

**Sixty years after Orne's *American Psychologist* article:
A Conceptual Framework for Subjective Experiences Elicited by Demand
Characteristics.**

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Abstract

Unlike objects, study participants form beliefs from cues present in a testing situation (demand characteristics). These beliefs can alter study outcomes (demand effects). Neglecting demand effects can threaten the internal and external validity of studies (including their replication). While demand characteristics garnered much attention following Orne's introduction of this notion, consideration of their effects has become sparse in experimental reports. Moreover, the concept remains confusing. Here, we introduce a conceptual framework for subjective experiences elicited by demand characteristics. The model distinguishes between participants' awareness of the hypothesis, their motivation to comply with it, and the strategy they use to meet situational requirements. We stress that demand effects may elicit genuine experiences. To illustrate the heuristic value of the model, we apply it to Evaluative Conditioning and the Rubber Hand Illusion. The General Discussion summarizes the main insights of our current analysis and discusses risk and opportunities associated with demand characteristics.

Sixty years after Orne's *American Psychologist* article:

A Conceptual Framework for Subjective Experiences Elicited by Demand Characteristics.

Introduction:

A persistent difficulty in the study of human thought and behavior lies in the use of methods borrowed from physics to study sentient and cognizant organisms (Rosenzweig, 1933). Unlike objects, people form beliefs from cues present in the study procedures in which they participate (demand characteristics). In turn, these beliefs can alter the study outcomes (demand characteristic effects). Neglecting demand characteristic effects can lead to incorrect interpretations, in much the same way as contaminated Petri dishes might lead to incorrect inferences in laboratory experiments.

Sterilization of equipment has become routine for scientists working with Petri dishes. In contrast, while demand characteristics generated a large body of research over the decades following Orne's introduction of the concept, interest in demand characteristics has waned over recent decades (Sharpe & Whelton, 2016). Consideration of demand characteristics is often sparse or even entirely absent in experimental reports (Klein et al, 2012), despite the concept being a standard component of introductory methodological courses in psychology.

Unfortunately, unlike sterilization in petri-dish based experiments, there is no general-purpose solution to the confounding effects of demand characteristics. Each experimental procedure presents unique challenges. Our aim in this article is to raise awareness of these issues and to provide a conceptual framework for gaining precision in our understanding of demand characteristics and, more specifically, the subjective experiences they elicit. In doing so, we draw attention to the degree to which demand characteristics may confound theories of mechanisms driving outcomes in contemporary psychology.

The manuscript is organized in three sections. In a first section, we discuss the historical roots and more contemporary developments in research on *demand characteristics*. Here, we stress their complex, far-reaching, and threatening nature, as well as their relevance to past and current methodological and theoretical debates. We also briefly discuss how researchers have aimed at controlling for the influence of demand characteristics. In a second section, we introduce a conceptual framework for subjective experiences resulting from demand characteristics. The model is organized on three levels: participants' relation to the experimenter hypothesis (i.e., hypothesis-aware, hypothesis-mistaken, hypothesis-free), motivation (i.e., to comply or be reactant) and strategy to meet the requirements of the situation (faking, imagining, or exerting phenomenological control). In a third section, we illustrate the model by discussing how it may be applied to and generate new insights in the study of Evaluative Conditioning and the Rubber Hand Illusion. The general discussion summarizes the main insights of the current analysis and discusses risks and opportunities associated with demand characteristics.

I. Introducing demand characteristics.

I.1. Demand characteristics as a broad and complex construct.

In his pioneer article on *demand characteristics*, Orne (1962) defined them as “the totality of cues which convey an experimental hypothesis to the subject” (Orne, 1962; p. 779). He saw those cues as very heterogeneous and often challenging to control. Besides the experimental procedure itself, Orne mentioned: “(...) rumors or campus scuttlebutt about the research, the information conveyed during the original solicitation, the person of the experimenter, and the setting of the laboratory, as well as all explicit and implicit communications during the experiment proper.” (Orne, 1962, p. 779). Consistent with this early definition, demand characteristics are commonly - and rightly - seen as experimental artifacts that weaken the validity of an experiment, as a “methodological issue” (Nichols &

Edlund, 2015). This is for instance evidenced in this contemporary definition: "an experimental artifact where participants form an interpretation of the experiment's purpose and subconsciously change their behavior to fit that interpretation" (Wikipedia contributors, 2020).

The understanding that Orne and other researchers developed of *demand characteristics*, however, became much more complex over the years. *Demand characteristics* can be cues situated inside or outside the lab. They can apply to experimental or non-experimental settings. They generally relate to undesirable effects but may also serve desirable ones. And they can trap study participant or patients as well as researchers or therapists. The complexity of the concept is apparent in the most recent definition of *demand characteristics* by Orne as: "the totality of cues and mutual role expectations that inhere in a social context, (e.g., a psychological experiment or therapy situation), which serve to influence the behavior and/or self-reported experiences of the research participant or patient." (Orne & Whitehouse, 2000, p. 469). This definition does not imply that demand characteristics should be limited to experimental settings and bring about only undesirable effects, and it makes no claim regarding how conscious their influence should be. This understanding of *demand characteristics*, which we endorse in the present article, is much broader than one that would limit them to subconscious experimental artifacts.

1.2. The historical roots of demand characteristics.

Although the concept of demand characteristics was initially developed by Orne (1959) to address specific issues in hypnosis research, wider interest in demand characteristics stems from Orne's later observation that research participants are often willing to comply with experimental instructions to complete boring and noxious tasks. Orne (1962) reasoned that study participants generally want to be helpful to the experimenter and serve Science. They want to be "good subjects" and infer that, if they are instructed to complete the tasks, this

means the tasks are worthwhile. The fact that participants form sensible expectations about the experimenter (i.e., "the experimenter is competent and considerate, so the task must be valuable") has much to do with communication pragmatics (Grice, 1975, Sperber & Wilson, 1986; Schwartz, 2010). To illustrate, when asked: "Can you hand me the pepper?", sensible respondents will go beyond the immediate meaning of the question and interpret it as an implicit request to hand on the pepper. Likewise, to quote Orne, "(...) if a test is given twice with some intervening treatment, even the dullest college student is aware that some change is expected, particularly if the test is in some obvious way related to the treatment." (Orne, 1962, p. 779).

At the time, however, Orne's theorization was inspired by the work of Gestalt psychologists. According to Gestalt theory, individuals inform their judgments and decisions by integrating cues present in their environment (see in particular Brunswik's lens model; Brunswik, 1955, 1957). This integration is thought to be holistic (i.e., the total is more than the sum of the parts). When applied to an experimental situation, a Gestalt theorization suggests that participants integrate more cues than the experimenter might think when interpreting their role in the experiment: "The response to any specific set of stimuli, then, is a function of both the stimulus and the subject's recognition of the total context." (Orne, 2009, p. 111). According to this analysis, any experimental situation is a problem (Rosenzweig, 1933) that participants attempt to solve by making inferences from a large set of cues, with the generally benevolent intention to help the experimenter.

I.3. The threatening nature of demand characteristics.

This analysis upset Orne's contemporaries. First, the reference to participants' phenomenology was considered heresy by the then-dominant behaviorist approach. The idea that "it is essential for the investigator to understand how a particular experimental situation is perceived by the subject to draw sensible inference from the subject's responses" (Orne, 1973,

p. 157) seemed inconsistent with behavioral investigations taking place at the time, for instance on classical and operant conditioning (see Section III.1).

Second, and more dramatically, Orne had opened a can of worms that questioned the interpretation (i.e., the internal validity) of influential social psychological studies completed at the time, including Berkowitz' studies on the "weapons effect" (e.g., Page & Scheidt, 1971), Milgram's studies on obedience (e.g., Orne & Holland, 1968), Zimbardo's prison experiment (e.g., Banuazizi & Movahedi, 1975), and Staats and Staats's studies on the classical conditioning of attitudes (Page, 1969). In particular, Orne questioned the fact that participants in Milgram's obedience studies had been deceived. Instead, he proposed that these participants may have played a role - occasionally, one that involved looking torn and anxious. This interpretation infuriated Milgram (but recently received empirical support; Hollander & Turowetz, 2017). In a violent assault on the notion, Milgram (1972; cited in Sharpe & Whelton, 2016) asked whether *demand characteristics* is a useful scientific notion at all or: "(...) shades into an autistic construction in which the themes of conspiracy, distrust, contaminants, and concealed motives play a commanding part?" (p. 150).

