigations of perception was nothing if not highly controlled. Moreover, this research was also taking discriminative behavior as the mark of sensory experience. Recall, for example, Wertheimer's experimental study of the phi-phenomenon, which consisted in laborious variations of the stimulus conditions (in this case, time lapse between two stimuli) in order to determine thresholds of discrimination (or thresholds of Gestalt switches), as indicated by behavioral (typically verbal) responses. In this vein, I would suggest that Gestalt psychological research must be regarded as psychophysical research. Second, and conversely, the psychophysics of Stevens and Boring shared one of the fundamental tenets of Gestalt psychology, namely the rejection of what Köhler (1913) had referred to as the "assumption of constancy," i.e., the notion that perceptions can be analyzed into simple sensations, which stand in a one-to-one relation to simple elements of stimuli.¹ Rejecting this assumption, Gestalt psychologists focused on conscious perceptions that are the results of the ways in which stimuli are arranged relative to each other rather than positing elements of sensation. Boring (1933) acknowledged that Gestalt Psychology "doomed elementarism" (Boring 1933, p. 21), but notes that Titchener responded to this with a shift away from the study of elements towards the study of "dimensions", such as quality, intensity, extensity, and protensity. Such dimensions, Boring emphasized, do not necessarily stand in a one-to-one relationship to dimensions of physical stimuli. In this vein, Boring and his student Stevens held that even though there are some experiential dimensions of hearing (such as loudness and pitch) that do correspond directly to specific dimensions of tonal stimuli (namely energy and pitch, respectively), there are also other tonal dimensions (volume, density) that are functionally dependent on a unique assemblage of energy and frequency, just like Gestalt phenomena are the results of unique stimulus-configurations. This comes out clearly in Edwin Borings 1935 article "The Relation of the Attributes of Sensation to the Dimensions of the Stimulus," where he describes empirical results of the above kind and goes on to state that "this invalidates the principle of the one-to-one correlation between the sensory attributes and the stimulus dimensions" (Boring 1935, p. 238).

Summing up, I argue that there were some similarities between psychophysics and Gestalt psychology in that both insisted on strictly controlled behavioral experiments, treating discriminatory reactions as the mark of conscious experience, and both rejected the notion of a simple correspondence between physical stimuli and experience. So, if it is the case (as I have contended above) that Koffka's remark about "shallow" Americans was directed against psychophysicists like Boring and Stevens, we need a better grasp on where the differences between their approaches lay, precisely. Was it that psychophysicists did not use phenomenological methods? Was it that they did not use *the right kind* of phenomenological methods? Or was it that (from the perspective of Gestalt psychology) psychophysicists did not go far enough in, and did not grasp the implications of, their use of phenomenological methods? In the remain-

¹ Notice that the assumption in question is not that for every stimulus there is one and only one sensation, but rather the other way around: for every sensation there is a stimulus in the world that invariably gives rise to that particular sensation, regardless of context.

der of this paper, I will argue that this last answer is the correct one. To prepare my argument for this point, I will begin by outlining two conceptions of what constitutes a phenomenological method in the context of psychophysical research. I will show that here, too, we find surprising commonalities between the Gestalt psychological and the psychophysical approach.

5 Two kinds of “phenomenological” methods in psychophysics: reflective and frontloading

I distinguish between two understandings of what one might be meant by “phenomenology” and how it plays out in experimental research. According to the first understanding of “phenomenology,” experiments are set up such as to enable subjects to reflect upon, and describe, the qualitative nature of their experiences. According to the second, experiments are set up in the light of a *prior* phenomenological analysis (by the experimenter or some other person). For reasons that will become clear momentarily, I refer to the former as an “reflective”, the second as a “frontloading” model of phenomenology.²

According to the first understanding of the role of phenomenology in perceptual research, such methods require subjects to describe not *what* they see, but *how* they see it (e.g., Hatfield 2005). Differently put, they have to reflect on their experience. Hatfield—in his defense of introspection—aligns this type of (reflective) phenomenological analysis with a fairly lean understanding of introspection. To introspect, on this understanding, is simply to describe the quality of one’s experience, and this can be done in a scientific and experimental manner precisely because subjects are effectively describing external stimuli with an eye on *how* they are experiencing these stimuli.³ Hatfield refers to this kind of activity as “introspection.” Chirimuuta (2014) has picked up this suggestion in order to investigate the nature of experiments that are specifically designed to tease out the “how”, i.e., the phenomenology, of subjects’ experience. She argues that in contemporary psychophysics, there is indeed a class of experiments that are “introspective” in the way Hatfield describes. These experiments are designed to prompt subjects to reflect on their experience, in contrast with other experiments that simply ask for quick responses. Chirimuuta shows that both kinds of methods are in fact used in psychophysical research and refers to the former as “introspection-reliant” (Chirimuuta 2014). To be sure, these methods do not explicitly ask subjects to reflect on how a stimulus is experienced, but rather give subjects instructions that force them to take a more reflective attitude, for example by requiring subjects to compare stimuli.

