EFFECTS OF LEARNING ON SOMATOSENSORY DECISION-MAKING AND EXPERIENCES

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Abstract

Medically unexplained symptoms (MUS), where patients experience disabling physical symptoms in the absence of medical pathology, are very common in medicine but poorly understood and difficult to manage. Recent models, reviewed in Chapter 1, identified MUS as somatic misperceptions and suggested that training to reduce somatosensory misperception more generally might result in decreased symptom reporting. Researchers have used an experimental paradigm called the somatosensory signal detection task (SSDT) to study somatosensory misperception in the laboratory. In this task, participants often report illusory touch sensations ("false alarms") when vibrations are absent, which are believed to be an experimental analogue of MUS. This thesis presents a series of studies examining the potential for training to change false alarm rates on the SSDT and other perceptual tasks, with a view to informing MUS treatment.

The training was based on operant conditioning and its effects on SSDT performance were investigated in four studies. Each study had two conditions (control vs. training) and three phases (baseline vs. manipulation vs. follow-up). Studies 1 and 2 used a within-subjects design. Studies 4 and 5 used a between-subjects design and addressed the methodological limitations of Studies 1 and 2. Using money as a reinforcer, Studies 1 and 4 randomly rewarded 50% of hits and punished 50% of misses in the manipulation phase of the experimental condition, with the aim of increasing the false alarm rate for individuals initially low in false alarms. Studies 2 and 5 randomly rewarded 50% of correct rejections and punished 50% of false alarms in the manipulation phase of the experimental condition, with the aim of decreasing the false alarm rate for individuals initially high in false alarms. Training in Studies 1 and 4 significantly increased false alarm and hit rates, made response criterion more liberal, but did not change sensitivity between the conditions. Training in Studies 2 and 5 significantly decreased false alarm and hit rates, made response criterion more stringent, but did not change sensitivity between the conditions. A new voice-hearing task was developed in Study 3 taking into account the limitations of existing paradigms and was used to examine transfer of training in Studies 4 and 5. Effects of training did not transfer to a second tactile task called the spontaneous sensation test but there was some evidence for transfer on the voice-hearing task, although the effects fell short of statistical significance. Voice false alarms correlated positively with the SSDT false alarm rate in the baseline phase, suggesting that a common mechanism underlies illusory perception in the different senses.

Study 6 was guided by the idea that successive SSDT trials would be sequentially dependent due to the activation of somatosensory schema underlying SSDT responses. All four SSDT studies were thus examined to see if responses in current trials were affected by stimuli and responses in previous trials. Data indicated that current yes responses were more likely to be preceded by another yes response or the presence of vibration on N-1 trials, whereas the light had no such effect. Sequential dependencies were approximately normally distributed and some were affected by the SSDT training. A significant positive relationship was found between somatization and sequential effect of N-1 light on misses, although no such association was found for SSDT false alarms. My findings add to previous efforts to train participants on the SSDT. The conditioning paradigm can potentially be used independently or in combination with other procedures aiming to change somatic experiences. The voice-hearing task is a useful paradigm to study psychotic phenomena and cross-modal transfer. In sum, the studies strongly suggest that illusory perceptual experiences are trainable, which might have important implications for the treatment of MUS.

Declaration

No portion of the work referred to in the thesis has been submitted in support of an application for another degree or qualification of this or any other university or other institute of learning.

Copyright Statement

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Dedication

To

Izazul Huque & Rawshanara Begum

(My parents)

CHAPTER 1

General Introduction. Medically Unexplained Symptoms: Theories and Studies

> Confident and armed with index cards, I looked out at the fifty or so friends and colleagues of my father's who had gathered around the memorial Norway spruce, launched into my first sentence, and began to shudder violently from the neck down. My arms flapped. My knees knocked. I shook as if I were having a seizure. . . . When the speech ended, the shaking stopped. I looked down at my legs. They had turned a deep red with a bluish cast.
>
> —Siri Hustvedt, The Shaking Woman or A History of My Nerves, pp. 14-15

1.1 Symptoms

Oxford Dictionaries Online (2016) defines *symptom* as "a physical or mental feature which is regarded as indicating a condition of disease, particularly such a feature that is apparent to the patient." The definition implies that symptoms are subjective, that is, they depend on personal beliefs and feelings rather than on facts (Wessely, Nimnuan, & Sharpe, 1999). Pennebaker (1982) took a broader view, defining a physical symptom as "a perception, feeling, or even belief about the state of our body" (p. 1). According to him, symptoms can, but may not, give information about our internal bodily states, and are often, but not always, caused by physiological processes in the body itself. The difference between a bodily sensation and a somatic symptom is in how we evaluate them: symptoms are sensations that we evaluate as indicating disease. This means that previously neutral sensations may become distressing if they are recognized as symptoms of serious illness and, conversely, worry about sensations arising from disease may be minimal if they are perceived as irrelevant and harmless to health and well-being. It follows that our perceptual and cognitive processes are important in determining whether an experience is perceived as a sensation or a symptom.

Dodd et al. (2001) described three interacting components of symptom experience: psychological, physiological, and sociocultural. The psychological component includes how individuals make sense of bodily symptoms. People become concerned with symptoms if they feel different from what they usually feel or think of as normal. They evaluate the severity and seriousness of symptoms to make

decisions about whether and how to seek medical help. It is known that such perceptual and cognitive processes may have a direct physiological effect on the body in their own right. For example, perceiving and evaluating dyspnea as threatening may affect respiration itself, which may in turn worsen the perception of symptoms and subsequent breathing (Dodd et al., 2001). Culture also influences the understanding, experience, and meaning of symptoms. For example, in Iranian culture, "heart distress" is seen not just as a sign of disease but also as an expression of negative mood (Good, 1977).

Symptoms are one of several different sources of information that have been used for centuries in medicine to diagnose, treat, and cure diseases (Silverman, 2007; Woodside & McClam, 2011). They play a crucial role not only in diagnosis of diseases but also in the self-management of health. Failure to notice unusual symptoms may result in a delay in seeking medical help, which might increase the severity and complexity of the problem to the point where it becomes life-threatening. On the other hand, misperception or excessive concern with symptoms may expose an individual to unnecessary and potentially harmful (due to the risk of side effects) medical investigations and medications, and lead to inappropriate use of health resources (Broadbent & Petrie, 2007).

Though symptoms are important in medicine, it is often difficult to measure them accurately because of their subjective nature (Elling & Elling, 2003). For example, pain thresholds vary among individuals and thus it is often problematic to determine the exact level of pain a person is experiencing. Therefore, along with symptom reports, physicians often examine their objective counterparts or *signs*. Stedman's medical dictionary (as cited in Falen & Liberman, 2006) defines a sign as "an objective symptom of disease discovered on examination of the patient by the physician" (p. 69). Signs are usually noticeable to others and can be assessed accurately with diagnostic equipment, such as a thermometer, sphygmomanometer, stethoscope, x-ray machine, etc.

1.2 Studying Physical Symptoms

Information about physical symptoms can be found from four main sources: patients' self-report, physician observation, physical examinations, and laboratory investigations. For research purposes, the most appropriate source or method of data collection depends upon the reason for collecting it, that is, the study objectives or research questions (Flowerdew, 2009; Kumar, 2011). Standard medical practice favors diagnosis based on medical history alone as it is less time consuming and more cost-effective than "blind physical exams and random tests" (Tan & Tombs, 1999, p.184). Physical and biochemical investigations are, however, suitable when research is about the organic basis of a disease. For example, Andersson et al., (1996) analyzed blood samples to investigate the physiological changes associated with electric hypersensitivity. Questionnaires or rating scales, on the other hand, are more practical and appropriate for collecting a wide range of health-related data including the severity of illness, distress, disability, satisfaction with care and risk factors (Saw & Ng, 2001). In addition, self-report instruments are essential to study abstract or theoretical constructs in healthcare science (Kimberlin & Winterstein, 2008). For these reasons, researchers depend heavily on self-report measures to study physical symptoms.

Diverse self-report measures have been used to assess physical symptoms, ranging from measuring common symptoms or bodily sensations, such as eyes water, running nose, twitching, etc. (Pennebaker, 1982) to those related to psychopathology, such as fainting spells, bodily pain, nausea, diarrhea, etc. (Bauer, Chen, & Alegría, 2012). Most physical health questionnaires ask respondents to indicate the frequency with which they have experienced different somatic symptoms. Zijlema et al. (2013) reviewed 40 of these questionnaires and found wide variation among them in terms of questionnaire structure, psychometric properties, and usability. They recommended researchers use the Patient Health Questionnaire-15 (Kroenke, Spitzer, DeGruy, & Swindle, 1998) and Symptom Checklist-90 (Derogatis, Lipman, Rickels, Uhlenhuth, & Covi, 1974), as both have good psychometric properties and are convenient for large-scale studies.

Both clinicians and non-clinical individuals can collect data on physical symptoms (Cournos, Lowenthal, & Cabaniss, 2008). However, research objectives and materials dictate who should administer health measurements. For medical purposes, the presence of a clinician is necessary to judge the meaning of symptoms (McDowell, 2006) or to evaluate the physical signs of a disease. On the other hand, trained non-clinician interviewers can carry out structured survey or interview questionnaires (McDowell, 2006). Even the presence of a researcher or interviewer is not necessary for self-administered questionnaires, which participants can answer themselves (Rubin & Babbie, 2009). Questionnaire measures of physical symptoms usually investigate the beliefs, feelings, or judgments of participants about perceived health and bodily symptoms (Sajatovic & Ramirez, 2012). Therefore, it is not considered essential in somatic symptom research to check whether there is an objective, medical explanation for symptoms by carrying out physical examinations or medical tests.

To study symptoms with questionnaires, emphasis is placed on correct and unbiased recording of respondents' answers. Though self-report measures have limitations, it is possible to collect quite useful information on somatic symptoms if researchers have adequate training on data collection and use psychometrically sound questionnaires (Ferraro & Su, 2000). An important use of subjective measures is that they allow researchers and practitioners to gain insight into a patient's perception of symptoms, which is not possible to get from laboratory or physical examinations (McDowell, 2006). In addition, they can be useful for screening people for health problems (Peveler, Kilkenny, & Kinmonth, 1997).

Experimental procedures have also been used to study bodily symptoms. For example, studies have used cholecystokinin tetrapeptide (CCK-4) intravenously to provoke symptoms of panic (Ströhle et al., 2005; van Megen, Westenberg, Den Boer, & Kahn, 1996). Van den Bergh et al. (1999) conditioned aerosols of butyric acid and ammonia (neutral stimuli) with a CO_2 enriched air mixture (unconditioned stimulus) to understand the role of classical conditioning in multiple chemical sensitivity. Melzig, Holtz, Michalowski, and Hamm (2011) used a fast breathing exercise to induce

physiological arousal and panic symptoms in order to study defensive mobilization in highly anxiety sensitive individuals. An obvious advantage of experimentation is that it establishes cause-and-effect relationships between variables. However, conducting true experiments to investigate medically significant symptoms is challenging and potentially unethical. The reason is obvious: any increment in physical symptoms because of an experimental procedure could produce problems in control groups or add to the suffering of patients and might worsen their perceived health status.

1.3 Symptoms: Models and Findings

The dominant model for understanding illness, health, and medicine in the present day is the biomedical model of health (Wood, 2012). The root of this model goes back to the works of Hippocrates, Galileo, and Descartes (Barkway, 2009; Wood, 2012). Its influence subsided as the concepts of witchcraft and demonic possession grew in the Dark Ages and Renaissance but was then revived in the nineteenth century during the public health movement (Barkway, 2009). The biomedical model asserts that illness and associated signs and symptoms are the result of disturbances in normal physiological processes due to genetic predispositions, injury, biochemical imbalances, viral or bacterial infection etc. (Lyons & Chamberlain, 2006). The model also asserts that disease can be diagnosed objectively with scientific methods (Walsh, 2004) and treated accordingly with medical interventions including drugs, medical devices and techniques (Ogden, 2012). This approach, however, does not consider social processes (Lyons & Chamberlain, 2006) or what patients feel or believe about their symptoms (Silverman, 2007).

Engel (1977) criticised the biological reductionism of the biomedical model and instead proposed an alternative explanation, the biopsychosocial model of health and illness. According to this approach, diseases and related symptoms result from the interactions between biological, psychological, and social factors. By this view, heart disease can better be understood by taking into account the cellular physiology, genetic predispositions, personality, life pattern, living and work environment of an individual, rather than by just carrying out medical examinations to look for problems in the heart. Recognizing the importance of multiple factors in the development and

maintenance of diseases, the General Medical Council has recommended integrating the biopsychosocial model in undergraduate medical education in the UK (Taylor, McAvoy, & O'Dowd, 2003).

According to the Parallel Process model of health (Leventhal, 1970), physical symptoms affect the perception of health threat and trigger three types of processing: emotional, motivational, and cognitive. Somatic symptoms bring about negative emotions, which make an individual fearful about their health condition. Symptoms also activate the cognitive system, whereby individuals try to understand what the symptoms mean (cognitive label) and how dangerous they are for their health. The motivational system then directs individuals to take steps to deal with fear and danger. Individuals then formulate strategies to reduce the health threat based on their understandings, beliefs, and interpretations of somatic symptoms.

The Common Sense model of self-regulation (Leventhal, Brissette, & Leventhal, 2003) views symptoms as the starting point of illness representation. According to this model, symptoms are the names or identities people give to their abstract experiences of health and illness. There are three sources of information that affect symptom experiences. These include disease or one's own physiological state, observation of illness in others, and the media. People use different heuristics to label, differentiate, and categorize information as symptoms. One of the heuristics is called the symmetry rule, which is used to identify or label any changes to the stable physiological state. Labelling symptoms is the first step toward deciding on what coping strategies and action plans individuals will adopt to reinstate health. People's understanding and decision about their bodily state, symptoms, and coping may change with the availability of new information or when interventions fail to cure disease.

Studies have identified a number of psychological factors that may influence the perception and reporting of physical symptoms (Pennebaker, 2000). For example, people may feel more fatigue (Fillingim & Fine, 1986) and heart palpitations (Pennebaker, 1981) and cough more (Pennebaker, 1982) in boring environments than in stimulating ones. Similarly, isolation or lack of social support may contribute to

symptom reporting (Biordi & Nicholson, 2013; Eastwood et al., 2013). These findings support Pennebaker's (1982) proposition that increased attention toward the self (due, for example, to a lack of environmental or social stimulation) facilitates the person's awareness of internal bodily cues and thus the perception of more physical symptoms. Health related beliefs, knowledge, and information also influence whether or what symptoms people will perceive. For example, compared to non-medical students, first year medical students report significantly more symptoms of the diseases they study (Moss-Morris & Petrie, 2001). It has also been reported that one may feel symptoms because of becoming familiar with an overt and unexplained illness or having a friend with psychogenic disease (Pennebaker, 2000). Even fake or fabricated symptom reporting may increase the subsequent identification and reporting of "genuine" symptoms (Merckelbach, Jelicic, & Pieters, 2011).

Reporting of symptoms may depend on an individual's age, gender, personality, and life experiences (Pennebaker, 2000). For example, studies have found that women report more symptoms than men (Murray, Dunn, & Tarnopolsky, 1982), individuals with high negative affectivity experience more symptoms than others (Suls & Howren, 2012), and victims of traumatic events, such as childhood trauma, interpersonal violence, injury, death of a loved one etc. report symptoms that may persist for many years (Chapman et al., 2012). Age is another factor that might affect perception and interpretation of symptoms. Studies find that elderly people (over 65 years of age) are more alert to physical symptoms and more likely to seek medical help than middle-aged people (45-55 years old; Leventhal, Leventhal, Schaefer, & Easterling, 1993; Leventhal, Easterling, Leventhal, & Cameron, 1995). These findings have been explained with the principles of conservation: a self-management strategy against somatic vulnerabilities and diseases that seems to be highly active during old age (Leventhal et al., 1993; Leventhal et al., 1995). Adult attachment style (enduring cognitive schemas about self-worth for receiving care and trustworthiness of others for providing care) is also related to symptom reporting, with patients with preoccupied and fearful attachment styles reporting more somatic symptoms than those with secure attachments (Ciechanowski, Walker, Katon, & Russo, 2002).

Each disease has its own pattern of accompanying symptoms, though they often overlap with those of other diseases (Fullick, 1998). Also, the extent of symptoms for the same disease may vary from one individual to another (Schneider et al., 2012).

1.4 Medically Unexplained Symptoms

Many of the symptoms encountered in medicine cannot be attributed to medical illness or injury, and thus are called medically unexplained symptoms (MUS). Joint pain, back pain, pain in the extremities, headache, and abdominal and cardiovascular symptoms are the common MUS found in the general population (Rief, Hessel, & Braehler, 2001). In more specialized settings, different diagnostic labels have been used to identify common MUS. For example, somatoform disorder (psychiatry), non-cardiac chest pain (cardiology), irritable bowel syndrome (gastroenterology), non-epileptic attacks and functional weakness (neurology), fibromyalgia (rheumatology), chronic pelvic pain (Gynaecology), atypical facial pain (dentistry), hyperventilation syndrome (respiratory medicine), multiple chemical sensitivity (allergy), etc. (see Brown [2007] for more details). The prevalence rate of MUS is very high. A Dutch study found DSM-IV somatoform disorders in 16.1% patients (N = 1046) (De Waal, Arnold, Eekhof, & van Hemert, 2004). In primary care practices, up to two third of the reported symptoms were identified as MUS (Steinbrecher, Koerber, Frieser, & Hiller, 2011). MUS (investigated as somatic manifestation of psychological distress) are global phenomena found in different cultures across geographical locations and economic development (Simon, Gater, Kisely, & Piccinelli, 1996). The cost (including healthcare use and productivity loss) associated with this disorder is very high. A UK study estimated it to be over £14 billion in 2008-2009 (Bermingham, Cohen, Hague, & Parsonage, 2010).

Though, MUS are very common in medicine, practitioners and researchers differ in their understanding and approaches to these conditions (Creed et al., 2010). Typically, the concept of MUS pertains to any symptoms that are persistent, debilitating (i.e., there is significant suffering and/or disability) and not explainable with reference to physical disease. A recent study suggests that the presence of three or more somatic symptoms is clinically significant, regardless of whether they are

explained or not (Escobar et al., 2010).

MUS are not specific for a particular body system, rather they are heterogeneous and can be seen in different parts of the body (Greco, 2012), either alone or in combination with other types of medical and psychiatric problems (Price, 2012). Kirmayer and Robbins (1991) described three types of MUS: (i) MUS in multiple systems of the body, so-called functional somatization; (ii) persistent and excessive illness worry along with MUS, known as hypochondriacal somatization; and (iii) MUS with comorbid psychiatric disorders like anxiety and depression, or presenting somatization.

Though MUS are common (Howman, Walters, Rosenthal, Good, & Buszewicz, 2012), they are highly controversial (Reid, Whooley, Crayford, & Hotopf, 2001). Even the most appropriate name for these symptoms is a source of dispute (Creed et al., 2010). Over the years, a number of other labels have been used for MUS, such as hysteria, psychogenic, non-organic, somatization, somatoform disorders, functional somatic symptoms, and conversion (Edwards, Adams, Brown, Pareés, & Friston, 2012; Greco, 2012). For the purpose of this thesis, I choose the term "MUS" because it is well-known among practitioners and researchers (Creed et al., 2010), self-explanatory (i.e. the name itself defines the symptoms that it refers to), and fits well with the aim of this thesis, which is non-diagnostic but the enhancement of academic understanding.

Unexplained somatic symptoms were a defining feature of somatoform disorders in DSM-IV-TR (American Psychiatric Association, 2000). However, DSM-5 asserts that the concept of MUS is unreliable and fosters mind/body dualism (American Psychiatric Association, 2013b). In addition, what is unexplained now might be explained in the future with new scientific discoveries. The opposite is also true. For example, formerly it was believed that most cases of chronic fatigue were caused by chronic infection with Epstein-Barr virus. However, studies have rejected this idea and therefore these disorders presently have no medical explanation (Straus; as cited in Sharpe & Carson, 2001). Similarly, the belief that stress causes peptic ulcer was discarded after the discovery of Helicobacter pylori (a type of bacteria) and nonsteroidal anti-

inflammatory drugs as the most likely causes of the disease. However, recent findings suggest that bacterial infection or use of drugs are not the sole explanations for peptic ulcer, but stress indeed contribute to the disease (Levenstein, Rosenstock, Jacobsen, & Jorgensen, 2015; Olson & Abeysinghe, 2014). Because of such difficulties, MUS have been excluded as a defining feature in the newly formed category of somatic symptom disorder (which replaces the somatoform disorders) in DSM-5. However, they remain key diagnostic features of conversion disorder (i.e. functional neurological symptom disorder) and pseudocyesis (phantom pregnancy) as in these cases it can be shown with certainty that the reported symptoms do not conform to medical pathophysiology (American Psychiatric Association, 2013a).

1.4.1 Models/theories of MUS. Attempts to explain the pathogenesis of MUS date back over 4000 years to ancient Egypt. Since that time, a number of explanatory concepts and models on MUS have been developed. These are briefly described in the following sections with the intention not of providing a comprehensive review of their strengths and weaknesses, but simply to describe the various views that are available in the literature and some of the relevant evidence.

1.4.1.1 Dissociation. Janet's dissociation theory (as cited in Brown, 2004) was possibly the first systematic attempt to account for MUS. He suggested that unexplained physical symptoms are seen only in "hysterical" individuals when they go through traumatic or otherwise intensely emotional events. According to this approach, distress and pain resulting from trauma reduce a person's natural ability to attend to different stimuli. This constricted attention process may initiate two different mechanisms that can give rise to physical symptoms. First, as time passes, the automatic narrowing down of attention may develop into a habitual tendency that restricts awareness of incoming sensory messages, giving rise to symptoms such as unexplained sensory loss. Second, information that cannot enter consciousness due to the spontaneous narrowing of attention may form memories that are "dissociated" from other aspects of knowledge, which give rise to physical symptoms when activated by related events. Ludwig's (1972) neurological theory borrowed much from Janet's account, proposing that excessive inhibition of cortical sensory processes

leads to impairments in attention and recent memory, with inadequate processing of sensory information resulting in the development of unexplained symptoms. Although both Janet and Ludwig regarded attentional dysfunction as being responsible for unexplained symptoms, neither specified the nature of that attentional deficit in detail.

More recent dissociation models (e.g. Ludwig, 1972; Whitlock, 1967) disagree with Janet's notion that only hysterical individuals experience unexplained symptoms. These models assert that dissociations between attention and incoming sensory messages are a normal coping response in individuals experiencing traumatic events, and that symptoms develop when such dissociations are overgeneralized and used excessively. Hilgard's (1977) neodissociation theory, in contrast, posits that dissociation is a normal cognitive process, rather than an unusual response to stress or trauma. According to him, our behaviors are controlled by a number of cognitive control systems which are hierarchically organized and autonomous but interconnected. An executive ego controls all these cognitive structures and selects specific control systems to manage different events. If the functions of this executive are inhibited, unconscious fragments of the ego may form and take control of behaviour. Any behaviour, under such a state, is perceived as involuntary. Hilgard, (1977) originally used this theory to explain hypnotic behaviour but it may also describe the processes underlying unexplained neurological symptoms, such as blindness or paralysis in the absence of any organic or neurological damage (Kihlstrom, 1992).

Studies on postictal amnesia (Kuyk, Spinhoven, & van Dyck, 1999), unexplained blindness (Bryant & McConkey, 1989), unexplained paralysis (Halligan, Athwal, Oakley, & Frackowiak, 2000), and auditory hallucination (Varese, Udachina, Myin-Germeys, Oorschot, & Bentall, 2011) support dissociation theories. However, many researchers have questioned the use of the term "dissociation" to explain phenomena like depersonalization and derealization, which may have completely different pathological mechanisms (Holmes et al., 2005; World Health Organization, 1992).

***1.4.1.2 Conversion*.** Breuer and Freud (1893-1895/1991) introduced the concept of conversion as an extension of Janet's dissociative model. According to them, recalling traumatic events is highly stressful for the individual. To defend the self from stress, the brain uses unconscious mechanisms, such as repression, to hide traumatic memories from conscious awareness. Breuer and Freud suggested that this process prevents neural energy associated with traumatic experiences from being discharged and that the defense mechanisms disrupt the energetic balance of the brain. To maintain the balance, the problematic energy is unconsciously converted to somatic symptoms that are in some way an expression of the traumatic event.

Breuer and Freud's view about the development of conversion disorder is supported by a number of studies. For example, Roelofs et al. (2002) compared traumatic experiences of conversion disorder patients to that of affective disorder patients. Patients with conversion disorder reported more traumatic events, such as physical and sexual abuse than the comparison group. Roberts et al. (2012) found significantly more somatic symptoms in psychogenic non-epileptic seizure patients with prior trauma compared to seizure-free individuals with posttraumatic symptoms. Sharpe and Faye (2006) reviewed 34 studies and found a link between child sexual abuse and non-epileptic seizure. Kranick et al. (2011) investigated previous life stress in patients with psychogenic movement disorders. They found significantly higher level of trauma and fear related to childhood abuse in these patients than healthy controls and patients with focal hand dystonia. A recent neurological study (Bryant & Das, 2012) demonstrated that treatment of hysterical mutism restored functional connectivity between speech network and networks that regulate anxiety. This finding suggests a relationship between MUS and the inhibition of emotion-related neural connectivity as is proposed in the conversion model. However, many have expressed doubts about the idea of unconscious symbolic expression of traumatic events as physical symptoms (Ron, 1994; Wessely, 2001), since psychological conflicts cannot be found in some patients and the emphasis on symbolic representation has the risk of creating and inducing false memories of traumatic events in patients (see, e.g. Lindsay & Read, 1994).

1.4.1.3 Somatization. Kirmayer and Taillefer (1997) have proposed a biopsychosocial model of somatization, which is also useful to explain normal illness behavior. According to this model, normal illness symptoms and everyday physiological changes may become disabling due to maladaptive cognitive and emotional responses, particularly in difficult or unhelpful social contexts.

According to this approach, individuals usually feel bodily sensations due to disease, negative emotional states, and everyday physical perturbations. They try to assess any risks the symptoms convey and accordingly make decisions about whether to see a physician. Past experiences, responses of friends and family, and interactions with social institutions, such as work, insurance, healthcare agencies etc. influence the decision-making process. Individuals become anxious and demoralized if the symptoms seem to indicate physical illness. To reduce anxiety and to become certain about their physical condition, they seek medical help and sometimes go through different assessment and treatment procedures. These are considered as normal illness behavior, which may turn into a distressing psychological and physical state if individuals are high in neuroticism and have maladaptive thoughts and beliefs about health and illness. People high in neuroticism become excessively anxious and worried about their physical condition. Such an affective state is aggravated when individuals start to believe and interpret the symptoms as evidence of life threatening disease. This, in turn, makes them more worried, more attentive to symptoms, and more likely to interpret their bodily state catastrophically. Thus, they fall into a vicious cycle of physiological, psychological, and social forces which eventually is expressed as somatization (i.e., experiencing multiple somatic symptoms from different bodily systems).

This model gives a good account of the development and maintenance of unexplained physical symptoms. However, according to Brown (2004) it is limited in at least three ways. First, the model cannot differentiate between presenting, hypochondriacal and functional somatization (Kirmayer & Robbins, as cited in Brown, 2004). Second, this model cannot explain many medically unexplained complaints, such as unexplained neurological symptoms (e.g., seizures in the absence of any

neurological abnormality). Third, the basic assumption that medically unexplained symptoms always result from psychological distress is not well established empirically.

1.4.1.4 Damasio's "off-line" body image and "as if" loop. Damasio (1994) proposed the ideas of the "off-line" body image and "as if" loop to explain how neural networks can influence the feeling of emotion without any active involvement of the peripheral body. According to this model (see Figure 1.1), our body state is represented across a wide range of brain areas, including somatosensory cortices including the insula and parietal regions, as well as the limbic system, hypothalamus, and brain stem. Two types of body maps are said to be formed in these areas, namely "on-line" and "off-line". On-line body maps represent the current state of the body, whereas off-line body maps indicate what the body usually feels like, rather than what is happening now. In other words, the off-line body image is a stable representation of the body in our memory. Damasio proposes that the activation of the off-line body representation may give rise to false sensations, as in "phantom limb" phenomena.

The "as if" loop is an extension of the concept of off-line body representation. Damasio suggests that the brain has neural structures that can give us the impression of "as if" we are in an emotional state and activate corresponding body parts accordingly. Usually, there are afferent and efferent connections between the brain and body. However, the "as if" neural loops residing in parietal, limbic, and frontal centers of the brain do not have any direct link with the body. Such neural circuits develop through repeated association between the actual body state and resulting mental images. According to Damasio, activation of the "as if" loop will create an impression of feelings in our mind/brain, even though there is no corresponding bodily state. Taken together, these ideas have the potential to explain MUS though very few studies have been carried out to test the propositions of Damasio's approach. However, some preliminary research findings are in accordance with the theory (Henningsen, 2003). For example, Naliboff et al. (2001) found elevated activation of rostral anterior cingulate and posterior cingulate cortices in response to anticipated but not delivered stimulation in patients with irritable bowel syndrome.

