

Why the “stimulus-error” did not go away

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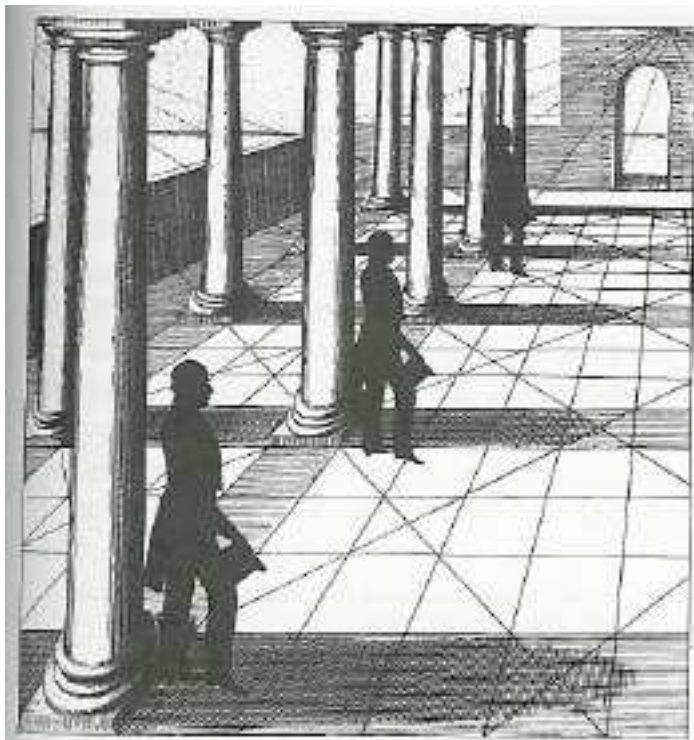
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Abstract:

Psychologists in the early years of the discipline were much concerned with the *stimulus-error*. Roughly, this is the problem encountered in introspective experiments when subjects are liable to frame their perceptual reports in terms of what they know of the stimulus, instead of just drawing on their perceptual experiences as they are supposedly felt. “Introspectionist” psychologist E. B. Titchener and his student E. G. Boring both argued in the early 20th century that the stimulus-error is a serious methodological pit-fall. While many of the theoretical suppositions motivating Titchener and Boring have been unfashionable since the rise of behaviourism, the stimulus-error brings our attention to one matter of perennial importance to psychophysics and the psychology of perception. This is the fact that subjects are liable to give different kinds of perceptual reports in response to the same stimulus. I discuss attempts to control for variable reports in recent experimental work on colour and lightness constancy, and the disputes that have arisen over which kinds of reports are legitimate. Some contemporary psychologists do warn us against a stimulus-error, even though they do not use this terminology. I argue that concern over the stimulus-error is diagnostic of psychologists’ deep theoretical commitments, such as their conception of sensation, or their demarcation of perception from cognition. I conclude by discussing the relevance of this debate to current philosophy of perception.

1. Introducing the Stimulus Error



-----Figure 1 near here-----

Figure 1 – Silhouette men wearing frock coats in perspective drawing/complex arrangements of black and white shapes, lines and dots.

When you view this figure, do you see a drawing of three men of roughly equal height, or, do you have sensations of three black irregular shapes, each of different sizes? If asked to report on your visual experience, how would you be inclined to respond? Such questions as these were much on the minds of psychologists in the first decades of the 20th century. In particular, the influential British born psychologist, E. B. Titchener urged that the first kind of response is illicit for the purposes of studying the mind because it commits the *stimulus-error*—roughly, the mistake framing perceptual reports in terms of what one knows of the stimulus, instead of just drawing on the perceptual experiences as they are felt.¹

¹ According to Boring (1921, 451), “[w]e commit the stimulus-error if we base our psychological reports upon objects rather than upon the mental material itself, or if, in the psycho-physical experiment, we make judgments of the stimulus and not judgments of sensation.” On this definition, it is the *subject* of experiments who is guilty of error, depending on whether he or she formulates reports in terms of objects and their properties rather than sensations themselves. But as Feest (forthcoming) has pointed out, an alternative way of thinking about the stimulus-error is as one which experimenters themselves succumb to when they mistakenly assume that

Though Titchener's brand of psychology fell into obscurity as the century progressed, and the term "stimulus-error" is no longer in use, the significance of the stimulus-error did not die away. The stimulus-error brings our attention to an issue of perennial importance to psychophysics and the psychology of perception: the fact that datasets are liable to have unwanted variability due to these different ways subjects may respond to the stimuli. It leads psychologists to consider if and how this variation can be controlled through training of subjects and the use of experimental instructions. Below I will compare Boring's presentation of the problem of unwanted variability in haptic perception (Section 3) to more recent findings of response variability in colour constancy experiments (Section 4).

I will discuss how psychologists then and now have dealt with the fact that subjects can give such different reports on the same stimulus—framed either in terms of ordinary perceptual objects (*object-responses*), putative raw sensations (*sensation-responses*) or, as we will see in the next section, hypothetical "mental elements". The philosophical interest of this topic comes about because the way that psychologists decide which of the different kinds of responses are valid or illicit is shaped by their prior theoretical commitments and cannot simply be decided by experiment. My central claim in this paper is that psychologists' stances towards response variability are theory driven. In the Section 2 I will show that Titchener's characterisation of object-responses as guilty of the stimulus-error is motivated by his theoretical suppositions, in particular the characterization of psychology as the measurement of purely mental phenomena.

In Section 5 I examine recent debates about how best to interpret data from colour and lightness constancy experiments. I argue that even though psychologists today do not talk of the "stimulus-error", some of them do dismiss responses informed by subjects' knowledge of the stimuli as being "cognitive" and "non-perceptual". This has interesting parallels with Titchener's injunction against the stimulus-error, even though the theory motivating contemporary psychologists, and their conceptions of perception and other mental activities, are clearly different from his. I will argue that attitudes towards the different kinds of responses are diagnostic of contemporary psychologists' deep theoretical commitments regarding the relationship between perception and cognition, and that the topic has implications for how philosophers of perception appeal to introspective evidence and scientific models. In short, philosophical

subjects' reports are indicative of sensory experiences themselves, while in fact they are descriptions of the objects of experiences. Both formulations of the error occur in Titchener's writings but we will be concerned primarily with the former one.

analysis of response variability and the stimulus-error sheds light on on-going controversies about how best to measure perceptual phenomena and reveals some of the conceptual fault-lines within perceptual psychology past and present.