Bitter exchanges were also witnessed in "weapons effect" research, in which a weapon's mere presence is thought to unconsciously trigger revengeful behavior (Berkowitz & LePage, 1967). This research, which anticipated in many regards the more recent literature on unconscious social priming effects (e.g., Bargh & Williams, 2006), was quickly seen as reflecting the role of demand characteristics. Page and Scheidt (1971) generally failed to replicate the effect but found it in participants aware of (and apparently willing to comply with) the experimenter's hypothesis. In a scathing attack on behavioral studies, these authors noted that demand characteristics "when allowed to operate unnoticed and uncontrolled by the unwary experimenter, can contaminate an otherwise well-designed experiment. In this sense, the most poorly controlled experiments, containing the most blatant demand cues, may be the

most effective in obtaining publishable results." (Page & Scheidt, 1971, p. 304; for a response, see Berkowitz, 1971).

The Pandora's box opened by Orne's original publication contributed to a (first) crisis of confidence where "(...) social psychologist's early enthusiasm (was) replaced by serious doubts about the future of their field" (Elms, 1975, p. 967). The existence of demand characteristics was questioning not just the internal validity of experiments (i.e., How should effects be interpreted?) but also their external validity (i.e., can we expect an effect found in one lab to be replicated in another lab and to generalize to non-laboratory settings if cues differ?; for a recent discussion, see Kihlstrom, 2021). These questions were painfully received by a field thriving to gain attention and recognition from decision-makers. Current questions about the relevance of lab studies for public policy (e.g., Ijzerman et al., 2020) have much to do with the interrogations raised by Page and Streidt (1971) more than half a century ago.

A second crisis was precipitated in social psychology ten years ago, partly resulting from a replication study conducted by Doyen and colleagues (2012). These authors found that the impact of priming cues (i.e., *elderly*) on participants' behavior (i.e., walking speed) depended on the experimenter's expectations about priming effects: participants walked slower or faster depending on whether the experimenter expected the primes to decrease or increase their walking speed. In the same year, this set of authors discussed the role of demand characteristics and, more generally, of situated social cognition (Smith & Semin, 2004, 2007) in social priming studies (Klein et al., 2012). Strikingly, social psychological research was pushed back to the early seventies (Page & Scheidt, 1971), with similar concerns (i.e., the role of demand characteristics) raised about a conceptually related phenomenon (i.e., unconscious stimulus-driven behavior).

I.4. Subjective experiences and strategies are at the core of demand characteristic effects.

The replication efforts that followed quickly led to the opposition between direct vs. conceptual replications that is also much relevant to Orne's early work. In conceptual replications, consistency in participants' interpretation of the experimental setting is thought to matter more than consistency in experimental procedures and materials (e.g., Crandall & Sherman, 2016; Stroebe & Strack, 2014). For instance, if the most stereotypical trait of group A is *aggressive* in country X but is *gentle* in country Y, it would hardly make sense to rely on country X's study material to seek replication of stereotyping effects in country Y.

One key challenge in this debate is how to replicate subjective experiences when major changes have taken place in contexts (including cultural contexts). As Orne (1962) noted, experimental social psychologists endorsed physics' rigorous model as an epistemological reference (see also Rosenzweig, 1933). However, "(psychologists) do not study passive physical particles but active, thinking human beings (...) the subject is not a passive responder to stimuli and experimental conditions. Instead, he is an active participant in a special form of socially defined interaction which we call "taking part in an experiment." (Orne, 2009, p. 110). It is no surprise that social psychologists interested in communication pragmatics favor conceptual over direct replications (e.g., Schwarz & Strack, 2014).

An important tension relates here to the passive or active role of the participants in the study setting. On the one hand, Orne (1962) argues that participants place themselves under the experimenter's control. Here, he sees participants as compliant puppets subjugated by the experimenter and meant to behave in a way that confirms the experimental hypotheses. On the other hand, Orne allowed for a much more active role of the participants. As we have seen, he refers to *mutual role expectations* and goes as far as suggesting that participants may trick the experimenters.

The active role played by study participants will be stressed in our model by discussing various strategies that they may implement for complying with or reacting to the hypothesis.

Likewise, we will discuss the case of participants who form hypotheses departing from those of the experimenter. In doing so, we will draw attention to the possibility that effects of demand characteristics involve genuine changes in experiences (Dienes, Lush et al, 2020; Lush et al, 2020), which is consistent with Orne's acknowledgement that demand characteristics can drive experience: “(...) it is likely that social cues can determine the subject's actual experience in the situation.” (Orne & Scheibe, 1964, p. 11).

1.5. How can demand characteristics effects be controlled for?

Whenever the influence of demand characteristics has been considered undesirable, experimenters have relied on procedures to counteract or probe their influence. We provide here a brief overview of the main methodological strategies used to counteract their effects. In doing so, we refer the reader to articles that provide a more in-depth discussion of these points.

When mutual expectations are assumed in experimenters and participants, double blindness may be considered an option. When expectations are assumed in the participants only, hiding (e.g., Stricker, 1967; Stricker et al, 1967) or distracting them (i.e., *red herring technique*, Laney et al., 2008) from the purpose of the experiment may help. These deception techniques have a long history in social psychology and have recently culminated in a thorough Swiss Cheese Model of deception, whose rationale consists of adding up several layers of deception to best fool participants (Olson & Raz, 2020). Needless to say, the use of deception comes with ethical problems, some of which have long been considered unacceptable from the perspective of related fields (e.g., Brannigan, 2004). Another procedure consists of limiting participants' control over their responses. This has been done for decades in attitude research by relying on indirect measures (Campbell, 1950), sometimes called *implicit* measures. As we will discuss later, however, implicit measures - besides important

questions raised on their very purpose (Corneille & Hütter, 2020) - are not immune to demand characteristic effects.

Alternatively, when the influence of expectations could not be prevented, experimenters have attempted to probe it using questionnaires (e.g., a funneled debriefing questionnaire; e.g., Bargh & Chartrand, 2010) or post-experimental interviews. This way, suspicious or complacent participants may be identified and removed from the dataset. It is assumed here that participants (i) can introspect their mental processes and intentions accurately and, when the case, (ii) can report them verbally, and (iii) are motivated and willing to disclose them. These assumptions, however, may not always be correct. For instance, Nichols & Maner (2008) found, during a post-experimental interview, that not a single one of their participants admitted knowing the hypothesis stated to them at the beginning of the study. One may speculate that being a “good subject” sometimes implies hiding one’s understanding of the experimenter’s hypothesis, making it “difficult to determine who is deceiving whom” (Mixon, 1972, p. 145). As a further concern, self-reports questionnaires sometimes show surprising relations to the studied effects. To illustrate, Nichols & Maner (2008) found that participants scoring higher in social desirability were *less* likely to align their behavior with a statement of the experimental hypothesis they had received at the beginning of the study. Likewise, participants considered at risk to fall prey to demand characteristics showed a *reversal* of effects in a study by Laney et al. (2008).

As often noted, post-experimental inquiries may themselves be sensitive to demand characteristics. Once participants have completed an experiment, one cannot tell whether they are claiming ignorance of the experimental aims because they think that is what the experimenter wants to hear or are claiming to have an understanding of the procedure which they did not have before or during the procedure (or at a stage in the procedure that would have influenced their results). This raises questions about the value of post-experimental

inquiries and highlights the difficulty of designing post-experimental measures of hypothesis awareness that find a balance between being too vague (and so may fail to capture the true state of knowledge of the respondents) and being too precise (and so bear the risk of informing participants of hypotheses they were not aware of in the first place; Rubin, 2017).

Additionally, the use of these procedures assumes that experimenters are motivated to do their best at accurately identifying complacent participants. However, as Orne (1962) noted: “even if the experimenter makes an effort to elicit the subject's perception of the hypothesis of the experiment, he may have difficulty in obtaining a valid report because the subject as well as he himself has considerable interest in appearing naive. » (p. 780). This important point was highlighted and generalized in Orne’s contribution to Rosenthal and Rosnow’s (2009) classic book on artifacts in behavioral research: “(...) the mere fact that an investigator goes to great lengths to develop a “cute” way to deceive the subject in no way guarantees that the subject is, in fact, deceived. Obviously, it is essential to establish whether the subject or the experimenter is the one who is deceived by the experimental manipulation.” (Orne, 2009, p. 112).

Perhaps the most thought-provoking procedure used to overcome the limitations of post-experimental inquiries are *quasi-controls*. Here, participants are instructed to imagine the effects of the manipulations in a pre-experimental inquiry (also called *non-experiment*) or to behave *as if* they were taking part in the treatment group (Orne, 2009). A classic example is provided by Bem (1967), who showed that observers provided with instructions and materials given to the actual participants can anticipate their (cognitive dissonance) responses. Likewise, in studies on hypnosis, simulator participants asked to behave as if they had undergone hypnosis produce behaviors so similar to the treatment group that experimenters cannot tell them apart (e.g., Damaser, et al., 1963; Orne et al., 1968).