One worry with this “reflective” approach, however, is that the more leeway subjects get in reflecting about their experiences, the more variable their responses will be. In fact, this was one major reason why Wilhelm Wundt (1907) was so critical of the “phenomenological” method employed by Bühler in his thought psychology

² The distinction between “reflective” and “frontloading” is not entirely satisfactory as it (wrongly) suggests that no reflection occurs in frontloading phenomenology. The point is that in the case of frontloading phenomenology, the *experimental subjects* are not required to reflect on their experience.

³ Notice that this notion of introspection differs from the one so emphatically rejected by Husserl in that it does not rely on an internal/external observation dichotomy.

(see the discussion in Sect. 3 above). Thus, Wundt charged that the hallmark of the experimental method in psychology was the strict control of experimental stimuli and possible responses in order to ensure that the experimental subjects' subjectivity could not get in the way of reliable results. (Bühler 1907; Wundt 1907; see also Feest 2014 for more details about this debate). In a similar vein, Chirimuuta points out that what she calls "introspection-reliant" methods in contemporary psychophysics are controversial, precisely because the more reflective subjects are allowed to be, the greater the worries about unreliability of their responses. Hatfield (2005) argues that these problems can be reduced by tighter experimental control and instructions. However, this seems to result in a trade-off between reliability and the use of phenomenological methods. To put it differently: If we understand phenomenological methods in an "reflective" mode as requiring experimental subjects to have some freedom in their responses, this will inevitably result in greater variability in their responses, but if we reduce variability by tightening the experimental conditions, this will automatically mean restricting that aspect of the method that made it phenomenological to begin with.

One response to this apparent dilemma is to engage in phenomenological analysis *prior to, or in the course of*, designing a given experiment. Experiments would then serve the function of testing specific questions about functional relationships between stimuli and experiences, but the question of what kinds of experiences we are interested in is itself already the result of *prior phenomenological* analysis that is either carried out by the researcher or gleaned from the literature. Consequently, the resulting experiments are designed to elicit precisely those experiences that have already been identified in the course of a prior analysis. Gallagher (2003) refers to this method as "front-loaded phenomenology" and explains it as follows: "The idea is to front load phenomenological insights into the design of experiments, that is, to allow the insights developed in phenomenological analyses ... to inform the way experiments are set up" (Gallagher 2003, p. 91; see also Gallagher and Sorensen 2006 and Käufer and Chemero 2013).⁴

I would like to suggest that this is indeed the sense in which Gestalt psychological (but not only Gestalt psychological!) research may be considered to be employing a phenomenological method. While the laws of Gestalt perception are the result of experimental research, Wertheimer and his colleagues already knew what they were looking for (the laws governing the formation and thresholds of specific Gestalts). They knew this based on a phenomenological analysis of their own conscious experience. And this informed the ways in which they set up their experiments. For example, if we think of the well-known story of how Wertheimer discovered the phi-effect (the effect of seeing movement when two lights are flashed in short succession), this was not on the basis of the phenomenological analysis of his subjects. Rather, he encountered a situation where he observed this phenomenon in himself and then set up an experiment that was carefully designed to elicit that perceptual phenomenon in his subjects and to investigate the precise conditions under which it occurs (see Ash 1995, op. cit.). Similarly, in other cases of Gestalt psychological research, I argue, it was the

⁴ Gallagher (2003) uses the term "front-loaded" whereas Gallagher and Sorensen (2006) speak of "front-loading" phenomenology. Here I use the two expressions interchangeably.

researchers, not their subjects, that were engaging in (frontloading) phenomenology, which in turn informed the kinds of experiments they conducted.

My claim that frontloading phenomenology can ensure more rigorous experiments and more reliable experimental data does not elucidate what precisely is the point of conducting such experiments. This question will be taken up in the following with a focus on the question of what exactly Gestalt psychologists were hoping to achieve by means of their (frontloading) phenomenology, and how their goals were different from those of other programs of experimental psychophysics at the time.