2. Introspection and the “Stimulus-Error”

In the late nineteenth and early twentieth centuries the newly institutionalised science of psychology was attempting to establish itself on terms equivalent to those of the physical sciences. For psychologists like W. Wundt and E. B. Titchener, who have retrospectively come to be known as the “introspectionists”², this meant that it was imperative to be explicit about the observational methods and experimental objects of the discipline. To summarise Titchener’s approach in a nutshell, the objects of psychology were declared to be the contents of the mind, and the scientific tool required for observing those contents was the highly trained introspective observer. Introspection was conceived as psychology’s proprietary form of scientific observation: “the right way to approach the study of psychological method [i.e. introspection] is to assume that it is, in all essentials, identical with the observational procedure of the natural sciences” (Titchener 1912:487).

Inspired by the success of the physical sciences in discovering the elements of matter, Titchener’s life’s work was to discover the elementary sensations that constitute the mind.³ The standard protocol in Titchener’s laboratory was to present trained individuals (typically his students) with novel stimuli (these could be visual, auditory, olfactory, etc.) and have them report their occurrent sensory experience. For example, in one kind of experiment discussed by Schwitzgebel (2011:75-80), two auditory tones of different frequency would be sounded together, and subjects were trained to observe a third tone, arising because of non-linearities of the ear, which is lower and quieter than the other two.

² See Danziger (1980). Titchener himself called his school of psychology “structuralist” (Evans 1984), where “structural” is contrasted with “functional” psychology (Titchener 1899). “Introspectionism” was a dismissive term invented later by behaviourist rivals. For the purposes of this brief survey I take Titchener as *the* representative of structuralist/introspectionist psychology. His school was certainly the most influential in the USA. But note that the activity of similar research programmes in Germany led to the imageless thought controversy, a dispute over introspectible qualities which damaged the later reputation of structuralism.

³ The “first object of the psychologist” is to “ascertain the nature and number of the mental elements” (Titchener 1896, p. 13) and so the psychologist “takes up mental experiences, bit by bit, dividing and subdividing, until the division can go no further. When that point is reached, he has found a conscious element” (Titchener 1896, p. 13).

“The psychologist arranges the mental elements precisely as the chemist classifies his elementary substances” (Titchener 1926, p. 49). See Beenfeldt (2013, chap. 4) for discussion.

Unlike the “simple ideas” of the British associationists (to whom Titchener was partly indebted [Beenfeldt, 2013]), Titchener’s elementary sensations could not ordinarily be observed, since they always made their appearance in compounds held together by the laws of psychological association (Titchener 1899:294). But with introspective training, an experimenter could learn to decompose the compound and isolate the individual sensation elements. On one published tally, over 40,000 mental elements had so far been isolated (Titchener 1896, p. 67).

In Titchener’s laboratory, introspectors were trained not to report on the object causing their experience, but rather on the purely mental sensations elicited because, as Titchener (1912:489) contends, the task of the psychology is to describe consciousness itself, not the physical stimuli which affect it, or the meanings which we attach to it. Thus reports which are influenced by the subjects’ knowledge of the stimulus are of no value to the psychologist. It is precisely this theoretical conception of the psychological method as aiming to recover purely mental elements which motivates the structuralists’ injunctions against the stimulus-error.⁴

In his magnum opus, *Experimental Psychology: a manual of laboratory practice*, Titchener warns against the “R-error” where “R” stands for *Reiz* (German for “stimulus”), while the English term “stimulus-error” is found at Titchener (1909, 145) along with some helpful examples:

When a student begins work in the psychological laboratory.... he is very likely to fall into what we term, technically, the stimulus-error. He is instructed to attend to sensation, but in reality he attends to stimulus. Instead of comparing two noise-intensities, he will compare the imagined heights from which the balls fall that give the noise-sensations; and, in general, he will concern himself not with greys but with grey papers, not with kinæsthetic sensations but with weights, not with visual magnitude but with the size of objects.

The task of structuralist psychology would be straightforward if sensory reports were the default or naïve ones, the sort that people tend to make independently of knowledge of a theoretical science. Avoiding the stimulus-error would then just be a question of recovering the natural or untutored attitude to one’s own experience. But, as Titchener writes here, the common sense approach to experience leaves people with a tendency to make stimulus reports. Careful training in introspective technique was required for all participants in his lab

⁴ It is worth mentioning that the issue of the potentially problematic conflation of sensory and stimulus reports first came to psychologists’ notice in the 1880’s and 90’s with the “quantity objection” to Fechner’s psychophysical methods (see Boring 1921:451-455; Heidelberger 2004:210).

because without it, Titchener (1920a, p. 23) writes, the observer is “warped and [biased] by common sense”. What structuralist psychology needs as data are reports of experience which are somehow uninterpreted—not informed by the meanings that we habitually attach to experiences (Titchener 1899:291).

However, Titchener’s assertions provoke concerns about what we would now call the “theory ladenness” of observation. In his laboratory, experimental subjects were usually junior scientists who were themselves immersed in the structuralist theory. So those making introspective observations were not ignorant of the theoretical import of the experiments. It is conceivable that theoretical expectations could have a biasing influence on the contents of sensory experience, just as Hanson (1958) and Kuhn (1962) famously, and controversially, argue occurs in the physics laboratory.⁵

It is interesting that Titchener (1899) accuses fellow psychologists of theory-laden observation—what he calls, “introspection through the glass of meaning”. He illustrates his case by discussing some published “absurdities” in the observation of musical intervals:

Such statements are palpably in conflict with fact; but I do not doubt that Herbart and Volkman made them ‘on the ground of introspection.’ Yes! They were introspecting, not the Is, but a logical Should-reason-ably-be; the theory was ready before introspection began and when the time came for introspection an idea representative of the octave or fifth or second, a logical meaning, stood in the path of direct vision, and they saw crookedly. (Titchener, 1899:291-292)

Even if the existence of theory laden observation in the physical sciences is disputed (see Fodor 1984, Votsis 2015), it is easy to show that it is a more pressing worry for structuralist psychology. For one thing, the process of training in introspective methods seems to have some impact on the features of sensory experiences (Schwitzgebel 2011:81-83).