To avoid confusion with competing definitions of the term *quasi-experiment*, we will refer here to studies in which expectancies are measured without participants taking part in the experimental procedure as *expectancy studies*. These procedures can reveal that study participants are likely aware of the experimental hypothesis and know how to comply with it. Perhaps even more critically, they can reveal that expectancies differ across control and experimental conditions. When this is the case, a difference in treatment is confounded with a difference in expectations, which poses a major threat to the interpretation of the effect (Boot et al, 2013).

We will pay close attention to *expectancy studies* in Section III.2. One should keep in mind that they possibly offer the best controls, although not perfect ones. As Orne (2009) explains: “They can never prove that a given finding in the experimental group is due to the situation's demand characteristics. Rather, they serve to suggest alternative explanations not excluded by the experimental design employed.” (Orne, 2009, p. 222).

II: A Conceptual Framework for Subjective Experiences Elicited by Demand Characteristics.

The previous section clarifies that the effects of demand characteristics on subjective experiences is neither simple nor univocal. Instead, individuals actively construe an understanding of the situation that can be consistent or inconsistent with the expectations of the experimenter (or practitioner). In turn, individuals may differ in their motivation to comply or react to this situation, and they can engage in various strategies to meet the requirements of the situation. In this second section, we present and discuss a conceptual framework that integrates these various points of attention.

The model focuses on subjective experiences elicited by demand characteristics. Downstream consequences of these experiences cannot be covered in the model as this would require the discussion of hundreds of studied effects. We will make it clear, however, that the

consequences of participants' knowledge, motivation, and used strategies on study outcomes (i.e., the proper demand characteristic *effects*) question their internal and external validity. Of importance too, the model should be considered a conceptual framework aimed at organizing knowledge, facilitating communication, and opening new research avenues. At present, it remains partly detached from measurement questions.

Insert Figure 1 here

The overarching construct in the model, *demand characteristics*, may be defined at a very general level as cues (including prior beliefs) shaping expectations in the situation. The situation may be a lab or field experiment or it may be a clinical setting. Many other cues can influence study outcomes, but in ways that are unrelated to hypothesis-awareness. For instance, participants expecting to receive money or credits if they complete an experimental task will generally perform a decent job because reciprocity norms are triggered by the reward. This expectation may influence the outcomes of an experiment (as participants otherwise may be unmotivated or leave the experiment midway) but in ways that are irrelevant to the tested hypothesis. These contributors should not be underestimated but we will focus our analysis on the contribution of hypothesis-related expectations.

Within the situation, individuals may differ based on their *knowledge of the hypothesis*. One can distinguish between participants who are aware of the hypothesis, mistaken about the hypothesis, or hypothesis-free. One may question whether individuals are ever fully immune to forming hypothesis-related expectations in a study or more broadly, an intervention setting (e.g., a clinical or organizational intervention). For our conceptual model, however, we may assume that these participants exist, in which case their data may be considered unproblematic. *Hypothesis-aware participants* have correctly identified the tested hypothesis. They are not naïve anymore. These are the most straightforward participants. Being aware of the hypothesis is, in principle, orthogonal to the question of whether

participants infer that the experimenter holds it. In many cases, the two states may overlap, but they do not need to. Hence, *hypothesis-aware* should be understood here as holding expectancies consistent with the experimental hypothesis. Finally, participants can be *hypothesis-mistaken*, in which case they have formed hypotheses that differ to the tested ones.

The second level in the model addresses the *motivation of the participants*. No matter whether they correctly identify the hypothesis or are mistaken about it, participants may or may not want to behave or experience states consistent with it. Those who want to are *compliant*. These participants “behave in an experimental context in a manner designed to play the role of a “good subject” or, in other words, to validate the experimental hypothesis.” (Orne, 1962, p. 778). These “good” participants may accidentally work against the hypothesis if they are mistaken about it (see Sherman, 1967, for an early analysis). To the contrary, *reactant* individuals may want to affirm their independence or are apprehensive of how their performance will be evaluated. They work against the hypothesis and may even be actively sabotaging the experiment. This can be illustrated with contemporary “survey trolls” who may inflate the measurement of conspiracy beliefs (e.g., Lopez & Hillygus, 2018). In principle, when reactant participants are mistaken by forming hypotheses that are opposite to the one held by the experimenter, they may ironically contribute to supporting it. In clinical contexts, patients who do not conform to a therapist’s efforts may be seen as reactant. They are occasionally said to be “non-collaborative” patients who show “decreased compliance with therapy” (e.g., Rosenberg & Siegel, 2018; Seibel & Dowd, 1999). We direct the reader to two recent reviews of psychological reactance, the second of which discusses measures of trait and state reactance (Miron & Brehm, 2006; Rosenberg & Siegel, 2018).

Finally, the third level of the model addresses the *strategy used by participants*. Participants may consciously *fake* an effect on behavioral or physiological measures. For those participants, the measure may not be indicative of the genuine effect anymore (e.g., a

positive evaluation on a Likert scale is not indicative of their true liking), or the measure may be valid but produced by a mechanism that differs from the one intended (e.g., arousal may be observed on a skin conductance measure but a participant produces this response by biting their tongue). Alternatively, participants may intentionally *imagine* effects and consciously produce them. Contrary to faking, imagination elicits genuine experiences. The “good” participants identified by Orne may enter this category. These participants are not fakers, but they are not totally naïve either. This case includes the sort of experiences one has when absorbed in a book or a movie – we know the depicted events are not real but we respond emotionally as though they were (as in ‘*alief*’, Gendler, 2008).

Finally, participants may exert *phenomenological control*. We will elaborate now some more on this less familiar and more counterintuitive strategy. Here, similar to imagination but dissimilar to faking, participants produce genuine effects in compliance or reactance to the hypothesis they have formed. Yet, contrary to participants who fake or imagine, they are unaware of it. We refer to this ability, operative within a variety of contexts which include hypnosis, as *phenomenological control*.

A clarification on phenomenological control

Many people are able to generate genuine experiences in response to expectancies arising from imaginative suggestion presented within hypnosis contexts. Trait response to imaginative suggestion is measured by scales consisting of imaginative suggestions for a variety of experiences (e.g., limb paralysis, amnesia or visual hallucinations; for reviews, see Acunzo & Terhune, 2021; Woody & Barnier, 2008). Stability of trait response to imaginative suggestion has been demonstrated over extended periods (up to 25 years; Piccione, Hilgard, & Zimbardo, 1989).

Hypnosis is a specific context for the delivery of imaginative suggestions which involves the designation of the individual delivering the suggestions as a *hypnotist* and a

hypnotic induction which is intended to lead to a special state (*hypnotic trance*) prior to the delivery of suggestions. It has long been known, however, that the hypnotic context is not required for successful response to imaginative suggestion (Hull, 1933). Likewise, scales avoiding the hypnotic context show comparable results to hypnosis scales (Barber, 1965; Oakley et al., 2021, Lush et al, 2021). We refer to the trait ability to respond to imaginative suggestion as *phenomenological control* (e.g., Dienes, Lush, et al., 2020). This term avoids association with suggestibility, which can lead to unwarranted association with gullibility or social compliance (Moore, 1964; Tasso et al., 2020; Coe, 1973).

Phenomenological control is not restricted to direct, verbal imaginative suggestion, but rather can occur in a variety of contexts (Halligan & Oakley, 2014) and in response to implicit suggestion (e.g., repeatedly passing iron rods at a short distance from the subject's body in the 18th century progenitor of hypnosis, mesmerism; Hammond, 2013; Pintar & Lynn, 2008). It is goal-directed but experienced as unintentional. Many anomalous phenomena may be examples of phenomenological control. For example, speaking in tongues (glossolalia) is reportedly experienced as unintentional, but is consistent with goals (e.g., demonstrating one's faith). Similarly, channeling spirits, spirit writing, dowsing, and so on may be examples of phenomenological control, in which experiences are voluntarily generated but experienced as involuntary (see Dienes, Palfi et al., 2020).

While hypnosis has proved to be a fruitful context for scientific research into phenomenological control, it now carries a large body of myths which have arisen from earlier scientific understanding and misrepresentation in popular culture (see Lynn et al, 2020 for a review of myths about hypnosis). For example, the misconception that response to imaginative suggestion is involuntary or involves being controlled by a hypnotist (a belief which may underlie an apparent reactance effect in hypnosis screening when contrasted with non-hypnotic responses on an otherwise identical scale; Lush et al, 202; see Spanos et al,

1987). While experiences of involuntariness are central to hypnotic responding (“the classical suggestion effect” Weitzenhoffer, 1980), *the response itself is not involuntary* (Spanos, 1986, Dienes & Perner, 2007). This dissociation of experiences of voluntariness and strategic, goal directed behavior is the central target for theories of phenomenological control mechanisms (see Dienes et al, 2020).