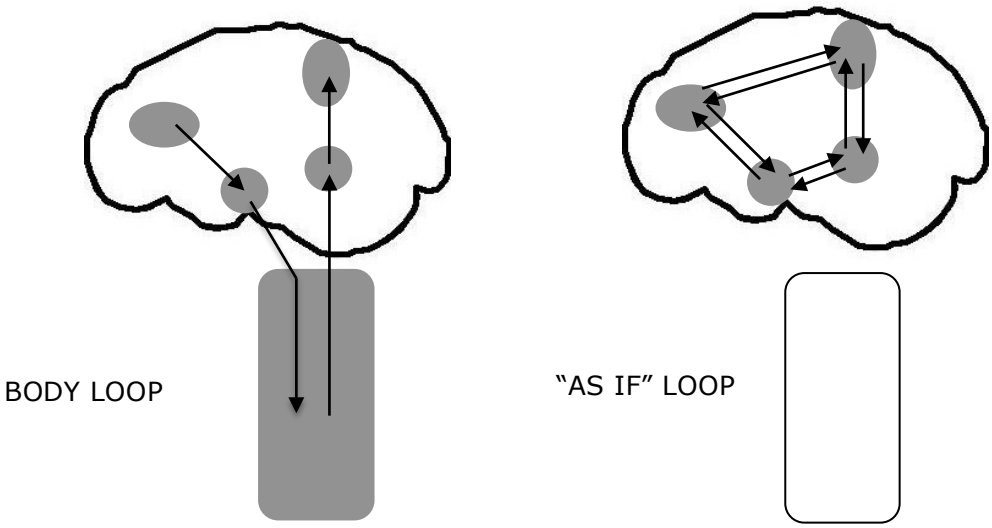


Figure 1.1. A diagram of the "body loop" and of the "as if" loop. The upper structure represents the brain and the bottom cylindrical structure represents the body. The "as if" loop operates independent of the body. Adapted from "Descartes' Error: Emotion, Reason and the Human Brain," by A. R. Damasio, 1994, p. 156.

1.4.1.5 The cognitive behavioral (CB) model. Beck's cognitive behavioural therapy (CBT) model is a biopsychosocial explanation of emotional distress. The central tenet of this model is that people develop beliefs (or schemas) about the self, others and the world early in life and that influences how we appraise situations and behave in them (Beck, 1976). Distorted or dysfunctional beliefs bring about psychopathology, and affects emotion and behaviour (Beck, 2011).

One recent description of a model based on the CBT approach is that of Deary, Chalder, and Sharpe (2007). According to the model, adverse experiences instigate physiological, cognitive, and behavioural responses to cope with stress. Interactions between, and excessive activation of these systems may bring out disease-like symptoms (i.e. MUS), which are perpetuated by a lack of explanation and misattribution of bodily sensations, and irrational beliefs about health and illness. Cognitive and attentional processes triggered by stressors involve the cognitive activation system, which produces a state of arousal, and the behavioural inhibition system, which stops the ongoing functioning of the attention system and redirects the resources to ameliorate the stressors. Activation of these physiological, cognitive, and

attentional systems increases sensitivity to (i.e. lowers the threshold for) threat and serves as a negative reinforcement by helping the person to handle the situation. These systems continue to operate if stressors are uncontrollable and unpredictable. This imbalances the level of hormones, metabolism, and immune functioning of the body, and brings about somatic symptoms. The cognitive system regards these symptoms as novel (i.e. medically significant) and therefore pays them more attention to process them further. This biases cognition, amplifies the symptoms further, increases sensitivity, activates biological stress systems and produces more symptoms—the vicious cycle, capable of reproduction and self-maintenance, continues. According to their model, factors that predispose individuals to MUS include genetics, early experience (e.g., negative life events), and personality (e.g., negative affectivity). Factors that precipitate MUS include major life events, such as death of loved ones, personal failure, accident, traumatic experience, etc. Physiological processes that are triggered by stressors include long-term potentiation (a type of sensitization process whereby synaptic strength is increased), activation of brain areas (e.g. hypothalamus), hormonal systems (e.g. pituitary and adrenal glands), and the hypothalamus pituitary adrenal (HPA) axis.

In accordance with the CB model, studies have found that cognition (i.e. distorted belief about health and illness) and behaviour are related to MUS. For example, Barsky et al. (2001) compared the estimation of risk for disease and health hazards between patients with and without hypochondriasis. Hypochondriacal patients overstated the risk significantly more than their counterpart. In another study, Rief, Nanke, Emmerich, Bender, and Zech (2004) found that patients with somatoform disorder report more causal explanations of illness (i.e. they have more irrational belief about health) than patients with no somatoform disorder. The behaviours of MUS patients are related to high usage of health care system. For example, Barsky, Orav, and Bates (2005) found that the use of health care resources and associated costs for patients with somatization is twice to that of non-somatizing patients. Somatizing patients also express dissatisfaction with medical care and physicians (Noyes, Langbehn, Happel, Sieren, & Muller, 1999).

Though studies support cognitive and behavioural aspects of the CB model, the proposed role of the HPA axis (Deary et al., 2007) was not found to be relevant to MUS in some studies. For example, a longitudinal cohort study in general population consisting of 741 male and female adults did not find any relationship between functional somatic symptoms and HPA axis function as measured by 24-h urinary free cortisol excretion (Tak, Bakker, & Rosmalen, 2009). Rief and Auer (2000) compared cortisol scores (measured from serum after the dexamethasone suppression test, saliva, and urine) of patients with somatization syndrome, patients with somatization and major depression, and healthy controls. The groups did not differ in any of the indices (i.e. cortisol scores) of the HPA axis. However, reviewing studies including patients with multiple MUS, Rief, Hennings, Riemer, and Euteneuer, (2010) concluded that many psychobiological pathways involving immunological activation and genetic factors (which function through HPA axis response, immune activation, and other biological systems) contribute to somatization. The other aspects of the CB model outlined by Deary, Chalder, and Sharpe (2007) are consistent with other models on MUS and the evidence pertaining to those.

1.4.1.6 Cultural models. MUS are global phenomena (Gureje, Simon, Ustun, & Goldberg, 1997) but there are significant differences in their expression, interpretation, and meaning between different cultural groups (Kirmayer & Young, 1998). It is believed that each culture has its own models or explanations about illness (Kirmayer & Sartorius, 2007). These models are encoded in cognitive schema, body practice, social rules, language, and institutions and thereby affect attention processes, interpretation style, and coping patterns of individuals brought up in that culture. Thus, people living in a particular society develop a unique cognitive pattern which affects their symptom experience, reporting, help-seeking, disability, adaptation, and treatment responses (Kleinman as cited in Kleinman, Eisenberg, & Good, 1978). Studies have demonstrated that feeling and reporting distress and discomfort associated with somatic symptoms depend not only on specific physiological disturbances but also on cognitive, social, and cultural processes (Kirmayer, Dao, & Smith, 1998). For example, Hwa-Byung is a cultural syndrome that

is usually seen in Koreans and Korean immigrants. In this disorder, psychological distress (e.g. suppressed anger) is manifested as somatic symptoms (e.g. palpitations, fatigue, indigestion, muscle pains, etc.) and emotional distress (e.g. depression, anxiety, anger, panic, etc.; Lee, Wachholtz, & Choi, 2014). Brain fag is another syndrome that characterizes African students' experience of a range of emotional and somatic symptoms, such as intellectual impairment, sensory impairment, sleeplessness, pain or burning in the head and neck, etc. caused by distress associated with student life and school conditions (Ayonrinde, Obuaya, & Adeyemi, 2015). Hughes (1985) listed 185 such culture-bound syndromes. The latest edition of the Diagnostic and Statistical Manual of Mental Disorders (5th ed.; DSM-5; American Psychiatric Association, 2013a) has emphasized cross-cultural variations in distress and accordingly updated diagnostic criteria to increase cultural sensitivity, and to improve diagnosis and care for people of all cultural backgrounds.

1.4.1.7 Operant conditioning theory and symptom reporting. Operant conditioning is a type of associative learning in which acquisition of a response depends on its consequences (Skinner, 1953). Operant conditioning principles describe how to strengthen or weaken responses (Skinner, 1963). Responses are more likely to be repeated if they are followed by pleasurable consequences (i.e., a positive reinforcer) or removal of unwanted consequences (i.e., a negative reinforcer). On the other hand, responses are less likely to be repeated if they are followed by negative consequences (i.e., a positive punisher) or removal of desirable consequences (i.e., a negative punisher). The process of strengthening a response is called reinforcement whereas the process of weakening a response is called punishment.

There are different schedules of reinforcement and punishment. In a continuous schedule, reinforcers or punishments are delivered every time after the target behavior occurs. In a partial schedule, reinforcers or punishments are delivered only some of the time. Partial reinforcement schedules produce higher rates of response than continuous reinforcement schedule (Sarafino, 2012). On the other

hand, a continuous punishment schedule is more effective in response suppression than other schedules (Pierce & Cheney, 2008).

Rewards and punishments can influence where and how individuals pay attention to their bodies and the resulting feeling of symptoms (Pennebaker, 2000). Rewards that may influence symptom reporting include getting attention from others (Durand & Barlow, 2009), release from responsibilities (Husain, Browne, & Chalder, 2007), and being able to control family matters (Pennebaker, 2000) and others' behavior (Kihlstrom & Kihlstrom, 1999).

People may learn to show physical symptoms early in their life. Research found that children with recurrent abdominal pain (RAP) are more likely to have a parent with gastrointestinal disorders (Bode, Brenner, Adler, & Rothenbacher, 2003) and higher levels of health anxiety (Ramchandani, Murray, Romano, Vlachos, & Stein, 2010). Hotopf (2003) explained RAP as a learned response through the processes of vicarious or social learning. Hotopf, Mayou, Wadsworth, and Wessely (1999) tested the hypothesis that experience of childhood physical illness would be associated with later experience of MUS. The study compared MUS patients with healthy individuals of the same age group. It was found that the development of MUS was related to poor parental health and one's own unexplained illness during childhood.

Childhood parental overprotection is another risk factor for functional somatic symptoms in young adolescents (Janssens, Oldehinkel, & Rosmalen, 2009) and chronic fatigue syndrome in adulthood (Fisher & Chalder, 2003). Overprotective parents probably pay more attention to the child's complaints, meaning that the child learns to identify and report neutral body symptoms as indications of disease. This, in turn, brings more attention and concern from parents, causing the child to report more physical symptoms and setting up a vicious cycle (Walker & Zeman, 1992; Walker et al., 2006; Walker, Garber, & Van Slyke, 1995).

Behavioural theorists believe that the same learning principles are applicable for the development and maintenance of both normal and abnormal behaviour (Trull, 2005). By the same token, learning theories (Husain et al., 2007; Tazaki & Landlaw, 2006) and chronic pain research (Rief & Broadbent, 2007) suggest that MUS probably

have a learned component. According to Breuer and Freud (1893-1895/1991), unexplained physical symptoms can have reinforcing value because they may reduce anxiety in individuals in some circumstances.

1.4.1.8 Somatosensory amplification model. Amplification is "the tendency to experience somatic and visceral sensation as intense, noxious, and disturbing" (Barsky, 1992, p. 28). Amplification involves anomalies in both attention and cognitive processes. Perception and amplification of symptoms can broadly be conceptualized as a two-step process. First, heightened activation in the attention system reinforces a tendency to look for and select both unpleasant (e.g. those related to diseases) and benign (e.g. those related to normal functioning of viscera, such as changes in heart rate) bodily sensations. Second, available information, knowledge, experience, expectations, and negative affect influence whether sensations are appraised as symptoms of diseases or related to normal physiological processes.

A central tenet of this model is that amplification has both state and stable trait-like properties and that individuals differ widely in their sensitivity and evaluation of bodily sensations. This explains why some people experience and report relatively more symptoms, whereas others have few, even within the same psychiatric or medical diagnosis. Barsky (1992) believes that both nature and nurture underlie amplification—it has genetic relevance as well as a learning component (e.g. childhood experiences).

The model accounts for some of the diagnostic features of somatic symptom and illness anxiety disorders as described in The Diagnostic and Statistical Manual of Mental Disorders (5th ed.; DSM–5; American Psychiatric Association, 2013a). For example, the diagnosis of somatic symptom disorder requires that patients perceive symptoms as distressing and remain cognitively, behaviourally, and emotionally preoccupied with symptoms. Similar patient features are described in illness anxiety disorder in which relevant somatic symptoms are absent or very mild in intensity.

This model has produced mixed study findings—some studies support the predictions of the model whereas others contradict. For example, Doering et al.

(2015) found higher somatosensory amplification related to increased reporting of somatic symptoms and biased assessment of general bodily symptoms as side effects of medication. Using structural equation modeling on data from 4573 participants, a strong association was found between somatization (as measured by the Patient Health Questionnaire-15 [PHQ-15]) and somatosensory amplification as measured by the somatosensory amplification scale (Witthöft, Fischer, Jasper, Rist, & Nater, 2016). Another study found an increase in non-specific somatic symptoms, such as headaches, dizziness, pain, etc. after a new high-voltage power line became operational in the area, related to causal beliefs and negative expectations about the health effects of power lines (Porsius, Claassen, Woudenberg, Smid, & Timmermans, 2016). In line with the prediction that amplification might be related to other psychiatric disorders in which patients report somatic symptoms, a study found somatosensory amplification as a significant predictor of somatization reported by 100 outpatients with major depression diagnosed according to DSM-IV criteria (Sayar, Kirmayer, & Taillefer, 2003).

Though the model considers heightened sensitivity to bodily sensations as an integral part of amplification, a study found that sensitivity to heartbeats in a heartbeat detection task was related to somatosensory amplification in the opposite direction to that predicted by the theory (Mailloux & Brener, 2002). More specifically, somatosensory amplification scores of the individuals who could detect heartbeat sensations were significantly lower than those who could not detect their heartbeat sensations. Brown, Poliakoff, and Kirkman (2007) studied 48 nonclinical participants in an experimental task with a cuing paradigm involving picture cues (i.e. photographs of neutral and threatening scene and body parts) and visual (i.e. LED flash) and vibrotactile targets. Contrary to the prediction of the amplification model, they found a significant negative correlation between body-focused attention and somatosensory amplification. Similarly, a study on patients with fibromyalgia did not find any difference in hypervigilance (as assessed by a tactile change detection task) between the patients and a matched control group (Van Damme et al., 2015).

Overall, the study findings indicate the importance of cognitive appraisal in experiencing symptoms, but also suggest that the attentional processes as described in the model may not be adequate to account for all cases of somatization.

1.4.1.9 Signal–filtering model. Rief and Barsky (2005) proposed the signal-filtering model of somatoform symptoms. According to this model, the brain receives sensory signals from throughout the body but healthy individuals are not aware of all these sensory activities because of neural filtering mechanisms. Somatoform symptoms arise if neural filtering is reduced or sensory inputs become too strong. The model has suggested a number of biological, cognitive, behavioral, and emotional factors that can disrupt the normal processing of sensory inputs (see Figure 1.2). Selective attention is one factor that is thought to be able to influence the screening processes of the filter system. A number of studies support this prediction. For example, a study varied body signals to manipulate attention to internal body processes (Pennebaker, 1982). It was found that attention to bodily processes increased symptom reporting whereas distraction decreased it. Similarly, in another study, the reporting of somatic symptoms of hypochondriacal patients increased when they gave more attention on the body (Schmidt, Wolfs-Takens, Oosterlaan, & van den Hout, 1994).

1.4.1.10 A Bayesian model of hysteria. Edwards et al. (2012) proposed a predictive coding (i.e., Bayesian) model of "hysteria", that is, functional neurological symptoms. This model is based on the hierarchical organization and functioning of the brain, and Bayesian inference, which takes into account empirical prior beliefs to determine the probability of occurrence of an event. According to this model, a multi-level hierarchical network predicts the most likely causes of sensations (but not their contents). Its levels are interactive and operate by top-down and bottom-up processes (i.e. there are forward as well as backward connections between the levels). The levels use prior predictions (i.e. beliefs and expectations based on previous experience) to infer the cause of incoming inputs, and the precision of these predictions determines perception and action. Precision of prediction at each level depends on the degree of attention that is directed towards that level—the higher the

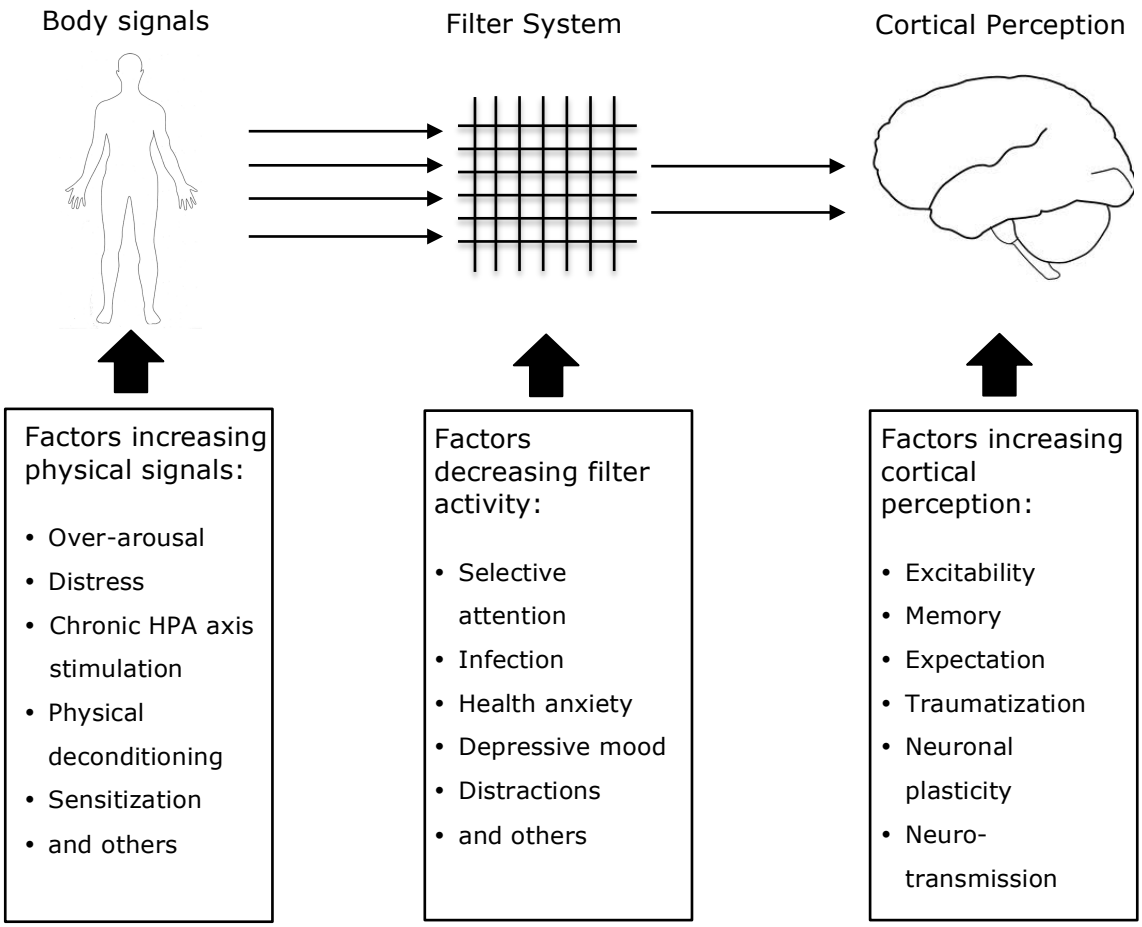


Figure 1.2. Signal-filtering model. Adapted from "Psychobiological Perspectives on Somatoform Disorders" by W. Rief and A. J. Barsky, 2005, as presented in "Explaining Medically Unexplained Symptoms-Models and Mechanisms," by W. Rief and E. Broadbent, 2007, *Clinical Psychology Review, 27*, p. 825.

attention, the higher the precision. When sensations have higher precision, they influence and change predictions at higher levels in the hierarchy.

The difference between predictions and inputs is the prediction error (or sensory surprise). A principal aim of the network is to reduce these errors, which is done in two ways: (i) by changing the content of sensations through action guided by top-down prior expectations; and (ii) by changing predictions based on the sensations (a bottom-up process). The motor system uses both processes while the perceptual system can only use the second process to reduce prediction errors. Neurologically, these processes correspond to changes in synaptic activity and synaptic strength of the neurons that encode predictions. Neuronal changes are brought about by fast

synchronous oscillatory activity and neurotransmitters, such as acetylcholine and dopamine.

According to the model, MUS may arise when attention increases the precision of pathological prior beliefs. A number of psychological factors underlie irrational beliefs about illness and undue attention to them, such as hypervigilance and excessive concern about disease, personal and cultural beliefs about health, cognitive bias (e.g. catastrophic thinking about illness, jumping to conclusion, etc.), illness experiences (e.g. viral infection, somatic symptoms, physical injury, etc. related to the self or others), and information about diseases. Neural functioning is also related to attention in a bidirectional manner. Neurons that encode pathological prior beliefs might cause undue attention, or undue attention might produce irrational prior beliefs (high in precision) that install pathology in neural functioning.

There is a slight difference between the processes that bring about medically unexplained sensory and motor symptoms. Medically unexplained sensory symptoms (such as pain, sensory loss, etc.) develop when pathological prior beliefs and expectations about illness at a high level of the hierarchy are afforded undue precision and accepted by the system as accurate predictions for random sensations (i.e. sensory noise), which in turn reinforce the sensations and thereby maintain the pathological prediction. Pyramidal cells, anterior cingulate, insula, precuneus, primary sensory cortex, and secondary sensory cortex are involved in this process. Medically unexplained motor symptoms (such as functional paralysis, tremor, etc.) develop through the same processes, but in this case an intermediate level of the hierarchy containing irrational beliefs about illness gets the highest precision and activates reflex mechanisms of the body to produce functional motor movements. Information about motor movements received in the higher level of the system does not match with existing predictions, however, resulting in perceived involuntariness and misattribution of the movements to illness. The model proposes that irrational beliefs about motor movements are held in the premotor cortex and supplementary motor area of the brain.

Propositions of the model about the origin of pathological beliefs and attention overlap with the other models on MUS. As we shall see, for example, the concept of "prior beliefs" and "precision" are equivalent to the ideas of "rouge representation" and "activation" of the integrative cognitive model of MUS (Brown, 2004) as described in the next section. The uniqueness of this model lies in its emphasis on the neurological basis of MUS. However, neurological evidence about its predictions based on patient data is not yet available.

1.4.1.11 Integrative cognitive model of MUS. Brown (2004) combined the basic elements of dissociation, conversion, and somatization models to propose an integrative account of unexplained physical illness. Brown's model identifies two features of the cognitive system that underlie the development and maintenance of MUS. First, our behaviors and perception can be under the influence of cognitive systems about which we have no awareness and thus have no control over. Second, our experiences are subjective and may not be in accordance with stimulation from the outside world. Rather, these experiences might be determined by memories from past experiences (or other mental representations) stored in the cognitive system. In other words, perception may take place merely by chronic or acute activation of mental representations, even though corresponding external stimulation is absent. Such processes, according to Brown, could give rise to MUS. For example, a person may feel pain just because of excessive activation of pain-associated memories and thus medical investigations would fail to detect any biomedical cause for the symptom.

According to the integrative cognitive model, the mechanisms underlying the generation of MUS are psychological in nature but these are not under the volitional control of the individual. Brown used the term "rogue representation" to refer to mental representations that are selected by the cognitive system as an account of what is in the world when these are actually inconsistent with sensory data. One's own experiences of physical states, observing and knowing physical states in others, prevailing conceptions of illness in society, and direct verbal suggestions from self or others may be the source of these rogue representations. Various factors may

contribute to the activation of these representations including negative affect, focusing attention on the body, interpreting benign symptoms as physical illness, illness behavior, and certain personality traits such as negative affectivity.

Lloyd, Mason, Brown, and Poliakoff (2008) developed an experimental paradigm called the somatosensory signal detection task (SSDT) in order to test predictions from the integrative cognitive model and study the somatic disturbances of MUS patients. In this task, participants place their non-dominant hand on a table in front of them. A computer monitor is kept on the table facing and centering the participant. Participants are presented with threshold-level tactile stimulation (painless vibrations) through a vibrating device (bone conductor) that is attached to the pad of their non-dominant index finger. The bone conductor and a nearby red LED (5mm) are kept mounted on a foam cube. This small light is used to present a visual stimulus (light flash). The computer monitor is used to give instructions to the participants and present a green arrow that cues the start of each trial.

The participants receive four different trial types: touch only, light only, light and touch, and no stimulus, which appear several times in a random order. In touch only trials, the tactile vibration (100Hz) is presented for 20ms in the middle of a 1020ms interval. Light only trials have a flash of the LED light for 20ms in the middle of a 1020ms interval. Both the vibration and light appear simultaneously in light and touch trials. Nothing is presented during the 1020ms interval in the stimulus absent trials. After each trial, participants indicate whether they felt any vibration by pressing 1, 2, 3, and 4 on the keyboard number pad corresponding to *definitely yes*, *maybe yes*, *maybe no*, and *definitely no* respectively. Thus the experiment produces four types of responses, namely hit (saying "definitely yes" or "maybe yes" when touch is present), false alarm (saying "definitely yes" or "maybe yes" when there is no touch), miss (saying "maybe no" or "definitely no" when touch is present), and correct rejection (saying "maybe no" or "definitely no" when there is no touch). Participants are kept naive about the significance of the light and are informed that vibration will not be present in all trials. In this experimental procedure, participants often make false alarms (i.e. report the touch as present when no stimulation has been delivered;

Lloyd, Mason, Brown, & Poliakoff, 2008). This tendency is stable, differs between individuals, and can be influenced by the presence of the light (Brown, Brunt, Poliakoff, & Lloyd, 2010; Katzer, Oberfeld, Hiller, & Witthöft, 2011; Lloyd et al., 2008; McKenzie, Poliakoff, Brown, & Lloyd, 2010). False alarm rates in the SSDT are also found to be positively related with symptom reporting (see below; Brown et al., 2012; Katzer, Oberfeld, Hiller, & Witthöft, 2011). The attentional and cognitive processes that bring about SSDT false alarms are said to be similar to the distortions in the memory and perceptual system underlying unexplained somatic symptoms as proposed in the integrative cognitive model of MUS (Katzer et al., 2011; Lloyd et al., 2008). The structure of the SSDT and possible mechanisms that generate the responses are illustrated in Figure 1.3.

The SSDT has been identified as a useful tool to test cognitive theories of MUS (Brown et al., 2010). As this task is based on signal detection theory, it is possible to separate the relative influence of perceptual and decisional components (as indicated by sensitivity and decision criterion) of responses. The stimuli are temporary benign sensations and do not bring about any clinical somatic symptoms. Therefore, it can easily be used to study healthy volunteers as well as clinical populations without some of the ethical issues posed by symptom provocation methods.

1.5 Studies with the SSDT

1.5.1 Exploring the SSDT paradigm and its features. The first SSDT study found that the task irrelevant light increased hit and false alarm rates and decreased response criterion, but did not significantly affect sensitivity (Lloyd et al., 2008). A number of studies have been carried out on the SSDT since then (see Appendix A for a summary of the studies). The majority of them examined the SSDT paradigm and its features. For example, McKenzie, Lloyd, Brown, Plummer, and Poliakoff (2012) investigated the underlying mechanisms of illusory tactile perception in the SSDT. They found that the association between false alarms and light did not form during the course of the experiment, but are more likely to be integrated in the perceptual system as a result of prior visuo-tactile experiences. In two other studies, McKenzie et al. (2010) replicated the original findings of Lloyd et al. (2008) study. In addition,

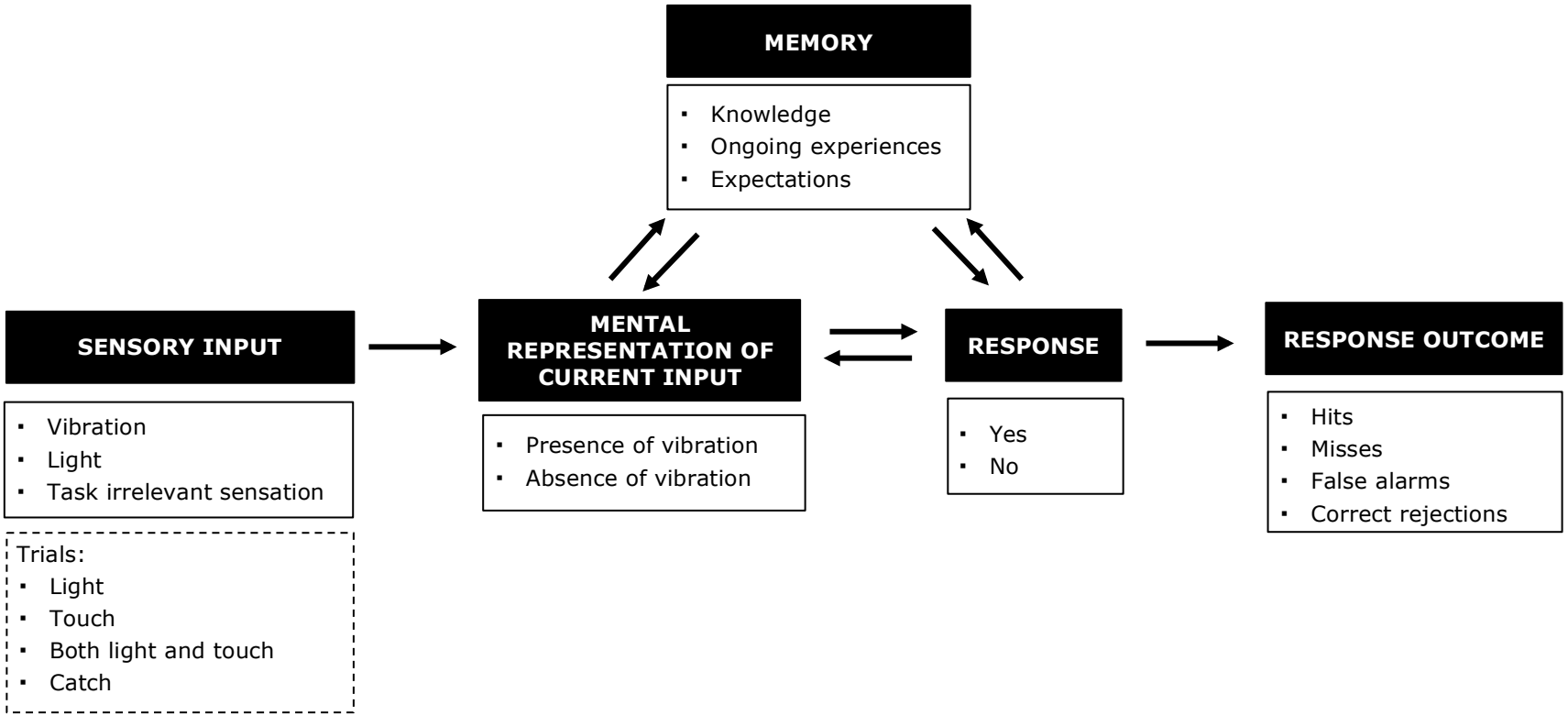


Figure 1.3. The SSDT events and underlying cognitive and perceptual processes. Interactions between the memory and perceptual systems to process sensory input produce a response which in turn provides feedback to the higher level systems.

they found increased sensitivity as a result of the light in Study 1, although this was not replicated in Study 2. The authors found similar effects when they compared visual and auditory start cues. They also examined consistency of the SSDT response outcomes across testing sessions (over an interval of a week in one condition and four weeks in another condition) and found hit rates, false alarm rates, and response criterion to be reliable over time, such that participants who were high in hit rate and false alarm rate or liberal on response criterion in the first session continued to give more hits and false alarms and remained liberal in second session. The authors interpreted the significant correlation between false alarms in separate sessions as a trait like stable characteristic. Katzer et al. (2011) modified the thresholding procedure of the SSDT and defined threshold as the intensity of vibration that could be identified correctly in 70.7% trials. They used a two- alternative forced-choice task instead of previously used one-interval ("yes/no") task to avoid the effects of possible response bias. However, their findings were similar to those of Lloyd et al. (2008). In addition, they found that threshold to detect vibration was stable over time (as measured before and after the SSDT) and false alarms were not misperceived tactile pulses in the finger. In an fMRI study, Lloyd, McKenzie, Brown, and Poliakoff (2011) found significant activation in the medial parietal and medial prefrontal cortex when participants produced false alarms compared to correct rejections; activations were not found in primary somatosensory cortex. The findings were interpreted as being consistent with the idea that top-down factors play a role in false alarms on the SSDT.