Furthermore, there is the question of what vocabulary to use to report on one’s sensations. As noted above, uninterpreted sensations are not just there in our untutored experience of the world; the naïve stance itself leads one into the stimulus-error. Thus everyday language, which blends talk of material objects with talk of subjective sensation, would not be an appropriate tool. In order to give acceptable reports on their sensations, subjects were tutored in a technical index of elementary sensations. So the project of structuralist psychology was reliant on the development of a technical vocabulary. Even if one is sceptical that

⁵ As Bogen (2014) puts it, there is a purely perceptual notion of theory laden observation by which “things don’t look the same to observers with different conceptual resources.” Sometimes philosophers of science take the “conceptual resources” to include ideas of common sense as well as scientific theory. In this discussion I only avert to the role of scientific concepts.

knowledge of structuralist theory would directly influence subjects' experiences in the lab, it is still likely that their observation *reports* would be theory laden because necessarily given in terms of a theoretical vocabulary.⁶ The irony is that in their efforts to avoid the stimulus error, the structuralists ended up collecting rather a lot of theory-laden data.

A further point, one which is most central to the argument of this paper, is that Titchener's very characterisation of stimulus reports as the "stimulus-error" was itself driven by theory. According to Beenfeldt (2013), Titchener's research programme was an experimental extension of the British associationist theory of mind, as represented initially by Hobbes, Locke and Hume, and latterly by James and John Stuart Mill. At its core is the empiricist notion of the "sensory given", a raw and uninterpreted layer of sensory experience which is the foundation of our sophisticated thoughts and beliefs about the world. Titchener, unlike Locke, did not believe that the elements of the sensory given were obvious to us, outside of controlled laboratory conditions. Thus there was no simple *observation* of mental elements which initiated the introspective programme; rather the programme takes off from the theoretical *posit*, that such elements must be there, and must be recoverable if we take enough pains. The methodological doctrines, such as the need to avoid the stimulus-error, flow directly from this theoretical assumption (Beenfeldt 2013:52-53). For if the naïve way of reporting is one that makes reference to ordinary physical objects, the motivation for rejecting it and requesting non-obvious sensation reports must be a theoretical one. Without a prior commitment to something like a sensory-given, the structuralists' alignment of ordinary perceptual reports with the stimulus error would be entirely mysterious.

In his recent discussion of the theoretical underpinnings of structuralist psychology, Hatfield (forthcoming, §§2.1-2.2) emphasises Titchener's greater commonality with the German tradition of sensory psychology, represented in particular by Hermann von Helmholtz and Wilhelm Wundt. What they share is a commitment to a stark distinction between elemental sensations and perceptions of objects.⁷ Again, with this theoretical presupposition in place it follows naturally that a central task of psychology is to conduct an analysis to

⁶ This is akin to what Bogen (2014) calls *semantical theory loading*.

⁷ "Titchener echoed the general sort of distinction made by Helmholtz, Wundt, Mach, and Ladd, in distinguishing (a) sensations as elements having the attributes of quality, intensity, temporal duration, and (sometimes) spatial extent – aspects of which one may hope to partially isolate in consciousness – from (b) perceptions compounded from sensations and imbued with meaning by their relation to other sensations" Hatfield (forthcoming, start of §2.2). As Hatfield discusses, Helmholtz held that we typically ignore our sensations but that we can potentially bring them to awareness; they are "unconscious" in the sense of forever being inaccessible to introspective awareness.

recover the elements of mental life, and then study the synthetic process through which meaningful, complex perceptions are obtained. If we fall into the stimulus-error and, as Titchener writes, “are constantly confusing sensations with their stimuli, with their objects, with their meanings” (1905:xxvi, quoted by Boring 1921:450), then we will be unable to examine the putative construction of perceptions out of sensations. To put this the other way round, the injunction against the stimulus-error is entailed by a particular conception of the goal of sensory psychology.

Feest (forthcoming) examines the debate between atomistically inclined psychologists, such as Titchener, and the emerging Gestalt school with its holistic theory of sensory experience. The Gestalt psychologists rejected the assumption that perceptual experiences are composed of simple sensational elements. With this comparison in mind, Feest also argues that what counts as a stimulus-error depends on prior theoretical commitments. From the perspective of Gestalt psychology, Titchener himself fell into the stimulus-error because he supposed that experience is decomposable just because physical stimuli are. This charge against the structuralists comes out clearly in Wertheimer (1922).

To summarise, the central lesson of this section is that the structuralists’ treatment of the stimulus-error was driven by their theoretical commitments. Whether or not one thinks that their observations were theory-laden in the Kuhnian sense—i.e., whether or not one believes that their sensory experiences and reports were directly moulded by their theoretical conception of the mind—it is still clear that what which reports *count* as leading to the stimulus-error depends on theories about the elements of perceptual states.⁸ Thus one might expect that the issue of the stimulus-error simply died when Titchener’s school of psychology, and its peculiar theoretical commitments, fell into disfavour. Yet, as we will see in the remainder of the paper, this was not the case. Understanding why this was so will bring to light some of the important controversies within perceptual psychology.

⁸ This raises the question of whether there can be a theory-neutral definition of the stimulus-error. (I thank an anonymous reviewer for bringing my attention to this question.) In Section 1 I defined the stimulus-error just in terms of a contrast between stimulus-reports and sensation-reports. But now we see that the matter is more complicated, because psychologists’ very conception of sensation-reports is bound to their wider theories of sensation, and the relationship between sensation and perception. For the purposes of this paper we can stick with the first pass, theory neutral definition of the stimulus-error. The fact that how psychologists define sensation-reports turns out to be theory-driven is not especially relevant for the remainder of this paper. The more important point for the later argument is that whether or not psychologists are concerned to rule out stimulus-reports and police against the stimulus error is largely determined by their theoretical commitments.

3. Response Variability and “Equivocal Correlation”

Given the dubious theoretical motivations for the stimulus-error doctrine, one might be tempted to declare that the characterisation of the stimulus-error as an error was itself the problem. Beenfeldt (2013:53), for example, treats the concept of the stimulus-error as something of a dirty trick to render structuralist psychology immune from experimental falsification. However, in raising the issue of the stimulus-error the structuralists identified a recalcitrant problem in perceptual psychology, that of response variability due to subjects’ differing interpretations of their task or different attitudes or ways of attending to the stimulus. The topic of this section is to show how the problem arises whether or not introspective methods are used.

In his 1921 article on the stimulus-error, Boring gives us reason to think that the issue is perennially relevant to perceptual scientists. Put briefly, his point is that whether psychologists like it or not there is no one deterministic relationship between perceptual stimulus and behavioural response. Instead, the relationship (“correlation”) is “equivocal” and dependent on factors internal to the experimental subjects such as attitude toward the stimulus (i.e. the stimulus one or the sensory one), focus of attention, response criterion and judgments made about the nature of the stimulus.