6 Aim and methods of (Gestalt) psychological research

One instructive place to turn when seeking more information about the means and ends of experimental research in Gestalt psychology in its relation to issues surrounding introspection and phenomenology is Koffka's 1923 paper "Zur Theorie der Erlebnis-Wahrnehmung."⁵ At first glance, this paper appears to be addressing the classical question of how to ensure that experimental subjects give veridical answers about the phenomenology of their experiences. This is indeed the question that other psychologists and philosophers had raised (and are raising to this day) with respect to methods of introspection in the past (see Feest 2012). However, Koffka quickly shifts gears to argue (effectively) that this question is ill-posed, since the kinds of experimental situations subjects are in are already highly constructed in order to prompt specific phenomenal experiences, and subjects are given very specific instructions to elicit exactly the kinds of verbal response experimenters are interested in. Put in the terms I have developed earlier, we can paraphrase Koffka here as saying that while we are interested in subjects' phenomenal experiences, the kinds of experiences they are likely to have in experiments are already severely limited by experimental stimuli and instructions, and this is precisely why we can get robust results. For Koffka, this highlights that experimentally constructed experiences, and the experiential reports they give rise to, are highly artificial (see also Feest 2014). Notice that this is compatible with my analysis, according to which Gestalt psychological experiments were designed such that the experimental subjects did not have much room to reflect on their perceptual experiences.

Koffka does not regard this artificiality as a problem, however, but instead makes the following two points: First, experimentally created data are always artificial. They are constructed relative to a given scientific purpose. This raises the question of what the purpose of perceptual research is, or ought to be. Koffka addresses this question with his second point, where he remarks on the fact that the subjective experience of a given stimulus configuration can vary significantly, depending on what feature of the configuration we are asked to focus our attention on. With this remark he addresses the worry that experimental instructions might alter the very experiences under investigation.⁶ Koffka agrees that the stimulus material alone does not determine what kind of

⁵ Roughly: "On the theory of the perception of experiences".

⁶ Within the history of psychology, this worry had been prominently voiced by Brentano (1973/1874), who pointed out that any experimental instruction to pay attention to an experience necessarily changes that experience.

experience will be had. For example: “I can design a psychological experiment in such a way that, depending on attentional direction, one object that is moving across a field, or two objects, each of which only move across a part of the field” (Koffka 1923, p. 383, my translation⁷). However, he does not see this as a threat to experimental studies in perceptual research. In fact, he views the very concern that experimental instructions or designs might meddle with the “true” experience as relying on a flawed premise, namely that there *are* true/unadulterated experiences, independently of context, and that it is the job of perceptual psychology to uncover them. It is easy to see that this assumption is precisely the premise of the structuralist research program with its aim of uncovering basic sensational elements. This premise about the aims of research was emphatically not shared by Gestalt psychologists. On the contrary, the aim of Gestalt psychological empirical research was to determine lawlike relationships of how what is experienced depends on (a) features of the environment, and (b) features of our attention. From this perspective, then, the fact that phenomenal experiences can be altered through variations in stimuli and instructions is not an obstacle for perceptual research, but rather *the very subject matter* of research. Gestalt psychological research thus exploits the exact feature under investigation, namely the fact that *sensory consciousness is systematically responsive to environmental structures and to attentional focus*.

Summarizing the above, we may say that from the perspective of Gestalt psychology the point of perceptual research was to investigate how phenomenal experience is functionally embedded in the environment, exemplified by experimental designs and instructions. In investigating this embeddedness, researchers artificially create effects that instantiate precisely the types of functional dependencies they are interested in. This interpretation is also supported by the phenomenologist Aron Gurwitsch (a younger contemporary of the Berlin-Frankfurt Gestalt psychologists), who writes that for Gestalt psychology “[p]erception is considered as a function of two variables, x_e and x_i , such that $P = F(x_e, x_i)$, where x_e signifies the external conditions and x_i the internal conditions of perception” (Gurwitsch 1966, p. 22).⁸ Gurwitsch directly relates this to the rejection of introspection, understood as a privileged mode of (inner) perception: “In a ‘functionalistic conception there is no place for privileged perceptions. All perceptions are on the same footing” (ibid. p. 23). It is worth unpacking what this means, precisely: The notion that there are functional relationships between external and internal conditions of perception on the one hand and the perceptions themselves may (at first glance) seem to suggest between “inner” and “outer”. However, the very idea of a strict functional dependency calls into question the very idea of a privileged “internal” perspective, which some have taken to be a hallmark of introspection, because it suggests that all experiences are directly responsive to stimulus conditions and experimental instructions. This means that reporting one’s experience simply requires being attentive to those (external) stimulus-conditions and instruc-

⁷ “Ich kann es im psychologischen Versuch so einrichten, daß ich je nach der Aufmerksamkeitsrichtung ein über ein bestimmtes Feld bewegtes Objekt oder zwei Objekte sehe, von denen sich jedes nur über einen Teil des Feldes bewegt”.