The myth that phenomenological control requires a trance state may partly explain a historical lack of awareness of the possibility that demand characteristics can drive genuine experience via phenomenological control. Orne did not raise the possibility that expectancy-related experiences generated in response to imaginative suggestion and expectancy-related experiences generated in response to demand characteristics elsewhere might arise from the same mechanism. In common with many hypnosis researchers at the time, Orne was interested in trance states (Spanos & Barber, 1974). Belief in the causal power of a trance state may have acted as a barrier, effectively walling off response to imaginative suggestion in the hypnotic context from response in other contexts unrelated to *hypnosis*.

Regardless of the exact reason for the oversight, that expectancies arising from demand characteristics may act like expectancies arising from imaginative suggestion (and therefore may support incorrect inference regarding mechanism) has been, until recently (Michael et al., 2012) overlooked both in imaginative suggestion research and the wider psychological literature; empirical investigations of trait phenomenological control in experimental effects which have not previously been attributed to demand characteristics have only recently emerged (Lush et al., 2020, 2021).

Expectancies arising from demand characteristics can drive reports of experience in psychology experiments (Kirsch & Council, 1989); for example, a forced-choice between whether or not salt is present in a sample of distilled water which contains no salt (Juhasz & Sarbin, 1966) produces reports consistent with experience of salt (in follow up interviews,

approximately 30% of participants reported they would “testify at a murder trial that they had tasted salt”). Participants report psychedelic flashbacks when told that a placebo is a drug which induces them (Heaton, 1975), or a range of placebo psychedelic effects in a mock-hallucinogen trial (Olson et al., 2020). Pleasant tingling sensations (ASMR effects) are reported for audio-clips which lack the characteristics thought to induce such effects when naïve participants are informed the stimuli have “been shown to produce the ASMR effect” (Cash et al., 2018).

In each of these examples, experimenters intended to alter experience by manipulating expectancies, but it is also possible that expectancies may drive experience in experiments when the experimenter does *not* intend it (Michael et al., 2012; Lush et al, 2020). Participants high in phenomenological control may meet their perceived experimental requirements (e.g., of being a ‘good participant’) by generating an experience according to expectancies arising from the interpretation of cues inherent in the experimental procedure (or pre-existing beliefs).

We emphasize that, while participants engage in phenomenological control voluntarily, they may not be aware that they have done so, and their experiences can be perceived as involuntary (Dienes et al., 2020). If participant expectancies are consistent with an experimental hypothesis, and the experimenter does not recognize that demand characteristics can drive reports of experience (and associated neurophysiological changes; see Dienes et al., 2020), phenomenological control effects can be incorrectly attributed to other mechanisms.

III. An Application of the Conceptual Model to Evaluative Conditioning and the Rubber Hand Illusion.

We now turn to the application of the model to Evaluative conditioning and Rubber Hand Illusion research. This selection was not random. First, the current authors hold

expertise in Evaluative Conditioning and in the Rubber Hand Illusion, respectively. Second, both phenomena enjoy contemporary interest. Third, the role of demand characteristics in Evaluative Conditioning is a classic topic of investigation, whereas the role of demands in the Rubber Hand Illusion is undergoing heated debates. Fourth, Evaluative Conditioning contrasts cognition (*demand characteristics*) against behaviorism (*attitude conditioning*) whereas the Rubber Hand Illusion is reminiscent of hypnosis.

For the sake of conciseness, we limit our analysis to the more common pathways: hypothesis aware participants who want to comply and exert faking, imagination, or phenomenological control. For both Evaluative Conditioning and the Rubber Hand Illusion, we first introduce the phenomenon of interest. Then, we discuss the role of hypothesis awareness in the effect. Finally, we examine how faking, imagination, and phenomenological control may have operated in compliant and hypothesis-aware participants. We illustrate the heuristic power of the model by discussing some of the more unsuspected cases (e.g. , phenomenological control in compliant and hypothesis-mistaken participants) in a third dedicated section (i.e., Section III.3)

III.1. Applying the Model to Evaluative Conditioning effects

III.1.a Introducing Evaluative Conditioning.

The concept of attitude is considered a key - if not the most important - notion in social psychology. Attitudes are latent constructs underlying people's negative or positive evaluations of social groups, people, objects, and ideas. They are thoughts to influence people's cognition and behavior. Therefore, social psychological research has developed an early and continued interest in how attitudes may be acquired and changed.

One of the simplest ways to create attitudes consists of pairing a neutral stimulus with another stimulus of negative or positive valence. In these procedures, the neutral stimulus is

called *conditioned stimulus* (or CS) and the affective stimulus is called *unconditioned stimulus* (or US). The effect that results from this pairing procedure (i.e., an evaluative change in the CS) is called *evaluative conditioning* (EC). The EC effect is robust. It has been observed with various sensory modalities, and it has important practical implications in the social psychological, health, and consumer domains (e.g., Hofmann et al., 2010; Walther, Nagengast & Trasselli, 2005; Hütter & Sweldens, 2018.) The study of EC effects traces back to the late fifties and has gained a strong revival of interest in the last two decades with the development of dual-process models of attitudes (e.g., Gawronski & Bodenhausen, 2014; for a recent discussion, see Corneille & Stahl, 2018).

III.1.b Hypothesis-awareness in EC research, and how it is controlled for.

In their pioneer work on the conditioning of attitudes, Staats and Staats (1958) paired meaningless letter strings (i.e., the CSs, e.g., YOF) with positive (e.g., healthy, beautiful) or negative (e.g., sick, ugly) words. Following this pairing, participants rated more favorably the CSs paired with positive than negative US words. It should not be too difficult for participants to figure out the hypothesis and how to comply with it in such experiments. This is supported by a recent line of *instruction-based* EC studies that are highly reminiscent of the *quasi-control* procedures we discussed in Section II (in particular the *non-experiment* procedure).

In instruction-based EC studies, participants are instructed how the CS and US were or will be paired but never experience their actual pairing. Instruction-based studies have found EC effects on evaluative ratings and more automatic evaluative measures (e.g., Gast & De Houwer, 2012). They have also successfully replicated mere exposure (Van Dessel et al., 2017) and approach-avoidance (Van Dessel et al., 2015) effects, again on both types of measures. These findings indicate that no experience of the CS-US pairings is required to elicit EC effects, thereby signaling the possible contribution of compliance to hypothesis-awareness in the original paradigm.

The question of whether verbal conditioning is influenced by hypothesis-awareness is all but new in conditioning research. Goldstein et al. (1972) tested its role in a study on verbal operant conditioning that made use of the Taffel procedure. In that study, participants were ostensibly reinforced by the experimenter (who said “Good”) each time they used a self-related pronoun (“I”, “We”). This reinforcement increased the use of pronouns. Goldstein et al. (1972) predicted and found a higher sensitivity to the reinforcers in volunteers, presumably characterized by a higher need for approval than non-volunteer participants. Researchers did not take long to investigate the role of hypothesis-awareness in EC effects. This was done by attempting (1) to identify hypothesis-aware participants and excluding them from the analyses, (2) to prevent the development of hypothesis-awareness.

Identification strategies were implemented in early EC studies. Insko and Oakes (1966) distinguished between *contingency awareness* (i.e., the awareness of the CS-US pairings) and *demand awareness* (i.e., the awareness of the purpose of the experiment; that is, hypothesis-awareness). As these authors pointed out, *contingency awareness* relates to whether there can be learning without awareness – a question that remains well alive in contemporary attitude research (e.g., for a recent discussion, see Corneille & Stahl, 2018). In contrast, *demand awareness* addresses whether “(...) the subjects become aware that the experimenter wants them to show a conditioning effect and thus give him what he is looking for?” (p. 488). Insko and Oakes measured this second type of awareness by using the following item: *Did you feel as if you were supposed to rate the nonsense syllables in any particular way? If so, explain.*” (p. 490)

Based on their findings, Insko and Oakes claimed that contingency but not hypothesis-awareness was critical to EC effects. Three years later, however, Page (1969) relied on a more thorough measurement (i.e., a 17-item questionnaire) and found evidence for EC effects only in participants who were both *contingency aware* and *demand-aware*. Page concluded that the

results supported the idea: “(...) that the so-called conditioned attitudes are entirely artifacts of demand characteristics” (p. 185). In a quick rebuke, Staats (1969) astutely pointed out that the thorough questionnaire used by Page (1969) may have produced demand artifacts. The author explained: “The questionnaire actually *demand*s that the subject indicate that he saw through the purpose of the experiment (...)” (p. 189). The latter point is consistent with the note of caution we made when discussing the difficulty of designing valid controls for demand characteristic effects. “Good subjects” may fake a lack of awareness of the tested hypothesis. Alternatively, the measurement itself is a cue in the situation that can alter thoughts and behaviors: suggestive or otherwise cooperative participants may be persuaded that they knew the tested hypothesis whilst they did not.