Boring makes the case that the stimulus-error is not exclusively the concern of adherents to the “psychology of datum” (i.e. those exploring the sensory “given” using introspectionist methods), but is also of concern to the “psychology of capacity”⁹. He writes that:

the effect of the "stimulus-error," from the point of view of a psychology of capacity, is....to render the correlations between stimulus and response equivocal and thus to jeopardize the rigor of conclusion that science demands. (Boring 1921, 465-6)¹⁰

⁹ According to Boring (1921:460), “[t]his psychology sees no distinctively mental measurement, but undertakes the physical measurement of bodily response as a function of the physical quantities of the stimulus. There is no sharp epistemological line discernible between this sort of measurement and other physical measurement, and it thus meets the requirement of modern behaviorism that psychology interpenetrate physical science without sensible demarcation. The psychology of capacity is also the psychology of mental tests and of Urban’s psychophysical experiments.” Boring mentions James McKeen Cattell as a leading proponent of the psychology of capacity. For the purposes of this paper I associate the term primarily with behaviourism.

¹⁰ It is worth quoting Boring (1921:462) at greater length: “If we are now to urge upon the psychology of capacity the avoidance of the stimulus-error, it is a fair demand that we state first the probable penalty that is incurred by a failure to accept our advice. Here we can not stand upon the epistemological ground that psychology observes mental processes and not stimuli, and that judgments of stimulus are therefore a priori inadmissible. This historical warning against the stimulus-error does not apply to the psychology of capacity which protests against a scientific dualism and deals by preference with stimulus and response. What we have to show is rather that the stimulus-error works against the establishment of the univocal

In other words, the stimulus-error brings our attention to an unwanted source of response variability---the way that psychophysical thresholds can change depending on whether the subject commits the stimulus error and bases his/her response on an interpretation of the stimulus, or if she employs the attitude favoured by the introspectionists and bases her response according to some kind of raw sensory experience (Boring 421:464-5).

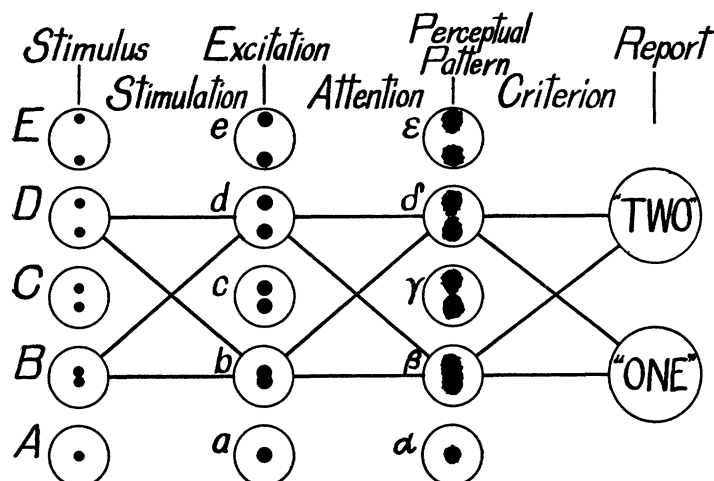
In support of this claim, Boring mentions Martius’s measurements involving estimates of the apparent sizes of objects at different distances (p.462-3), and Friedländer’s experiment on the haptic discrimination of lifted weights (p.464). Martius’s experiments are comparable to size constancy tasks in contemporary psychophysics, where subjects are asked to match the sizes of objects (such as bars or circles) viewed simultaneously at near and far distances. Here subjects’ responses can either be indicative of the relative sizes of the retinal projections made by the objects (hence varying with distance from perceiver) or closer to the actual objects’ sizes (hence invariant with distance from perceiver). According to the old structuralist dogma, the latter kind of response would be illicit because it commits the stimulus-error. But here Boring is commenting more neutrally on the phenomenon of response variability, understanding such response differences as due to subjects’ internal predispositions and states. As we will see below, constancy experiments of various kinds do tend to show the attitudinal effect that Boring describes.

Boring’s final example is the measurement of the tactile two-point threshold—the measurement of the minimum distance between two pressure points on the skin which reliably gives the impression of two separate stimuli. In such an experiment the behaviourist “psychologist of capacity” is only concerned with the stimulus and verbal report. However, Boring observes, the relationship or “correlation” between stimulus and report is variable (“equivocal”) due to differences in the intermediate factors mentioned above – attention, attitude and response criterion.

Figure 2 is Boring’s (1921:p.466 ff.) illustration of this point. The states labelled α - ϵ are supposed to be what the subject actually feels---what the subject in an introspective experiment would be asked to report on, independently of any judgments regarding the original stimulus. In a behavioural experiment, the subject would be asked to report “one” or “two” depending on whether the stimulus feels like a single or double point. However, as Boring notes, the subject’s criterion for determining which of states α - ϵ is to be associated with reports of “one” or “two” is somewhat flexible. A subject may feel a merged,

correlations between stimulus and response that a psychology of capacity demands, that it interferes with the prediction of the response for a given stimulus.”

dumbbell shaped pattern on her skin (β) but report “two” because she knows that this kind of sensation was probably caused by a two point stimulus. This results in the measurement of a lower two-point threshold, in comparison to subjects who report “two” only in cases where they have a sensation of two clearly separated points (ϵ). If all the experimenter records is the initial stimulus and the final verbal report, she is left in the dark about the path from stimulus to report, and hence the ways in which attitude and attention have influenced the subjects’ reports.



-----Figure 2 near here-----

Figure 2 (from Boring 1921, 466).

When two small pressure points are applied close together on the skin, these are felt as one pressure point if the distance between them is very small. For an intermediate range of close together stimuli, an experimental subject may report that they perceive ‘one’ or ‘two’ points, depending on whether they concentrate on the skin sensation, or whether their report is influenced consideration of the likely stimulus.

Boring (1921, 470) concludes that, “the failure to control the attitudinal factor... results perforce in an equivocal determination of these responses, which is nothing more nor less than a “stimulus-error””. His advice to the behaviourists is to “make use of introspective data” (p.471) and to take the same care as the introspectionists typically do, in controlling “by instruction both attention and criterion” (p.469); control of the stimulus alone will not suffice. However, his concerns about the effects of shifting response criteria were in some instances met without resort to these measures. *Signal detection theory* is a set of techniques used by later psychophysicists to estimate the discriminability or detectability of stimuli regardless of the subject’s response bias towards more

conservative or more permissive reports (Green and Swets 1966). In the case of the two-point threshold, the stimulus attitude effectively gives a more permissive report criterion, whereas the sensation attitude leads to a conservative bias. Thus a signal detection analysis would be applicable.