1.5.2 Clinical relevance of SSDT response outcomes. A number of studies have investigated the relationship of SSDT response outcomes with psychopathology. A significant positive relationship was found between somatic symptom severity as measured by the PHQ-15 and false alarms in the SSDT (Brown et al., 2012; Katzer et al., 2011), though this was not replicated in another study (Katzer, Oberfeld, Hiller, Gerlach, & Witthöft, 2012). False alarms were not related to trait anxiety, depression, hypochondriasis, somatosensory amplification, or the tendency to express psychopathology in the form of somatic symptoms (Brown et al., 2012), although health anxiety correlated with false alarms when trait anxiety was controlled in one

study (Katzer et al., 2011). Liberal response criterion was also been found to correlate significantly with PHQ-15 and health anxiety in that study (Katzer et al., 2011). Individuals high in somatoform dissociation as measured by the somatoform dissociation questionnaire-20 (SDQ-20) were more liberal in response criterion and produced more false alarms than low SDQ-20 individuals when their depression, trait anxiety, and somatosensory amplification was controlled (Brown et al., 2010). Katzer et al. (2012) however did not find difference in false alarm rate between participants with somatoform disorders (SFD group) and healthy individuals. The SFD group was more liberal in response criterion than controls in the 20 light absent trials of the first block. False alarms and light-modulated bias in SFD group had significant positive correlations with pseudoneurological symptoms as measured by a subscale of the Screening for Somatoform Symptoms-2 (SOMS-2, which measures medically unexplained somatic complaints during the last two years). The SFD group also had lower threshold than the control group, which also correlated significantly with PHQ-15 score, severity of depressive symptoms (on the Beck Depression Inventory-II) and health anxiety (on the Whitley Index). Katzer et al. (2011) found that sensitivity positively correlated with trait anxiety and increase in sensitivity caused by light had a significant negative correlation with total as well as vegetative scores in SOMS-2 (Katzer et al., 2012). On the whole, these clinical studies seem to support cognitive models of MUS and the potential of the SSDT as a tool to investigate symptom reporting.

1.5.3 Altering SSDT response outcomes. Different techniques have been tried out to manipulate SSDT response outcomes. For example, Brown et al. (2010) used a memory task to activate vibration-related schema. In the training phase, half of the stimuli (pictures) were accompanied by different vibrations. In the recall phase, 75% of the pictures presented to a maximal recall group were used in the training phase, whereas 25% of the pictures presented to a minimal recall group were used in the training phase. When a picture was identified as being accompanied by vibration, participants were asked to recall and assess the vibration. When a picture was identified as not being accompanied with vibration participants were asked to assess

the picture. The training, however, did not produce the desired affect—false alarm rate did not differ between minimal and maximal recall groups. McKenzie et al. (2012) used an associative learning procedure in which supra-threshold tactile stimuli and light were presented concurrently to high and low association groups. The high association group received bimodal trials three times more frequently than the low association group, who, on the other hand received the light only trials three times more often than their counterparts. Both groups received feedback on every trial. A control group did not receive any training. It was found that the low association group made fewer false alarms than both the high association and control groups. False alarm rates, however, did not differ between high association and control groups. Mirams, Poliakoff, Brown, and Lloyd (2010) studied whether vision of the body affects SSDT performance. Participants performed the SSDT under two testing conditions: in one, the hand was visible (vision condition), in the other, the arm, hand, and fingers were covered with a black cape (no-vision condition). It was found that viewing the hand significantly increased the false alarm rate but did not affect the hit rate, sensitivity, and response criterion. In another study, Mirams, Poliakoff, Brown, and Lloyd (2012) investigated whether the SSDT response outcomes can be changed with interoceptive and exteroceptive attention. In the interoceptive attention task, participants counted pulse sensations in the fingertip and received feedback about their performance. In the exteroceptive attention task, participants counted the number of times grating domes with a threshold level grating orientation were presented horizontally and vertically and received feedback about their performance. It was found that the interoceptive attention task increased false alarm rate and liberal response criterion in light absent trials and reduced the light's effect on response, but did not affect sensitivity. On the other hand, the exteroceptive attention task resulted in fewer touch reports. In general, the findings indicate that internal and external body-focused attention may have differential effects on somatic perception. Meditation was also found to be effective at changing SSDT response outcomes (Mirams, Poliakoff, Brown, & Lloyd, 2013). In this study, an experimental group practised body-scan meditation in the laboratory and also at their homes over a

period of seven days, whereas a control group listened to stories over the same period of time. The meditation was found to increase sensitivity and decrease false alarm rate, but did not affect hit rate or response criterion. In a recent study, Perera, Newport, and McKenzie (2015) replicated the findings of the first SSDT study (Lloyd et al., 2008) and also found that real-time vision of an illusory finger changed SSDT response outcomes. There were three illusory conditions: stretched finger, shrunken finger, and detached finger. The stretched finger condition increased hit rate and decreased response criterion (i.e. response criterion became more liberal), shrunken finger increased both hit rate and sensitivity, and detached finger decreased false alarm rate and increased response criterion (i.e. response criterion became more stringent). These findings suggest that distortions of the veridical view of the body can alter perceptual and decisional components associated with somatosensory illusions.

1.6 Conclusion and General Aims of the Research

A number of theories and models have been proposed to account for medically unexplained symptoms. Although these models view MUS from different perspectives, there are similarities between them. Contemporary theories, such as Damasio's "off-line" body image and "as if" loop (Damasio, 1994), signal–filtering model (Rief & Barsky, 2005), and integrative cognitive model of MUS (Brown, 2004) have emphasized anomalies in perceptual and cognitive system to account for MUS. Studies have used the SSDT to study the perceptual and cognitive processes believed to underlie MUS. The relationship between SSDT false alarms and somatic symptoms has inspired researchers to investigate whether it is possible to train participants to change their false alarm rates, which could have important clinical implications. For example, being able to train individuals to produce fewer false alarms on the SSDT might have implications for suppressing "rogue representations" in the perceptual system and thus symptom reporting (Brown, 2004).

We saw in the preceding section that researchers have tested a number of training procedures with the SSDT with mixed results. The memory manipulation technique of Brown et al. (2010), for example, failed to change false alarm rates, and

neither did training to associate suprathreshold vibrations with the light stimulus (McKenzie et al., 2012). There were methodological limitations in these studies. For example, the training protocol of McKenzie et al. (2012) was problematic as they did not consider the possibility that training with stronger (i.e., suprathreshold) tactile stimulation might not generalize to much weaker (i.e., threshold level) stimuli. In addition, they did not assign participants randomly in control group. The memory task of Brown et al. (2010) was not adequate to activate vibration-related memory that the training aimed to achieve. The body-scan meditation training of Mirams et al. (2013) reduced false alarms, but the validity of the findings might have been compromised by confounding variables and lack of control as majority of the training sessions (i.e. five out of seven) took place at home (in the absence of the experimenter).

Taking the shortcomings of previous training procedures into consideration, a potential alternative might be to use operant conditioning for the training. A number of studies (see Appendix B for a summary of these studies) found that reinforcement and punishment changed sensitivity and bias in visual and auditory signal detection tasks (e.g. Johnstone & Alsop, 2000; Lie & Alsop, 2010). The findings of these studies suggest that operant conditioning principles could be effective to alter responses on the SSDT. In this thesis, I present four studies looking into this possibility and related two additional studies. The principal aim that guided these studies was to develop and test a training protocol that can be used to alter somatosensory perception in the SSDT, specifically false alarms. The primary objective of Studies 1 and 4 (see Chapters 2 and 5) was to investigate whether people who rarely false alarm on the SSDT can be trained to make more false alarms using a training procedure based on operant conditioning principles. The objective of Studies 2 and 5 (see Chapters 3 and 6) was to investigate whether the tendency to false alarm on the SSDT could be reduced using operant conditioning. As perceptual training generalizes to other perceptual experiences (Bratzke, Seifried, & Ulrich, 2012; Liu & Weinshall, 2000; Ragert, Schmidt, Altenmüller, & Dinse, 2004), the second objective was to investigate whether training on the SSDT transfers to affect responses on a different perceptual

task. In accordance with this objective, a new task called the voice-hearing task has been developed and validated in Study 3 (see Chapter 4) to use it with the SSDT in Studies 4 and 5. The third objective was to investigate whether false alarm rates were related to somatic symptoms as found in the previous SSDT studies. All four SSDT studies also investigated the other SSDT response outcomes, such as hits, bias, and sensitivity. As it is very common in psychophysical judgment tasks that previous events (i.e. stimuli and responses in previous trials) affect current responses (Lockhead, 1992), the objective of Study 6 was to examine whether responses in current SSDT trials depended on events (i.e. response, presence of vibration, and presence of light) in previous trials, and whether training altered this relationship. To this aim, all the SSDT data (from studies 1, 2, 4, and 5) were processed and analyzed, and sequential dependency between current responses and preceding events was examined (see Chapter 7).

CHAPTER 2

Study 1: Can Illusory Somatosensory Experiences be Increased With Training?

> . . . and then came the violent migraine that lasted for almost a year, the year of Fiorinal, Inderal, Cafergot, Elavil, Tofranil, and Mellaril, of a sleeping-drug cocktail I took in the doctor's office in hopes that I would wake up headache-free. No such luck.
>
> —Siri Hustvedt, The Shaking Woman or A History of My Nerves, p. 16

All people, at one time or other, experience physical symptoms, such as joint pains, back pains, headaches, fatigue, dizziness, racing heart, etc. (Broadbent & Petrie, 2007). These are seen in healthy individuals as well as in people diagnosed with different types of physical and psychological disorders (Pennebaker, 2000).

The causes of bodily symptoms are often determined using different diagnostic tests. In many cases, however, a symptom's causes may not be evident (Hansen, Rosendal, Fink, & Risør, 2013). Indeed, a large proportion of symptoms are actually medically unexplained (Nimnuan, Hotopf, & Wessely, 2001) and the correlation between physical symptom reports and objective health status is actually quite low (Pennebaker, 1982).

The mechanisms of these medically unexplained symptoms (MUS) are poorly understood. According to the integrative cognitive model, many MUS are distortions in somatosensory experience caused by the over-activation of mental representations in the cognitive system (Brown, 2004). To test the predictions of this model, and to study the somatic disturbances of MUS patients, Lloyd et al. (2008) developed an experimental procedure called the somatosensory signal detection task (SSDT). In the SSDT, participants are presented with a threshold level tactile pulse to the index finger of their non-dominant hand in half of the trials and no stimulus in the rest of the trials, either with or without the concurrent presence of a non-target LED light. The participant's task in each trial is to give a "yes" or "no" response to indicate whether they perceived the tactile stimulation, resulting in four types of responses - hits (saying "yes" when touch is present), false alarms (saying "yes" when there is no touch), miss (saying "no" when touch is present), and correct rejection (saying "no"

when there is no touch). Studies with the SSDT have found that participants have a tendency to report feeling the vibration even when there is no tactile stimulation, particularly when a concurrent light is present. Such "illusory touch" experiences (i.e., false alarms) are believed to be similar to MUS (Lloyd et al., 2008) in that both are caused by the same memory activation processes as proposed in the integrative cognitive model of MUS (Brown, 2004). Studies have found that the false alarm rate is normally stable over time (McKenzie et al., 2010) and correlates with symptom reporting (Brown et al., 2012; Katzer et al., 2012).

Mirams et al. (2010) examined the effects of attending to the body on SSDT response outcomes and found that viewing the body increased false alarm rates in light present trials. To investigate whether the false alarm rate could be altered with training, McKenzie et al. (2012) used a training protocol to form two different levels of association (i.e. weak and strong) between suprathreshold vibrations and the light stimulus. The results indicated that the false alarm rate was no different in a strong association group compared to controls; in contrast, a weak association group had fewer false alarms than the controls, regardless of whether the light was present or not. In addition, training did not affect the identification of vibration when it was actually present (i.e., the hit rate). Limitations of this study include the fact that participants were not randomly assigned to the control group, and a failure to account for the association between the light and vibration extinguishing over time. In addition, the authors overlooked the likelihood that the contiguity between the light and threshold level vibration in experimental (i.e. the SSDT) trials might interfere with the effects of any previous associations that had been formed between supra-threshold tactile and visual stimuli during the training.

An alternative approach to investigating the effects of training on the SSDT would be to use operant conditioning. A number of studies with human participants have reported how the use of reinforcement and punishment changes performance in signal detection tasks. For example, Lie and Alsop (2009) rewarded correct responses by giving points and punished incorrect responses by deducting points in a visual perception discrimination task. They found that both reinforcement and punishment

influenced response criterion and increased sensitivity, specifically when they were combined in a schedule. In another visual discrimination study, participants avoided the response that was associated with a high rate of punishment (Lie & Alsop, 2010). Bias and sensitivity also depend on the frequency of reinforcement and type of reinforcer being used. For example, controlled reinforcement procedures produced a general pattern of bias and ROC plot (i.e. the response criterion remained relatively stable over changes in discriminability), whereas uncontrolled reinforcement procedures brought about variable patterns of bias and ROC points in a visual perception discrimination task (Johnstone & Alsop, 2000). In a two-choice signal detection task, training with monetary reward was more effective than training with points to change response bias (Johnstone & Alsop, 1996). Though signal detection studies predominantly used visual tasks to study the effects of reward and punishment, it is likely that the same operant conditioning principles would be equally applicable to signal detection tasks in other modalities, such as the SSDT, which is used to study somatosensory experiences. This possibility (i.e. whether operant conditioning would affect responses to the SSDT) was investigated in the current study.

Studies have found that perceptual learning generalizes to separate stimuli and tasks. For example, training to detect the motion directions of two stimuli in an easy motion direction task improved performance in untrained directions and, with practice, the learning generalized to a more difficult task in a new direction (Liu & Weinshall, 2000). Ragert et al. (2004) compared the tactile discrimination thresholds of professional piano players and non-musician controls on tactile tasks irrelevant to piano playing. It was found that discrimination thresholds on the tip of both index fingers of highly trained piano players were significantly lower than the controls. This superior discrimination ability (in a task unrelated to piano playing) had a significant positive correlation with the amount of daily piano training with both the fingers. These findings, although unrelated to the SSDT, suggest that training in the SSDT may transfer to other tactile experiences, such as perceiving spontaneous sensations as measured by the spontaneous sensation (SPS) test (Michael & Naveteur, 2011;

Michael et al., 2012). Further support for this possibility comes from the finding that interoception (i.e. awareness of internal bodily state) affects SPSs in the SPS test (Michael, Naveteur, Dupuy, & Jacquot, 2015), and both response criterion (Mirams et al., 2012) and sensitivity (Mirams et al., 2013) in the SSDT, which suggests that there might be common perceptual processes underlying these tactile experiences. This likely transfer effect of the SSDT training was also studied in the present experiment.

Previous studies reported a significant relationship between false alarm rates on the SSDT and severity of somatic symptoms as measured by the Patient Health Questionnaire-15 (PHQ-15; Brown et al., 2012; Katzer et al., 2011). However, the relationship was not found in the study by Katzer et al. (2012). To explore this possibility (i.e. whether somatization is related to somatosensory distortions on the SSDT) and to examine the likely relationship between perception of SPSs and somatization, participants were asked about their somatic symptoms in this study.

The false alarm rate on the SSDT differs between individuals—some rarely false alarm, whereas others produce many (Brown et al., 2012). The present study was carried out with low false alarm participants (the procedure for identifying high and low false alarm participants is described in section 2.2.4 on page 67).

In sum, the objectives of the present study were to investigate whether:

1. People who rarely false alarm on the SSDT can be trained to report more false alarms through operant conditioning.
2. Hit rate, sensitivity, and liberal response bias also increase along with false alarms.
3. The SSDT training affects subsequent reports of sensations in the hand in a separate task (i.e. the SPS test).
4. Total SPSs and SSDT false alarm rates correlate at baseline.
5. Total SPSs and SSDT false alarm rates in the baseline phase correlate with symptom reports (i.e. scores on the PHQ-15).

Research indicates that anxiety (Malow, 1981) and handedness (Rhodes & Schwartz, 1981) might affect tactile perception in signal detection tasks. In addition, sleepiness adversely affects performance (Gillberg & Akerstedt, 1998). Therefore,

these variables were also considered in the present study (by assessing them with relevant measures) to control and identify their confounding effects on the SSDT parameters.

2.2 Method

2.2.1 Study Design

A mixed design was used where condition (experimental vs. control) and phase (baseline, manipulation, and follow-up) were the repeated-measures variables and session (experimental session first vs. control session first) was the between-group variable. There were two purposes for including session as a variable: (i) to control the effect of order and (ii) to see if experience in the first session affects performance in the second session. Light (present vs. absent) was an additional repeated-measures variable for the SSDT. The purpose of using light was to maximise the false alarm rate, as studies have found that participants are more likely to give false alarms in the presence of the light (Lloyd et al., 2008). Thus, light was not directly relevant to the objectives of the present study and therefore findings on the effects of the light are presented in Appendix C as secondary analyses. The dependent variables were hit rate, false alarm rate, bias, sensitivity, number of SPSs, intensity of SPSs, pleasantness of SPSs, certainty of SPSs, and extent of SPSs. As stated in the hypotheses (see the section on results), it was expected that there would be main and interaction effects of condition and phase on the dependent variables. It was also expected that session would not affect the dependent variables due to the long interval in between them (i.e. at least seven days).

2.2.2 Participants

The study, as approved by the University of Manchester ethics committee, was carried out with 33 (65.2%) female and 19 (36.53%) male volunteers aged between 18 and 38 years (M = 22.15, SD = 4.22). The participants were students (n = 50; 96.15%) and staff (n = 2; 3.85%) of the University of Manchester. Participants received either 12 academic credits or £15 as compensation for their time and effort.

To determine the sample size, all published studies on the SSDT were considered. Studies that collected data from patient groups and investigated

symptoms or variables having clinical significance were excluded. Thus, four of the studies were identified as potentially relevant to the present experiment. In the second step, the mean of the means and the pooled standard deviation for false alarms in light absent trials of the selected studies were calculated. In the third step, G*Power (Faul, Erdfelder, Lang, & Buchner, 2007) was used to determine the required sample size considering the study design, obtained mean and standard deviation values, with alpha = .05 (two-tailed), and effect size = .50. It was found that testing 44 individuals would have 90% power to reject the null hypothesis. However, 52 participants were recruited to allow for drop outs and unusable data. The inclusion criteria were: aged between 18 to 40 years and ability to understand instructions in English. The exclusion criterion was having any medical condition that might affect the sense of touch.

2.2.2.1 Recruitment. The study was advertised (See Appendix D) online using University of Manchester Intranet Sites. Posters were also displayed on the noticeboards of university buildings. Participants willing to take part in the experiment were requested to register online and read the electronic version of the participant information form (see Appendix E), a hard copy of which was available on the day of the experiment. If they wanted to continue with the experiment, they were asked to sign the consent form (see Appendix F), answer the PHQ-15, give contact details so that they could be sent reminders about their sessions, and select their preferred time and date online. Participants could also contact the researcher via email to participate or to hear more about the research.

2.2.3 Materials

2.2.3.1 Questionnaires. The following questionnaires were used in the present study.

2.2.3.1.1 The PHQ-15. The PHQ-15 (see Appendix G) is part of the full Patient Health Questionnaire developed by Spitzer, Kroenke, and Williams (1999), and is designed to assess individual differences in somatic symptom severity (Kroenke, Spitzer, & Williams, 2002). Respondents indicate how bothered they have been about 15 physical symptoms during the last 4 weeks (*not bothered*, *bothered a little,* or

bothered a lot corresponding to scores of 0, 1, and 2 respectively). A number of studies have reported that the PHQ-15 is a highly reliable and valid instrument (Interian, Allen, Gara, Escobar, & Díaz-Martínez, 2006; Kroenke et al., 2002; Kroenke, Spitzer, Williams, & Löwe, 2010; Spitzer et al., 1999).

2.2.3.1.2 Short-Form of the State Scale of the Spielberger State-Trait Anxiety Inventory (STAI). This is a six-item scale (see Appendix H) for assessing the anxiety level of a person at a particular moment (Marteau & Bekker, 1992). There are four response options for each item ranging from 1 (*not at all*) to 4 (*very much*). Total scores can vary between 6 and 24 where higher scores indicate elevated anxiety. The scale is sensitive to fluctuations in state anxiety and it has acceptable reliability and validity (Marteau & Bekker, 1992; Tluczek, Henriques, & Brown, 2009).

2.2.3.1.3 Edinburgh Handedness Inventory. Edinburgh Handedness Inventory (EHI; Oldfield, 1971) is a widely used, reliable and valid instrument to determine whether an individual is a left, right, or mixed-hander (see Appendix I). It is comprised of 10 items representing different daily life activities, such as writing, throwing, using scissors, etc. Participants answer by specifying the hand they prefer for these actions. The obtained score ranges from -100 indicating complete left-handedness to 100 indicating complete right-handedness.

2.2.3.1.4 The Karolinska Sleepiness Severity Scale (KSS). Akerstedt and Gillberg (1990) developed this rating scale (see Appendix J) to measure current level of sleepiness. The scale ranges from 1 (*very alert*) to 9 (*very sleepy, great effort to keep awake, fighting sleep*). A score of 7 or more indicates excessive sleepiness. A Japanese adaptation of the scale was strongly related to behavioural and electroencephalographic indicators of sleepiness, supporting its validity (Kaida et al., 2006). However, reliability is difficult to determine as it is sensitive to fluctuations in sleepiness with time (Shahid, Wilkinson, Marcu, & Shapiro, 2012).

2.2.3.2 SSDT materials and procedure

2.2.3.2.1 Measurement of tactile perception threshold. A forced-choice adaptive procedure was used to determine the vibrotactile perceptual threshold of each participant. During the task, participants sat on a comfortable chair and kept

their non-dominant hand on a table in front of them. A computer monitor, aligned centrally in front of the participant on a table, was used to provide instructions and present a green arrow (962 x 722 pixels) that cued the start of each of two 1020ms consecutive intervals that comprised a trial. The arrow appeared in the middle of the monitor for 250ms, pointing downwards towards the participant's finger. The first and the second arrow contained the number one and two respectively to designate which interval was being presented. In each trial, a custom built amplifier (Dancer Design, Merseyside, United Kingdom) converted square sound wave input from a computer to 100 Hz tactile pulse (painless vibrations) and delivered it to a vibrating device (bone conductor) for 20ms. A double-sided adhesive circle was used to attach the bone conductor to the pad of the participant's non-dominant index finger. The bone conductor was mounted on a foam cube alongside a 5mm red LED, which was not used during the thresholding procedure. The vibration appeared randomly in the middle of one of the two intervals. After each trial, participants were asked to indicate using a PC keyboard which of the two intervals contained a short tactile vibration. The rest message "Please take a quick rest. Press space to continue" appeared on the screen after every 80 trials, although participants were able to rest at any time. During the thresholding phase, participants wore headphones that delivered white noise to mask the sound of the bone conductor and to block background noise.

A parameter estimation by sequential testing (PEST) computer algorithm was used to determine the participant's tactile threshold for use in the SSDT. The PEST began with a strong, easily perceptible, but painless vibration of 31.16 dB that was equal to a pressure of 274 m/s. A Wald SPRT [N(c) (no. of correct responses) - Pt. N (T) (probability threshold value (0.75) multiplied by current trials completed) W (W's limits were: 1 - -1)] was used to change the vibration strength. Selection of the vibration level depended on the responses given on all trials since it reached its current intensity level. In this process, the threshold for the participant was the intensity of vibration that was correctly detected in 75 per cent of the trials. Though the number of trials required for the determination of threshold varied from individual to individual, the computer algorithm was programmed to run a maximum of 250

trials. If the maximum number of trials was reached, the average vibration intensity of the last 50 trials was taken as the threshold level of the participant. An E-Prime programme (Psychology Software Tools, Inc., Pittsburgh, PA) was written to deliver stimuli and record responses.

2.2.3.2.2 Measurement of SSDT parameters. The SSDT parameters were measured with the same apparatus used to determine threshold. The task was comprised of blocks of 80 trials consisting of four, randomly interspersed trial types (touch only, light only, light and touch and no stimulus) each lasting 1020ms. Both control and experimental conditions had a total of eight blocks. The first two blocks were the baseline blocks. Average false alarm rate in light present trials in these blocks of the experimental condition was used to determine whether a participant was a high or low "false alarmer". The next four blocks were the manipulation blocks that delivered reinforcement and punishment in the experimental condition but no manipulation in the control condition. The last two blocks were the follow-up blocks with the usual SSDT trials. The start of each trial was signalled for 250ms by a green arrow on the computer monitor. In touch only trials, a threshold level tactile vibration was presented for 20ms in the middle of the trial interval. In light only trials, the LED light was flashed for 20ms in the middle of the trial interval. In light and touch trials, both the vibration and light flash appeared simultaneously in the middle of the trial interval. In no stimulus trials, nothing was presented during the trial interval. After each trial, participants indicated whether they felt any vibration by pressing 1, 2, 3, and 4 on the keyboard number pad corresponding to *definitely yes*, *maybe yes*, *maybe no*, and *definitely no* respectively. There were 12 practice trials to familiarize participants with the task. Participants were kept naive about the significance of the light and were informed that vibration would not be present in all trials. The same rest message, as mentioned in the thresholding phase, appeared at the end of each block, although participants could rest at any time if they wished. White noise was also presented during the SSDT trials.

2.2.3.2.3 Processing SSDT data. Four SSDT variables were derived, namely hit rate, false alarm rate, bias and sensitivity. To determine these, the frequency of

"definitely yes" and "maybe yes" responses were added to get total "yes" responses, whereas "definitely no" and "maybe no" were added to get total "no" responses. Then the following formulae were used:

Hit rate = (Total no. of yes responses in vibration present trials + .5) / (Total no. of vibration present trials + 1)

False alarm rate = (Total no. of yes responses in vibration absent trials + .5) / (Total no. of vibration absent trials + 1)

Sensitivity (d') = Z (Hit rate) – Z (False alarm rate)

Bias (c) = -.5 {Z (Hit rate) – Z (False alarm rate)}

In the above formulae of hit rate and false alarm rate, .5 was added to the numerator and 1 to the denominator so that they can be used to calculate Z values to determine sensitivity and bias. This log linear correction is essential as Z values for hit or false alarm rate of 0 or 1 are infinite (Snodgrass & Corwin, 1988).

The SSDT variables were calculated separately for each block. For each statistic (i.e. hit rate, false alarm rate, response bias, and sensitivity), the mean of Blocks 1 and 2 were then calculated to get baseline measures, the mean of scores from Blocks 3, 4, 5, and 6 were calculated to obtain measures for the manipulation phase, and the mean of scores from Blocks 7 and 8 were calculated to obtain measures for the follow-up phase. In each case, means for the experimental and control conditions were calculated separately, as were those for light present and light absent trials.

2.2.3.3 Measurement of SPSs. An adapted version of the protocol and procedure described by Michael and Naveteur (2011) was used to measure participants' spontaneous sensations. Participants were asked to relax and to keep their non-dominant hand palm down on a piece of smooth white A4 paper. At this time, the wrist of their other hand remained at resting position on the corresponding thigh under the table so that they could not see it. Participants were then asked to focus their attention on their non-dominant hands for 10 seconds, indicated by verbal 'start' and 'stop' signals from the experimenter. Immediately after the stop signal, participants were given a standard picture of a non-dominant hand (see Appendix K)

to mark any areas where they felt sensations during the 10-second attention period, and to write names (e.g., "tingling", "throbbing") for these. They then rated the intensity and pleasantness of each sensation, along with how confident they were that a sensation was present (see Appendix L). The intensity scale ranged from 1 (*weak*) to 10 (*strong*). The pleasantness scale ranged from -4 (*unpleasant*) to 4 (*pleasant*), where 0 indicated neutral sensation. The certainty scale had six options ranging from 0 (*uncertain*) to 5 (*certain*). The SPS test was repeated twice, making three trials overall, which were then combined to get average scores for each of the SPS test statistics. There was a practice trial on the first administration of the SPS test to familiarise participants with the task.