Besides or in addition to identification strategies, researchers have tried to *prevent* the development of hypothesis-awareness. A first strategy consists of disguising the true purpose of the experiment (e.g., Staats & Staats, 1958) or at least using instructions that do not stress too obviously the evaluative implications of the procedure. The latter is generally achieved by merely asking participants to passively witness CS-US pairings without further evaluative instruction. In other studies, where the size of EC effects was largely decreased, participants were actively distracted from processing the evaluative quality of the stimuli. This was done for instance by asking participants to process the age of human CS characters (e.g., Gast & Rothermund, 2011) or the CSs’ brightness (Stahl et al., 2016). Although these studies were interested in examining the role of processing goals in EC effects, it is reasonable to assume that they oriented participants towards non-evaluative expectations.

The gold standard for preventing the contribution of hypothesis-awareness to EC effects, however, is to rely on unconscious learning procedures. The rationale is that participants can hardly draw conscious expectations about cues they do not consciously perceive. As a result, they cannot comply with these expectations either. This has been

achieved in subliminal EC studies. Contrary to early conclusions, however, no subliminal EC effect is found when using strong designs, measures, and analyses (see e.g., Dedonder et al., 2014; Heycke et al., 2018; Heycke & Stahl, 2020; Högden et al., 2018; Stahl et al., 2016).

If little evidence exists that EC effects can be acquired unconsciously, this implies that the role of *hypothesis-awareness* in EC effects cannot be easily rejected. It should be noted, however, that “unconscious” EC studies do not just prevent hypothesis-awareness but also contingency-awareness, the latter of which is critical to the obtention of EC effects.

III.1.c Faking, Imagination, and Phenomenological control in EC studies, for hypothesis-aware and compliant participants.

The above analysis indicates that various identification and prevention strategies have been implemented to control for the role of hypothesis-awareness in EC effects, with mixed success. As the model indicates, however, being aware of the experimental hypothesis and willing to comply with it does not imply the effect is not genuine. It is not genuine for faking participants only.

Faked EC effects have been traditionally addressed by using more automatic evaluative measures. In EC studies, being a “good subject” entails reporting a positive evaluation about CSs paired with positive stimuli vs. a negative evaluation about CSs paired with negative stimuli. When using measures such as an Implicit Association Test, the possibility for faked reporting is limited by the less direct and controllable nature of the evaluation (but see Fiedler & Bluemke, 2005). Because EC effects have been found on a diversity of indirect and more automatic measures, it seems unreasonable to conclude that EC effects uniquely reflect fake responding. More generally, we believe it is also unreasonable to assume that all study participants are fakers.

Participants can also engage in *imagination*. Here, they are aware of the experimental hypothesis and may consciously form genuine evaluative experiences that are consistent with it. Obviously, they can do so only when they are conscious of the CS-US pairings. If they are not, they cannot comply. As we discussed above, this may explain the lack of (robust, replicated) EC effects in procedures that prevent participants from a conscious encoding of the CS-US pairing. Once “good subjects” have formed genuine evaluative impressions, however, these impressions will naturally affect both "explicit" and "implicit" evaluative measures. This importantly means that the use of more automatic measures of evaluations is no guarantee that EC effects are immune to demand characteristic effects. However, these measures can reduce the risk of fake responding. EC effects observed in either experienced or instruction-based procedures may generally capture imaginative processes: participants correctly guess what is expected from them and both consciously and intentionally produce genuine experiences that fit what they see as the requirement of the situation.

In theory, demand characteristics may also lead to the control of phenomenology to meet expectancies. In EC, this could lead to the formation of genuine evaluative impressions which, although an intentional attempt to meet expectancies arising from demand characteristics, are nevertheless experienced as unintentional. The case of *phenomenological control* has not been explicitly addressed yet in EC, but one recent study is close to it. Van Dessel et al. (2019) relied on hypnotic suggestion to enhance the processing of counter-attitudinal information in EC procedures. Their study indicates that participants integrate counter-attitudinal information into their final impression of the CSs to a larger extent (as measured by an IAT) after receiving the following hypnotic suggestion: “You will now receive information that you will process more strongly than you normally can. Please remember well that the information that you will hear next, will sink in more deeply than is typically the case.” (p.3). Akin to verbal instruction delivered along with non-deceptive

placebos (see the General Discussion), participants in that research received information on the phenomenology of hypnosis upon entering the lab. Because phenomenological control was not assessed in that study, it is however difficult to identify the contribution of imagination and phenomenological control in the observed effects. The use of a less controllable measure of the CS evaluation, however, reduces in that study the risk of faking.

The above analysis suggests that awareness of the hypothesis and an intention to comply with it may contribute to EC effects. How much does it matter? It is important to distinguish here between external and internal validity issues.

Regarding external validity, there is always a risk that effects found in the lab do not generalize to other labs (leading to unsuccessful replication) or to the everyday life (dampening the pragmatic value of the studied effects) if cues differ and participants derive different expectations from those. To illustrate, a toilet paper brand may become more positively evaluated when associated with puppies in the lab because participants believe the situation requires from them that they produce this evaluation. However, this effect may have little relevance for designing an advertising campaign because pedestrians coming across puppies-toilet paper pairings displayed on a billboard may not consider it their job to start liking the brand. This does not mean that EC effects have low replication potential (they have proved robust) or pragmatic interest (most ads can be seen as evaluative conditioning procedures). However, as the procedure becomes more complex and subtle, generalization issues may ensue.

Regarding threat to internal validity, it depends on the specific research question. From a functional learning perspective, EC is defined as a change in evaluative behavior (i.e., the response) that is contingent on a change in the state of the world (i.e., the pairing of the CS-US stimuli). Whether this relation is driven by processes A, B, C, or D (i.e. the unobservable psychological mechanism) may not matter so much as long as the relation is found. For this

approach, whether the effect is faked or not may be the most substantive question (although a radical approach may not even consider it to be relevant, as suspicion of faking would imply an assumption about the operating mental processes). From a cognitive psychology viewpoint, however, the question matters a great deal. For instance, compliance-driven imagination should be expected to influence both direct and indirect measures (as it may alter the stored mental representations) whereas faking should influence direct measures only (as it does not involve a change in mental representations - just a change in behavior). Needless to say, imagination, phenomenological control, and reactance are also relevant to research in cognition in their own right. From a social-interactionism viewpoint, or for personality researchers interested in *compliance*, the question also matters. For instance, the fact that EC effects correlate positively with neuroticism and agreeableness (Vogel et al., 2019) may support the predictive validity of these constructs: more agreeable people may be more likely to play the role of *good subjects*, and people scoring higher on neuroticism may experience higher evaluation apprehension.

III.2. Applying the Model to the Rubber Hand Illusion.

III.2.a Introducing the Rubber Hand Illusion (RHI):

The Rubber Hand Illusion (RHI; Botvinick & Cohen, 1998) was the first of an influential class of multisensory illusions that are claimed to demonstrate experimentally induced changes in the experience of body ownership (for a review, see Braun et al., 2018). In a typical RHI experiment, participants are seated before a rubber hand with their real hand concealed from view, while these two hands (their own and the fake one) are repeatedly stroked in synchrony. Although this procedure can generate a broad range of experiences in interviews (Lewis & Lloyd, 2010; Valenzuela Moguillansky et al., 2013), the effects of interest are generally (but not always, e.g. Haans et al., 2012) limited to experiences of felt touch referred to the fake hand and of ownership of the fake hand. In short, participants come

to experience the fake hand as their own following the synchronous brushing of their concealed hand (which they don't see) with the fake one (which they see). Feelings of ownership for the fake hand are typically measured on Likert scales. The three statements most commonly interpreted as evidence of RHI experience are “It seemed as if I were feeling the touch of the paintbrush in the location where I saw the rubber hand touched”, “It seemed as though the touch I felt was caused by the paintbrush touching the rubber hand” and “I felt as if the rubber hand were my hand” (see Lush, 2020; Wu, in press; Riemer et al., 2019 for a detailed discussion of RHI agreement statements).