However, signal detection theory only helps to eliminate response variability if the effect of changing attitude is just to modify the relative proportion of correct responses and errors. If observer/observation variability leads to a qualitatively different pattern of responses then this raises another set of theoretical and experimental puzzles, as we will see in the case of colour and lightness constancy.

4. Variable Reports in Constancy Experiments

Colour constancy is often characterised as the stability of colour *appearances* (the hue and saturation that objects appear to have) despite changes in the chromaticity of ambient illumination. However, changes in illumination do cause noticeable changes in colour appearances so it is open to debate whether colour constancy is better characterised as the ability to match coloured *stimuli* across changing illumination. It should be noted that since the fall of structuralism, psychologists studying sensory responses (typically psychophysicists) had not carried on policing against the stimulus-error. However, as Hatfield (forthcoming) relates, during the mid 20th century psychologists studying size and shape perception continued to explore the way that subjects' responses could be shaped by task instructions. In size constancy experiments, some psychologists used response attitude as an independent variable to be manipulated via experimental instruction. The typical finding is that subjects show greater size constancy if prompted to give stimulus reports.¹¹

In the field of colour perception, Arend and Reeves (1986:1743) criticised an earlier study by McCann et al. (1976) on the basis that the instructions given to their subjects was open to either the stimulus or the sensory (“appearance”) interpretation.¹² In their own study, Arend and Reeves gave their subjects two

¹¹ To my knowledge, psychologists after Boring (1921) do not use the terminology of “observational attitudes”. Mid 20th Century psychologists such as Holaday and Carlson employ the terms “intentional set” or “instructional set” instead (see Hatfield (forthcoming), section 3). By the time of the research discussed in this section and the next, psychologists talk of different “conditions”, where task instructions to give sensory/appearance or stimulus responses are an independent variable in the experiment.

¹² “A second problem for constancy interpretation is that the observer's task may have permitted intermixture of two types of perceptual judgment. It is not clear whether subjects were matching hue and saturation or were selecting papers with the same apparent surface color. As the experiments reported below show, this distinction has a major effect on matching data” (Arend and Reeves 1986:1743).

different kinds of instructions: either to match hue and saturation *or* to match stimuli so that they looked as if made from the same colour paper. Arend and Reeves report that in the first task (sensation/appearance reports) subjects showed little colour constancy, whereas for the second task (stimulus reports), subjects showed approximate colour constancy (p.1746).

Since Arend and Reeves' study it is standard practice for psychophysicists to state more precise operationalisations of colour constancy than the simple "matching" idea of McCann and colleagues (see e.g. Troost and de Weert 1991; Cornelissen and Brenner 1995; Foster 2003; Reeves et al 2008; and Smithson 2005 for review). Thus one might conclude that while Boring and Titchener were right to draw attention to the "stimulus-error" as a source of response variability, their strictures against stimulus reports are unfounded; indeed, it appears to be the case that certain perceptual phenomena, like colour constancy, are *better* measured through stimulus reports. What is interesting is that there is still an on-going controversy about whether all such stimulus reports are genuinely *visual* and not, rather, post-perceptual judgements or inferences made by subjects about the likely source of stimulation. According to my analysis, the concern about policing the stimulus-error has resurfaced in recent perceptual psychology and, as I will argue in the next section, researchers' varying opinions as to whether stimulus responses are valid or illicit are indicative of their deep theoretical commitments concerning how perception should be defined and explained. As we saw in the discussion of Titchener above, views about what counts as a stimulus-error are shaped by theory rather than by data. Although psychologists today do not use the term, the stimulus-error has not gone away.

5. Theoretical Perspectives on Stimulus Responses¹³

5.1 "Sensory" vs. "Cognitive" Theories of Colour Constancy

In the introduction to their replication of Arend and Reeves' study, Troost and de Weert (1991:591) point out that there has been a longstanding controversy between those advancing "sensory" and "cognitive"¹⁴ explanations of colour

¹³ A note on terminology: contemporary psychologists tend not to employ a clearly defined *sensation-perception* distinction. While this distinction is relevant to the neo-Helmholtzian views discussed below, in the interests of space I do not dwell on it. The more important distinction for my argument below is between the *visual* and the *non-visual/cognitive*. Sometimes (following a particular psychologist) I refer to the visual experience as "sensory", and sometimes as "perceptual". Nothing much follows from the use of these different terms.

¹⁴ This label is loaded because this word is normally used to describe something apart from purely visual or perceptual processes. Yet advocates of the "cognitive" approach to constancy assert that these inferential steps, etc., occur *within* vision and help explain perception itself, as Troost and de Weert (1991:591) themselves point out: "by *cognition*, we refer to structural

constancy. Sensory accounts propose that constancy is due to adaptational mechanisms at the earliest stages of visual processing. The visual system need not form any representation of the illuminant, and it is assumed that the observer is not aware of the changing lighting conditions. Edwin Land’s (1964) *Retinex* model is an influential version of a sensory account, and this was the theoretical basis for the McCann study.

In contrast, Rock’s (1977) “unconscious inference” explanation of colour constancy is a paradigm case of a “cognitive” account. On this neo-Helmholtzian view, and also the Marrian *inverse optics* accounts¹⁵, it is assumed that the visual system *does* form an explicit (though perhaps unconscious) representation of illuminant chromaticity, and uses this to calculate a value of the surface colour which is stable despite changing illumination. Such inferences are thought to occur at the unconscious, sub-personal level, even though inference and cognition are typically characterised as person level phenomena. Arend and Reeves (1986:1749) present the idea as follows:

hues and saturations might change when the illuminant changes but be perceived to result from constant surface colors and varying illumination..... That is, perfect constancy could still obtain if the viewer, by a perceptual computation, were able to see the paper as an object of the same surface color under illumination perceived to be greener than the direct sunlight.

Note that the underlying computation, and the resulting colour constancy, are categorised as perceptual: one *sees* that the paper has not changed colour as the lights are altered.

In contrast, if researchers’ theoretical starting point is the sensory one, as with McCann et al. (1976), it makes sense that their operationalization of constancy should just be the simple hue and saturation matching task. This is because the sensory account does not allow for perceptions of constancy that are independent of apparent colour sensations. It is assumed that the effect to be measured is just appearance matching, and observers will not even be warned

properties of the visual system that reveal the abstract perceptual organization of a scene, such as implemented decision rules in pattern recognition We certainly do not mean conscious reasoning or willingness to see.”