⁸ While this article was published in a collected volume in 1966, it is based on a lecture Gurwitsch gave in 1934.

tions. No additional “inward” looking is required.⁹ This explains, once again, why a phenomenological approach need not be introspective.¹⁰

But if Gestalt psychology, as stated above, investigates how experiential phenomena are functionally embedded in the environment, and if it does so by means of experimental designs and instructions, the follow-up question is what are, from a Gestalt psychological perspective, criteria for *good* experimentally produced experiential phenomena? Koffka’s answer, quite simply, was that good experimentally produced experiential phenomena are those that have clear Gestalts: “In general, good phenomena are ‘gestalty’ phenomena; in contradistinction to mere and-additions and the chaotic” (Koffka 1923, p. 393, my translation¹¹). So, the aim of Gestalt psychological experiments was to trigger phenomenal experiences that gave rise to highly salient and robust discriminatory reactions, where the robustness of the phenomenal experience was judged by the reliability of the experimental results. We have, thus, a rather broad definition of what is—for the purposes of a psychological experiment—a “good” phenomenon, namely any phenomenal experience that robustly gives rise to discriminatory reactions. A “good” experiment, then, is one that reliably evokes the relevant phenomenal experience, thereby giving rise to robust experimental results. And a good experimenter is one that has successfully engaged in frontloading phenomenology, enabling him or her to set up the experiment in question.

I argue that if we abstract away from the language of “Gestalt,” the method pursued by Gestalt psychologists is not so dissimilar from more traditional psychophysical experimentation, pursued in the tradition of Fechner in the nineteenth century and taken up by figures like Boring and Stevens in the twentieth century. First, both aimed at the experimental production of robust discriminatory reactions to experimental situations, i.e., at the investigation of functional dependencies between stimuli and responses. Second, I claim that both presupposed and relied on what I have above called “frontloaded” phenomenology. This assertion may come as a surprise especially with regard to S. S. Stevens, who, after all, wrote that “to experience is, for the purpose of science, to react discriminatively” (Stevens 1935a, p. 521). If we take this assertion at face value, we might suppose that he denied the reality of phenomenal experience altogether. However, this couldn’t be further from the truth. In fact, it is precisely because Stevens took for granted the reality of very specific phenomenal experiences that he was able to ask the question of how their existence could be experimentally demonstrated. Take for example, Stevens’s work on tonal volume and tonal density as a function of frequency and energy. He motivates his research in the following way: “We have, on the one hand, adequate introspective evidence that volume is phenomenally a separate and distinct attribute of tonal stimuli; but, on the other hand, experiments

⁹ The idea expressed here is similar to the notion of the transparency of experience discussed in the more recent philosophical literature about introspection (e.g., Tye 2002).

¹⁰ Without being able to explore this further here, I would like to suggest that we can also see a parallel between the Gestalt psychological and “Husserlian” phenomenological approach here: Husserl’s distinction between a noetic and a noematic stance indicates that a distinction between “inner” and “outer” can take place within the phenomenological mode. (Husserl 1917).

¹¹ “Allgemein: gute Phänomene sind gestaltete Phänomene, von ihnen unterscheiden sich die bloßen, Und-Verbindungen‘und das Chaotische” (Koffka 1923, p. 393).

in which volume has been used as the basis of judgment have not yielded stable quantitative results” (Stevens 1935b, p. 398).

What I take Stevens to be saying here is that within a psychophysical experiment, we want to reliably elicit discriminatory behavior, but that in order to do so, we have to “frontload” a phenomenological analysis of the kind of experience we are interested in (in this case, the phenomenal experience of tonal volume). Only then can we construct an experiment that contains suitable stimuli and experimental instructions. This is precisely the position I have above attributed to the Gestalt psychologists of the Berlin/Frankfurt school. So, Stevens is certainly appealing to his (or our) phenomenology to motivate the search for an experiment that might clearly individuate the phenomenon. Interestingly, Koffka himself uses a very similar example in his demonstration of an experimentally created “Gestalt” in support of a particular experiential quality, namely “vocal character” (brightness or darkness of a tone, in addition to pitch and loudness that more obviously correspond to specific aspects of physical stimuli) (Koffka 1923). This example is intriguing because it is not a classic Gestalt phenomenon.¹² Yet, on his definition, it is a Gestalt, insofar as it occurs in response to a specific stimulus-configuration, rather than in response to a specific stimulus “element”. As we saw, this is precisely the kind of case Boring (1935, cited above) also had in mind when he wrote that the assumption of one-to-one correspondence between “dimensions” of stimuli and sensations had to be abandoned.

The upshot of this discussion is that the kind of perceptual research conducted by Gestalt psychologists like Koffka and Wertheimer on the one hand and American psychophysicists like Boring and Stevens on the other were was much more similar than one might at first suspect: (a) Both held that unique kinds of experience could be elicited on the basis of specific types of stimulus configurations and instructions; (b) both conducted highly controlled experiments in order to elicit experiences that could give rise to robust experimental effects to empirically delineate the phenomena they were interested in; and (c) both did so on the basis of frontloading phenomenology.