The RHI is commonly thought to provide insight into mechanisms underlying the sense of ownership (Braun et al., 2018). While there is some disagreement as to the degree to which the RHI is related to body ownership experience more generally (David et al., 2014), the assumption that the RHI provides evidence of the role of multisensory integration mechanisms in the experience of body ownership informs much theoretical work (see Heed & Röder, 2012; Vignemont, 2018). While some claim that the illusion is primarily or entirely bottom-up (e.g., Armel & Ramachandran, 2003), or even cognitively impenetrable (Hohwy, 2010; Ehrsson, 2004, quoted in Yong, 2011), others argue that the illusion is additionally influenced by top-down mechanisms that are distinct from demand characteristics, for example “an interaction between current multisensory input and internal models of the body” (Tsakiris, 2010, p. 706).

A few authors reject the fundamental assumptions supporting dominant interpretations of RHI evidence. Dieguez (2018) argues that the RHI is not an illusion, but rather arises from expectations about the experimental procedure and an ability to suspend disbelief. Alsmith, (2015) argues that RHI reports may reflect imaginative perceptual experiences that are not experienced as intentional. Lush et al., (2020; see also Lush, Dienes, et al., 2021) argue that the RHI may be entirely attributable to phenomenological control.

III.2.b Hypothesis-awareness in RHI research, and how it is controlled for.

Orne argued that “the demand characteristics most potent in determining subjects' behavior are those which convey the purpose of the experiment effectively but not obviously” (Orne, 1962, p. 780). Consider an RHI experiment from the perspective of a participant. The context of the experiment will be sufficient to generate expectancies of some effect (the scientist must be interested in some result of the forthcoming procedure). The exact nature of the expectancies formed will be based on any available cues as to the possible nature of the expected effect. In a typical RHI experiment, the participant is seated in front of a box with their right hand placed flat inside the box. A fake right hand is positioned within the box in the same orientation as their own hand, but at a distance of 15cm to the left of their hand. The fake hand can be seen through an opening in the box, but the participant's hand is concealed. The participant is instructed to stare at the fake hand and the experimenter (positioned on the opposite side of the box) then begins to stroke the fake and real hands in synchrony and in the same locations on each hand so that the felt and seen brush strokes closely match.

That one is expected to feel the touch on the fake hand may be a natural conclusion to draw, considering that people have much experience of simultaneity of visual and tactile signals when their body parts are touched. Indeed, pre-existing beliefs constitute part of the demand characteristics of an experiment and may lead to *hypothesis awareness*. That such an experience is to be interpreted as an experience that the fake hand is one's own hand may be a common interpretation of this scenario. There are two standard control methods used in RHI studies, both of which draw on the methods reported by Botvinick & Cohen (1998):

(a) a comparison of agreement scores, within the synchronous brushing condition, for ‘illusion’ statements (referred touch and ownership, which are predicted by the experimenter to arise from multisensory stimulation) *versus* ‘control’ statements (e.g., visual hallucinations, the hand turning ‘rubbery’).

(b) a comparison of agreement scores for ‘illusion statements’ between the synchronous and asynchronous brushing conditions.

A crucial assumption underlying the use of these two control methods is that expectancies for illusion and control conditions are matched. However, expectancy studies in which participants are merely instructed about RHI procedures show that the assumption is not met: akin to the instruction-based and quasi-control experiments discussed in the previous sections, the pattern of expectancy ratings matches that of typical reports of illusion and control measures (Lush, 2020; replicated by Lush, Seth, et al., 2021; Reader, 2021). The assumption of equal expectancies also may not be met for other less commonly employed control conditions; for example, anatomically implausible positioning of a fake hand (Chancel & Ehrsson, 2020) or asynchronous movement (Kalckert & Ehrsson, 2012).

It is important to emphasize here that demand characteristics are present at any stage of a procedure, and prior to it (they are not limited to pre-existing beliefs, recruitment materials, experimental instructions). For example, it may be that ‘ownership’ is only expected once an experience of referred touch has occurred (see Wu, in press), or following exposure to the ownership questionnaire item in a control condition (an order effect; Lush 2021). We will return to consideration of cues from which participants may draw expectancies regarding the experimental hypothesis and related effects in the RHI in the section on phenomenological control in hypothesis-mistaken participants.

III.2.c Faking, Imagination, and phenomenological control in RHI studies, for hypothesis-aware and compliant participants.

Because controls have been employed that are not or may not be matched for expectancies, it cannot be ruled out that RHI reports are partly or even fully attributable to hypothesis awareness and compliance with it. Hence, we turn now to a consideration of the strategies that participants may have used in RHI studies.

Questionnaire responses can be easily *faked*. To protect against this possibility, ‘implicit’ measures of illusion experience have been developed. Proprioceptive drift - an experience of the participants’ hand drifting toward the fake hand - is the most common, and sometimes is presented without accompanying subjective report (e.g., Tsakiris & Haggard, 2005). However, there is no evidence that this measure is robust against demand characteristics, and it is sensitive to the exact phrasing of the question posed (e.g., asking the participant where they “feels like” their finger is vs. where their finger “really is” (Tamè et al., 2018). Expectancy studies show that the assumption of equivalent expectancies is not met for the commonly used asynchronous control condition in this measure (Lush et al., 2021). The contribution of faking, therefore, cannot be ruled out in proprioceptive drift. Skin conductance response is another common indirect measure (Armel & Ramachandran, 2003), but it can be faked too (e.g., by changing facial expression; Levenson et al., 1990). An asynchronous condition is also ineffective as a control for faking in this measure (Lush et al., 2021).

While much of the quantitative evidence for the RHI could, taken at face value, be accounted for by faking, it seems implausible that so many participants would attempt to actively deceive the experimenter (while a sizeable minority do not report RHI experience, agreement with RHI statements is common; Roseboom & Lush, in press). However, RHI measures may reflect *imagination*. RHI measures of agreement with statements which begin “I felt as if” “or “It seemed as though” could be reasonably interpreted by participants as invitations to report imagined experience, and therefore involve intentionally imagined experiences which are distinct from faking (the participant does not mean to mislead the experimenter about their experience). Dieguez (2018) compares RHI reports to imaginative responses in other situations, for example feeling fear while watching a horror film (despite knowing that the action depicted is all fake and presented on a 2D screen). It would be reasonable to interpret similar agreement statements presented after watching a horror movie

as evidence of imaginative experience (e.g., “I felt as if I was scared while watching the movie”). Similarly, Wu (2018) argues that reports of changes in the experience of hand location could arise from imagining one’s hand is where the fake hand is. Physiological changes associated with the RHI procedure may also reflect *imagination*. For example, skin conductance response can be altered through simulating emotions (Stern & Lewis, 1968) or by imagining ownership of a fake hand (Hägni et al., 2008).

In at least some cases, RHI experience seems to be something other than *imagination* or *faking*. For example, both participants and RHI researchers trying the effect themselves can report striking and apparently surprising RHI experience which they do not seem to consider to be acts of imagination (Lewis & Lloyd, 2010; Yong, 2011). Such reports parallel those of experience in hypnosis and may reflect goal directed and strategic generation of experience which is experienced as involuntary, or *phenomenological control*.

There are substantial relationships between trait *phenomenological control* and RHI measures (Lush, Dienes, & Seth, 2021b), and without participants relatively high in trait *phenomenological control* (above the median), group-level evidence for ownership experience may be rare (Roseboom & Lush, 2020). Indirect RHI measures are intended to show evidence of experience, but are, to date, ineffective at distinguishing between whether the experience is attributable to *phenomenological control* or other mechanisms. Imaginative suggestions can lead to neurophysiological change (e.g., fMRI; for reviews see Halligan & Oakley, 2013; Jensen et al., 2017; Landry et al., 2017) and change in other physiological measures (e.g., skin conductance; Barber & Coules, 1959; including for ownership of a virtual hand; Hägni et al., 2008; see Dieguez, 2018 for a similar argument regarding 'alief'). In sum, evidence for the RHI may reflect hypothesis awareness and compliance to the hypothesis via *faking*, *intentional imagination*, or *phenomenological control*.

Demand characteristics may drive RHI effects rather than, or in addition to, multisensory mechanisms, or other top-down effects. Whether this matters or not depends on the questions for which we take the RHI to be informative. If we wish merely to claim that synchronous brushing of fake and real hands can drive behavioral changes in self-reports, then even faking is not a problem. If we wish to claim that it drives changes in experiences, but remain uncommitted to the mechanism or to claims about a sense of ownership, then hypothesis awareness is problematic only with regard to faking.

If we are interested in the *mechanisms* driving experience in the RHI, then more serious issues arise. As we discussed, genuine experiences may be changed by consciously intended imagination or phenomenological control. Without controlling for hypothesis awareness and therefore participants' expectancies (and suggestion difficulty in the case of phenomenological control; see Lush, 2020), it is not possible to ascertain whether or not RHI effects are arising from, for example, multisensory integration mechanisms. It may be, for example, that demand characteristics inherent in the application of multisensory stimuli are related to RHI experience, rather than experience arising directly from multi-sensory mechanisms. Similarly, without controlling for *hypothesis awareness*, claims that there are top-down effects in the RHI that are not attributable to demand characteristics (e.g., mechanisms involving internal body models: Tsakiris, 2010) are not parsimonious.