2.2.3.3.1 Processing SPS data. Means from the first three SPS trials in both the experimental and control conditions were computed to determine baseline SPS frequency, intensity, pleasantness, certainty, and extent. Similarly, means from the last three SPS trials were calculated to determine follow-up SPS statistics. To determine the extent of SPS, each hand figure was scanned with an hp Scanjet 4500c and saved as a JPEG image. Image processing and analysis software (ImageJ 1.46r; Abràmofff, Magalhães, & Ram, 2004) was used to measure in square pixels the area of each spontaneous sensation. All images were processed with the same computer.

2.2.4 Procedure

This study was carried out concurrently with another (see Study 2 in Chapter 3) that investigated whether it was possible to reduce the false alarm rate in high false alarm participants. The control condition was identical in each study and allocation of participants to the two studies was determined automatically by their baseline performance on the SSDT in light present trials of the experimental condition. Participants were allocated to this study if their false alarm rates were below .16 and to Study 2 if their false alarm rates were equal to or above .16. This demarcation was the median false alarm rate as determined from the data of previous SSDT studies. Two testing sessions, one for the control and another for the experimental condition, were separated by at least seven days. Each session had the same series of 12 tasks, the only difference being that the experimental condition involved training

(conditioning) in the middle four (of eight) blocks of SSDT trials whereas in the control condition there was no such training (normal SSDT blocks were delivered). The order of conditions was determined randomly for each participant to ensure that the group attributes were equivalent and any observed effects of condition were not due to subject variables but attributable to the manipulation. Both sessions were carried out in a quiet and light-attenuated room. At the start of each session, participants were asked to remove any jewellery from the fingers and wrists of their non-dominant hand. Some participants wore wristbands that could not be removed without cutting or breaking them; in these cases, they were asked to ensure that these bands were worn in both sessions. Approximately 15 seconds before the tasks, participants were asked to clean their hands with alcohol-based hand rub.

2.2.4.1 Control condition. The protocol for the control condition is shown in Figure 2.1.

2.2.4.2 Experimental condition. The protocol for the experimental condition is displayed in Figure 2.2. The procedures for the experimental and control conditions were identical apart from task 8, where the conditioning manipulation (i.e. training) was delivered. As in the control condition, participants completed four blocks of 80 SSDT trials during this phase but received reward and punishment following particular responses with a view to conditioning them. More specifically, participants received 10 points in half of the hit trials (i.e., where they correctly indicated that a stimulus was present) and lost 10 points in half of the miss (i.e., where they incorrectly indicated that a stimulus was absent) trials. Before starting these trials, participants were informed that they would receive 1p for every point they accumulated during this part of the experiment. A random selection process (built into the E-Prime programme) was used to determine which of the hit and miss trials would result in consequences. This was done to control participants' expectancy about specific trials or responses that might result in reward or punishment. The random selection process also conformed to a variable ratio schedule that produces higher rates of persistent responding (Sarafino, 2012). Participants were informed whether they had won or lost points by 3-second feedback messages on the computer monitor, which also indicated

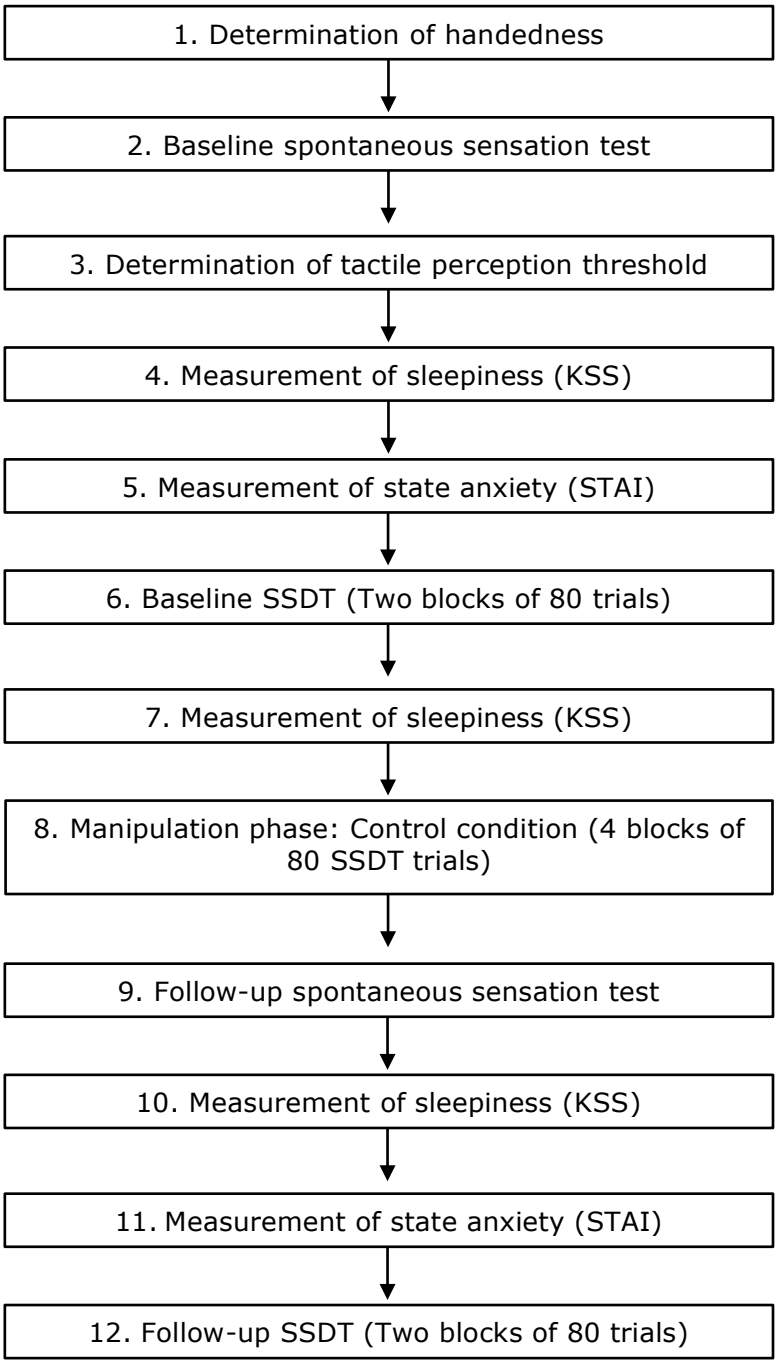


Figure 2.1. A flow chart representing the control condition of Studies 1 and 2

their running point total (e.g., "You have won 10 points. Your total score is 300"; "You have lost 10 points. Your total score is 280"). Different colours were used to present the messages: yellow for win and red for loss. Instructions about the possibility of winning or losing points and obtaining corresponding feedback messages were given at the start of this phase. At the end of the experiment, participants received one penny for every point that they accumulated during this phase (in cases where they

had fewer than zero points they did not receive anything apart from the usual honorarium for taking part).

It was expected, in accordance with the findings of different human and animal experiments (e.g. Johnstone & Alsop, 2000; Lie & Alsop, 2009; McCarthy & Davison, 1979), that participants would be biased towards the response that resulted in reward and avoid the response that brought loss. To strengthen the training effect (Johnstone & Alsop, 1996), this conditioning phase consisted of four blocks of 80 trials instead of two. In addition, participants were encouraged to try to win as many points as possible to increase the reinforcement value of the reward (see Appendix M for the verbal instruction used in this phase).

At the end of each session, participants were asked what they thought the purpose of the experiment was and what their expectations were, in order to identify any participant expectancy effects.

2.2.5 Data Preparation

2.2.5.1 Exploring outliers and assumptions. The data were initially examined for outliers and for their consistency with the assumptions of mixed Analysis of Variance (mixed ANOVA). For this purpose, histogram, boxplot, and the outlier labelling rule (Hoaglin, Iglewicz, & Tukey, 1986; Hoaglin & Iglewicz, 1987) were used to determine outliers for each of the SSDT and SPS test parameters. Then Z scores of skewness and kurtosis, Kolmogorov-Smirnow test, and Shapiro-Wilk test were used to see if the distribution of scores for the SSDT and SPSs satisfied normality assumptions.

It was found that the distributions of SSDT hit rates and SPS certainty were approximately normal with no outliers. However, other parameters of the SSDT and SPS test had some outliers and problems with normality. The score of four outliers for sensitivity and two for intensity were replaced using the formula of mean plus two times the standard deviation (Field, 2009) to make their distributions normal. Square root transformation of data normalised false alarm rate, bias, and total no. of SPS measures. However, SPS pleasantness and extent did not conform to normality even after addressing the outliers and transforming the scores. Therefore, it was decided to

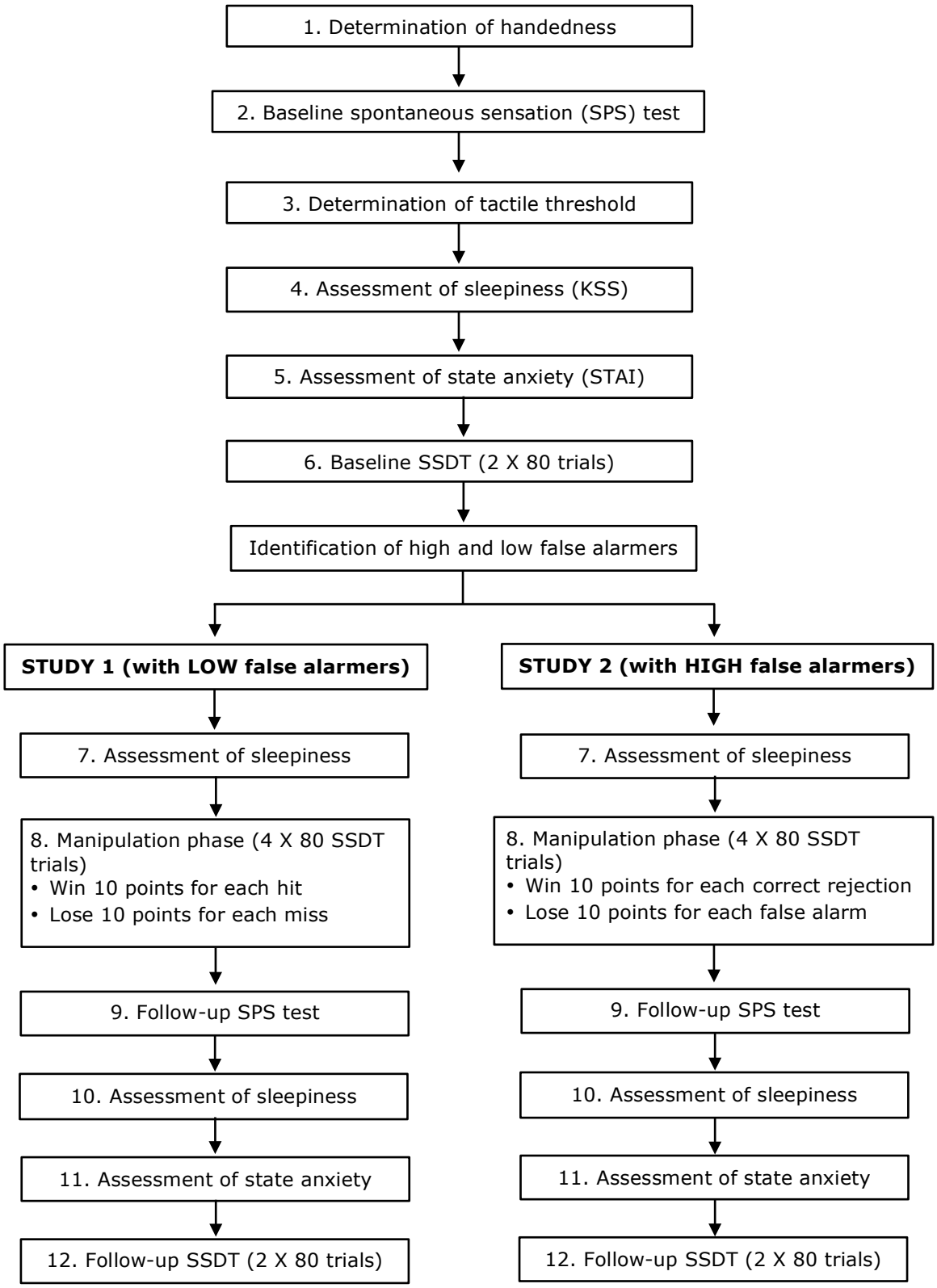


Figure 2.2. A flow chart representing the experimental condition of Studies 1 and 2.

use non-parametric tests to analyse them.

Though the transformation of scores ensured homogeneity of variance for most of the variables, Levene's test indicated that the variances were significantly different between the two groups for baseline false alarm rate in the light absent control condition, $F(1, 44) = 9.02$, $p < .01$, follow-up sensitivity in the light present control condition, $F(1, 44) = 4.07$, $p < .05$, and follow-up SPS frequency in the control condition, $F(1, 44) = 5.20$, $p < .05$. As the assumption of homogeneity of variance was satisfied in most of the cases and ANOVA is fairly robust to the violation of this assumption when sample sizes are equal (Field, 2009), we decided to continue with our decision of using mixed ANOVA for the analysis.

Mauchly's sphericity test found that four effects violated the assumption of sphericity: condition x phase for hit rate ($\varepsilon = .84$, $p < .05$), light x phase for hit rate ($\varepsilon = .85$, $p < .05$), main effect of phase for sensitivity ($\varepsilon = .79$, $p < .01$), and condition x light x phase for sensitivity ($\varepsilon = .82$, $p < .05$). The Huynh-Feldt correction was therefore used to produce valid *F*-ratios as all these estimates of sphericity were greater than 0.75 (Girden as cited in Field, 2009).

2.2.5.2 Exclusion of data. Data were scrutinized to identify individuals whose baseline hit rate in the experimental condition was extremely high (> 95%) or low (< 5%). It was found that three participants had 99% and one had 96% hit rate. These participants' data were excluded from the analysis on the grounds that the task had been insufficiently ambiguous for these individuals. Two more participants were excluded for falling asleep during the experiment. The final analysis of the SSDT data, total SPSs, SPS intensity, and extent of SPSs, therefore, was carried out with 46 individuals with a low false alarm rate on the SSDT, of whom 22 completed the experimental condition first. Participants who did not report any SPS either in the baseline or follow-up phases of the control and experimental conditions were excluded from the analysis on SPS certainty and pleasantness. Thus, there were 27 participants for these analyses, of whom 11 completed the experimental condition first.

2.2.6 Statistical Analysis

A mixed ANOVA was used to analyse SSDT response outcomes where phase (baseline vs. manipulation vs. follow-up), condition (control vs. experimental), and light (present vs. absent) were the within-group independent variables and session (control condition in the first session vs. experimental condition in the first session) was the between-group independent variable. The same analysis was used to analyse SPS data on frequency (i.e. total number of SPSs reported), intensity, and certainty, where phase (baseline vs. follow-up) and condition (control vs. experimental) were the within-group independent variables and session (control condition in the first session vs. experimental condition in the first session) was the between-group independent variable. As mentioned previously, non-parametric tests were used to analyse data of the other two SPS variables, namely SPS pleasantness and extent.

We were not interested in the effects of light on the SSDT response outcomes. Therefore, main and interaction effects of light are presented in Appendix C as secondary analyses. For the same reason, data on state anxiety and sleepiness have been dealt with as secondary analyses in that section.

Correlational analysis between the SSDT false alarm rates in the baseline, total number of SPSs in the baseline, and PHQ-15 scores are not described in this chapter. Rather, these data are combined with the corresponding data of Study 2 to increase sample size and variability in the data. The correlational analysis, therefore, is presented in Chapter 3.

As the different levels of the independent variables for both the SSDT and SPS test are compared several times, there is a possibility of Type I error due to multiple comparisons and multiple testing. The level of significance was therefore adjusted using Bonferroni correction. As examining changes in false alarm rates was our primary objective, the standard alpha value of .05 was used in this case. For the rest of the SSDT analyses (i.e. for hit rate, response bias, and sensitivity), the alpha was adjusted to .02. Similarly, the alpha was .05 for the analyses of total SPSs and .01 for the rest of the SPS responses (i.e. intensity, pleasantness, certainty, and extent).

2.3 Results

2.3.1 Hypothesis 1: Conditioning will Increase False Alarm Rate, Hit Rate, Sensitivity and Liberal Response on the SSDT

Descriptive statistics for the SSDT response outcomes are presented in Table 2.1.

2.3.1.1 False alarm rate. The main effect of phase on false alarm rate was significant, $F(2, 88) = 13.90$, $\eta^2_p = .24$, $p < .0001$, as were the main effects of condition, $F(1, 44) = 4.54$, $\eta^2_p = .09$, $p < .05$, and session, $F(1, 44) = 4.30$, $\eta^2_p = .09$, $p < .05$. Bonferroni corrected post-hoc test indicated that in the first session, participants reported more false alarms when it was the experimental than the control condition (mean difference = .07; 95% CI = .002, .14; $p < .05$).

As predicted, there was a significant interaction between condition and phase, $F(2, 88) = 29.32$, $\eta^2_p = .40$, $p < .0001$. Figure 2.3a portrays the interaction effect. Bonferroni corrected post-hoc tests revealed that the baseline false alarm rate in the control condition was significantly higher than in the experimental condition (mean difference = .08; 95% CI = .04, .11; $p < .0001$); however, the false alarm rate was significantly higher in the experimental condition than the control in both the manipulation (mean difference = .12; 95% CI = .06, .19; $p < .005$) and follow-up phases (mean difference = .09; 95% CI = .04, .14; $p < .005$). In the control condition the false alarm rate decreased over time, such that the follow-up rate was significantly lower than the baseline (mean difference = .04; 95% CI = .01, .07; $p < .005$). In the experimental condition, in contrast, participants produced significantly more false alarms at follow-up than at baseline (mean difference = .05; 95% CI = .00, .10; $p < .0001$).

There were no significant interactions between phase and session, $F(2, 88) = .61$, $\eta^2_p = .01$, $p = .55$, condition and session, $F(1, 44) = 1.87$, $\eta^2_p = .04$, $p = .18$, and phase, condition, and session, $F(2, 88) = .07$, $\eta^2_p = .002$, $p = .93$.

2.3.1.2 Hit rate. There was a significant main effect of phase, $F(2, 88) = 15.41$, $\eta^2_p = .26$, $p < .0001$, as well as a significant condition by phase interaction,

$F(1.90, 83.80) = 19.81$, $\eta^2_p = .31$, $p < .0001$. The interaction effect is shown in Figure 2.3b.

Bonferroni corrected post-hoc tests revealed that in the experimental condition, the hit rate was higher in the manipulation compared to the baseline (mean difference = .21; 95% CI = .12, .29; $p < .0001$) and follow-up phases (mean difference = .17; 95% CI = .09, .24; $p < .0001$). In contrast, in the control condition, the hit rate was higher in the baseline than in the manipulation (mean difference = .09; 95% CI = .03, .15; $p < .005$) and follow-up phases (mean difference = .16; 95% CI = .07, .24; $p < .0001$). Comparison of the experimental and control conditions further revealed that their hit rates did not differ in the baseline (mean difference = .09; 95% CI = -.01, .18; $p = .07$) but did in the manipulation (mean difference = .21; 95% CI = .10, .32; $p < .0001$) and follow-up phases (mean difference = .11; 95% CI = .002, .22; $p < .05$).

There were no significant main effect of condition, $F(1, 44) = 3.13$, $\eta^2_p = .07$, $p = .08$, session, $F(1, 44) = .60$, $\eta^2_p = .01$, $p = .44$, and interactions between phase and session, $F(2, 88) = .29$, $\eta^2_p = .01$, $p = .75$, condition and session, $F(1, 44) = 1.83$, $\eta^2_p = .04$, $p = .18$, and phase, condition, and session, $F(2, 88) = .82$, $\eta^2_p = .02$, $p = .44$ were also non-significant.

2.3.1.3 Sensitivity. There was a significant main effect of phase on sensitivity, $F(1.78, 78.79) = 8.26$, $\eta^2_p = .16$, $p < .005$. Bonferroni corrected post-hoc tests revealed that participants' sensitivity (regardless of condition) did not differ between baseline and manipulation phases (mean difference = .07; 95% CI = -.13, .27; $p = 1.0$). However, baseline sensitivity was significantly higher than follow-up sensitivity (mean difference = .32; 95% CI = .08, .57; $p < .01$). Similarly, sensitivity in the manipulation phase was significantly higher than that in the follow-up phase (mean difference = .25; 95% CI = .08, .42; $p < .005$).

The main effects of condition, $F(1, 44) = .397$, $\eta^2_p = .009$, $p = .532$, session, $F(1, 44) = 4.06$, $\eta^2_p = .09$, $p = .05$, and interactions between condition and phase, $F(2, 88) = 2.12$, $\eta^2_p = .046$, $p = .127$, condition and session, $F(1, 44) = .29$, $\eta^2_p =$

Table 2.1

Mean (standard deviation) SSDT Response Outcomes in the Baseline, Manipulation, and Follow-Up Phases of the Experimental and Control Conditions in the Sessions of Study 1

SSDT responses	Control condition						Experimental condition					
	Baseline		Manipulation		Follow-up		Baseline		Manipulation		Follow-up	
	LA	LP	LA	LP	LA	LP	LA	LP	LA	LP	LA	LP
First session = control condition												
FA rate[a]	.34 (.12)	.30 (.12)	.29 (.13)	.28 (.15)	.25 (.13)	.28 (.16)	.39 (.17)	.42 (.17)	.36 (.16)	.42 (.17)	.32 (.16)	.43 (.16)
Hit rate	.53 (.29)	.59 (.27)	.64 (.27)	.40 (.29)	.48 (.28)	.54 (.31)	.62 (.25)	.59 (.29)	.71 (.23)	.39 (.24)	.48 (.28)	.55 (.24)
Sensitivity	1.29 (1.05)	1.87 (.99)	1.35 (1.07)	1.79 (1.08)	1.18 (1.11)	1.55 (1.24)	1.52 (.99)	1.59 (.87)	1.05 (.97)	1.22 (1.08)	1.01 (.88)	1.06 (.85)
Bias[a]	1.59 (.17)	1.56 (.17)	1.66 (.17)	1.61 (.14)	1.72 (.17)	1.62 (.17)	1.50 (.22)	1.45 (.18)	1.61 (.18)	1.52 (.18)	1.68 (.16)	1.54 (.15)

(continued)

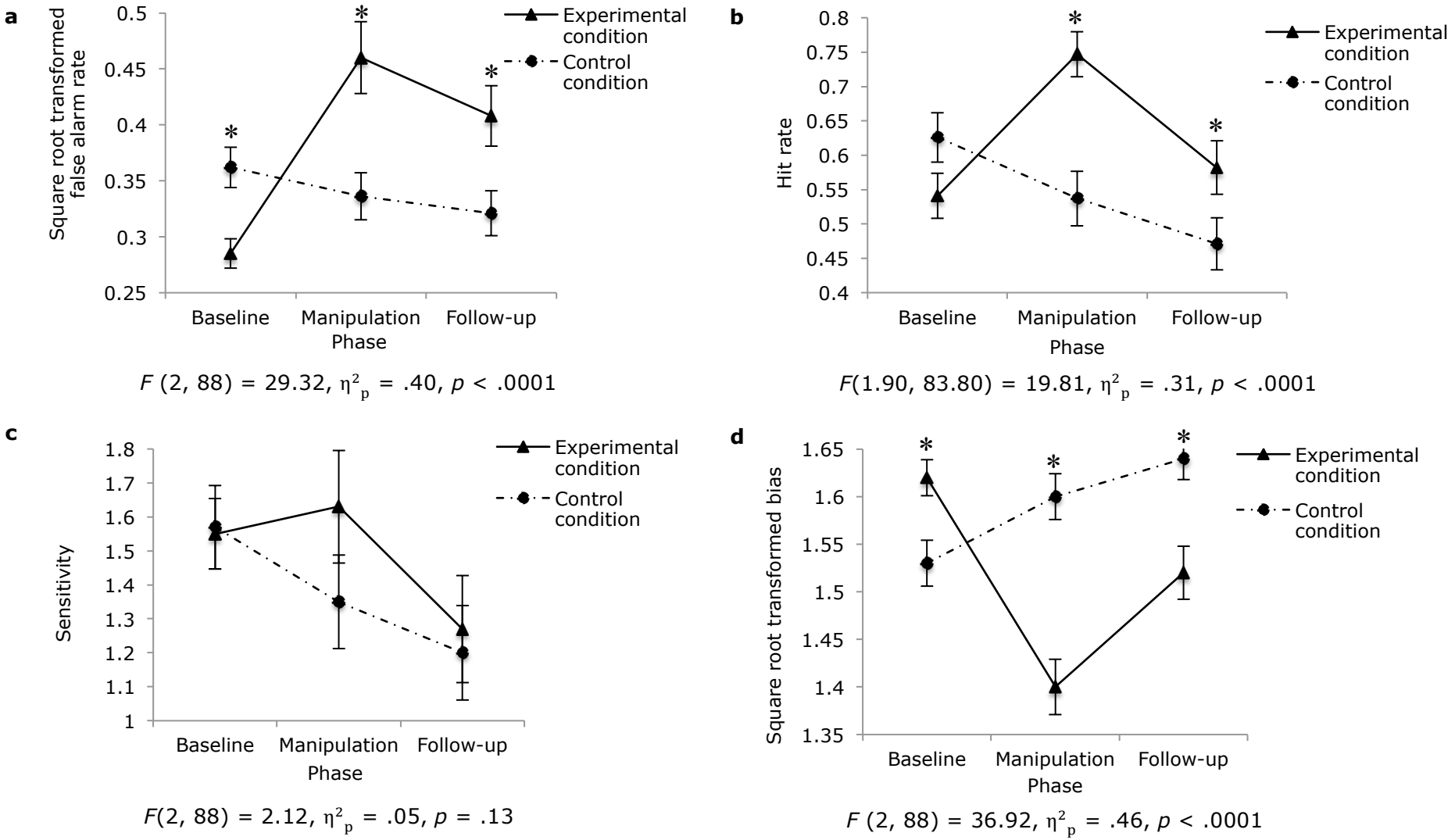
Table 2.1

Mean (standard deviation) SSDT Response Outcomes in the Baseline, Manipulation, and Follow-Up Phases of the Experimental and Control Conditions in the Sessions of Study 1

	Control condition						Experimental condition					
	Baseline		Manipulation		Follow-up		Baseline		Manipulation		Follow-up	
SSDT responses	LA	LP	LA	LP	LA	LP	LA	LP	LA	LP	LA	LP
First session = Experimental condition												
FA rate[a]	.28 (.12)	.27 (.09)	.42 (.24)	.44 (.25)	.36 (.19)	.40 (.20)	.30 (.12)	.29 (.08)	.46 (.20)	.51 (.19)	.43 (.18)	.43 (20)
Hit rate	.55 (20)	.81 (.21)	.64 (.22)	.58 (.25)	.77 (.20)	.67 (.26)	.46 (.25)	.75 (.25)	.51 (.26)	.51 (.30)	.66 (.26)	.57 (.27)
Sensitivity	1.55 (.63)	1.95 (.73)	1.80 (1.12)	1.92 (1.23)	1.40 (1.01)	1.61 (1.20)	1.23 (.65)	1.46 (.89)	1.28 (1.04)	1.48 (1.13)	.92 (1.03)	1.16 (1.12)
Bias[a]	1.62 (.12)	1.58 (.14)	1.43 (20)	1.35 (.24)	1.56 (.18)	1.47 (.19)	1.65 (.15)	1.63 (.13)	1.46 (.18)	1.35 (.19)	1.55 (.20)	1.51 (.20)

Note. FA = false alarm; LP = light present condition; LA = light absent condition.

[a]Square root transformed score.



F (2, 88) = 29.32, η^2_p = .40, p < .0001

F(1.90, 83.80) = 19.81, η^2_p = .31, p < .0001

F(2, 88) = 2.12, η^2_p = .05, p = .13

F (2, 88) = 36.92, η^2_p = .46, p < .0001

Figure 2.3. Condition by phase interactions for false alarm rate, hit rate, sensitivity, and bias. Error bars are standard errors.

.01, p = .59, phase and session, $F(2, 88) = 1.16$, $\eta^2_p = .03$, p = .32, and phase, condition, and session, $F(2, 88) = .71$, $\eta^2_p = .02$, p = .50, were not significant. The phase by condition interaction is shown in Figure 2.3c.

2.3.1.4 Response bias. There were significant main effects of condition, $F(1, 44) = 9.00$, $\eta^2_p = .17$, $p < .005$, and phase, $F(2, 88) = 17.88$, $\eta^2_p = .29$, $p < .0001$, and a significant condition by phase interaction, $F(2, 88) = 36.92$, $\eta^2_p = .46$, $p < .0001$. The interaction effect is shown in Figure 2.3d.

Bonferroni corrected post-hoc tests revealed that bias towards the "no" response was significantly higher in the experimental than the control condition in the baseline phase (mean difference = .09; 95% CI = .04, .15; $p < .005$). However, it became significantly higher in the control than the experimental condition in both the manipulation (mean difference = -.20; 95% CI = -.28, -.13; $p < .0001$) and follow-up phases (mean difference = -.12; 95% CI = -.18, -.05; $p < .005$).