Troost and de Weert take Hering (1874/1964) and von Kries (1905) to be the originators of the sensory account, while attributing the first formulations of the cognitive approach to Helmholtz (1867/1962), Katz (1911/1935) and Koffka (1935). As it would take us too far away from our main topic, in what follows I will not be discussing the historical versions of these accounts. I will be focusing on the ways that subsequent versions of these theories shape contemporary psychologists’ attitudes towards response variability.

¹⁵ As Pizlo (2001:3146) characterises the inverse optics approach: “*perception is about inferring the properties of the distal stimulus X [i.e. the external physical object] given the proximal stimulus Y [i.e. the pattern of retinal sensation]*”. Marr (1982) is a classic exposition.

against making stimulus matches. This is because the sensory model presupposes at the outset that the relevant perceptual phenomenon is the result of the early visual system working to counteract the effects of the changing illuminant chromaticity. If constancy is perfect, the subject simply will not see any change in surface or illuminant colour. The idea of stimulus matching---with its supposition that constancy *is compatible with* apparent changes in colour---lies beyond the conception of colour constancy that comes with the sensory model.

In principle, both the sensory and “cognitive” mechanisms may be employed by the visual system. Indeed, many researchers now endorse the multi-mechanisms view (Smithson 2005). Unlike McCann et al., Arend and Reeves assume at the outset that stimulus matches are genuine operationalisations of perceptual constancy. They conclude from their finding of weak constancy with the appearance match and better constancy with paper match that the sensory mechanisms “make little contribution to color constancy within a single scene” (1749).

In sum, “cognitive” theories assume that the visual system employs separate representations of object and illuminant, while sensory theories assume there is no recoverable perception of the illuminant. The mere fact that two kinds of constancy responses can be elicited, depending on experimental instructions, would appear to be straightforward evidence for the existence of both mechanisms, and a refutation of the assumption made on the basis of the sensory model, that colour constancy should only be operationalized with hue and saturation appearance matches.

However, there is still an on going debate about whether the stimulus responses measure a genuine perceptual phenomenon at all. This is how Troost and de Weert (1991:595) put the issue. It is worth quoting them at length:

The results of the object-appearance condition must be judged with care. Instead of matching *what one sees* (exact matching), the subjects had to match *what they should see*. Therefore, these results do not point to a difference in sensation but rather to the ability of subjects to separate illuminant and object components and adjust their matches in accordance with their estimations. We think that since, in the object-matching condition, subjects strongly rely on indirect knowledge about objects and illuminants that have to be made explicit, the uncertainty of the matches is higher than in the exact-matching condition. Thus, in our view, the differences in br [Brunswick ratio, a measure of constancy] between the object- and exact-matching conditions are caused by different judgments rather than different sensations. If this is the case, it is simply not allowed

to relate these results to *color constancy as a visual phenomenon only*.
[emphasis added]

Here Troost and de Weert compare and contrast the process behind the sensation-response (“exact matching”) and the stimulus-response (“object matching”). The latter process, they argue, requires “indirect knowledge about objects and illuminants” and cannot be taken as a “visual phenomenon only”. In other words, their analysis rejects the neo-Helmholtzian idea that the object matches can be explained purely by processes of inference and “estimation” within the visual system.

One might object to my analysis here on the grounds that Troost and de Weert are averting to conscious and person level inferences and judgments, and that these would count as post-perceptual even to a committed neo-Helmholtzian. Thus, I have failed to show that Troost and de Weert are ruling out *any* kind of non-sensory constancy mechanism. This objection brings to our attention the fact that there are *two* ways of thinking about the stimulus-responses: the first is to conceive of the inferential steps behind stimulus responses as sub-personal, unconscious, and proprietary to the visual system; the other is to think of them as person level, conscious and post-perceptual cognitive judgments. My point is that Troost and de Weert’s experimental data do not give them empirical grounds for asserting that the stimulus-responses are due to post-perceptual inferences. In these kinds of constancy tasks it is simply unclear what the division of labour is between vision and post-perceptual judgment. So psychologists are forced to appeal to background theory about how much can be expected from a purely visual mechanism.

An example of lightness constancy, shown in Figure 3, helps illustrate how unclear the divide between vision and judgment is, introspectively. The top surface of the chequered solid is perceived to be more strongly illuminated than the side and front facing surfaces. This is obvious because of they way that the grey levels are darker in the side and front surfaces, which appear to be standing in shadow. However, when asked to make a stimulus-response we would still say that the three sides of the top light cube are made of the same light grey material and not intrinsically darker at the sides. It is unclear to me, introspectively, if this is a cognitive assessment of the cube, or if the sameness of the material is presented to me visually. It would certainly be presumptory to rule out a sub-personal inferential explanation without further evidence and investigation. As we will see next, the same dispute over neo-Helmholtzian and post-perceptual explanations of stimulus-responses arises in discussions of lightness constancy.