The RHI is considered to provide insight into the sense of body ownership. However, and this is the threat posed by demand characteristics to internal and external validity, if the RHI is an imaginative suggestion effect then it does not necessarily provide evidence for a distinct sense of ownership, nor that such a sense exists outside the experimental situation. To illustrate with a straightforward example, experiences of ownership of someone else's face can be created in the lab (e.g., Tajadura-Jiménez et al., 2012). Yet, it is implausible that normal people experience others' face as their own in the everyday life (threat to external

validity), and more research may be needed to ascertain that such effects reflect multi-sensory integration mechanisms over and above receptivity to imaginative suggestion (threat to internal validity).

III.3. Walking through the other pathways of the model:

In the previous two sections, we only discussed the case of hypothesis-aware and compliant participants. A comprehensive discussion of the other cases, both for EC and the RHI, would make for an oversized manuscript. In many cases too, the discussion would be highly speculative as the majority of those pathways have not been empirically investigated yet. This, by the way, supports the heuristic value of the model in organizing past knowledge *and* pointing to neglected research directions.

The case of hypothesis-aware but reactant participants, and of (compliant or reactant) hypothesis-mistaken participants have not been discussed yet. In all these cases, the three strategies (faking, imagination, and phenomenological control) may be involved. There is, for instance, nothing that should prevent a reactant participant to fake or actively imagine experiences that are opposite to their understanding of the situational requirement. The case of phenomenological control in reactant participants is perhaps more tricky, but is conceivable. For instance, a participant with prior experience of a particular phenomenological control effect which they attribute to something other than phenomenological control (e.g., paranormal experiences) may show reactant phenomenological control if participating in a test of that effect conducted by a researcher they believe to be skeptical about it, or a patient with a functional disorder may show worsening symptoms if accused of malingering. Compliance and reactance may also be exerted through the three identified strategies for *hypothesis mistaken* participants: being incorrect about a hypothesis does not prevent to comply with it or react to it by using these strategies. As a case in point, we discuss now phenomenological control in compliant but hypothesis mistaken participants.

RHI research provides a rich source of candidate phenomenological control effects in hypothesis mistaken participants. It is not uncommon for some participants to report experience for control statements, for example, agreeing that their hand felt “rubbery” or that the fake hand changed in appearance (see Riemer et al., 2019). Lush et al., (2020) report a relationship between agreement scores for a control statement comparable to the relationship between phenomenological control and illusion measures so that participants high in phenomenological control are more likely to report agreement for control experience (note that lower average response for control experiences than illusion experiences may reflect increased difficulty in generating that experience as well as lower expectancies; see Lush, 2020). Similarly, interviews conducted with RHI participants reveal a wide range of experiences that are not captured by the traditional questionnaire (Lewis & Lloyd, 2010; Valenzuela Moguillansky et al., 2013).

Because demand characteristics vary with study designs, expectancy effects may vary across studies. For example, merely adding the addition of measurement of expectancies for proprioceptive drift to an RHI pre-experimental inquiry results in expectation of experience for a standard control statement describing an experience of the participants’ hand drifting toward the fake hand (Lush et al., 2021). The addition of new demand characteristics introduced by changes to the basic RHI study design may lead to *hypothesis-mistaken* participants reporting a variety of reported experiences across studies. For example, the RHI procedure applied to abstract clay objects can generate reports of a feeling of heaviness or that a participant’s fingers “feel bent over, just like the clay” for a spider-like object (Hall & Poliakoff, 2020). Repetitive synchronous tapping on a marble hand and a participant’s hand generates reports of stiffness, numbness, and ‘pins and needles’ (Senna et al., 2014). In a ‘magnetic hand illusion’ (the space above the fake hand is ‘brushed’), participants report experiencing opposing magnetic forces or ‘forcefields’ (Hohwy & Paton, 2010; an experience

of magnetic force is a common imaginative suggestion effect). When the RHI is induced unimodally with a light, red light generates reports of heat and blue light cold felt on the fake hand (Durgin et al., 2007). Finally, consider an RHI effect initially developed as a control method but reinterpreted as a novel illusion when participants unexpectedly reported experience: the invisible hand illusion, in which empty space is ‘stroked’ (Yong, 2013). While the proposal remains to be tested, such phenomena could be driven by phenomenological control.

General Discussion

Early concerns were raised by psychologists about using a model borrowed from physics to study cognizant human beings. As Lana (2009) astutely pointed out, however, Orne's insights proved a powerful reminder of quantum indeterminacy: "One finds a definite relationship between the observational process of the experimenter and the natural process of the subject." (p. 94). The sense-making process elicited in the social exchange between experimenters (or clinicians) and their participants (or patients), and how this process impacts thoughts, bodily experiences, and behaviors, makes *demand characteristics* an eminently social psychological notion. Demand characteristics are relevant to a breadth of social psychological phenomena (e.g., social conformity, obedience, “social priming”, attitude, and social cognition research) as well as to a great diversity of other psychological domains, including clinical psychology, neurosciences, and psychopharmacological research.

Over the last six decades, demand characteristic effects have never left the preoccupations of researchers. Rather, they have represented a looming threat to internal and external validity for which no straightforward, one-size-fits-all, solution exists. Many procedures have been developed to prevent, probe, and reduce the influence of demand characteristics, none of which allows for a perfect reduction of the uncertainty. The very possibility of conducting compelling empirical studies on demand characteristic effects was

questioned early on by Sigall and colleagues (1970) who argued: “To use a social psychological experiment to investigate problems (indeed, artifacts) that plague social psychological experiments is somewhat akin to placing two mirrors face-to-face and trying to point out the original image in one of them. How do we know that the data we are using to discuss possibly biased data are not, in and of themselves, biased?” (p. 8). We discussed as a case in point the measurement of hypothesis-awareness, where participants can fool experimenters into believing that they did not (Nichols & Maner, 2008) or that they did (e.g., Staats, 1969; Rubin, 2017) identify the purpose of the experiment.

Our conceptual framework highlights the existence of three strategies that may be involved in the production of demand effects. We distinguished between strategies leading to fake (faking) but also to genuine (imagination and phenomenological control) experiences. In the latter, we further distinguished between experiences that produced consciously (imagination) or unconsciously (phenomenological control). These distinctions, along with the motivational goals they serve (complying or reacting), among participants who correctly or incorrectly guess the hypothesis, allow for a much finer-grained understanding of demand characteristic effects. In the remainder of this article, we discuss risks and opportunities associated with these effects.

Risks associated with demand characteristics.

Considering its presence for six decades as a scarecrow (Sharpe & Whelton, 2016), *demand characteristics* appears to be a fixed, yet polysemous, part of the vocabulary of psychological science. We recommend that a distinction is drawn between *demand characteristics* as cues in the situation (including prior beliefs), *subjective experiences* elicited by those cues, and *demand effects* as the outcome of these experiences on a studied phenomenon. We also recommend that subjective experiences elicited by demand characteristics are specified when studying demand effects. For instance, is a researcher

referring to effects driven by participants aware or mistaken about the hypothesis? And in either case, which strategy is involved? This should help clarify, for instance, whether one is conceiving the observed phenomenon as genuine or faked, and consciously produced or not.

We would like to insist that the existence of *demand effects* does not exclude the possibility of genuine experiences, and that these experiences may sometimes be unconsciously implemented. In addition to the evidence discussed here regarding the RHI, evidence supporting the theory that demand characteristics drive phenomenological control in psychological research has been reported for three effects so far: visually-evoked auditory response (vEAR – reports of auditory experience in response to silent video; Lush, Dienes, Seth, et al., 2021), mirror touch synaesthesia (reports of feeling seen touch) and vicarious pain (reports of feeling pain when viewing people apparently in pain; Lush et al, 2020).

That expectancies can drive experience in psychological experiments is a looming threat to established theories of mechanisms driving effects of interest. This issue can be considered a directly analogous methodological issue to the recognition that expectancies can drive placebo response in clinical trials (for the role of expectancies in placebo and imaginative suggestion, see Kirsch, 1985). In both cases, the effects of an intervention (or experimental manipulation) should be attributed to expectancies unless evidence can be provided to the contrary. Here again, control is not easily dealt with. For instance, when treating depression, control participants may not experience side effects of the pharmacotherapy. As a result, they may figure out that they belong to the placebo condition. When an active placebo is used that matches the control condition for expectation, the treatment effect may become negligible (Cuijpers & Cristea, 2015). This issue is widespread in psychotherapy and psychological research. In a recent article, Boot and colleagues (2013) rightly noted: “Despite the need to match expectations between treatment and control groups, almost no psychological

interventions do so. This failure to control for expectations is not a minor omission—it is a fundamental design flaw that potentially undermines any causal inference” (p. 445).