There were no significant main effect of session, $F(1, 44) = .61$, $\eta^2_p = .01$, p = .44, and interactions between phase and session, $F(2, 88) = .01$, $\eta^2_p < .001$, p = .99, condition and session, $F(1, 44) = 3.93$, $\eta^2_p = .08$, p = .05, and phase, condition, and session, $F(2, 88) = .32$, $\eta^2_p = .01$, p = .72.

2.3.2 Hypothesis 2: Conditioning During the SSDT Trials will Increase Reporting of SPSs at Follow-up

Descriptive statistics on the SPS response outcomes are shown in Table 2.2.

2.3.2.1 Total SPSs. There was a significant main effect of phase, $F(1, 44) = 5.00$, $\eta^2_p = .10$, $p < .05$, with Bonferroni corrected post-hoc tests revealing that sensations were significantly more common at follow-up than at baseline, regardless of condition (mean difference = .11; 95% CI = .01, .21; $p < .05$).

There was a significant interaction effect between condition and session, $F(1, 44) = 6.54$, $\eta^2_p = .13$, $p < .05$. The interaction effect is shown in Figure 2.4A. It was found that total number of SPSs in the experimental condition was more than that in the control condition, when the experimental condition was the first session (mean difference = .17; 95% CI = .04, .31; $p < .05$). However, such an effect was not found

with the control condition in the first session (mean difference = .06; 95% CI = -.07, .19; p = .34).

There were no significant main effects of condition, $F(1, 44) = 1.46$, $\eta^2_p = .03$, $p = .23$, session, $F(1, 44) = .003$, $\eta^2_p < .001$, $p = .95$, and interactions between condition and phase, $F(1, 44) = 1.40$, $\eta^2_p = .03$, $p = .24$, phase and session, $F(1, 44) = .003$, $\eta^2_p < .001$, $p = .96$, and phase, condition, and session, $F(1, 44) = .82$, $\eta^2_p = .02$, $p = .37$.

2.3.2.2 Intensity of SPSs. The interaction between phase, condition, and session was significant, $F(1, 44) = 8.14$, $\eta^2_p = .16$, $p < .01$. The interaction is shown in Figure 2.4B.

Bonferroni corrected post-hoc tests revealed that if the control condition was in the first session, SPS intensity did not differ between the baseline and follow-up phases both in the control (mean difference = -.85; 95% CI = -1.74, .04; p = .06) and experimental conditions (mean difference = -.17; 95% CI = -1.25, .92; p = .76). On the other hand, follow-up SPSs were felt significantly more intensely than those in the baseline phase (mean difference = 1.42; 95% CI = .29, 2.55; $p < .05$) of the experimental condition when it was the first session but such an effect was not found in the control condition when it was the second session (mean difference = -.20; 95% CI = -1.13, .73; p = .67). Intensity of SPSs did not differ between the conditions either in the baseline (mean difference = -.36; 95% CI = -1.21, .49; p = .40) or in the follow-up phase (mean difference = .32; 95% CI = -1.19, .55; p = .46) if the first session was the control condition. If the first session was the experimental condition, intensity of SPSs in the control and experimental conditions did not differ in the baseline phase but were reported more intensely in the follow-up phase of the experimental than of the control condition (mean difference = 1.07; 95% CI = .16, 1.98; $p < .05$).

The main effects of phase, $F(1, 44) = 3.66$, $p = .06$, $\eta^2_p = .08$, condition, $F(1, 44) = .37$, $p = .55$, $\eta^2_p = .01$, session, $F(1, 44) = .03$, $p = .88$, $\eta^2_p = .001$, and interactions between phase and condition, $F(1, 44) = 1.34$, $p = .25$, $\eta^2_p = .03$,

Table 2.2

Mean Number, Intensity, Certainty, Pleasantness, and Extent (Standard Deviation) of Spontaneous Sensations in the Baseline and Follow-up SPS Tests in the Experimental and Control Conditions Across Different Session Orders.

			First session	
Variables	Condition	Phase	Control	Experimental
Number of SPSs[a]	Experimental	Baseline	.67 (.44)	.76 (.47)
		Follow-up	.79 (.48)	.95 (.47)
	Control	Baseline	.74 (.38)	.67 (.48)
		Follow-up	.84 (.36)	.70 (.64)
Intensity of SPS	Experimental	Baseline	2.66 (2.40)	2.24 (2.04)
		Follow-up	2.83 (2.26)	3.66 (2.55)
	Control	Baseline	2.30 (1.74)	2.79 (2.61)
		Follow-up	3.15 (2.10)	2.59 (2.58)
Certainty of SPS	Experimental	Baseline	2.91 (1.41)	2.98 (1.70)
		Follow-up	2.78 (1.28)	3.83 (.89)
	Control	Baseline	2.65 (1.07)	3.43 (1.36)
		Follow-up	2.34 (1.37)	3.50 (1.26)
SPS pleasantness[b]	Experimental	Baseline	-.17 (-.62 to .33)	-.17 (-.33 to .00)
		Follow-up	.00 (-.63 to .00)	-.33 (-.78 to .00)
	Control	Baseline	.00 (-.29 to .61)	.00 (-.33 to .00)
		Follow-up	.00 (-.46 to .29)	-.17 (-.72 to .00)
SPS extent[b]	Experimental	Baseline	9457 (741.42 to 54225.25)	12653.17 (2337.08 to 51749.08)
		Follow-up	17026.33 (1935.50 to 44762.08)	24733.67 (4603.92 to 47564.50)

(continued)

Variables	Condition	Phase	First session	
			Control	Experimental
	Control	Baseline	17062 (3325.92 to 85247.08)	20544 (.00 to 97907.17)
		Follow-up	20199 (7856.58 to 76388.17)	7947.17 (.00 to 39217.25)

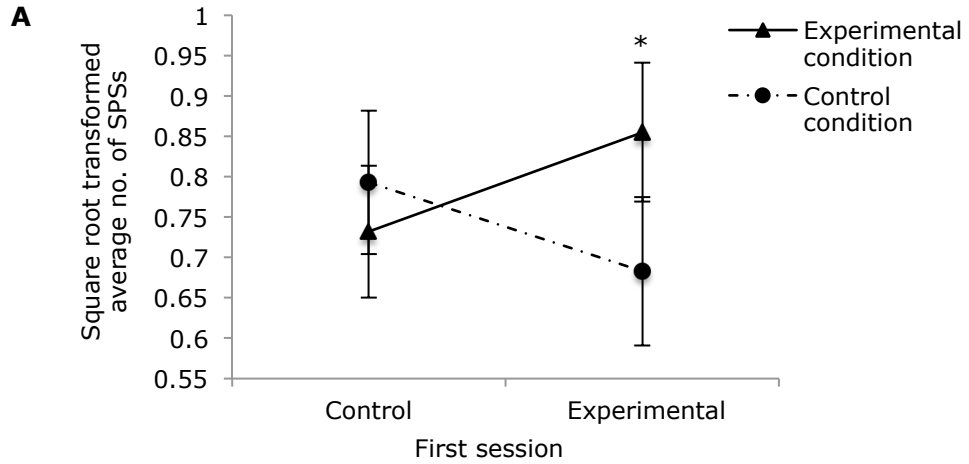
Note. [a]Square root transformed data. [b]Descriptive statistics are median and interquartile range within parentheses.

phase and session, $F(1, 44) = .03$, $p = .86$, $\eta^2_p = .001$, and condition and session, $F(1, 44) = .28$, $p = .60$, $\eta^2_p = .01$, were not significant.

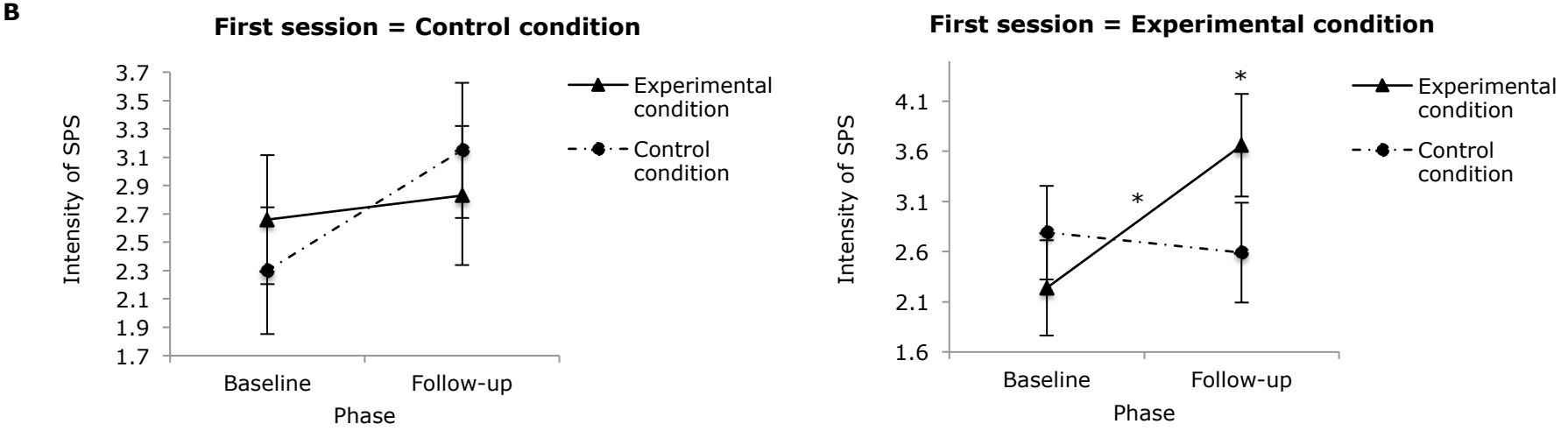
2.3.2.3 Certainty of SPSs. The main effect of condition was not significant, $F(1, 25) = .22$, $p = .65$, $\eta^2_p = .01$, and neither were the main effects of phase, $F(1, 25) = .62$, $p = .44$, $\eta^2_p = .02$, session, $F(1, 25) = 4.10$, $p = .05$, $\eta^2_p = .14$, or the interactions between phase and condition, $F(1, 25) = 2.61$, $p = .12$, $\eta^2_p = .09$, phase and session, $F(1, 25) = 1.29$, $p = .27$, $\eta^2_p = .05$, condition and session, $F(1, 25) = 1.78$, $p = .20$, $\eta^2_p = .07$, and phase, condition, and session, $F(1, 25) = 1.06$, $p = .31$, $\eta^2_p = .04$.

2.3.2.4 Pleasantness of SPSs. Friedman's ANOVA indicated that SPS pleasantness did not differ significantly between the phases of the control and experimental conditions, $\chi^2(3) = 5.83$, $p = .12$.

Mann-Whitney test was carried out to examine the effects of session on SPS pleasantness. There was no significant difference between the groups (i.e. those participating in the control vs. experimental condition in the first session) in the baseline, $U = 80$, $z = -.4$, $p = .72$, $r = -.08$, and follow-up phases of the experimental condition, $U = 71$, $z = -.86$, $p = .42$, $r = -.17$, and likewise in the baseline, $U = 64.5$, $z = -1.19$, $p = .25$, $r = -.23$, and follow-up phases of the control condition, $U = 62$, $z = -1.31$, $p = .21$, $r = -.25$.



$F(1, 44) = 6.54$, $\eta^2_p = .13$, $p < .05$



$F(1, 44) = 8.14$, $\eta^2_p = .16$, $p < .01$

Figure 2.4. A) Condition by session interaction for number of SPSs. B) Condition by phase by session interaction for intensity of SPS. Error bars are standard errors.

2.3.2.5 Extent of SPS. Friedman's ANOVA indicates that extent of SPS did not differ between the baseline and follow-up phases of the control and experimental conditions, $\chi^2(3) = 1.91$, $p = .59$. A Mann-Whitney test demonstrated that extent of SPS did not differ between the groups (i.e. control vs. experimental condition in the first session) in the baseline, $U = 262$, $z = -.04$, $p = .97$, $r = -.01$, and follow-up phase of the control condition, $U = 189$, $z = -1.66$, $p = .10$, $r = -.24$, or in the baseline, $U = 240$, $z = -.53$, $p = .60$, $r = -.08$, and follow-up phase of the experimental condition, $U = 233$, $z = -.68$, $p = .51$, $r = -.10$.

2.4 Discussion

As hypothesized, the operant conditioning manipulation increased the false alarm rate (i.e., the tendency to say "yes" when there was no tactile stimulation) of participants who had initially exhibited few false alarms, both during the training procedure and at follow-up. Conditioning also affected participants' hit rate and response bias (i.e., their overall tendency to say "yes") but there was no difference in sensitivity between the conditions. In general, the findings are in accordance with the principles of operant conditioning theory (Skinner, 1953). Similar results have been reported in signal detection studies of conditioning in other sensory modalities. For example, Lie and Alsop (2009, 2010) found that operant conditioning affected hit rate, sensitivity, and response bias for visual stimuli, whereas Carterette, Friedman, and Wyman (1966) found that correct feedback increased the hit rate but decreased the false alarm rate in an auditory perception task.

It should be noted that participants were not rewarded for false alarms but for hits, that is, saying "yes" when the vibration was present. On the other hand, they were punished for misses, that is, saying "no" when the vibration was present. The results indicate that this procedure conditioned participants to say "yes" more frequently, resulting in significant increases in both the false alarm and hit rates, with a corresponding effect on response bias but not on sensitivity. These findings demonstrate how reinforcing a particular response can influence the frequency of different but characteristically similar responses.

Importantly, the effect of conditioning on false alarms was not limited to the conditioning trials but continued into the follow-up phase when reinforcement and punishment were no longer being applied, with the follow-up false alarm rate remaining significantly higher than that in the baseline. This indicates that the conditioning procedure had an enduring effect on participants' somatosensory decision processes, which perhaps lasted beyond the experimental (i.e. training) session. Since false alarm rates are highly reliable (McKenzie et al., 2010) and the same group of participants performed in both the control and experimental conditions, it was expected that their baseline false alarm rates would be comparable across the conditions. However, it was found that the baseline false alarm rate in the control condition was significantly higher than that in the experimental condition (see Figure 2.3a) and the baseline response criterion was significantly more liberal in the control than in the experimental condition (see Figure 2.3d) . The hit rate also showed a similar, though non-significant disparity (see Figure 2.3b). Evidently, half of the participants who received training in the first session were still liberal in response criterion (i.e., their learned tendency to say "yes" was still there) after about a week when they returned back in the second session to perform in the control condition. This raised the baseline group average for false alarm and hit rates in the control condition, hence the differences in the baseline phase.

It was also hypothesized that conditioning particular responses on the SSDT trials would have a more general effect on subsequent somatosensory decision making, such that participants would report more spontaneous sensations on an unrelated task. Although the expected condition by phase interaction was not found, participants reported comparatively more spontaneous sensations after the experimental manipulation when conditioning was presented in the first session. The same effect was also found for the intensity of spontaneous sensations. It may be that conditioning responses on the SSDT only affects subsequent perceptual processing when participants are naïve to the testing procedure and ambiguity is high. This is indirectly supported by the comparison of mean false alarm rates for the experimental condition across the sessions, which indicate that false alarm rate in the

experimental condition was higher when it was presented in the first session (.293, .487, and .433 for baseline, manipulation, and follow-up phases respectively) than when it was presented in the second (.276, .433, and .382 for baseline, manipulation, and follow-up phases respectively). Though the interaction between condition, phase, and session was not statistically significant, the mean false alarm rates indicate the likely presence of intervening factors when the experimental condition is presented second.

Overall, it seems that conditioning of SSDT responses had some effect on the tendency to report spontaneous sensations, but not consistently. However, it is questionable whether the SPS test in its present form and content is appropriate for use together with the SSDT. SPSs are involuntary sensations (i.e. they do not require any external trigger), typically measured during a relaxed state of the body. However, keeping the non-dominant hand in a fixed position for long periods on SSDT trials is likely to have residual effects on the joints, muscles, and tendons of the hand. This, in turn, may be a source of various sensations in the entire hand, which come to light in the SPS. For example, many participants reported short-term numbness in the area of the index figure that was attached to the bone conductor. If such sensations are due to the SSDT, presenting the SPS test immediately afterwards is likely to compromise its validity as a measure of the tendency to experience spontaneous sensations. A better alternative in this case might be to ask participants to focus elsewhere on the body rather than the hand (this was addressed in Chapters 5 and 6).

CHAPTER 3

Study 2: Can Illusory Somatosensory Experiences be Decreased With Training?

> From the chin up, I was my familiar self. From the neck down, I was a shuddering stranger.
>
> —Siri Hustvedt, The Shaking Woman or A History of My Nerves, p.172

We saw in Study 1 (Chapter 2) that SSDT training increased the false alarm rate of low false alarm individuals, although it was not clear whether the conditioning generalized to the SPS task. In this chapter, we consider whether the same conditioning principles can be used to decrease participants' false alarm rate on the SSDT, and whether this generalizes to other perceptual experiences on the SPS. To address these possibilities, the following objectives have been set for the present study to investigate whether:

1. The false alarm rate of high false alarm individuals can be decreased by conditioning certain SSDT responses and whether this affects other SSDT response outcomes (i.e., hit rate, response bias, and sensitivity).
2. Participants experience fewer SPSs after SSDT training and whether the training also affects other SPS response characteristics (i.e., intensity, pleasantness, certainty, and extent).
3. False alarm rate and total SPSs correlate at baseline.
4. Baseline false alarm rate and total SPSs relate to somatic symptom severity as measured by the PHQ-15[1].

3.2 Method

The questionnaires, SSDT, and SPS materials and procedure of this study were the same as those of Study 1. There were three phases in both the control and experimental conditions, namely baseline, manipulation, and follow-up; participants responded to the questionnaires and performed the tasks in each of these phases in a sequence as depicted in Figures 2.1 (page 69) and 2.2 (page 71). The only difference

[1] This was the fifth objective of Study 1. The analyses are presented here as they involved combining the data from Studies 1 and 2 in order to increase the sample size and ensure adequate variation.

between the present and the previous study was that a different group of participants (i.e., those with a high rather than a low false alarm rate) was tested here with an alternative training protocol in the experimental condition.

3.2.1 Participants

There were 30 participants in this study. They were students (n = 27, 90%) and staff (n = 3, 10%) of the University of the Manchester, aged between 18 and 39 years (M = 22.97, SD = 5.07); 21 (70%) were female. Among the participants, 27 were right-handed and the rest were left-handed as their scores on the Edinburgh Handedness Inventory indicate (Oldfield, 1971).

Sample size calculation followed the same steps as described in Study 1 (see Section 2.2.2). Using G*Power (Faul et al., 2007), it was found that a sample size of 34 would produce a power of .8 with alpha = .05 (two-tailed) and effect size = .5.

As described previously, a computer programme allocated participants automatically (unknown to the experimenter) to either Study 1 or Study 2 depending on their baseline false alarm rate in light present SSDT trials in the experimental condition. On this basis, 30 participants (false alarm rate ≥ 0.16) were allocated to Study 2 and 52 participants (false alarm rate < 0.16) to Study 1. It was decided to stop further recruitment and testing when the sample size reached 30 for Study 2, as this was close to what had originally been aimed for (the power analysis suggested a sample size of 34) and it was evident that continuing recruitment would result in unnecessarily testing more participants for Study 1.

3.2.2 Experimental condition

In the manipulation phase of the experimental condition, participants won 10 points (i.e., 10 pence) in half of the randomly selected correct rejection trials (i.e., where they correctly indicated that the stimulus was absent) and lost the same amount in half of the randomly selected false alarm trials (i.e., where they incorrectly indicated that a stimulus was present). Participants got computer generated visual feedback about their performance in the same way as in Study 1.

3.2.3 Data preparation

3.2.3.1 Exploring outliers and assumptions. Data in Study 2 were examined to identify outliers and to check whether the assumptions of mixed ANOVA were met. Each of the SSDT variables had 24 distributions resulting from the combinations of four factors: phase (baseline, manipulation, and follow-up), condition (control and experimental), light (present and absent), and session (control condition in the first session and experimental condition in the first session). Similarly, there were eight distributions for each of the SPS variables, which were the combinations of three factors: phase (baseline and follow-up), condition (control and experimental), and session (control condition in the first session and experimental condition in the first session). All these distributions were inspected for outliers. It was found that the hit rate, false alarm rate, bias, and extent of SPS distributions had a total of 13, 11, 2, and 10 outliers respectively (with a maximum of three outliers in a distribution), which were replaced by values equal to the mean plus two times the standard deviation (Field, 2009). Correcting the outliers brought all the SSDT and SPS distributions to normal except those of false alarm rate, sensitivity, SPS pleasantness, and SPS extent. Log transformation was carried out to make them normal. The correction procedure, however, could not normalize four follow-up false alarm rate distributions: false alarm rate in follow-up light absent trials of the experimental condition when the experimental condition was the first session, W (17) = .83, p < .01, follow-up light absent trials of the control condition when the experimental condition was the first session, W (17) = .86, p < .05, follow-up light absent trials of the experimental condition when the control condition was the first session, W (13) = .63, p < .001, and follow-up light present trials of the experimental condition when the experimental condition was the first session, W (17) = .84, p < .01. As most of the false alarm rate distributions were normal, group sizes were equal (the same 30 participants took part in both the control and experimental conditions), and ANOVA is quite robust to violations of normality (Field, 2009), we decided to continue with the parametric test to perform the analysis.

Levene's test indicated that participants who received the control condition in the first session had significantly different variances from those who had the

experimental condition first for the false alarm rate in the follow-up light absent experimental condition, F (1, 28) = 6.724, $p < .05$, false alarm rate in the follow-up light present experimental condition, F (1, 28) = 6.643, $p < .05$, bias in the baseline light present experimental condition, F (1, 28) = 4.272, $p = .05$, and bias in the manipulation light present experimental condition, F (1, 28) = 6.739, $p < .05$. As ANOVA is fairly robust to the violations of homogeneity of variance when sample sizes are equal (Field, 2009), we elected to continue with our original decision of using mixed ANOVA for the analysis.

Mauchly's test indicated that the assumption of sphericity was violated for the phase x condition x light hit rate, χ^2 (2) = 6.983, $p < .05$; phase x condition sensitivity, χ^2 (2) = 10.45, $p < .01$; phase x condition x light sensitivity, χ^2 (2) = 7.566, $p < .05$; and phase x condition x light bias, χ^2 (2) = 10.883, $p < .01$. Therefore, degrees of freedom were corrected (Girden as cited in Field, 2009) using Greenhouse-Geisser ($\varepsilon = .68$ for the phase x condition sensitivity and .67 for the phase x condition x light bias) and Huynh-Feldt estimates of sphericity ($\varepsilon = .89$ for the phase x condition x light hit rate and .76 for the phase x condition x light sensitivity).

To determine the relationship between the SSDT false alarm rates in light present and light absent conditions in the baseline, total SPSs in the baseline, and PHQ-15, task scores only in the first session were used; those of the second session were discarded to avoid the possibility that responses in the second session might be influenced by what participants experienced in the first session. To decide on whether Pearson's correlation could be carried out with the variables, the distributions of the variables were examined. It was found that the distribution of PHQ-15 scores was non-normal, D (76) = .18, $p < .001$, and none of the transformation techniques corrected the problem. It was therefore decided to use Spearman's correlation to determine the relationships between the variables.

3.2.3.2 Exclusion of data. None of the participants had an extremely low or high hit rate (i.e. hit rate below .05 or above .95) in the baseline meaning that they

understood the task and the vibration level was sufficiently ambiguous to them. Therefore, data from all 30 participants were used in the analysis.

3.2.4 Statistical Analysis

In the present mixed ANOVA design, phase (baseline, manipulation, and follow-up), condition (control and experimental/training), and light (present and absent) were the within-subjects variables and session (control condition in the first session and experimental condition in the first session) was the between-subjects variable. As we were not interested in the effects of light on the dependent variables (i.e., SSDT response outcomes), they are presented in Appendix N as secondary analysis. Similarly, sleepiness and state anxiety have been analyzed as secondary objectives.

As the false alarm rate in the SSDT was the main variable of interest, the level of significance for its analysis was .05. For the other SSDT variables (i.e., hit rate, response bias, and sensitivity), this was Bonferroni adjusted to .02 to avoid the possibility of type I errors. Similarly, the alpha was .05 for total SPSs (the main variable of interest) and .01 for the other SPS variables (i.e. intensity, pleasantness, certainty, and extent).

3.3 Results

3.3.1 Hypothesis 1: Conditioning Will Decrease False Alarm and Hit Rates and Make Response Criterion More Conservative (i.e., Less Likely to Say "Yes"), With Sensitivity Remaining Unchanged

Descriptive statistics on the SSDT response outcomes are shown in Table 3.1.

3.3.1.1 False alarm rate. There were significant main effects of phase, $F(2, 56) = 63.70$, $p < .001$, $\eta^2_p = .70$ and condition, $F(1, 28) = 5.35$, $p < .05$, $\eta^2_p = .16$. The main effect of session was not significant, $F(1, 28) = 3.61$, $p = .07$, $\eta^2_p = .11$.

The interaction between phase and condition was significant, $F(2, 56) = 50.60$, $p < .001$, $\eta^2_p = .64$. Post hoc paired comparisons with Bonferroni's correction, as presented in Figure 3.1, indicate that in the baseline phase, participants reported significantly more false alarms in the experimental than in the control condition, mean difference = .24, 95% CI [.07, .40], $p < .01$. In contrast, the false alarm rate in the experimental condition was significantly lower than that in the control condition both

in the manipulation, mean difference = -.24, 95% CI [-.41, -.06], $p < .05$, and follow-up phases, mean difference = -.47, 95% CI [-.60, -.33], $p < .001$. In the control condition, the baseline false alarm rate was significantly higher than the manipulation phase false alarm rate, mean difference = .15; 95% CI [.03, .27], $p < .05$. False alarm rates in the other phases of the control condition did not differ significantly, mean difference between the manipulation and follow-up phases = -.02, 95% CI [-.13, .08], $p = 1.00$, and mean difference between the baseline and follow-up phases = .13, 95% CI [-.02, .27], $p = .10$. In the experimental condition, the baseline false alarm rate was significantly higher than both the manipulation, mean difference = .62, 95% CI [.44, .80], $p < .001$, and follow-up, mean difference = .83, 95% CI [.66, 1.00], $p < .001$, false alarm rates. False alarm rate in the manipulation phase of the experimental condition was significantly higher than that in the follow-up phase, mean difference = .21; 95% CI [.07, .35], $p < .01$.

There was a significant interaction between condition and session, $F(1, 28) = 18.30$, $p < .001$, $\eta^2_p = .40$. Bonferroni corrected post hoc tests, as shown in Figure 3.2, indicate that, when the control condition was the first session, participants produced significantly more false alarms than in the experimental condition, mean difference = .44, 95% CI [.24, .65], $p < .001$; the overall false alarm rates did not differ between the conditions when the experimental condition was the first session, mean difference = -.13, 95% CI [-.31, .04], $p = .15$. Participants who had the control condition in the first session reported more false alarms in the control condition than the participants who had the experimental condition in the first session (and thus had the control condition in the second session), mean difference = .48, 95% CI [.18, .78], $p < .01$. The groups did not differ in the experimental condition, however; mean difference = -.09, 95% CI [-.28, .09], $p = .32$.

Interactions between phase and session, $F(1, 56) = .15$, $p = .86$, $\eta^2_p = .01$, and phase, condition, and session were not significant, $F(1, 56) = .16$, $p = .87$, $\eta^2_p = .01$.