-----Figure 3 near here-----

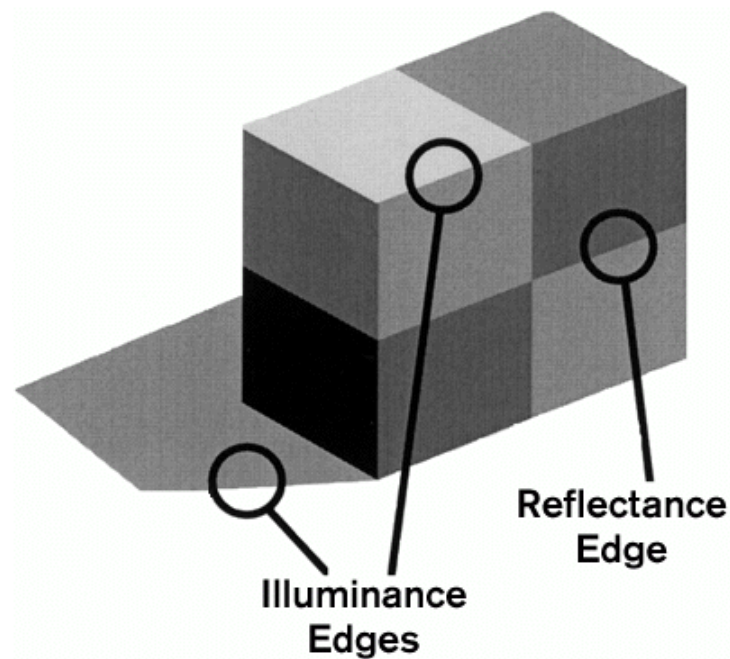
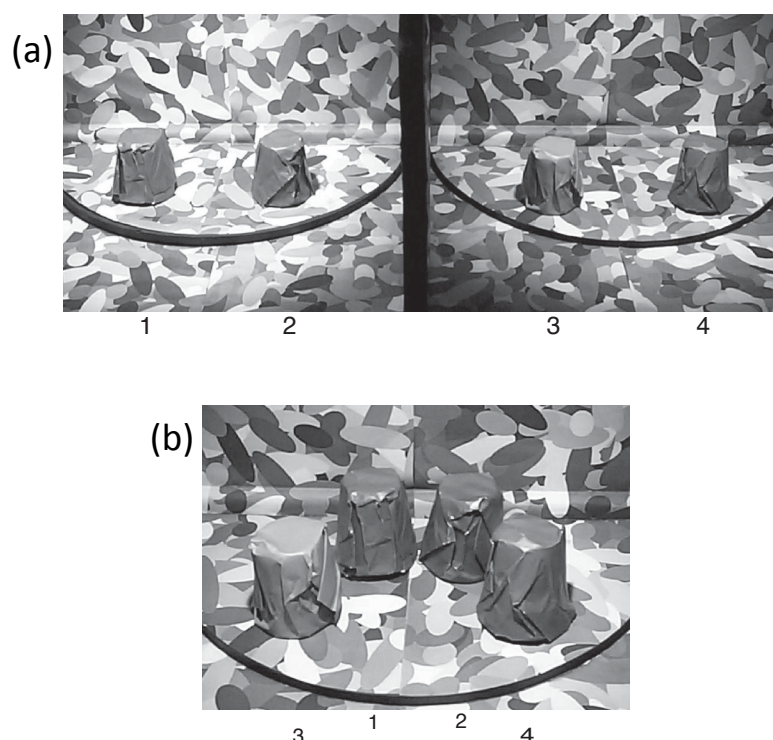


Figure 3: Illustration of lightness constancy. The lighter and darker cubes are each perceived to have a uniform intrinsic grey colour despite the difference in grey pixel values at the top and sides, due to the presence of shadows. Image from: <http://ai.ato.ms/MITECS/Entry/gilchrist.html> by Alan Gilchrist. Permission needed.

5.2 Lightness Constancy and the Return of the Controversy

Robilotto and Zaidi (2004) performed a series of experiments on lightness constancy – the ability to recognise sameness of grey level of a material surface, despite changes in overall illumination level, from dim to bright lighting. Their subjects were required to determine which out of four stimuli presented under two different illumination conditions (Figure 4a) was the odd one out due to a different surface lightness (see Figure 4b for correct answer).



-----Figure 4 near here-----

Figure 4: Stimuli used by Robilotto and Zaidi (2004) in lightness constancy experiment. Observers were asked which of the four cups depicted in (a) has a different lightness (grey level of paper) from the others. (b) shows that the correct answer is cup 3. See AUTHOR for detailed discussion of their methods. [Permission needed]

Even though Robilotto and Zaidi’s task instructions were such as to prompt a stimulus report,¹⁶ significant response variability was still observed. The majority of subjects’ data were consistent with them using a strategy based on appearance comparisons---simply picking the cup that looked most different in grey level from the other three; yet two out of the seven subjects’ data suggested that their responses were based not on the sensory strategy but on a process of estimating the difference in illumination across the two scenes, and then working out the relative grey levels of the stimuli. The latter strategy is the one that *inverse optics* models of lightness constancy propose happens within the visual system at the sub-personal level.

¹⁶ “You will be presented with four pieces of gray crumpled paper. Three of the papers will be of an identical shade of gray. The fourth paper will be a slightly lighter or slightly darker shade of gray. You will be asked to decide which of the four papers is of a DIFFERENT MATERIAL from the other three” (Robilotto and Zaidi 2004:782, emphasis original).

What is striking is that Robilotto and Zaidi's analysis of inter-subject differences (in their own experiment and in discussion of another laboratory's results) is that it rests on a robust distinction between "sensory" and "non-sensory" qualities: "[i]ndividual differences thus are likely to be due to attempts to infer a non-sensory quality, rather than due to the particular task or instruction" (Robilotto and Zaidi, 2004:792). The implication is that when an observer strategy conforms to the predictions of inferential models of vision, doubt should be cast on whether we are really dealing with a visual phenomenon, but instead a matter of post-perceptual judgment or cognition.¹⁷

One reason to share this suspicion regarding reports which go beyond sensory appearances is the finding of *over-constancy*. Robilotto and Zaidi (2004) note that the two subjects following the inverse optics strategy consistently over-estimate the difference in illumination between the two scenes. This suggests to them that subjects are deliberately employing a non-visual strategy during the experiment in order to arrive at the correct answer regarding the properties of the external physical stimulus.¹⁸ Troost and de Weert (1991:596) also found evidence for over-estimation of the illuminant difference when subjects were required to perform paper matches across stimuli presented successively in time. They suggest that this is an effect of memory.

However, this suspicion regarding non-sensory reports is controversial amongst vision researchers. For instance, Robilotto and Zaidi's work can be compared with a lightness constancy experiment published by a different group led by Laurence Maloney (Boyaci et al 2003), where the authors do not mark a distinction between visual (sensory) and inferential reports. Maloney's work is firmly in the inverse optics tradition (Maloney et al. 2005), where visual perception is *defined* as an inferential process. We are not assumed to have conscious access to the uninterpreted sensory input that forms the basis of our perceptual inferences. As I argue at length elsewhere (AUTHOR), there is a difference in theoretical opinion which concerns the question of how to define vision in contrast with judgment or cognition. The dispute cannot straightforwardly be settled by experiments such as the ones I have described, because scientists' theoretical commitments so fundamentally shape experimental design and data interpretation.

To conclude this section, from the recent literature on colour and lightness constancy I have discussed variations in response patterns which are due to

¹⁷ It should be noted that elsewhere Robilotto and Zaidi are explicit in their rejection of inverse optics models of constancy and elsewhere argue that their lightness constancy studies provide evidence against the validity of the inverse optics approach (Robilotto and Zaidi 2006).