7 “Shallow” and “deep” (frontloading) phenomenology

In the previous section I have argued that there is a (perhaps surprising) convergence between the experimental practices of psychophysicists and Gestalt psychologists. But if I am right with my analysis, what are we to make of Koffka’s distinction between shallow, “American” methods and deep, “German” and “phenomenological” methods? Clearly, Koffka saw relevant differences between them. That this unease was mutual comes out in Edwin Boring’s 1933 book, *The Physical Dimensions of Consciousness*. Even though he acknowledged (as we saw in Sect. 4 above) that Gestalt psychological research had dealt a fatal blow to the structuralist program of identifying “elements” of sensation, and even though (as I have just argued) his and Steven’s own project of experimentally identifying “dimensions” of consciousness was methodologically not dissimilar from Gestalt psychological procedures, Boring also made some critical remarks about Gestalt psychologists, to whom he attributed a method of “experimen-

¹² By this I mean here that it cannot be subsumed under the well-known laws of Gestalt perception.

tal phenomenology.” Of this method he said that while it had a place “as a temporary procedure in a young science,” it came with too much “[dangerous] freedom” (Boring 1933, p. 22). In this vein, he stated that “[p]henomenology provides no rigid rubrics for analysis and there lies in it the danger of a chaotic multiplication of descriptive terms and a consequent loss of the systematic integration that is necessary in a satisfactory science” (Boring *op cit.*, pp. 22/3). This statement suggests that, contrary to my above analysis, Boring took Gestalt psychologists to be practicing a kind of reflective phenomenology (as opposed to a frontloading phenomenology). Thus, even if I am right in my contention that there were similarities in the (phenomenological) methods practiced by psychophysicists and Gestalt psychologists, Boring clearly expressed the view that Gestalt psychologists had gone too far. This directly mirrors Koffka’s assertion that some American psychologists had not taken phenomenological methods far enough. In the remainder of this section, I will explicate the nature of this disagreement. I will argue that Boring and Stevens’s version of frontloading phenomenology would have been regarded as shallow by Gestalt psychologists, because the two groups did not agree about the theoretical implications of such an analysis. In turn, Boring’s critique of Gestalt psychological “experimental phenomenology” relied on some misunderstandings of their theoretical and metaphysical commitments.

7.1 Phenomenology and “true” theory

My point of departure here is Koffka’s assertion that “[a] good description of a phenomenon may by itself rule out a number of theories and indicate definite features which a true theory must possess” (Koffka 1935, p. 73). This statement has to be seen before the background of a broader commitment to the value of theorizing in psychology. Thus, Koffka criticizes psychology for blindly collecting facts, while neglecting to put them together in a theoretically meaningful way. In this vein, he writes that while in a certain way a person who knows twenty facts knows more than one who only knows two facts, “in another sense the latter person, if he knows those two items in their intrinsic relation, so that they are no longer two but one with two parts, knows a great deal more than the former” (Koffka 1935, 5). Interestingly, as we saw above, Boring also worried about “a multiplication of descriptive facts.” However, his concern was that it was precisely the Gestaltists’ phenomenological method that would result in such a multiplication, by virtue of providing “no rigid rubrics for analysis” (Boring 1933, *op cit.*). So, clearly, the disagreement cannot simply have been that Gestalt psychology was theory-driven, whereas Boring-style psychophysics merely collected descriptive facts. Both sides argued for the importance of theory. What was at issue, I argue, was a fundamental disagreement over what were appropriate theoretical “rubrics of analysis” and to what extent they could be revealed by an appropriate phenomenological and experimental method. Therefore, the two, even though they did not differ much in their rejection of (what the Gestalt psychologists called) the constancy hypothesis and in their employment of (what I have referred to as) frontloading phenomenology could not see eye to eye with regard to the shape that a theory of perceptual psychology should take.

At the heart of their disagreement lay a difference in opinion as to how radically frontloading phenomenology might shape theory formation. For Koffka, the point of radically frontloading phenomenology was not merely that it would guide experimental data production, but that it would shape the very kind of theory he pursued. By contrast, Boring and Stevens started out with a particular theory (about the dimensionality of consciousness) but did not take their results to inform the notion of *theory* as such. Koffka and the Gestalt psychologists employed frontloading phenomenology in a “deeper” sense as informing their very ideas about the “true” theory, needed not just in psychology, but in other sciences as well.

In a nutshell Koffka argued that since the basic units of our phenomenal experience are always structured and dynamic (i.e., “gestalts”), the notion of a (structured and dynamic) gestalt should also be the basic rubric of scientific analysis. Differently put, he argued that not only the identification of the *explanandum*, but also the formulation of the *explanatory categories* of psychological theories needed to be fundamentally infused with the insights of frontloaded phenomenological analysis. For him, such an analysis would reveal that what was required was “a thoroughly dynamic theory in which the processes organize themselves under the prevailing dynamic and constraining conditions” (Koffka 1935, p. 105). Clearly, what he was envisioning here was something like a dynamic systems account of mental/neurophysiological processes. Importantly, for him the relevant dynamic system was the brain. In the following, I will briefly outline similarities and differences between Gestalt psychology and Boring-style psychophysics with regard to this issue, highlighting in particular a few misunderstandings on the part of Boring.