3.3.1.2 Hit rate. A mixed ANOVA revealed that the main effects of phase, $F(2, 56) = 91.73$, $p < .001$, $\eta^2_p = .77$ and condition, $F(1, 28) = 8.57$, $p = .01$, $\eta^2_p = .23$

Table 3.1

Mean (standard deviation) SSDT Response Outcomes in the Baseline, Manipulation, and Follow-Up Phases of the Experimental and Control Conditions in the Sessions of Study 2

SSDT responses	Control condition						Experimental condition					
	Baseline		Manipulation		Follow-up		Baseline		Manipulation		Follow-up	
	LA	LP	LA	LP	LA	LP	LA	LP	LA	LP	LA	LP
First session = control condition												
FA rate[a]	-.82 (.48)	-.48 (.28)	-.94 (.43)	-.62 (.31)	-.97 (.41)	-.56 (.37)	-.84 (.29)	-.53 (.16)	-1.34 (.37)	-1.19 (.38)	-1.73 (.24)	-1.36 (.36)
Hit rate	.57 (.31)	.75 (.15)	.47 (.30)	.71 (.25)	.37 (.25)	.55 (.24)	.59 (.17)	.74 (.15)	.35 (.22)	.51 (.25)	.15 (.13)	.22 (.15)
Sensitivity[a]	.52 (.11)	.52 (.11)	.51 (.09)	.54 (.09)	.48 (.10)	.46 (.09)	.57 (.08)	.56 (.11)	.55 (.12)	.58 (.11)	.51 (.11)	.51 (.10)
Bias	.35 (.87)	-.25 (.56)	.63 (.81)	-.11 (.83)	.80 (.72)	.11 (.81)	.38 (.35)	-.12 (.24)	1.10 (.55)	.72 (.40)	1.62 (.56)	1.25 (.51)

(continued)

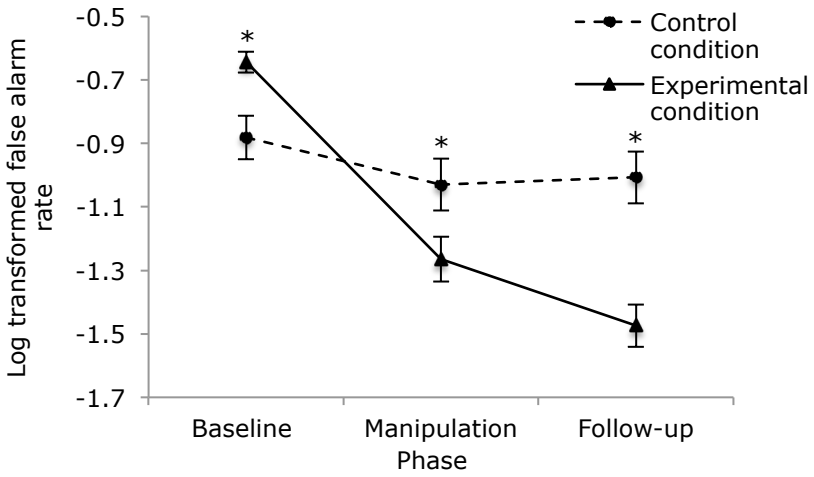
Table 3.1

Mean (standard deviation) SSDT Response Outcomes in the Baseline, Manipulation, and Follow-Up Phases of the Experimental and Control Conditions in the Sessions of Study 2

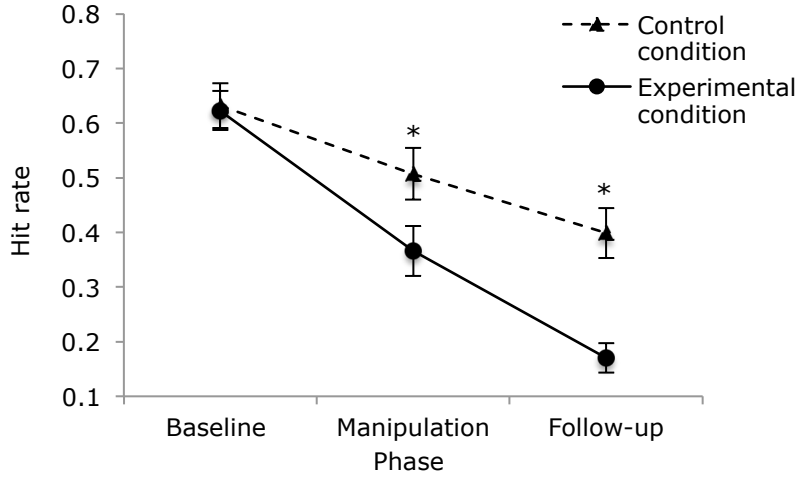
	Control condition						Experimental condition					
	Baseline		Manipulation		Follow-up		Baseline		Manipulation		Follow-up	
SSDT responses	LA	LP	LA	LP	LA	LP	LA	LP	LA	LP	LA	LP
First session = Experimental condition												
FA rate[a]	-1.23 (.49)	-1.00 (.45)	-1.35 (.53)	-1.20 (.58)	-1.32 (.52)	-1.18 (.55)	-.70 (.26)	-.49 (.17)	-1.36 (.45)	-1.12 (.46)	-1.48 (.40)	-1.33 (.56)
Hit rate	.53 (.29)	.68 (.21)	.38 (.28)	.47 (.27)	.26 (.29)	.41 (.28)	.50 (.24)	.65 (.23)	.24 (.24)	.36 (.30)	.13 (.15)	.18 (.17)
Sensitivity[a]	.58 (.11)	.58 (.12)	.54 (.10)	.56 (.11)	.48 (.13)	.53 (.12)	.51 (.08)	.52 (.10)	.49 (.13)	.51 (.11)	.45 (.09)	.42 (.15)
Bias	.73 (.66)	.45 (.72)	1.04 (.74)	.80 (.73)	1.23 (.77)	.87 (.78)	.38 (.52)	-.05 (.48)	1.26 (.60)	.89 (.72)	1.58 (.54)	1.35 (.76)

Note. FA = false alarm; LP = light present condition; LA = light absent condition.

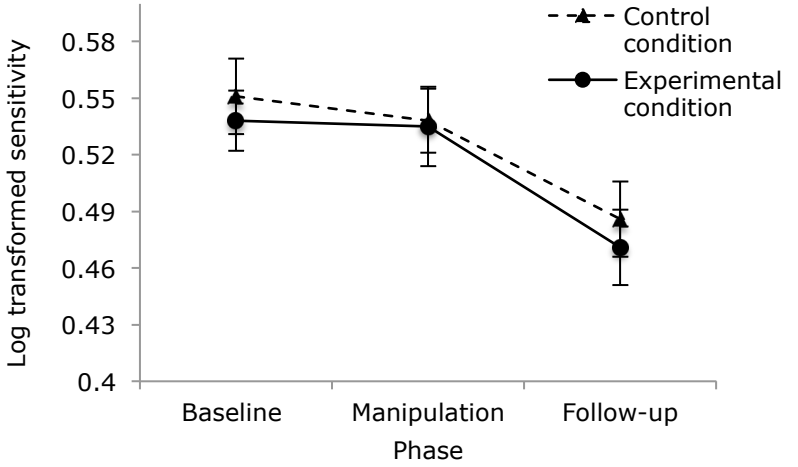
[a]Log transformed score.



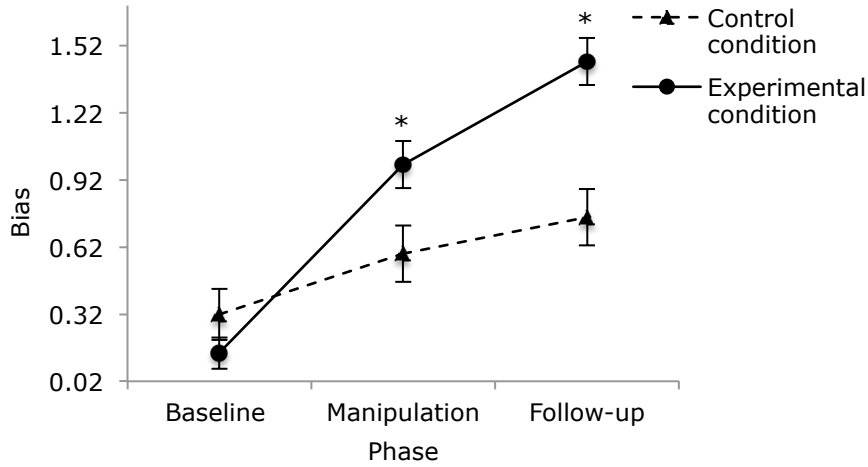
$F(2, 56) = 50.60$, $p < .001$, $\eta^2_p = .64$



$F(2, 56) = 12.45$, $p < .001$, $\eta^2_p = .31$



$F(2, 56) = .15$, $p = .86$, $\eta^2_p = .01$



$F(2, 56) = 29.95$, $p < .0001$, $\eta^2_p = .52$

Figure 3.1. Phase by condition interactions for the SSDT response outcomes. Error bars are standard errors.

were significant. The main effect of session was not significant, $F(1, 28) = 2.89$, $p = .10$, $\eta^2_p = .09$. The interaction between phase and condition was significant, $F(2, 56) = 12.45$, $p < .001$, $\eta^2_p = .31$ (see Figure 3.1). Bonferroni corrected post hoc tests indicate that, in both the conditions, hit rate in the baseline was significantly higher than in the manipulation phase, mean difference in the control condition = .13, 95% CI [.06, .19], $p < .001$, mean difference in the experimental condition = .26, 95% CI [.16, .35], $p < .001$, and follow-up phases, mean difference in the control condition= .23, 95% CI [.14, .33], $p < .001$, mean difference in the experimental condition = .45, 95% CI [.36, .55], $p < .001$. In both conditions, the manipulation phase hit rate was significantly higher than that in the follow-up phase, mean difference in the control condition = .11, 95% CI [.05, .17], $p < .001$, mean difference in the experimental condition = .20, 95% CI [.10, .29], $p < .001$. Post hoc tests further indicate that hit rates in the control and experimental conditions did not differ significantly in the baseline phase, mean difference = .01, 95% CI [-.10, .12], $p = .86$. Hit rate in the control condition was significantly higher than that in the experimental condition both in the manipulation and follow-up phases, mean difference in the manipulation phase = .14, 95% CI [.04, .24], $p < .01$, mean difference in the follow-up phase = .23, 95% CI = [.13, .33], $p < .001$.

There were no significant interactions between phase and session, $F(1, 56) = 1.18$, $p = .31$, $\eta^2_p = .04$, condition and session, $F(1, 28) = .19$, $p = .67$, $\eta^2_p = .01$, and phase, condition, and session, $F(2, 56) = .99$, $p = .38$, $\eta^2_p = .03$.

3.3.1.3 Sensitivity. Mixed ANOVA showed that the main effect of phase was significant, $F(2, 56) = 17.64$, $p < .001$, $\eta^2_p = .39$. Bonferroni corrected post hoc tests indicate that sensitivity in the follow-up phase was significantly lower than that in the baseline, mean difference = -.07, 95% CI [-.10, -.03], $p < .001$, and manipulation phase, mean difference = -.06, 95% CI [-.09, -.03], $p < .001$. However, there was no significant difference in sensitivity between the baseline and manipulation phases, mean difference = .01, 95% CI [-.02, .03], $p = 1.00$.

The main effects of condition, $F(1, 28) = .20$, $p = .67$, $\eta^2_p = .01$, and session, $F(1, 28) = .23$, $p = .64$, $\eta^2_p = .01$, were not significant.

The interactions between phase and condition, $F(2, 56) = .15$, $p = .86$, $\eta^2_p = .01$ (see Figure 3.1), phase and session, $F(2, 56) = .93$, $p = .40$, $\eta^2_p = .03$, condition and session, $F(1, 28) = 4.75$, $p = .04$, $\eta^2_p = .15$, and phase, condition, and session, $F(2, 56) = .10$, $p = .90$, $\eta^2_p = .004$, were not significant.

3.3.1.4 Bias (response criterion). Mixed ANOVA indicates that the main effects of phase, $F(2, 56) = 114.43$, $p < .001$, $\eta^2_p = .80$, and condition, $F(1, 28) = 9.19$, $p < .01$, $\eta^2_p = .25$, were significant. The main effect of session was not significant, $F(1, 28) = 3.95$, $p = .06$, $\eta^2_p = .12$.

There was a significant interaction between phase and condition, $F(2, 56) = 29.95$, $p < .0001$, $\eta^2_p = .52$. Bonferroni corrected post hoc tests, as presented in Figure 3.1, indicate that there was no significant difference in bias between the two conditions in the baseline phase, mean difference = .17, 95% CI [-.11, .46], $p = .23$, but participants responded "yes" significantly less both in the manipulation, mean difference = -.40, 95% CI [-.62, -.18], $p < .01$, and follow-up phases, mean difference = -.70, 95% CI [-.95, -.45], $p < .001$, of the experimental condition compared to the corresponding phases of the control condition. In both the control and experimental conditions, participants were significantly more likely to say "yes" in the baseline than in the manipulation, mean difference in the control condition= -.27, 95% CI [-.40, -.14], $p < .001$, mean difference in the experimental condition = -.84, 95% CI [-1.09, -.59], $p < .001$, and follow-up phases, mean difference in the control condition = -.43, 95% CI [-.61, -.27], $p < .001$, mean difference in the experimental condition = -1.30, 95% CI [-1.58, -1.01], $p < .001$. Bias towards responding "yes" in the manipulation phase of both the conditions were significantly higher than that in the follow-up phase, mean difference in the control condition = -.16, 95% CI [-.30, -.03], $p < .05$, and mean difference in the experimental condition = -.46, 95% CI [-.68, -.24], $p < .001$.

The interaction between condition and session was significant, $F(1, 28) = 6.27$, $p = .02$, $\eta^2_p = .18$ (see Figure 3.3). Bonferroni corrected post hoc tests on the interaction between condition and session indicate that, in the control condition, participants who had the control condition as the first session were significantly more biased towards the "yes" response than the participants who had the experimental condition in the first session, mean difference = -.60, 95% CI [-1.08, -.11], $p < .05$. The groups did not differ in the experimental condition, however; mean difference = -

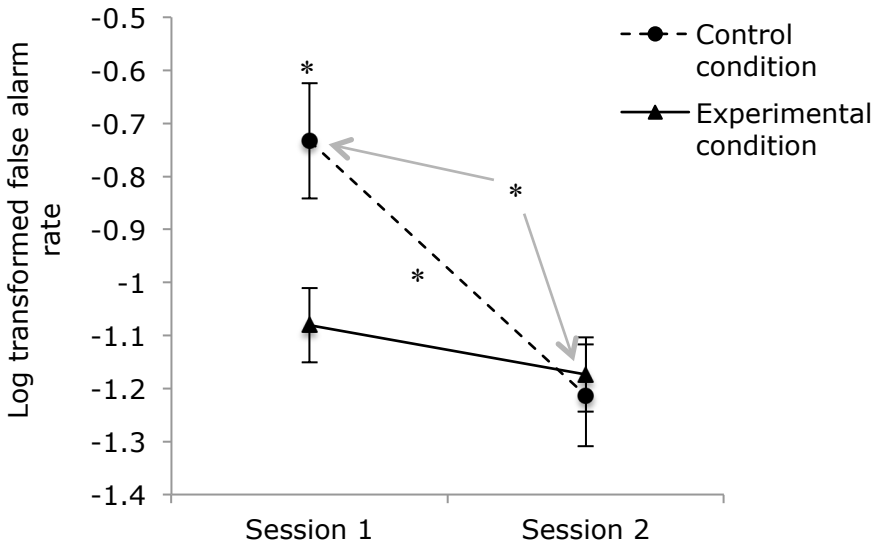


*Figure 3.2. C*ondition by session interaction for the SSDT false alarm rate. Error bars are standard errors.

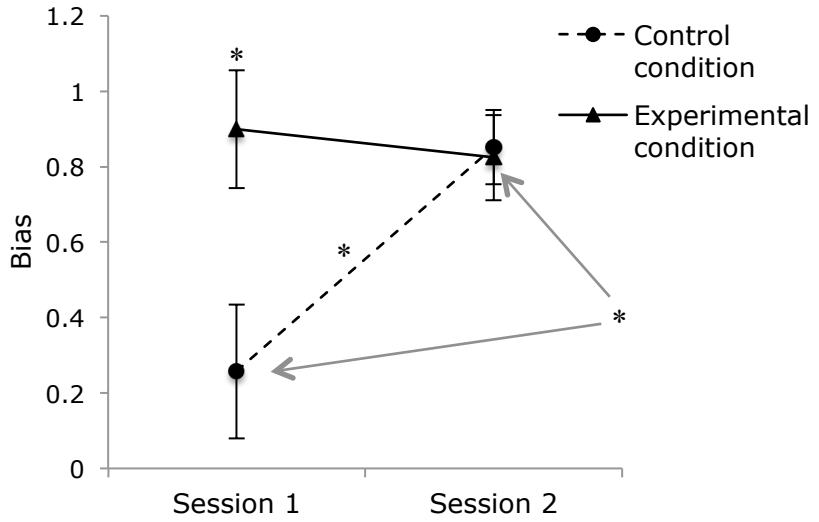


Figure 3.3. Condition by session interaction for the SSDT response bias. Error bars are standard errors.

.08, 95% CI [-38, -.23], $p < .62$. Participants whose first session was the control condition were significantly more biased towards saying "yes" in the control than in the experimental condition, mean difference = -.57, 95% CI [-.89, -.25], $p < .01$. For participants whose first session was the experimental condition, bias did not differ significantly between the two conditions, mean difference = -.05, 95% CI [-.33, -.23], $p = .74$.

3.3.2 Hypothesis 2: SSDT Conditioning Will Transfer to SPS Responses, Resulting in Reports of Fewer SPSs and Changes in Their Properties (i.e. Intensity, Pleasantness, Certainty, and Extent)

Descriptive statistics on SPS responses are presented in Table 3.2.

3.3.2.1 Number of SPSs. Mixed ANOVA indicates that the main effect of phase was significant, $F(1, 28) = 8.73$, $p < .01$, $\eta^2_p = .24$. In contrast to the hypothesis, Bonferroni corrected post hoc test indicates that participants reported significantly more SPSs in the follow-up than in the baseline phase, mean difference = .26, 95% CI [.08, .43], $p < .01$.

The main effects of condition, $F(1, 28) = .32$, $p = .58$, $\eta^2_p = .01$, and session, $F(1, 28) = 3.32$, $p = .08$, $\eta^2_p = .11$, were not significant.

There was a significant interaction between condition and session, $F(1, 28) = 6.18$, $p < .05$, $\eta^2_p = .18$. Bonferroni corrected post hoc tests, as shown in Figure 3.4, indicate that number of SPSs did not differ between the conditions when the control condition was the first session, mean difference = .18, 95% CI [-.11, .47], $p = .21$, but when the experimental condition was the first session, participants reported significantly more SPSs in the experimental than in the control condition, mean difference = .28, 95% CI [.03, .54], $p < .05$. In the control condition there was no significant difference between the groups with regard to the number of SPSs reported, mean difference = -.16, 95% CI [-.60, .29], $p = .47$. In the experimental condition, the participants who completed the experimental condition in the first session reported significantly more SPSs than the participants who completed the control condition in the first session, mean difference = .62, 95% CI [.11, 1.14], $p < .05$.

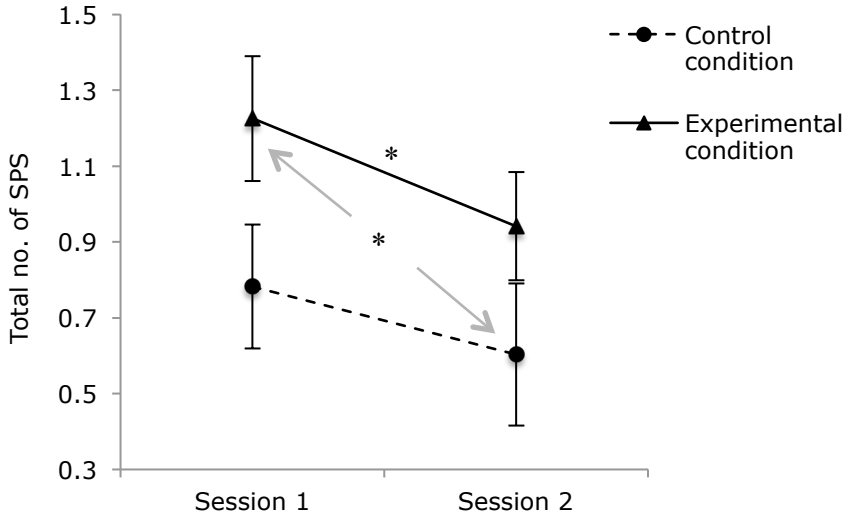


*Figure 3.4. C*ondition by session interaction for the total SPSs reported. Error bars represent standard errors.

Table 3.2

Mean (Standard Deviation) SPS Task Outcomes in the Baseline and Follow-Up Phases of the Experimental and Control Conditions in the Sessions of Study 2

SPS task variables	Control condition		Experimental condition	
First session = Control condition	Baseline phase	Follow-up phase	Baseline phase	Follow-up phase
Total no. of SPSs	.67 (.54)	.90 (.67)	.59 (.63)	.62 (.62)
Intensity of SPS	3.55 (1.92)	3.39 (1.24)	4.35 (2.09)	3.22 (1.07)
Pleasantness of SPS[a]	.26 (.21)	.22 (.14)	.30 (.10)	.33 (.06)
Certainty about SPS	2.90 (1.65)	2.32 (1.31)	3.07 (1.48)	2.45 (1.29)
Extent of SPS[a]	4.78 (.56)	4.56 (35)	4.57 (.62)	4.25 (.58)
First session = Experimental condition				
Total no. of SPSs	.80 (.62)	1.08 (.79)	.98 (.67)	1.47 (.92)
Intensity of SPS	3.86 (1.87)	4.20 (1.40)	3.55 (1.35)	4.56 (1.14)

(continued)

SPS task variables	Control condition		Experimental condition	
Pleasantness of SPS[a]	.34 (.13)	.27 (.13)	.29 (.18)	.29 (.16)
Certainty about SPS	2.94 (1.04)	3.34 (1.09)	3.33 (1.27)	3.58 (1.08)
Extent of SPS[a]	4.62 (.57)	4.58 (.41)	4.48 (.31)	4.73 (.43)

Note. [a]Log transformed score.

There were no significant interactions between phase and condition, $F(1, 28) = .001$, $p = .97$, $\eta^2_p < .001$, phase and session, $F(1, 28) = 2.16$, $p = .15$, $\eta^2_p = .07$, and phase, condition, and session, $F(1, 28) = 2.37$, $p = .14$, $\eta^2_p = .08$.

3.3.2.2 Intensity, Pleasantness, and Certainty about SPS. None of the main and interaction effects was significant (see Appendix O).

3.3.2.3 Extent of SPS. None of the main and interaction effects was significant (see Appendix O) except phase by session interaction which was nearly significant, $F(1, 15) = 6.78$, $p = .02$, $\eta^2_p = .31$.

3.3.3 Hypothesis 3: There Will be a Positive Correlation Between Baseline SSDT False Alarm Rate, Baseline Total Number of SPSs, and Symptom Reporting

Spearman's correlation coefficients between the variables are presented in Table 3.3. In contrast to our hypothesis, none of the coefficients was significant.

Table 3.3

Summary of Intercorrelations, Medians, and Interquartile Ranges (IQR) for SSDT False Alarm Rates in the Baseline in Light Present and Light Absent Trials of the First Session, Total Number of SPSs Reported in the Baseline of the First Session, and PHQ-15 Scores (n = 76)

Measures	1	2	3	*Median*	*IQR*
1. PHQ-15	--			4	3 to 8
2. FA-LA	-.13	--		.13	.09 to .21
3. FA-LP	.06	42*	--	.13	.09 to .28
4. Total SPSs	.17	.14	.09	.67	.33 to 1.3

Note. PHQ-15 = The Patient Health Questionnaire-15; FA = false alarm; LA = light absent trials; LP = light present trials; SPS = spontaneous sensation.
*$p < .001$.

3.4 Discussion

The SSDT findings in the present study are the mirror image of the corresponding findings in Study 1, where we sought to increase the false alarm rate. As predicted, rewarding correct rejections and punishing false alarms decreased the false alarm rate and hit rate and conditioned participants to say "yes" less often, whereas sensitivity did not differ between the groups across the phases. These findings, together with those of Study 1, confirm our presumption that operant conditioning can effectively change the SSDT false alarm rate in both directions. The effectiveness and persistence of the training in changing the false alarm rate is further evident in the phase by condition interactions of Study 1 and 2. In both studies, and contrary to expectation, a significant difference in the baseline false alarm rate was found when a comparison was made between participants with reference to whether they experienced SSDT training in the first session. When participants underwent SSDT training in the first session, the resulting effects seemed to carry over to influence their baseline responses in the control condition in the next session, even though there was gap of at least seven days between the sessions. The significant condition by session interactions for both the false alarm rate and response bias in Study 2 provided additional evidence in support of the enduring effect of the training. The false alarm rate was significantly lower in the experimental condition when it was the second session, whereas the false alarm rate did not differ between the conditions when the experimental condition was the first session. This suggests that learning (of the SSDT training) in the first session was carried over to influence responses in the second session. Likewise, there was a significant difference in response criterion between the sessions when the control condition was the first session, but such a difference was not found when the experimental condition was the first session. This suggests that the response criterion of the first session (as conditioned by SSDT training) was carried over to affect responses in the second session.

Contrary to expectation, the SSDT conditioning did not reduce the rate of SPS; rather, the findings were similar to those of Study 1, such that participants reported

more SPSs after the training that reduced SSDT false alarm rates. In addition, like the condition by session effects on SSDT bias and false alarm rates in Study 2, the experimental condition produced more SPSs than the control condition if it was the first session but no such effect was found when the control condition was the first session (the same results were found in Study 1). Although the order of conditions was counterbalanced across participants, the presence of order and practice effects in both Studies 1 and 2 suggests that a between-subjects design would be more appropriate for research of this sort.

Another limitation of this study and Study 1 is that the majority of participants in the experimental condition won more than they lost, which most likely affected their mood and motivation during the procedure. This is evident in sleepiness scores indicating that participants were more alert in the experimental than in the control condition (see secondary analyses on sleepiness in Appendix N). Reward-related changes in mood and motivation of participants could have systematically affected their subsequent experiences of SPS, particularly when they had the opportunity to win extra money in the first session of the study (i.e., when the experimental condition was first) but not in the second. This might explain why participants reported more SPSs following the training condition. Though no study has yet investigated possible relationship between affect-related physiological arousal and reporting of SPS, previous research suggests that affective states (i.e. moods) affect noticing and attending normal body sensations (Watson & Pennebaker, 1989). One way of dealing with this possible confounding effect would be to introduce rewards into the control condition to make the conditions more comparable, in a manner that creates a pleasant experience of winning without introducing any form of learning. This would control for the emotional and motivational effects of being rewarded, without training particular responses on the task.

The SPS test is also potentially problematic because the focus for the participant's responses on this task (i.e., the non-dominant hand) is also the site of stimulation on the SSDT task. As the SSDT involves attending to and detecting tactile stimulation in the index finger over many trials, it is likely that prolonged attention to

the hand will cause to perceive more SPSs (Michael & Naveteur, 2011; Michael et al., 2012) no matter what the SSDT training entails. Fatigue in the non-dominant hand due to its over use might be another reason why participants reported more SPSs in Study 2 when SSDT conditioning was expected to lessen it. An alternative approach would be to ask participants to focus on the entire body instead of just on the non-dominant hand to identify and report SPS.

It is also possible that the SPS task is not the most appropriate test to detect the generalization effect of SSDT conditioning. Compared to experiences on the SSDT, the concept of SPS perhaps is much more general and ill-defined for participants. In the SSDT, participants know exactly what stimulation to look for and over what period, meaning that decisions about the presence of the stimulus are much more straightforward. In the SPS task, it is less clear what participants should attend to and when such experiences might arise. As there is no specific target sensation, the number and types of tactile experiences that participants can report depend entirely on them, meaning that experimenters do not have necessary control over the test and variability is very high. For example, total SPSs reported in Studies 1 and 2 ranged between 0 to 5 and 0 to 8 respectively. Combining all the phases and conditions, around 20% participants in Study 1 and 18% participants in Study 2 did not report any SPS. The lack of experimental control increases the risk that an unknown number of extraneous variables might confound responses to the task, which is a potential threat to the validity of findings particularly if confounding effects vary between individuals and groups. With this in mind, a task similar to the SSDT in a different sensory modality might be more suitable to study the presumed transfer of any conditioning effects. Non-significant relationships between the SSDT false alarm rate and total SPSs in the baseline further suggests that the SPS test is unsuitable for studying perceptual transfer of conditioning on the SSDT.

Contrary to the findings of previous SSDT studies (e.g. Brown et al., 2012; Katzer, Oberfeld, Hiller, & Witthöft, 2011), the correlation between baseline false alarm rate and somatization was not significant. This questions the reliability of the relationship between these variables. One possible explanation for the absence of this

effect might be that participants in the present studies (Studies 1 and 2) answered the somatic symptom measure (PHQ-15) online at least one week before performing the SSDT. Though PHQ-15 is a reliable measure and participants report symptoms experienced in the last four weeks, the time gap might introduce variability that would be absent if the questionnaire was administered on the same day of carrying out the SSDT.

CHAPTER 4

Study 3: Development and Validation of a Voice-Hearing Task

> As I lie in bed on the threshold between wakefulness and sleep, I often hear both male and female voices utter short emphatic sentences and, every once in a while, my name.
>
> —Siri Hustvedt, The Shaking Woman or A History of My Nerves, p. 226

Transfer of SSDT training to responses to a novel task within the same perceptual modality (i.e. the SPS task) was not clearly evident in Studies 1 and 2. It seems that the SPS task, in its present form, was not suitable for studying the effects of SSDT conditioning on other tactile experiences, perhaps because the concept and experiences of SPS were too broad and heterogeneous among participants and the body part used for this task (i.e. the non-dominant hand) may have suffered from fatigue because of its prolonged involvement in the SSDT task. To address these shortcomings, it was suggested that the SPS task should be modified to include the whole body and to introduce a signal detection task in a different modality.

On the SSDT, false alarms are typically explained as the result of top-down processes, that is, excessive activation of touch representations in memory (Lloyd et al., 2008). There is a similar account of auditory hallucination-like experiences as the result of perceptual expectations (Vercammen & Aleman, 2010; Waters et al., 2012) and intrusive thoughts and memories related to audition (Badcock & Hugdahl, 2012). In the same way that the SSDT measures somatosensory distortions (Brown et al., 2012), voice detection tasks measure hallucinatory experiences relating to audition which are seen not only in clinical populations but also in healthy individuals (Waters et al., 2012). In fact, numerous laboratory investigations into schizotypy have used various procedures to induce anomalous auditory experiences in healthy participants to study the cognitive and neurological processes underlying auditory hallucination. As the models underpinning the SSDT and auditory signal detection tasks share some common views about anomalous perceptual experiences, a voice-hearing task seems appropriate to investigate cross-modal transfer of the SSDT training.

One of the common techniques used to study hallucinatory voices in laboratory settings is to present white noise alone or together with words, phrases, or sentences,

and to ask participants to indicate if they heard a voice during each trial. In a study on fantasy proneness, for example, participants listened to three minutes of white noise and were asked to press a button every time they clearly heard one or more segments of the song *White Christmas* embedded in it (Merckelbach & van de Ven, 2001). In another study by Hoskin, Hunter, and Woodruff (2014), 72 sentences (divided into high and low constraint frames based on how easy it was to predict the last word) were presented in a neutral male voice in 288 trials. White noise replaced the last word (a noun) in half of the trials and masked it in the other half (signal-to-noise ratio was set so the target speech was detectable in 80% of the trials). The participant's task, was to indicate whether they heard any speech in white noise each trial and rate how confident they were about their responses (on a continuum from 1 meaning 'uncertain' to 4 meaning 'certain').