¹⁸ As Robilotto and Zaidi (2004:792) write in their discussion, "it is becoming clear that some observers try to do conscious corrections that lead to individual differences".

differing task instructions or differences in subjects’ interpretation of their task. Such findings corroborate Boring’s (1921) analysis, whereby different “attitudes”---that of the stimulus vs. that of sensation---lead to “equivocal correlation” between physical stimulus and subject’s response. Furthermore, we have seen three instances where researchers are skeptical that the data arising from the stimulus attitude really bear on the perceptual phenomenon of constancy. As Troost and de Weert (1991:595-6) summarise:

The results of both the simultaneous- and the successive-object conditions suggest that knowledge about illuminant and objects can strongly influence color judgments; it does not imply that there are differences in sensory data¹⁹ between the two conditions [appearance vs. paper matches].

Like the structuralist psychologists, Robilotto and Zaidi and Troost and de Weert treat reports informed by the subjects’ judgments about the nature of the stimulus to be inadmissible in experiments targeting sensory (visual) experience. Behind Titchener’s restriction was a concern to limit the observations of psychology to truly mental phenomena; contemporary psychophysicists, in contrast, are concerned to demarcate the right explananda for the psychology of *vision* as opposed to cognitive science more generally. We have also seen that scientists such as Maloney, who are not bothered by this new version of the stimulus-error argument, work within the neo-Helmholtzian tradition which posits that perception is an inferential process and does not restrict the appropriate target of enquiry to hypothetical sensory states which are entirely unaffected by inferences about distal stimuli. My central point is that these conflicting ways of analyzing and evaluating subjects’ responses are fundamentally theory driven, just like Titchener’s earlier characterisation of the stimulus-error.

6. Conclusion and Two Lessons

The central finding of this paper is that the issue of the “stimulus-error” has had a life beyond the early introspectionist schools of psychology. Boring (1921) was prescient in his analysis of the ramifications of the stimulus-error for later perceptual science which employs behaviouristic methods. Boring’s problem of

¹⁹ Like Robilotto and Zaidi, Troost and de Weert do not give an explicit definition of their sensory vs. non-sensory distinction. As I read these authors, the “sensory” encompasses all conscious visual phenomena, whereas “non-sensory” refers to post-perceptual cognitive processes or effects.

“equivocal correlation” has caused much ink to be spilled in the recent literature on colour and lightness constancy. Although they do not use the term, some researchers still call subjects’ responses into question when they are symptomatic of the stimulus attitude. Furthermore, we have seen that contemporary researchers’ attitudes towards the stimulus-error are indicative of their deep theoretical commitments over how to define perception, and what counts as a genuinely visual response to a psychophysical stimulus.

To finish the paper I would like to spell out an important lesson for philosophers of perception. Colour constancy has been much discussed in the recent literature on the ontology of chromatic properties. For example, many colour *realists* (those who argue colours are perceiver- and illuminant-*independent* properties of ordinary material objects) take constant colour perception to provide direct phenomenological evidence for their view (Tye 2000:147; Byrne and Hilbert 2003:9).

In response, Cohen (2008, 2009) has appealed to Arend and Reeves’ (1986) findings in order to make the case that the failure of constancy for appearance matches is *prima facie* evidence against colour realism. In favour of his own *relationist* ontology (where colours are perceiver- and illuminant-*dependent* properties of ordinary material objects), Cohen (2008) puts forward a counterfactualist theory of colour constancy. The basic idea is that the visual system is able to generate two different kinds of responses to pairs of colour stimuli. One response simply compares their occurrent appearances (leading to the observable difference in surface colour across changing illuminants), while the other answers the question of whether the two surfaces would match in their appearance in the counterfactual situation of their illuminants being the same.²⁰ These capacities are to be explained in terms of “neo-Helmholtzian subpersonal mechanisms for generating conclusions about the world on the basis of current visual input—mechanisms whose operations are not accessible to conscious introspection” (Cohen 2008:84).

The interesting thing here is that Cohen is troubled by the potential objection that his counterfactualist is an “over-intellectualisation” of colour constancy, and thus could not account for the finding of colour constancy in animals such as honey bees and in human infants. In order to deflect it, Cohen hastily commits himself to a schematic picture of the visual system which posits neo-Helmholtzian inferential mechanisms. He cannot afford to consider that the

²⁰ Cohen (2008:80): “what is constant in cases of colour constancy—is not their occurrent apparent colour, but their counterfactual apparent colour. The visual system’s responsiveness to this counterfactual dimension of comparison drives one of our reactions to cases of colour constancy (namely, the invariance/surface match reaction). On the other hand, I claim that our visual systems are also responsive to the distinct dimension of occurrent apparent colour....”.

stimulus or paper matches are the result of post-perceptual judgements, as Troost and de Weert take it that they do. We see that even in the philosophy of perception, analysis of stimulus-judgments is based on antecedent theoretical commitments rather than the experimental or introspective data themselves.

Similarly, in making the case that constancy phenomena lend support to colour realism, Allen (2007, 144) writes that:

turning on a desk lamp in an already day lit room brings about a very noticeable change in the appearances of the objects it illuminates. But we do not ordinarily think that turning on a desk lamp actually changes the colours of the objects it illuminates. The objects' colours appear to remain constant throughout the change in the illumination.

It is telling that Allen moves from talking of a “noticeable change in the appearance”, to talk of how we do not “think” that changes in object colour occur, to the assertion that “[t]he objects' colours appear to remain constant.” The passage calls for clarification, but it is certainly telling that in order to argue that there is an apparent phenomenon of constancy Allen finds himself assimilating thought to perception.

The moral of the story is that in order to employ constancy phenomena in the service of ontological claims, these two philosophers have had to make strong theoretical assumptions about the lack of separation between perception and inference. These commitments are mentioned in passing, if at all, and do not seem to be given the weight they deserve. Yet, it would be perfectly reasonable for an opponent to enter the scene and invoke something like the “stimulus-error” argument: Allen and Cohen are simply not describing a visual effect. The result would be dialectical stalemate since, as we have seen, the scientific story is far from settled and is not resolvable through current experimental practice.

More generally, we should be suspicious of philosophers' arguments from supposedly bare “phenomenological facts”. We have seen in the discussion of introspective psychology that an observation as straightforward as the sensing of pressure points on the skin can give variable results depending on whether one's “attitude” is directed to the cutaneous sensations or towards the distal stimulus. What is more, the responses one gives will be informed by one's beliefs about what could possibly be there. Yet philosophers of perception very rarely even consider the possibility that their own phenomenological reports and intuitions are shaped by their theoretical commitments (AUTHOR). When thinking about perceptual constancy and response variability, philosophers and psychologists of perception are all tightly bound within an evidential circle. In this paper I hope to have shown that greater awareness of the interdependency of these commitments can only be beneficial for future research.

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