7.2 Psychophysical isomorphism and parallelism

In his 1935 book, Koffka explicitly emphasized that the dynamic processes in question were ultimately *neural* processes, and that these processes were expected to be “isomorphic” with experienced structures. It was on the basis of the assumption of *isomorphism* that he was able to argue that “psychological observations” could be used “as material for a physiological theory” (Koffka 1935, p. 61). In other words, he envisioned that the “true” theory of psychology was ultimately going to be a physiological theory. This is an intriguing proposal, which points to another parallel between the Gestaltist approach and that of Edwin Boring, as Boring, in his 1933 book, *The Physical Dimensions of Consciousness*, had endorsed what he called a “relational theory of consciousness” (222), which, for him, was firmly rooted in the idea that the nervous system is a relational organ, which allows for reaction, response, reflex, discrimination, and differentiation. “Psychophysiological events,” Boring states, “constitute dynamic wholes” (Boring 1935, p. 230), and the dynamics in question were neural dynamics (hence the title of his book as referring to *physical* dimensions). Given these parallels in outlook, one might have expected Boring to be sympathetic to the notion of isomorphism articulated by Gestalt psychology. In the following I will spell out why he was not. My analysis will bring to the fore that Boring (a) misunderstood what Gestalt psychologists meant by “isomorphism” and (b) conflated phenomenological analysis with introspection, incorrectly charging Gestalt psychology with the latter and thereby

failing to acknowledge that his own approach relied on (frontloaded) phenomenological analysis as well. I will also argue that (c) at the root of Boring's construal of Gestalt psychology was his rejection of what he called "parallelism."

There is some agreement in the literature that the Gestalt-psychological notion of isomorphism can be traced back to some remarks Wertheimer made in 1911, in relation to his experiments of the phi-phenomenon (Koffka 1935; Luchins and Luchins 2015/1999). For Wertheimer, these experiments not only showed that a sensationalist atomism in psychology had to be false, but also suggested that an atomistic neurophysiological theory of sensory processing had to be rejected along with it. As Koffka explains it, such an atomistic theory assumes that neurophysiological processes are simple and separable, and that basic sensations are therefore localizable. His point is that once we understand that there are no atomistic sensations, it follows that there also cannot be corresponding atomistic processes in the brain. It is in this sense, then, that frontloaded phenomenology of motion perception (as underwritten by the experimental results of the phi-phenomenon) has implications for neurophysiological theory. As Luchins and Luchins (2015/1999) show, Wertheimer and Köhler subsequently used the expression "isomorphism" in slightly different (though not incompatible) ways: It was only Köhler who explicitly explored the idea of an isomorphism between experiential structure and cortical organization (Luchins and Luchins 2015/1999, p. 76 ff.), whereas for Wertheimer, the isomorphic relationship "between organization of the phenomenal field and that of the geographical field" was more central (Luchins and Luchins 1935, p. 77). Hence, it seems that in the work of Gestalt psychologists, the notion of a psychophysical isomorphism referred to structural similarities both between phenomenal consciousness and cortical organization and between phenomenal organization and the geographical environment.

As it turns out, the notion of isomorphism was a key point of disagreement between Gestalt psychologists and Edwin Boring, as is evidenced by a letter from Max Wertheimer to Wolfgang Köhler.¹³ In it, Wertheimer reports having received a copy of Boring's 1935 article "The Relation of Attributes of Sensation to the Dimensions of the Stimulus" along with a letter from Boring, from which Wertheimer quotes extensively. In that letter, Boring states that he wrote this article "with you and Koffka in mind". A bit further down, Boring is quoted as saying: "I am bothered about isomorphism. I do not believe that it is right. I have now read Koffka's account, his argument for it, and I am even more doubtful. ... What I mean is that isomorphism is a constancy hypothesis and is held for the same reasons that support the conventional peripheral constancy hypothesis: viz, authority, tradition, inertia of thought, ignorance of fact".¹⁴ So, Boring claims that the thesis of isomorphism is due to precisely the error that Gestalt psychologists had put their finger on, i.e., the error of assuming there to be a one-to-one relationship between stimuli and experience on the one hand and experience and brain organization on the other. This is clearly a misunderstanding of what Gestalt psychologists meant when they rejected the constancy hypothesis. They did not reject that there were structural similarities between the physical world and

¹³ The letter is undated, but since it contains reference to a letter from Boring, containing an article from 1935, it is a fair guess that it was written in, or shortly after, 1935.