Some auditory signal detection studies have investigated both the occurrences and contents of auditory hallucinations. For example, Vercammen and Aleman (2010) presented five to seven word sentences masking the last word (i.e. the target) with white noise. There were total of 150 trials among which 50 had predictable targets (i.e. the words that 75% of the respondents in a pilot study used to complete the sentences), 50 had unpredictable targets (i.e. the words that none of the respondents in the pilot study used to complete the sentences), and 50 had no targets except white noise. Signal-to-noise ratio was set to a level at which 70% of the trials were correctly identified (by a pilot sample). Participants were instructed to press a response button if they heard a word and to utter the word loud if they were sure about it, but to say unsure if they were uncertain about the identity.

In some studies, voice stimuli were distorted (instead of using white noise) to make them ambiguous to participants. Feelgood and Rantzen (1994), for example, used a one trial task in which they spliced randomly selected 1-second sections of a five-minute long backwardly played male voice and asked participants to detect and write words and phrases that they perceived in it.

Cueing paradigms have also been used to study hallucinatory voices. Ilankovic et al. (2011), for example, compared speech perception between paranoid

schizophrenic patients and healthy volunteers. They used the participant's own face and another person's face as cues and 192 personal adjectives, half of which were recorded in the participant's own voice and the other half in unfamiliar male and female voices as targets. The pitch was shifted by -4 semitones to distort half of the own and half of the alien voices. The words (i.e. targets) were presented through headphones and the participant's task was to indicate whether they heard their own or someone else's voice.

Though a number of paradigms have been used to study auditory hallucinations, the findings are comparable (see Appendix P for details). Participants, in general, reported auditory false alarms and hallucination prone individuals and hallucinatory patients had significantly more distortions in auditory experiences than healthy individuals and controls.

A common strategy of the auditory tasks, as described here, is to present ambiguous voice stimuli so that top-down processes, related to suggestibility and expectations, influence auditory perceptions of participants. Auditory false alarms, thus produced, are congruent with the SSDT false alarms because they are also believed to be the results of top-down processes involving touch representations in the memory. However, using familiar stimuli, such as meaningful words, phrases, and sentences probably is not the ideal way to study perceptual anomalies as unrelated memory processes might confound task performance. Studies have found that individuals can recognize both familiar speech and non-speech sounds under difficult listening conditions due to their familiar sound recognition ability (Kidd, Watson, & Gygi, 2007). Though the auditory tasks employed different techniques to make the stimuli ambiguous, the possibility that the level of ambiguity may not be the same for all participants due to individual differences in familiarity with, and sensitivity towards, words cannot be ignored. Evidence that participants rate background white noise as less loud when target stimuli are familiar (Jacoby, Allan, Collins, & Larwill, 1988) supports this possibility. One might argue that familiarity was not a problem as the tasks discriminated between patients and healthy individuals in terms of how many hallucinatory voices they reported, but it is imperative that the influence of

irrelevant cognitive processes remain under control to better understand the perceptual and cognitive mechanisms underlying anomalous experiences. This can be achieved by using nonsense auditory stimuli. Consistent with this, Davis, Johnsrude, Hervais-Adelman, Taylor, and McGettigan (2005) found that familiarity with distorted stimuli facilitated learning but the effects disappeared when phonologically similar but unfamiliar non-word sentences were used. An auditory task with nonsense voices will match well with the SSDT as both of them present nonsense stimuli to participants.

A limitation of discrete-trial auditory signal detection tasks is that they are likely to be less sensitivity to individual differences in task performance; they dictate when to respond or limit how many responses (e.g. false alarms) one can produce (unless they administer several thousand trials, which may not be suitable for some studies). Two participants, for example, scoring the maximum are considered to be similar on the psychological construct measured, but they might have performed differently if they had complete freedom and control over their responses. A continuous one-trial task might be the solution as, within a predefined trial length, there is no restriction or insistence on responding; as such, participants depend more on their judgment than on repeated instructions from a task. In addition, it may not be sensible to use a discrete-trial auditory signal detection task alongside the SSDT as they might appear to similar to participants, leading them to give similar responses across the tasks due to the common method.

Another criticism is that psychometric properties of the existing auditory tasks are unknown. Though some of the tasks were used in multiple studies (e.g. the tasks of Barkus et al. [2007], Merckelbach and van de Ven [2001], and Hoffman et al. [1995]), none of them were tested for stability of task performance over time. As a result, we do not know whether the tasks would yield the same (or similar) results if they were repeated. Without determining reliability of a task, we cannot be sure of study findings—they might be produced by some potential extraneous variables. The advice of Wilkinson and the APA Takforce on Statistical Inference (1999) to authors reflects this concern. They urged researchers to provide reliability coefficients of measures used in studies even if that is not directly related to study objectives. In

some studies, the absence of reliability information was accompanied by subjective decision about the amplitude of auditory stimuli and inadequate description of how the signal-to-noise ratio was decided (e.g. the tasks of Bentall and Slade [1985a], Galdos et al. [2011], Randell, Goyal, Saunders, and Reed [2011], and Vercammen et al. [2008]). Also, it is not explicit how neutrality of speech stimuli was determined (e.g. the task of Galdos et al. [2011]) or what the voice stimuli contained (e.g. the task of Barkus et al. [2007]). The problems of ignoring psychometric properties while constructing psychophysical tasks are evident in Bentall and Slade's (1985a) interpretation of their findings that "subjects differed widely in the frequency with which they used the different rating categories available to them, but most used predominantly 1s and 5s, perhaps reflecting the fact that the [auditory signal detection] test was not as difficult as had been anticipated" (p. 162). Table 4.1 summarizes some of the limitations of the existing auditory tasks used to study auditory hallucinations.

Due to the limitations of the available auditory signal detection tasks, we have developed a brief voice-hearing task and determined its psychometric properties (i.e. reliability and validity). This is a continuous one trial task and allows participants to give as many responses as they want, which we believe has made the task sensitive to individual differences. We have used nonsense voices as stimuli to control for the potential confounding effects of variations in stimulus familiarity. As the task takes only a few minutes to complete, we expect it to be suitable for administering alongside the SSDT. Though it is a brief and a simple task, each step of its development was highly systematic, methodical, and logical.

4.2 Method

4.2.1 Design

To determine consistency in hearing illusory voices on the newly developed task over time, test-retest reliability was assessed. To evaluate construct validity (i.e. whether the task measured what it was supposed to measure), aberrant perception, bodily symptoms (both general and current), and affect measures were used. As the voice-hearing task is designed to investigate anomalous perception of hearing voices

Table 4.1

Limitations of the Auditory Tasks Used to Study Auditory Hallucinations

Study	Problems with the stimuli and task	Variations in stimulus familiarity as a confound	Discrete trials and therefore restricted range	Too long	Inadequate no. of trials
Merckelbach and van de Ven (2001)	--	✓	--	--	--
Hoskin, Hunter, and Woodruff (2014)	--	✓	✓	✓	--
Vercammen, de Haan, and Aleman (2008)	✓	✓	✓	--	--
Bentall and Slade (1985)	✓	✓	✓	✓	--
Barkus, Stirling, Hopkins, McKie, and Lewis (2007)	✓	--	✓	✓	--
Moseley, Fernyhough, and Ellison (2014)	--	✓	✓	✓	--
Vercammen and Aleman (2010)	--	✓	✓	--	--
Randell, Goyal, Saunders, and Reed (2011)	✓	✓	✓	--	✓
Galdos et al. (2011)	✓	✓	✓	✓	--
Hoffman et al. (1995)	✓	✓	--	--	--
Feelgood and Rantzen (1994)	✓	--	--	--	--
Ilankovic et al. (2011)	--	✓	✓	✓	--

Note. "Problems with the stimuli and task" comprises all the auditory tasks characterized by complete absence or inadequate information on stimuli features, such as stimuli selection criteria or signal-to-noise ratio, etc. This also includes the experimental paradigms which may not be convenient for some studies as one or more trained raters (along with an experimenter) are required to interpret the responses in these tasks.

(i.e. voice false alarms), it was expected that false alarms in the task would have significant positive correlation with auditory hallucination proneness, but weak or no relationship with bodily aberration, symptoms, and affect.

4.2.2 Participants

A total of 117 participants (89 females) comprising students and staff of the University of Manchester and aged between 18 and 43 years (mean age = 24.04 years, *SD* = 5.43) took part in the study. Of these, 52 (35 females; age range 18-43 years; mean age = 27.31, *SD* = 5.40) participated in the test-retest reliability aspect of the study. Undergraduate Psychology students received course credits for participation and others received monetary compensation at the end of the experiment. The University of Manchester Research Ethics Committee approved the study.

4.2.3 Materials

4.2.3.1 The voice-hearing task.

***4.2.3.1.1 Task description*.** The voice-hearing task consisted of a continuous, 4.5 minutes stream of white noise over which nonsense speech stimuli of different amplitudes were randomly presented. The same structure was followed to develop a one-minute practice task. In both the practice and main tasks, participants pressed the spacebar each time they thought they had heard a voice. Prompt responses to voice stimuli were identified as hits and others (i.e. responses in the absence of any stimuli) were considered to be false alarms.

4.2.3.1.1.1 Speech stimuli. PassMaker (version 1.2; Rohr, 2013) was used to generate random English letter strings to compose 70, seven-letter nonsense 'words', which were then converted into WAV speech files using a text-to-speech software programme (Balabolka, Version 2.9; Morozov, 2014; Ivona, Version 2014) with rate = 0, pitch = 0, volume = 100, duration = 800ms and voice = Brian (male).

To ensure that the voice stimuli were nonsense, 10 native English speakers (seven PhD students and three faculty members of the University of Manchester) used a three-point rating scale (*very similar, a little bit similar, or not at all similar*) to evaluate whether each utterance was similar to a real English word. Participants

listened to words one at a time (at full volume on the laptop) in a quiet room. Eight (80%) raters identified 17 and seven (70%) raters identified 16 utterances not at all similar to actual English words. The former 17 utterances were selected for use in the main test and the latter 16 were selected for the practice test.

Auditory thresholds of a pilot sample consisting of 19 participants aged 18-40 years (mean age = 25, *SD* = 2.47) were determined to select the amplitudes of the voices to be used in the voice-hearing task. For this, a computer algorithm called the parameter estimation by sequential testing (PEST) was written on E-Prime software. The PEST began with a loud and easily perceptible nonsense voice of 31.16 dB played against 44100Hz continuous white noise (maximum amplitude normalized to -55 dB). The voice appeared in the middle of a randomly selected one of two consecutive 1020ms intervals in a trial. A downward pointed 250ms green arrow indicated the start of each interval. The arrows were marked as 1 and 2 to help participants to identify the interval that contained the voice. Participants responded using a keyboard (pressed 1 and 2 to indicate the first and the second intervals respectively). The sound stimuli (i.e. white noise and voices) were presented thorough headphones. The rest message "Please take a quick rest. Press space to continue" appeared on the screen after every 80 trials, although participants were able to rest at any time. Correct responses in consecutive trials resulted in lowering of voice intensity whereas successive incorrect responses were followed by an increase in voice amplitude. More specifically, Wald's Sequential Probability Ratio Test (SPRT) [N(c) (no. of correct responses) - Pt. N (T) (probability threshold value (0.75) multiplied by current trials completed) ≥ W (W's limits were: 1 - -1)] was used to change the amplitude of the voice which depended on the responses given on all trials since it reached its current intensity. The threshold was defined as the voice amplitude at which the voice was correctly detected in 75% of the trials. Though the number of trials required to determine threshold varied between individuals, the computer algorithm was programmed to run a maximum of 250 trials. The thresholds of all the participants were determined before reaching the 250th trial. However, if the maximum number of

trials were reached, the average sound intensity in the last 50 trials would be taken as the auditory thresholds for the participants.

The mean threshold (as determined by E-Prime) was -5936.8421 (*SD* = 160.591, range -6300 to -5700). It was decided to use -5900 (an approximation to the mean threshold for the pilot sample) as the central amplitude of the voice-hearing task and go three steps (one step = -150, roughly equal to the SD of the thresholds obtained from the pilot sample) up and two steps down for the other amplitudes. Thus the sound intensities that were used in the voice-hearing task were -5450, -5600, -5750, -5900, -6050, and -6200. As the voice-hearing task did not involve any thresholding step, using a range of sound amplitudes around the mean threshold obtained from the pilot sample maximized the likelihood of encompassing participants' actual auditory thresholds, whilst introducing a degree of variability in the perceptibility of the voices. This would make the task sufficiently ambiguous (and therefore increase the probability of false alarms) whilst remaining face valid to participants.

The practice task consisted of high amplitudes of sound as well as those used in the main task, in order to (i) familiarize participants with the task; and (ii) determine their general reaction time to clearly audible voices, which was later used to define the criterion for false alarms in the main task. Thus, there were 18 different amplitudes in the practice test: -1000, -1500, -2000, -2500, -3000, -3500, -4000, -4500, -4700, -4900, -5000, -5200, -5450, -5600, -5750, -5900, -6050, and -6200.

Voices were played randomly against the background of white noise in both the practice and main tasks. The intervals between consecutive voices were optimised such that voices did not appear to be overlapping with one another and the test did not become too lengthy. Thus, in the practice test, a random interval between a minimum of 1 second and a maximum of 2 seconds was used in-between two successive utterances. An approximately one-minute practice trial required 24 sound files to cover all the volume levels.

In the main test, a randomly determined gap of 3 to 10 seconds was used between two successive voices. This task consisted of 18 sound files with 16

utterances played once and one played twice. Both the sound files and amplitudes were randomly selected. It took around 4.5 minutes to complete the main test.

***4.2.3.1.2 Data analysis*.** In both the practice and main tests, participants gave multiple responses in between successive voice stimuli as they responded each time they thought that a voice was present. In the practice test, the first response given after the presentation of a voice stimulus was examined to determine if it was an outlier relative to the reaction time of other first responses to the remaining stimuli (Hoaglin & Iglewicz, 1987). All the outliers were discarded as they were unlikely to represent participants' actual response time to voice stimuli. The additional responses to a stimulus (i.e. second, third, forth, and fifth and so on) were identified as false alarms and were excluded as they were not true responses to voice stimuli. The mean and standard deviation of the remaining practice test reaction time data were then determined to identify hits and false alarms in the main test. Hits in the main test were defined as any response time within 2SD from the mean of practice data (after excluding false alarms and outliers as described above) and the rest were defined as false alarms (i.e. response time below or above 2SD from the practice mean). False alarms in the main test also constituted additional responses to a voice stimulus as in the practice test.

4.2.3.2 Questionnaires.

4.2.3.2.1 The Launay-Slade hallucination scale (LSHS). A modified version (Bentall & Slade, 1985b) of the Launey-Slade Hallucination Scale (Launay & Slade, 1981) was used to measure individuals' tendency to hallucinate (see Appendix T). The items were about auditory hallucinations, for example, "In the past, I have had the experience of hearing a person's voice and then found that no one was there"; visual hallucinations, for example, "On occasions, I have seen a person's face in front of me when no one was in fact there"; vivid thoughts, for example, "Sometimes a passing thought will seem so real that it frightens me"; intrusive thoughts, for example, "No matter how hard I try to concentrate, unrelated thoughts always creep into my mind"; and vivid daydreams, for example, "The sounds I hear in my daydreams are generally clear and distinct". Participants selected one of the five alternative

responses (e.g. 'Certainly Applies', 'Possibly Applies', 'Unsure', 'Possibly Does Not Apply' and 'Certainly Does Not Apply') to indicate the extent to which each item was applicable for them. Total scores ranged between 0 and 48; the higher the score the more an individual was likely to report hallucinatory experiences. The test-retest reliability of the scale is considered very good, $r = .8421$, $p < .01$ (Bentall & Slade, 1985b). The present study also found good internal consistency of the scale, Cronbach's $\alpha = .79$.

4.2.3.2.2 The perceptual aberration scale (PAS). The 35-item perceptual aberration scale (Chapman, Chapman, & Raulin, 1978) assessed how frequently participants had unusual perceptions related to body image, vision, and audition (see Appendix U). Five types of body image aberration were measured (by 28 items), namely (i) unclear boundaries of the body, for example, "Sometimes I have felt that I could not distinguish my body from other objects around me"; (ii) unreal feelings of detached body parts, for example, "I have sometimes felt that some part of my body no longer belongs to me"; (iii) feeling that the body is decaying, for example, "I have sometimes had the feeling that my body is decaying inside"; (iv) unusual experience about the size, relative proportions, or spatial relationships between the body parts, for example, "Sometimes part of my body has seemed smaller than it usually is"; and (v) perceiving an altered appearance of the body, for example, "I have had the momentary feeling that my body has become misshapen". The other items were about visual and auditory aberrations, for example, "For several days at a time I have had such a heightened awareness of sights and sounds that I cannot shut them out". Participants were asked to indicate whether an item was true or false for them. The total score ranged between 0 and 35 with higher scores indicating more perceptual aberrations. The scale had good internal consistency reliability in previous studies (all Cronbach's $\alpha > .88$ as obtained from college students, people with schizophrenia, normal controls, and outpatients [Chapman et al., 1978]) as well as in the present study, Cronbach's $\alpha = .82$.

4.2.3.2.3 The patient health questionnaire-15 (PHQ-15). This is the same questionnaire that was used in studies 1, 2, 4, and 5 (see Appendix G). Participants

were asked to indicate to what extent 15 physical symptoms (such as, headaches, stomach pain, dizziness, etc.) bothered them during the past four weeks (Spitzer et al., 1999). The scale has good psychometric properties (Zijlema et al., 2013). Cronbach's alpha coefficient in the present study was .78.

4.2.3.2.4 The positive and negative affect scale (PANAS). The PANAS measured participants' general experience of positive and negative affect (see Appendix V). It has 20 items (10 for positive affect and 10 for negative affect) with a 5-point scale ranging from 1 (*very slightly or not at all*) to 5 (*extremely)*. Total scores for each affect measure range from 10 to 50 with higher scores indicating heightened experience of the affects in general. Studies have reported excellent reliability and validity of the scale (Watson, Clark, & Tellegen, 1988; Watson, Wiese, Vaidya, & Tellegen, 1999). Cronbach's alpha coefficients in the present study were .88 and .91 for positive and negative affects respectively.

4.2.3.2.5 Pennebaker symptom checklist. A modified version of the Pennebaker symptom checklist (Pennebaker, 1982) was used to measure whether participants experienced symptoms (e.g., headache, watery eyes, racing heart, etc.) while performing the task (see Appendix W). This modified version contained two items (i.e. pain and fatigue) in addition to the original 12 items on the checklist. Scoring for each item ranged from 0 (meaning complete absence of the symptom) to 6 (meaning acute state of the symptom). The total score ranged between 0 and 72 with higher score indicating elevated experience of symptoms while performing the task. The 12-item version has previous been found to have good internal consistency (Cronbach's α = .75) and moderate test-retest reliability with an interval of 1 month between sessions (r = .21; Pennebaker, 1982). The internal consistency reliability of the modified scale in the present study was quite good, Cronbach's α = .84.

4.2.4 Procedure

4.2.4.1 Test-retest reliability. Study advertisements (see Appendices Q and AD) were posted on a University research volunteering website and notice boards around the campus. Interested participants used a study website to register and to access the participant information sheet (see Appendices R and AE), consent form

(see Appendix S), and available time slots to take part in the study. They participated in the study as per their convenience but it was ensured that an interval of approximately three weeks was maintained in between the sessions. The auditory task was described to the participants at the outset of each session. They then performed the practice test followed by the main task. It took approximately six minutes to complete both the tasks. In the second session, participants answered the questionnaires following completion of the auditory tasks. In both sessions, participants took part at about the same time of the day.

4.2.4.2 Validity. Along with the participants who took part in the reliability study, a different group of participants (predominantly psychology undergraduates) were tested in the validation study. The recruitment procedure for these participants was the same as the reliability study. The testing procedure was the same as that in the second session of the reliability study (i.e. performing the practice and main auditory tasks followed by the questionnaires). Two researchers carried out this part of the study—a male researcher (i.e. myself) tested the participants who took part in both the reliability and validity studies and a female researcher (a 3rd year undergraduate student) tested the participants (as part of her undergraduate research project) who took part only in the validity study.

4.2.4.3 Statistical analysis. Descriptive statistics were used to summarize the scores on the measures. Correlation coefficients between the voice-hearing task parameters on the two testing occasions and between the task parameters and scores on the questionnaires were determined to assess test-retest reliability and validity of the task respectively. It is to be noted that for the participants who took part in the reliability study, their auditory task data in the first session has been used for the validity study to ensure comparability of data, as approximately half of the validity study participants performed the task once (i.e. were not tested in two separate occasions). Data were examined to check whether they satisfy the assumptions of parametric correlation (i.e. Pearson product-moment correlation) and multiple regression. It was found that the variables were non-normal with many outliers. Transformation of scores did not correct the problem and other options, such as

removing or replacing extreme outliers were not found to be the ideal solutions (as they would distort a signification portion of the data). It has therefore been decided only to use nonparametric statistics (e.g. median, interquartile range, Spearman's correlation) to analyze reliability and validity data of the auditory task.

4.3 Results

4.3.1 Test-retest reliability of the voice-hearing task

Descriptive statistics for hits, false alarms, and total voices on the two testing occasions are presented in Table 4.2 which demonstrates that participants produced approximately equal number of hits and false alarms across the sessions, although there was wide individual differences in task performance. There was a significant positive correlation between task performance on the two testing occasions (Table 4.3), r = .391, .822, and .821 for total hits, total false alarms, and total voices respectively (all ps < .01).

Table 4.2

Median and Interquartile Range of Hits, False alarms, and Total Voices on the Two Testing Occasions of the Voice-hearing Task (n = 52)

Testing Session	Hits		False alarms		Total voices	
	Mdn	IQR	*Mdn*	IQR	*Mdn*	IQR
First	8	5-10	8.5	5-17.25	17.5	11.25-26.75
Second	8	5-12	11	6-20.75	20	12.25-31.25

Note. IQR = interquartile range.

4.3.2 Validity of the voice-hearing task

Descriptive statistics of the questionnaires and voice-hearing task parameters are presented in Table 4.4. Spearman's rho (Table 4.5) demonstrates that voice false alarms had a significant correlation with hallucination proneness, r = .23, p < .05. The remaining correlation coefficients between the task parameters and responses on the questionnaires were not significant. In other words, voice-hearing task performance was unrelated to body aberration, symptom, and affective state of individuals.

Table 4.3

Spearman's Correlation Coefficients Between the Voice-Hearing Task Parameters on the Two Testing Sessions (n = 52)

Variables	1	2	3	4	5
1. Hit 1	-				
2. FA 1	.28*	-			
3. Total 1	.59****	.91****	-		
4. Hit 2	.39***	.48****	.59****	-	
5. FA 2	.25	.82****	.74****	.42***	-
6. Total 2	.38**	.82****	.82****	.71****	.92****

Note. Hit 1 = total hits in the first session; FA 1 = total false alarms in the first session; Total 1 = total voices reported in the first session; Hit 2 = total hits in the second session; FA 2 = total false alarms in the second session; Total 2 = total voices reported in the second session.
*$p < .05$. ** $p < .01$. ***$p < .005$. ****$p < .001$.

Table 4.4

Median and Interquartile Range of the Voice-Hearing Task Parameters and Questionnaires (n = 117)

Variables	*Mdn*	IQR
Voice-hearing task		
Hits	9	6-14
False alarms	7	5-11
Total voices	19	13-26
Questionnaires		
Launay-Slade hallucination scale	15	10-21
Perceptual aberration scale	3	1-6
PANAS positive affect	29	25-33
PANAS negative affect	18	12-23
Patient health questionnaire-15	7	5-11
Pennebaker symptom checker	6	2-13

Note. IQR = interquartile range; PANAS = the positive and negative affect scale.

Table 4.5

Spearman's Correlation Coefficients between the Voice-Hearing Task Parameters and Questionnaires (n = 117)

Variables	1	2	3	4	5	6	7	8	9
1. Hit 1	-	-.10	.64**	-.05	.12	-.09	-.12	.17	.04
2. FA 1		-	.63**	**.23***	.02	.06	-.06	-.02	.02
3. Total 1			-	.06	.09	-.03	-.12	.07	.02
4. Launay-Slade hallucination scale				-	.53**	.47**	-.06	.48**	.50**
5. Perceptual aberration scale					-	.45**	-.15	.40**	.48**
6. PANAS negative affect						-	-.06	.58**	.46**
7. PANAS positive affect							-	-.18	-.12
8. Patient health questionnaire-15								-	.70**
9. Symptom checker									-

Note. Hit 1 = total hits in the first session; FA 1 = total false alarms in the first session; Total 1 = total voices reported in the first session.
*$p < .05$. ** $p < .001$.

4.4 Discussion

In order to investigate cross-modal transfer of the SSDT conditioning (Studies 4 and 5), we developed a voice-hearing task and investigated its psychometric properties. The new task is brief and distinct from the SSDT, free from possible confounding effects of familiarity with stimuli, sensitive to individual differences, and highly systematic in the development of stimuli and processing of response data.

Correlational analysis demonstrated that the voice-hearing task responses were fairly consistent over time. The relationship between false alarms in the two sessions was very strong, that between hits was weak, and between hits and false alarms in both the sessions was weaker than the other correlation coefficients obtained. In other words, as expected, false alarms on the voice-hearing task were very highly consistent over time, hits less so, and the relationship between hits and false alarms was modest. The findings are comparable to those of McKenzie, Poliakoff, Brown, and Lloyd (2010) who also found stable false alarms but unstable hits on the SSDT over time. This pattern of findings suggests that there seems to be a different process operating in hits and false alarms but there is also something that overlaps between them; perhaps this is a response criterion in the general sense.

The findings that false alarms on the task were highly stable (more than hits) not only indicate its reliability but also suggest its usefulness to study auditory hallucination and similar experiences in other sensory modalities (like vibrotactile false alarms on the SSDT). This is supported by the significant positive correlation between voice false alarms and psychosis proneness which, in turn, suggest that the false alarms in the task are likely to be the result of expectations and beliefs (i.e. top-down processes) about hearing of voices which are akin to the processes considered to be instrumental in experiencing auditory hallucinations (Vercammen & Aleman, 2010; Waters et al., 2012). That correlations of psychosis proneness with hits and total voices were not significant further substantiate the hallucinatory feature of voice false alarms in the task. Relationships of the voice-hearing task with the other measures characterize its nature and scope too. Correlational analysis has suggested that the task is not related to affect, health or body related anomalies. Such

specificity in scope (i.e. the task measures auditory hallucination like experiences only) is desirable as the task is expected to be precise in determining the cross-modal effects of the SSDT training without being mingling with other processes.

The voice-hearing task has adopted a unique design and procedure which are likely to mitigate potential limitations of the existing auditory signal detection tasks. We therefore believe that this new task, along with investigating the SSDT conditioning effects, can potentially be used with other signal detection tasks and to study auditory hallucinations in both healthy and clinical groups. Flexibility in structure and procedure is another key advantage of the new task. Its features, such as contents of the voices (e.g. meaningfulness, emotional valence, etc.), task duration, criteria used to differentiate hits and false alarms, etc. can easily be modified to fit the objectives of a study. The voice-hearing task is therefore expected to have a wide range of potential uses in the study of perceptual processing and anomalous experience.

CHAPTER 5

Study 4: Does Training Increase Illusory Somatosensory Experiences and Generalise to Separate Perceptual Tasks?

> Nobody really knew what was wrong with me. . . . but why I had become a vomiting, miserable, flattened, frightened ENORMOUS headache, a Humpty Dumpty after his fall, no one could say.
>
> —Siri Hustvedt, The Shaking Woman or A History of My Nerves, pp. 16-17

The results of Study 1 and 2 (reported in Chapters 2 and 3 respectively) demonstrated that SSDT response outcomes (i.e., hits, false alarms, and bias) can be changed with training using reward and punishment. However, there was no concrete evidence for transfer of training to other somatosensory experiences as measured by the spontaneous sensation (SPS) test. Participants reported significantly more SPSs in the follow-up phase of Studies 1 and 2 no matter what condition (control vs. training) they were in. Analysis further showed that significantly more SPSs were reported in the experimental condition of both the studies if it was the first session (i.e., there was no difference between the conditions if the first session was the control condition). In retrospect, it was perhaps not surprising to obtain such confusing results, as the literature on perceptual training is very mixed: some studies find transfer of perceptual training within and across the modalities whereas others do not. For example, temporal discrimination training in the auditory modality (using feedback on errors in perceptual judgment) improved performance in the same as well as in a different modality involving a visual temporal discrimination task (Bratzke, Schröter, & Ulrich, 2014). Similarly, Nagarajan, Blake, Wright, Byl, and Merzenich (1998) found training to discriminate between temporal intervals marked by somatosensory vibrations transferred to a different skin location on the same hand and the corresponding skin location on the other hand. The training also improved discrimination of similar temporal intervals when presented as an auditory tone. Studies have also demonstrated transfer of perceptual training to motor performance. For example, training on an auditory temporal interval discrimination task facilitated performance on a motor task that involved participants using their thumb (which was

kept hidden from view) to press a button twice in succession to match the standard intervals used in the perceptual paradigm (Meegan, Aslin, & Jacobs, 2000). In contrast, Lapid, Ulrich, and Rammsayer (2009) found that training on an auditory temporal discrimination task transferred to the auditory but not to the visual modality, whereas Bratzke, Seifried, and Ulrich (2012) found that auditory temporal discrimination training transferred to the visual modality but not vice versa.