Arguably, however, an even greater threat elicited by demand characteristics has to do with *ecological validity*, one of the four main concerns in the replication crisis (Fabrigar et al., 2020). Specifically, even though a valid interpretation may be raised in a given experimental context, cues inherent in the lab setting may elicit motivations and strategies that do not survive outside the lab, or when moving temporally or spatially from one lab to another, or even when merely changing the identity of the experimenter. In Section I, we have pointed out that this problem was central to the first and second (still ongoing) replication crisis in social psychological research. We believe that this external validity issue is at the core of ongoing debates on the respective value of direct versus conceptual replications – a debate that, similar to the seventies, is associated today with its share of interpersonal animosity; see for instance Lee, 2021; Sherman & Rivers, 2021). On a more positive note, many effects do robustly replicate and do have relevance for the everyday life, in which case we may surmise that influential cues remain constant across the investigation contexts.

Facing replication issues, researchers may be tempted to attribute *post-hoc* replication failures to changes in contexts or to hidden *cues*. This clearly is not the way to go. As Zwaan et al. (2018) put it: “reliance on context sensitivity as a post hoc explanation, without a commitment to collect new empirical evidence that tests this new idea, renders the original theory unfalsifiable. (p. 6)”. There certainly is no simple solution to these questions. However, the current analysis points to a couple of recommendations. First, researchers should avoid conflating *demand characteristics* with *demand effects*. As we have seen, demand characteristics can produce a broad range of contradictory effects. Second, temperance is likely to help and may be achieved on two levels: (i) by nuancing critiques of established work when a new study points to the role of demand characteristics in the effect of

interest (or, relatedly, when findings fail to replicate), and (2) by endorsing a cautious mindset when it comes to making policy recommendations based on lab studies (no matter their validity and lab replication potential).

Opportunities associated with demand characteristics.

Turning to the more positive side of demand characteristics, we can think of three implications. First, this notion may serve as a general reminder of the unwanted role of expectations in psychological research. For the reasons just discussed, a lack of attention to demand characteristics may threaten internal and external validity, feed replication failures, and encourage unwarranted interventions and policy-making recommendations. Hence, it is useful to retain the construct in the behavioral methods vocabulary. Second, we have argued that the mechanisms underlying the formation and influence of expectations may be of interest in their own right. Third, one can capitalize on participants' cue-based expectations to bring about desirable experiential and behavioral changes. This is most evident in clinical and placebo studies, which we now briefly discuss.

There are two sides to demand characteristics. On the one hand, expectations can create effects that are mistakenly attributed to treatments. On the other hand, an effect *is* created, and it may be a valuable one. The notion that compliance with cue-induced expectations or even mere verbal instruction can produce desirable outcomes has long been part of psychotherapeutic interventions. Directly mapping their rationale onto Orne's (1962) demand characteristics article, Kanter, Kohlenberg, and Loftus (2002) coined the term "therapeutic demand characteristics", defined as "the sum total of cues that convey the therapist's wishes, expectations, and worldviews to a client and become significant influences on the client's behavior, specifically influencing the client to confirm the therapist's wishes, expectations, and worldviews with subsequent behavior" (p. 3; see also Kanter et al., 2004). Paradoxical interventions have even been designed for capitalizing on a patient's reactance, such as with

symptom prescription strategies (e.g., Rohrbaugh et al., 1981; Tennen et al., 1981). As a side note, and on a very general level, one may also build on faking to bring about changes. This is implemented with social control, whereby people produce behaviors they would otherwise not produce based on expectations about the negative consequences of not behaving a particular way. These expectations may be triggered by cues present in the environment (e.g., an automatic reminder of a fine when exceeding a speed limit) or through direct verbal instruction (e.g., a verbal threat).

From a more clinical perspective, there is a long tradition of clinical application of imaginative suggestion effects, primarily within the context of hypnosis (see Lynn et al., 2019). Considered in this light, reports of effective exposure therapy based on the rubber hand illusion (Nitta et al., 2018; see Terhune & Oakley, 2020 for a recent discussion of exposure therapy in hypnosis), or rubber hand dependent placebo analgesia (Coleshill et al., 2017; see Milling et al., 2005, for an example of analgesic effects of imaginative suggestion outside the hypnotic context) may be operating by the same mechanisms as existing imaginative suggestion-based treatments.

While imaginative suggestion effects were attributed to the manipulation of magnetic fluid in the 18th-19th centuries, and hypnotic trance in the 19th-20th centuries (Pintar & Lynn, 2008), these historical interpretations are no longer consistent with scientific evidence. Despite the, at the time, scientifically plausible but incorrect explanations of mechanism, mesmerism, and hypnosis have effective clinical applications. For practical purposes, then, it may not much matter for clinical applications if the RHI is a suggestion effect (besides problems arising from trait differences in response; see Zbinden & Ortiz-Catalan, 2021 for a discussion of such problems in the application of the RHI to prosthetic limbs).

Placebos as treatments are also based on expectations, and their use is widespread. International surveys indicate that 17% to 80% of doctors have prescribed placebos (e.g.,

Fässler et al., 2010). Prescription of “impure” placebos (interventions having a known effect, but not for the ailment under consideration) have been implemented by up to 97% of healthcare providers in the UK (Howick et al., 2012). The relation between placebo effects, demand characteristics, and expectations is evident when considering that placebo effects may be seen as an instantiation of conditioning effects (De Houwer, 2018), the latter of which we have seen are highly sensitive to propositional knowledge and verbal instruction. Of interest, expectations can also produce nocebo effects. In particular, verbal suggestion on negative side effects may cause allodynic and hyperalgesic effects as strong as those elicited by direct experience (Blasini et al., 2018; Rodriguez-Raecke et al., 2010)

Admittedly, using placebos (or sham surgery) is ethically controversial as clinical efficiency is achieved through deception. Healthcare providers are also generally uncomfortable or ambivalent about using them (Fässler et al., 2010). Yet, these ethical considerations can be alleviated by the use of a non-deceptive placebo (for a recent discussion, see Colloca & Howick, 2018). In non-deceptive placebo studies, the experimenter informs participants about the nature of placebo effects and tells them that they belong to the placebo group. Whereas non-deceptive placebo effects are generally observed on self-reports, a recent study has generalized them to a neural measure of emotional distress (Guevarra, Moser, Wager & Kross, 2020, p. 2). Findings such as these highlight the influence that mere verbal instructions and propositional learning may have on subjective and objective health indicators (for a recent discussion, see De Houwer, 2018).

While Kirsch (1994) describes imaginative suggestion as a non-deceptive placebo, and there are cases which bear comparison with phenomenological control effects (e.g., non-deceptive open-label placebo; Charlesworth et al, 2017), evidence for relationships between placebo response and phenomenological control is mixed (see Parsons et al, 2021). Placebo aside, imaginative suggestion effects have been applied for clinical purposes for centuries

(Pintar & Lynn, 2004). Phenomenological control effects in contexts other than hypnosis may be useful for this purpose. If clinically relevant effects of rubber hand illusion-type procedures (e.g., treatment for OCD or analgesia) are phenomenological control effects, they may still be effective when they are described as such, and provide a means of providing clinical benefits of imaginative suggestion to patients for whom the hypnotic context is unappealing.

Conclusion

In their remarkable discussion of the history of demand characteristics, Sharpe and Whelton (2016) compared the notion with an old and resilient scarecrow. Together with these authors, we believe that the implications of demand characteristics cannot be overstated and we surmise that the concept, how psychologically unpleasant it may be, will stick around for a very long time in behavioral, clinical, and neuroscience research, haunting the present and future generations of researchers. As we have seen, demand effects threaten both the internal and external validity of studies. The internal validity issue is concerning and thorny: in particular, differences between treatment and control conditions are often confounded with expectations (Boot et al., 2013) and it is never entirely clear how a perfect control may be achieved for the latter. Almost ten years ago, Michael and colleagues (2012) argued that insufficient attention has been paid to the effects of expectancies in psychological science, and the situation is little different today. The external validity issue threatens the reproducibility of studies and dampens their practical value. The challenges posed by demand characteristics for these issues is great and requires frank confrontation. Proceeding to a closer examination of how expectations are elicited and how they influence (bio)psychological outcomes across psychological domains represents a unique opportunity for our field to push the boundaries of knowledge and for designing new interventions.

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