¹⁴ Letter from Wertheimer to Köhler, op cit. Source: New York Public Library.

experience. What they rejected, rather, was the assumption that experiences are composed of simple sensations that stand in a one-to-one correspondence with elements of the stimulus. In this vein, Wertheimer utters some surprise about Boring's verdict. He writes: "This is quite strange. He obviously misunderstands what we mean by isomorphism. ... Well, the most simple answer would be ... that isomorphism in our sense does not refer to a one-to-one relation between parts, but to a correspondence between holistic properties".¹⁵ Luchins and Luchins (2015/1999) come to a similar verdict (Luchins and Luchins 2015/1999, p. 90).¹⁶

To get back to the question with which we started this section: How was Boring's rejection of the isomorphism-thesis compatible with his stated aim to formulate a physical theory of the dimensions of consciousness? Boring provides two answers. One has to do with the state of research at the time; the other reflects a more fundamental philosophical commitment (and perhaps misunderstanding of Gestalt psychological commitments). With regard to the former, Boring points out that "[t]he physiologists said that the brain is, in general, a net-work of connections, not a field where dynamic forces, such as the Gestalt psychologists find in perception, can exist. The future will decide that point" (Boring 1942, p. 90). Differently put, then, while Boring was firmly committed to the idea that experiential dimensions were physiological dimensions, he regarded it an open question what an adequate physiological theory of these "dimensions" would ultimately look like. It seems, however, that a lot hangs on how to read the notion of a "correspondence between holistic properties" (Wertheimer, *op cit*). For example, Wagemans et al. (2012) suggest that Köhler had in mind a relationship of resemblance but argue that "[b]y this he meant functional instead of geometrical similarity indicating that brain processes do not take the form of the perceived objects themselves" (Wagemans et al. 2012, p. 1176). If this is indeed all that was intended, Boring might have been less skeptical of the isomorphism hypothesis.

However, it is quite possible that this would have made no difference to Boring as I would like to suggest that for Boring there was a deeper philosophical issue at stake, in that he suspected Gestalt psychologists to be committed to nineteenth-century parallelism, according to which there are two equally permissible perspectives on the mind/brain: an introspective one and a physiological one. This view was an explicit target of his 1933 book. I suggest that he saw two problems with this, one ontological and one epistemological. Ontologically speaking, for him, the very notions of parallelism and isomorphism smacked of dualism.¹⁷ Epistemologically, he objected

¹⁵ "Nun, das ist wunderbar. Er misverstehet [sic] wohl, was wir mit Isomorphismen meinen ... Nun, die einfachste Antwort waere ... dass sich die Isomorphie nicht bezieht auf eine one to one relation hinsichtlich von Stucken, sondern auf Entsprechung von Ganzeigenschaften" (Letter from Wertheimer to Köhler 1935. Source: New York Public Library Archive).

¹⁶ Luchins and Luchins also point out that Boring's is in fact the standard mathematical notion: "[I]n mathematics an isomorphism between two systems requires a one-to-one correspondence between their elements (that is, each element of one system corresponds to one and only one element of the other system, and conversely), which also preserves structures" (Luchins and Luchins 2015/1999, p. 70).

¹⁷ See Heidelberger (2003) for a discussion of this tradition. The term "parallelism" indeed might give rise to this view, since it seems to suggest two sets of independent properties—one mental one physical—that happen to be in perfect harmony. However, I would argue that what 19th-century writers like Fechner had in mind is better described as a dual-aspect theory that tries to stay neutral with respect to metaphysical issues.

to the idea that the “introspective” viewpoint enjoyed some kind of epistemic standing (or even priority), arguing instead that consciousness be regarded as inferred from behavior. In this spirit, he contrasted his own view with “introspectional psychology, both the Gestalt branch and the ‘existential’ [Titchener’s] branch,” which “regards experience as given in its own right within psychology. Phenomenal experience stands on its own feet” (Boring 1933, p. 223). For Boring, having phenomenal experience “stand on its own feet,” amounted to positing units of experience as discrete and separate from the brain. “The most striking thing about consciousness,” he argues, “is its continual flux and change. It is not split up into elements or Gestalten but is truly as continuous as a stream with eddies and currents and pools. I never perceive any phenomenon as fixed” (Boring 1933, p. 227).

I argue that Boring is misconstruing Gestalt psychology here. But more importantly, he is also misconstruing his own method. First, as I have argued above, it was a central component of the Gestalt approach that they did not regard “gestalten” as fixed, but rather saw them as dynamically responsive to environmental features. Secondly, and relatedly, they did not endorse the kind of (inward-looking) introspectionism Boring is criticizing here, precisely because, while they did indeed regard a central epistemic status to phenomenal experience, they regarded experience as functionally embedded in the environment. In this regard, then, these passages once again underwrite my contention that the positions of Boring on the one hand and Gestalt psychologists on the other were not as dissimilar as Boring made them out to be. But this discussion also highlights an important point about what I have called “frontloading phenomenology.” By emphasizing that conscious experience can only be inferred from experimental data, Boring effectively denies the role of the experimenter in setting up the experiments that generated the data in the first place. In order to set up such experiments, I have argued in this paper, both Gestalt psychologists and more traditional psychophysicists such as Boring and Stevens, had to rely on prior phenomenological analysis of the kind that I call “frontloading phenomenology.” By emphasizing the importance of phenomenological analysis, Gestalt psychologists acknowledged this point. By denying it, Boring revealed a blind spot in his own approach.¹⁸

8 Conclusion

In this paper I have raised the question in what sense of the word the experimental method employed by Gestalt psychologists was “phenomenological” and how their approach was related to the phenomenological tradition spearheaded by Edmund Husserl. With regard to the latter question, I highlighted the historical connections between the two movements and argued that members of the Berlin/Frankfurt school of Gestalt psychology shared Husserl’s wariness of the inner/outer distinction (and hence of introspection) as well as his contention that experienced phenomena should be taken on their own terms. With regard to the former question, I distinguished between two notions of “phenomenological”: one in which the experimental subject

¹⁸ I would like to thank Alistair Isaac for suggesting this to me. Isaac (2017) makes a related point when he argues that Boring and Stevens import substantive—if unacknowledged—assumptions about their subject matter into their experiments.

is prompted to engage in reflections on their experience and one in which the phenomenological work is “frontloaded,” i.e., done prior to the experiment. I argued that members of the Berlin/Frankfurt school Gestalt psychology should be regarded as having practiced something more like the latter. However, I also emphasized that in their actual experiments, the Gestalt psychological approach was pretty straightforwardly psychophysical, insofar as their protocols were very tightly controlled and aimed for robust experimental effects.

I backed up my thesis about the psychophysical character of Gestalt psychological research by looking at the relationship between the perceptual research of Edwin Boring and S.S. Stevens on the one hand, and Koffka on the other. As it turns out, not only did the two projects have some surprising commonalities, but the two groups also explicitly, and critically, engaged with each other’s work. With regard to commonalities I argued that psychophysicists like Boring and Stevens also engaged in what one might refer to as “frontloading phenomenology,” in that they drew on their own phenomenal experience to identify the objects of their research, though they failed to acknowledge this.

Despite the fact that both groups of scientists employed a kind of frontloading phenomenology, and despite the fact both were situated within a psychophysical paradigm, they were critical of one another: As I laid out, Boring charged that “experimental phenomenology” could not provide rigorous rubrics of analysis. Conversely, Koffka charged (though without mentioning any names) that mainstream psychophysics had thrown out the phenomenological baby with the introspectionist bathwater. I showed that Boring’s negative verdict resulted from a (mis)understanding of “phenomenology” as being based on introspection (in the sense of a free-wheeling inward-looking analysis), which in turn he took to be deeply entangled with mind/body-dualism. Conversely, Koffka’s critique of the “shallowness” of mainstream psychophysics was informed by the idea that phenomenological analysis should inform not just the analysis of the explanandum, but also the construction of a psychological/neurophysiological theory. I referred to this commitment as “deep” frontloading phenomenology. Briefly, for members of the Berlin/Frankfurt school of Gestalt psychology, the structured nature of experience was a dynamic function of both external stimuli and attentional state, and moreover, the structuring of this experience was very likely mirrored in the dynamic behavior of the brain, a fact they referred to as “isomorphism.” The fact that Gestalt psychologists used the term “isomorphism” in a non-standard way may have contributed to Boring’s misunderstanding.

In conclusion, let me add a few words about the scope of my argument in this paper. Given my focus on particular proponents of both Gestalt psychology and psychophysics, the question may be raised whether my analysis can be extended to other approaches of Gestalt psychology, psychophysics, and phenomenology respectively. Concerning Gestalt psychology, for example, it needs to be emphasized that the Berlin/Frankfurt school was historically not the only Gestalt psychological movement, to be contrasted, for example, with the Graz school of Meinong and Bernussi (see, for example, Albertazzi et al. 2001). I would suggest that the specific convergence between the phenomenological and the psychophysical tradition in the Berlin/Frankfurt school accounts for the features I have highlighted here, making it unlikely that we will see them in other Gestalt psychological traditions. Likewise, as pointed by Chirimuuta

(2014; cited above), current psychophysical research differs in the degree to which it prompts subjects to reflect on the experiences prompted by experimental stimuli. Thus, clearly, not all phenomenology in current psychophysics is “frontloaded.” Lastly, I certainly do not mean to deny the rich and ongoing work in naturalized and experimental phenomenology that has developed in the last couple of decades (see, for example, Gallagher and Schmicking 2010). My argument in this paper pertains to one particular current in twentieth-century experimental psychology. Looking at more recent research in experimental phenomenology will have to wait for another article.

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