To account for such inconsistent findings, Proulx, Brown, Pasqualotto, and Meijer (2014) suggested that the transfer of perceptual learning is facilitated when stimuli presented to different sensory modalities are related in their salient features (such as location, duration, etc.). Though the SPS task and SSDT use the same part of the body (i.e., the non-dominant hand), they differ in key respects. The SSDT stimulus (i.e., tactile vibration) is well defined in terms of its intensity and presentation but the stimulations underlying SPSs are complex, reflecting multiple body receptors (e.g. cutaneous receptors and interoceptors), brain areas, and psychological processes (Michael & Naveteur, 2011). It is unknown whether SPSs can actually be false alarms (i.e. participants might incorrectly report sensations when in fact there was no body sensation) and no task has yet been developed to determine that. It is possible that some sensory activities are always present when we experience SPSs and thus they might be unrelated to SSDT false alarms. To determine whether SSDT training generalizes to other modalities, it is arguably more appropriate to use a perceptual task that generates and measures false alarms. In this respect, the voice-hearing task (VHT; presented in Chapter 4) seems suitable, as it is similar to the SSDT *apropos* its stimulus features (i.e., both the tasks use threshold level nonsense stimuli) and response outcomes (i.e., both the tasks produce hits and false alarms).

Another finding of study 1 and 2 that contrasts with previous studies (e.g., Brown et al., 2012; Katzer, Oberfeld, Hiller, & Witthöft, 2011) is the absence of a significant relationship between false alarm rates on the SSDT and severity of somatic symptoms. As has been mentioned in Chapter 3, it is possible that an interval of seven days between measuring somatic symptoms and performing in the SSDT may have introduced unknown variables that confounded the relationship. To examine this

possibility, both the symptom measure (i.e. the Patient Health Questionnaire-15) and SSDT were administered during the same testing session.

In Chapter 4, we have seen that false alarms on the VHT had significant correlation with psychosis proneness as measured by the Launay-Slade hallucination scale (Bentall & Slade, 1985b). This scale covered hallucinatory tendency including auditory and visual hallucinations, vivid and intrusive thoughts, and vivid daydreams. However, it is unknown whether voice false alarms have relationships with other features of psychosis, such as hypomania, impulsivity, social anhedonia, etc. The present and the next study (see Chapter 6) give us further opportunity to examine the scope of voice false alarms (i.e. to identify additional features of psychosis that relate to voice false alarms) by administering the VHT and a scale measuring features of psychosis different from what was assessed during the development of the task.

In this study and the next (see Chapter 6), we used the VHT to investigate whether conditioning on the SSDT transfers to a different sensory modality, whilst attempting to replicate our initial results when some of the limitations of Study 1 and 2 are addressed. In addition, correlates of voice false alarms (as investigated in Study 3) are further examined. The objectives of the present study were to:

1. Use a between-subjects design and an improved control condition to replicate the primary findings of Study 1 (i.e., that rewarding hits and punishing misses on the SSDT would lead to an increase in the false alarm rate that would persist in the follow-up phase). We also aimed to replicate the secondary findings that conditioning led to a persistent increase in hit rate and alteration in response bias, but kept sensitivity relatively stable.

2. Investigate whether conditioning of SSDT responses would increase reporting of SPS on a measure focusing on the whole body rather than just the hand, thereby addressing the concern that recording SPS only in the non-dominant hand (as was done in Studies 1 and 2) may become contaminated by fatigue and changes in sensations in the hand due to its prolonged use and focusing on its index finger during the SSDT trials.

3. Investigate whether conditioning of SSDT responses would transfer to a similar but unrelated perceptual task in the auditory modality (i.e., false alarms on the VHT).

4. Investigate whether SSDT false alarm rates would correlate with total SPSs and voice false alarms at baseline.

5. Investigate whether SSDT false alarm rate relates to severity of somatic symptoms (as measured by the patient health questionnaire-15) and voice false alarms to psychosis proneness (as measured by the mini-psychosis proneness scale).

5.2 Method

5.2.1 Study Design

A mixed design was used to carry out the study, where condition (control vs. experimental/training) was the between-subject variable and phase (baseline vs. manipulation vs. follow-up) and light (present vs. absent) were the within-subject variables. As explained in Chapters 2 and 3, the presence of the light on the SSDT was not the main variable of interest in this study and thus its effects are presented in Appendix X as secondary analysis. The SSDT measures (i.e., hit rate, false alarm rate, bias, and sensitivity), voice hearing task measures (i.e., voice hits, voice false alarms, total voices), and SPS task measures (i.e., number of SPSs, intensity of SPS, pleasantness of SPS, and certainty of SPS) were the dependent variables.

5.2.2 Participants

A total of 75 participants (Female = 41, 54.67%) comprising students (n = 65, 86.67%) and staff of the University of Manchester volunteered for the study. Their age ranged between 19 and 39 years (M = 23.92. SD = 4.96). All but four of the participants were right handed as determined using the Edinburgh Handedness Inventory (Oldfield, 1971).

Descriptive statistics (i.e., mean and standard deviation) of the changes in false alarm rate (i.e., baseline false alarm rate – follow-up false alarm rate) in the control and experimental conditions of Study 1 were used to calculate the required sample size for this study. Using G*Power (Faul et al., 2007), it was found that nine participants in each group (total 18) would be sufficient to detect the training effects

on false alarms, given that effect size (d) = 1.44, power (1- β) = .80, and level of significance (α) = .05. As this study was carried out simultaneously with another study with the aim of decreasing false alarms (Study 5) and participants' baseline false alarm rates were unknown, we ended up testing 75 low false alarm individuals (which gave the study power > .99 with d = 1.44 and α = .05) to obtain a sufficient number of high false alarm participants for Study 5.

Inclusion criteria were being aged between 18 and 40 years and having a good understanding of instructions in English. Exclusion criteria were non-corrected visual impairment, having a medical condition that might affect the sense of touch and hearing, and participation in previous SSDT studies (as these individuals would already be familiar with the objectives and structure of the task).

5.2.2.1 Recruitment of participants. The recruitment procedure was the same as that for Studies 1 and 2 except that there was no online PHQ-15; all the questionnaires were administered on the day of the experiment after finishing the tasks.

5.2.3 Materials

5.2.3.1 Questionnaires. The same set of questionnaires (i.e., the Patient Health Questionnaire-15 [α = .68 in this study], short-form of the state scale of the Spielberger State-Trait Anxiety Inventory [in the present study, α = .70 and .66 in the baseline and follow-up phases respectively], the Karolinska Sleepiness Severity Scale, and Edinburgh Handedness Inventory) of Studies 1 and 2 were used in this study. In addition, the Mini-Psychosis Proneness scale (PPQ) was used to assess psychosis proneness of participants. This 12-item questionnaire (Hay et al., 2001; see Appendix Y) is based on Chapman and Chapman's six scales on psychosis (Chapman et al., 1984; Chapman, Chapman, & Raulin, 1976, 1978; Eckblad & Chapman, 1983, 1986; Eckblad, Chapman, Chapman, & Mishlove, 1982). The items are categorized into four scales, namely perceptual aberration-magical ideation (e.g., "Sometimes part of my body seems smaller than it really is"), hypomania-impulsivity/non conformity (e.g., "In unfamiliar surroundings, I am sometimes so assertive and sociable, that I surprise myself"), social anhedonia (e.g., "Although there are things

that I enjoy doing myself, I usually seem to have more fun when I do things with other people") and physical anhedonia (e.g., "I seldom care to sing in the shower"). Each item has two response options: true and false, which are scored 1 and 0 respectively, with the exception of three items that are reverse scored. Thus, total scores range between 0 and 12, with higher scores indicating greater psychosis proneness. Hay et al. (2001) reported satisfactory construct and predictive validity for each of the subscales. In the present study, however, the internal consistency reliability was very low, α = .21, .26, .59, and .15 for perceptual aberration-magical ideation, hypomania-impulsivity/non conformity, social anhedonia, and physical anhedonia respectively.

5.2.3.2 SSDT: Materials and procedure. The SSDT setup and procedure were the same as those in Studies 1 and 2 (see section 2.2.3.2). Tactile (i.e., vibration) perception threshold was determined for each participant, followed by 12 SSDT practice trials and eight blocks of 80 SSDT trials, where the first two blocks were the baseline, the next four were for training (i.e., conditioning the SSDT response outcomes with reinforcement and punishment), and the last two were for the follow-up phase. The frequency of hits, false alarms, misses and correction rejections were used to calculate hit rate, false alarm rate, sensitivity and bias. Like Studies 1 and 2, the average false alarm rate in light-present trials in the baseline blocks determined allocation of participants in this and the next study (i.e. Study 5).

5.2.3.3 SPS: Measurement and synthesis of data. The SPS test used in Studies 1 and 2 was modified to address the limitations identified there. Participants were asked to relax and focus on their whole body (instead of the non-dominant hand as in Studies 1 and 2) for 20 seconds (compared to 10 seconds in the previous studies; the duration was increased to allow participants to have adequate time to attend to the entire body). There was a practice trial followed by three main trials, each indicated by a verbal start and stop signal. After a trial, participants were given a body figure (see Appendix Z) to circle the areas where they felt the sensations. They were also given the same rating scale used in Studies 1 and 2 to indicate frequency, intensity, pleasantness, and certainty about the sensations (see Appendix

L). The three trials were averaged to obtain mean baseline and follow-up SPS response outcomes for each participant. In contrast to Studies 1 and 2, extent (i.e. body area) of SPS was not determined as it was not possible to draw a body figure on paper (like the hand figure of the previous studies) of the approximate size of the actual human body.

5.2.4 Procedure

The procedure was the same as that of Studies 1 and 2 except that the VHT was included in the task sequence and different groups of participants were allocated to the control and experimental/training conditions (i.e., between rather than within participants design). Study 5 (i.e., false alarm decreasing study) was carried out simultaneously and, like before, an E-Prime program controlled selection of participants for the two studies. This was a double blind technique: neither the researchers nor the participants knew who was in which study. The allocation of participants to the control and experimental conditions was randomly determined. The Microsoft Excel RAND function was used for this purpose, which generated a random number for each participant. The numbers (representing the participants) were then sorted in ascending order. First half of the participants were allocated to the control condition and the rest to the experimental condition. The baseline false alarm rate in the light present condition (0.15, compared to 0.16 in the previous studies) was used to identify low and high false alarm participants, with those below this rate being identified as low false alarm participants and selected for the present study; the remainder were identified as high false alarm individuals and were allocated to Study 5. The new false alarm criterion was approximate to the median false alarm rate found in Studies 1 and 2 combined and it was expected to ensure approximately equal number of participants for Studies 4 and 5. Like before, the only difference between the control and experimental conditions was in the manipulation phase (the middle four blocks): experimental participants were conditioned according to their responses and which study they were in whereas control participants received fixed pseudo reward and punishment. Unlike Studies 1 and 2, participants were not

required to remove any jewellery at the start of a session because the SPS task involved the whole body, not any specific part, such as the hand.

5.2.4.1 Control condition. The protocol for the control condition is shown in Figure 5.1. At the outset of the manipulation phase, participants were informed that they would get regular feedback about their performance in the form of winning or losing points after a certain number of trials. They were also told that this would improve their tactile perception and decision making. After every 40 trials, they saw a message on a computer screen stating how many points they had won or lost and what the cumulative score was. At the end of this phase, participants were informed that their total score was 250 points for which they got £2.50. This is the average amount of money that participants won in Studies 1 and 2 but was unrelated to the participants' actual performance. Table 5.1 shows how the amount of reinforcement and punishment was manipulated. The purpose of this pseudo conditioning was to control for the effects of winning money in the experimental condition, which was uncontrolled in the previous studies and may have affected the results. It was

Table 5.1

Predetermine Fixed Amount of Reward and Punishment Used in the Manipulation Phase of Studies 4 and 5 Which was Independent of Participants' Responses

After	Feedback message		
	You've won	You've lost	Your cumulative total point is
40th trial	130	125	5
80th trial	100	10	95
120th trial	50	75	70
160th trial	140	130	80
200th trial	60	80	60
240th trial	100	40	120
280th trial	200	40	280
320th trial	80	110	250

expected that this procedure would create a pleasant experience of winning comparable to that in the experimental condition, but they would not learn which responses resulted in the winning or losing of points because feedback was not given for individual trials; in this sense, the reward was expected to act as a general motivator rather than to train particular responses.

5.2.4.2 Experimental condition. The protocol for the experimental condition is shown in Figure 5.2. The procedure for the control and experimental condition was the same except that in the manipulation phase participants were allocated to this study or the next (i.e., Study 5) depending on their baseline false alarm rate in light present trials.

An important change that has been made in Studies 4 and 5 is in the amount of money that participants could win or lose in a trial. In the experimental condition of Studies 1 and 2, participants won or lost 10 points (i.e., 10p) for half of the randomly selected correct or incorrect responses respectively. In the present study the value of reinforcement and punishment was reduced to 5 points (i.e. 5p). More specifically, participants won 5 points for half of their randomly selected hits and lost the same amount for half of their randomly selected misses. This reduction in the amount of reward did not reduce participants' motivation to win as they were unaware of the total amount of money that could be won. Moreover, participants came to know about the possibility of winning money just before the experiment and it seemed that they felt positive about it and were motivated to win as much as possible. The reduction was made to ensure efficient use of research funds with the understanding that it would unlikely to diminish the conditioning effects of winning or losing money in the studies.

Another important addition in the manipulation phase of this study (both in the control and experimental conditions) was the use of small emoticons just above the message regarding the winning or losing of points. A yellow smiley-face emoticon was used when participants won points and a red sad-faced emoticon was used when participants lost points. It was expected that the use of happy and sad emoticons would strengthen the reward and punishment and also would act as a visual aid to the

feedback message (Derks, Bos, & von Grumbkow, 2007; Huang, Yen, & Zhang, 2008).

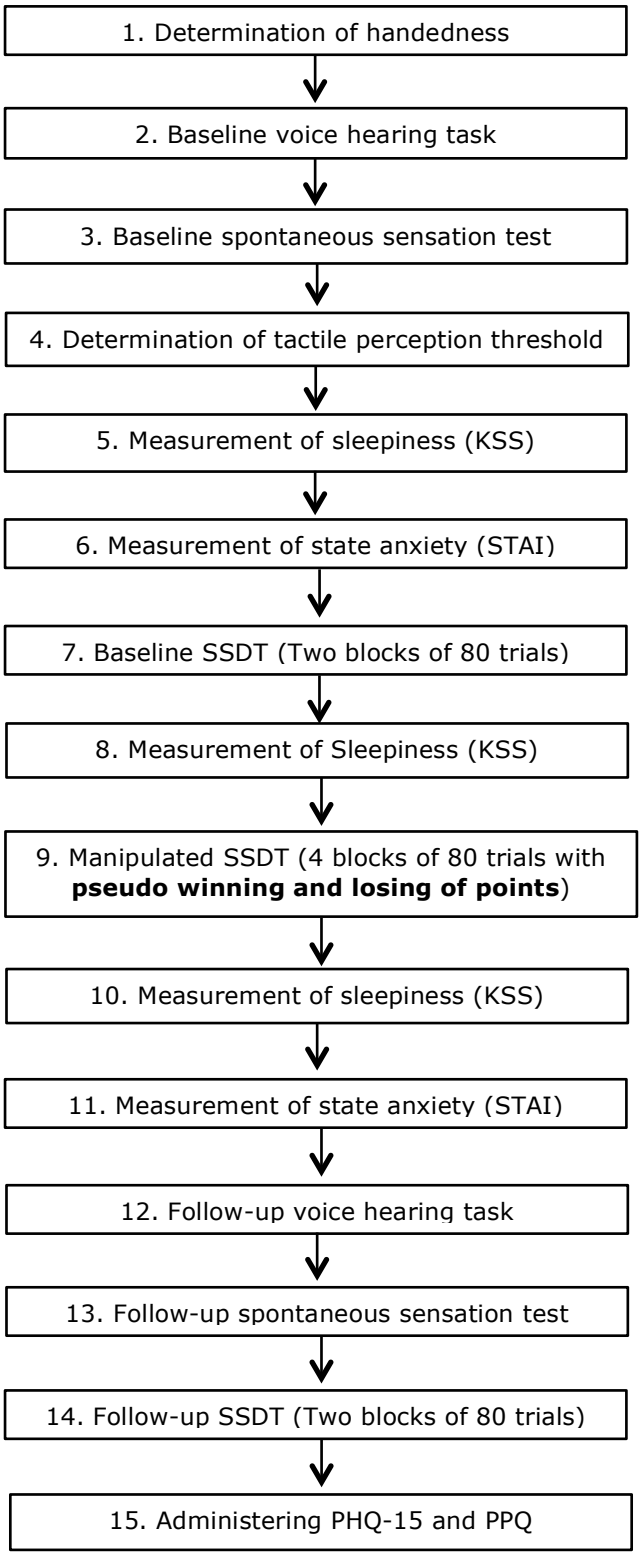


Figure 5.1

Sequence of tasks carried out in the control condition of Studies 4 and 5

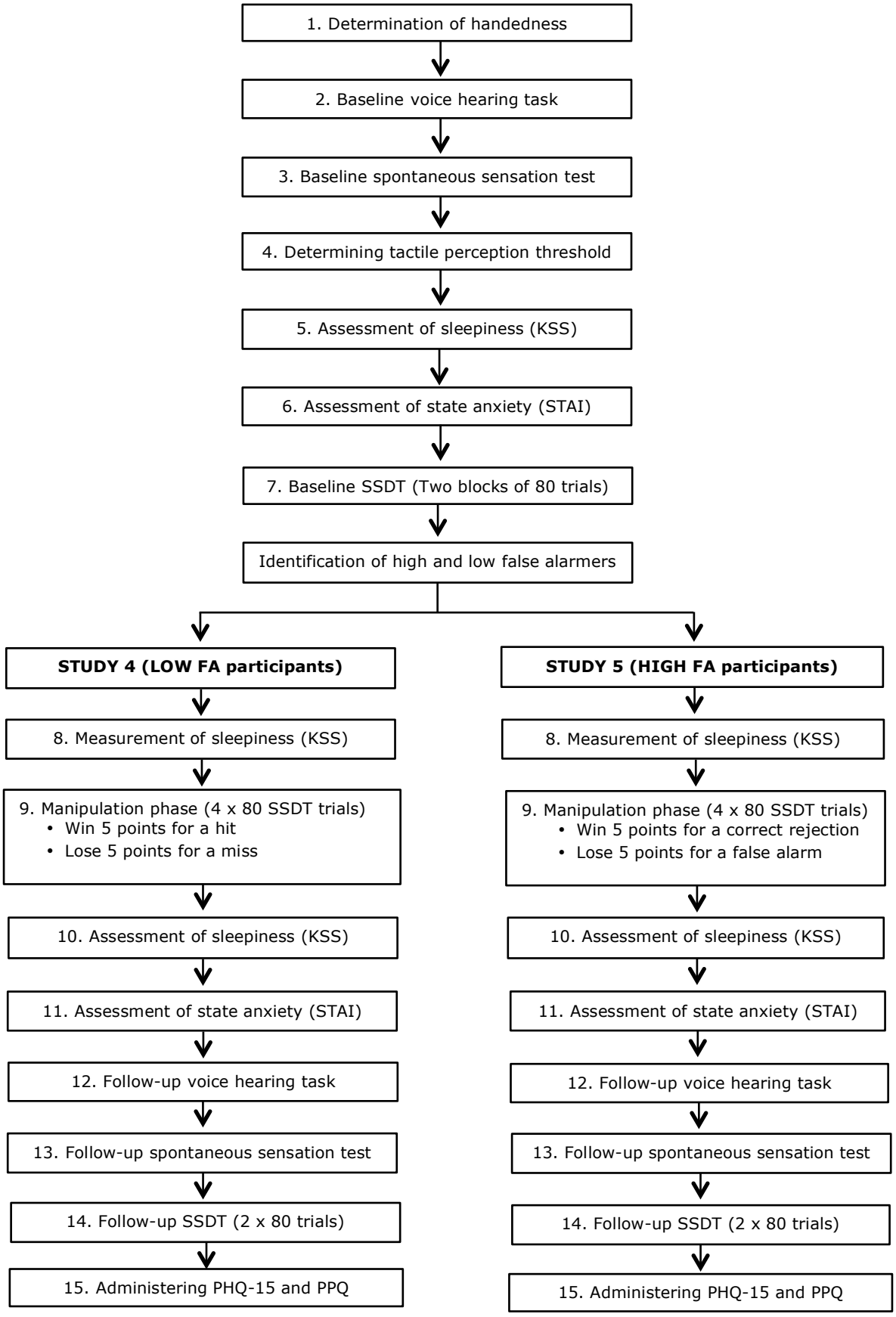


Figure 5.2

Sequence of tasks carried out in the experimental condition of Studies 4 and 5

At the end of the study, participants were asked what they thought the study was about, to see if they formed any idea or expectation about the experiment that might have undesirable effects on their responses. In addition to fixed compensation of eight experimental credits or £10 for psychology students and £10 for non-psychology students and staff, participants were given one penny for each point they won in the training phase.

5.2.4.3 Data preparation.

5.2.4.3.1 Exploring outliers and assumptions. Data from the SSDT, SPS test, and voice-hearing task were examined using the statistical approach described in Chapter 2 to identify outliers and violation of mixed ANOVA assumptions.

None of the SSDT response outcomes were normally distributed. Also total SPSs, intensity of SPS, voice false alarms, and total voices were non-normal. To make the distributions normal, different data transformation techniques were tried. Log transformation was found satisfactory for the SSDT false alarm rate and sensitivity data, reciprocal transformation for the SSDT hit rate data, and square root transformation for the SSDT bias, total SPSs, intensity of SPS, voice false alarms, and total voices data. However, none of the transformation solved the normality problem of the control condition baseline and follow-up SSDT false alarm rate in light present trials, $D(41) = .22$, $p < .001$ and $D(41) = .20$, $p < .001$ respectively; experimental condition baseline false alarm rate in both light absent and light present trials, $D(34) = .23$, $p < .001$ and $D(34) = .24$, $p < .001$ respectively; control condition baseline and follow-up SPS pleasantness, $D(39) = .21$, $p < .001$ and $D(39) = .17$, $p < .01$ respectively; and baseline total voices in the control condition. As the sample size was big ($n > 30$), mixed ANOVA is robust to some deviations from normality (Field, 2009), and most of the distributions in the analysis were normal, we decided to proceed with the parametric test.

Though the transformation of scores corrected problems in the data for most of the distributions, Levene's test indicated that the assumption of homogeneity of variance was violated by light present manipulation hit, $F(1, 73) = 4.14$, $p = .05$; light present follow-up hit, $F(1, 73) = 4.89$, $p = .03$; light present manipulation

sensitivity, $F(1, 73) = 4.03$, $p = .05$; and light absent follow-up sensitivity, $F(1, 73) = 4.88$, $p = .03$. It is to be noted that the variances in the baseline did not differ significantly between the groups and we expected that the SSDT manipulation might have differential effects on participants resulting in unequal variances between the groups. As the groups had equal variances for most of the dependent variables, we decided to use the parametric test (i.e., mixed ANOVA) to analyze the data.

Mauchly's sphericity test showed that the assumption of sphericity was violated by the main effect of phase on the false alarm rate ($\varepsilon = .70$, $p < .001$), main effect of phase on the hit rate ($\varepsilon = .85$, $p < .01$), phase x light on the hit rate ($\varepsilon = .84$, $p < .01$), main effect of phase on sensitivity ($\varepsilon = .91$, $p < .05$), phase x light on sensitivity ($\varepsilon = .90$, $p < .05$), and main effect of phase on bias ($\varepsilon = .82$, $p < .01$). Greenhouse-Geisser correction was therefore applied to the main effect of phase on the false alarm rate due to the sphericity value less than .75; Huynh-Feldt correction was applied for the other analyses as their sphericity estimates were greater than .75 (Girden as cited in Field, 2009).

5.2.4.3.2 Exclusion of data. None of the participants' hit rate was excessively high or low indicating that the task was sufficiently ambiguous to them and therefore data from all the participants were used to analyse the SSDT responses. However, participants who did not report any SPS either in the baseline or follow-up phase were excluded from the analyses of SPS pleasantness and certainty. Thus, there were 39 control and 26 experimental participants for these analyses.

5.2.5 Statistical Analysis

Mixed ANOVA was used to determine the main and interaction effects of phase, condition, and light on the SSDT outcomes; main and interaction effects of phase and condition on the SPS test measures; and main and interaction effects of phase and condition on voice hearing task outcomes.

Light was not a variable of interest in the present study. Therefore, its main and interaction effects are presented in Appendix X as a secondary analysis. Also, changes in state anxiety and sleepiness were examined in the secondary analysis.

To determine correlation coefficients between the SSDT, voice hearing task and SPS test measures in the baseline phase, data from this and the next study (i.e. Studies 4 and 5) are combined and the results are presented in the next chapter. This allows us to examine the relationships in a large sample with greater variability. Similarly, data from Studies 4 and 5 are combined to explore the correlations between baseline false alarm rate and PHQ-15 and between baseline voice false alarms and psychosis proneness; these findings are also presented in the next chapter.

The SSDT outcomes were calculated from the same set of data and therefore testing multiple hypotheses about them would likely increase type I error. As the primary objective of the present study was to investigate false alarm rate, the conventional 5% significance level was used for this variable. As the secondary objective was to examine hit rate, bias, and sensitivity, a Bonferroni correction set the significance level to .02 for them. Similarly, the level of significance was .05 for total SPSs and .02 for intensity, pleasantness, and certainty. Likewise, the level of significance was .05 for voice false alarms and .03 for hits and total voices.

5.3 Results

5.3.1 Hypothesis 1: Rewarding SSDT Hits and Punishing Misses Will Increase the False Alarm and Hit Rates and Make the Response Criterion More Liberal, but Sensitivity Will be Comparable to That of the Control Condition (Replication of the Findings of Study 1).

Descriptive statistics on the SSDT response outcomes across phases and conditions are shown in Table 5.2.

5.3.1.1 False alarm rate. Mixed ANOVA showed that the main effects of phase, F (1.54, 112.17) = 21.81, p < .0001, η_p^2 = .23, and condition, F (1, 73) = 22.88, p < .0001, η_p^2 = .24, and the interaction between phase and condition (Figure 5.3 B), F (2, 146) = 18.21, p < .0001, η_p^2 = .20 were significant. Bonferroni post hoc tests indicate that the baseline false alarm rate did not differ between the control and training groups (mean difference = -.05; 95% CI = -.20, .11; p = .53), but the training group produced significantly more false alarms both in the manipulation

(mean difference = .47; 95% CI = .26, .68; $p < .0001$), and follow-up phases (mean difference = .59; 95% CI = .40, .79; $p < .0001$) than the control group. For the control group, the manipulation false alarm rate was significantly higher than the follow-up false alarm rate (mean difference = .15; 95% CI = .04, .26; $p < .005$), but it did not differ significantly from that in the baseline (mean difference = .09; 95% CI = -.09, .28; $p = .65$). The difference in the false alarm rate between the baseline and follow-up phases in the control condition was not significant (mean difference = -.06; 95% CI = -.11, .22; $p = 1.00$). In the training condition, false alarm rate was significantly increased compared to baseline in both the manipulation (mean difference = .51; 95% CI = .31, .72; $p < .0001$) and follow-up phases (mean difference = .49; 95% CI = .31, .67; $p < .0001$); however, the false alarm rate did not differ significantly between the manipulation and follow-up phases (mean difference = .026; 95% CI = -.093, .145; $p = 1.00$).

5.3.1.2 Hit rate. Mixed ANOVA indicated that the main effects of phase, $F(1.74, 127.32) = 12.89$, $p < .0001$, $\eta_p^2 = .15$, and condition, $F(1,73) = 8.72$, $p < .005$, $\eta_p^2 = .107$, and the interaction between them (Figure 5.3 A), $F(2, 146) = 24.54$, $p < .0001$, $\eta_p^2 = .25$, were significant. Bonferroni post-hoc tests indicate that the difference in the baseline hit rate between the control and training groups was not significant (mean difference = .02; 95% CI = -.04, .08; $p = .54$). However, the training group made significantly more hit responses than the control group in both the manipulation (mean difference = .13; 95% CI = .08, .18; $p < .0001$) and follow-up phases (mean difference = .12; 95% CI = .06, .17; $p < .0001$).

The hit rate of the control group did not differ between phases (mean difference of baseline vs. manipulation phase = .01; 95% CI = -.03, .05; $p = 1.00$; baseline vs. follow-up phase = .04; 95% CI = -.01, .08; $p = .12$; manipulation vs. follow-up phase = .02; 95% CI = -.01, .05; $p = .21$). In the training group, hit rate was significantly increased compared to baseline in both the manipulation (mean difference = .13; 95% CI = .09, .18; $p < .0001$) and follow-up phases (mean difference = .10; 95% CI = .05, .15; $p < .0001$); however, the hit rate did not differ

significantly between the manipulation and follow-up phases at the Bonferroni corrected alpha (mean difference = .03; 95% CI = .00, .07; p = .05).

5.3.1.3 Response Bias. Mixed ANOVA demonstrated that the main effects of phase, $F(1.70,123.94) = 34.10$, $p < .0001$, $\eta_p^2 = .32$, and condition, $F(1,73) = 31.33$, $p < .0001$, $\eta_p^2 = .30$, and the interaction between them (Figure 5.3 C), $F(2, 146) = 33.28$, $p < .0001$, $\eta_p^2 = .31$, were significant. Bonferroni post-hoc tests indicate that the control and experimental groups did not differ in the baseline phase (mean difference = -.01; 95% CI = -.08, .06; p = .78) but that training participants had a significantly lower response bias than control participants (i.e., were more likely to say "yes") in both the manipulation (mean difference = -.27; 95% CI = -.35, -.19; $p < .0001$) and follow-up phases (mean difference = -.27; 95% CI = -.35, -.19; $p < .0001$).

Baseline bias in the control condition did not differ significantly from the manipulation (mean difference = .02; 95% CI = -.05, .10; p = 1.00) and the follow-up phases (mean difference = -.06; 95% CI = -.13, .01; p = .15); however, control participants were significantly more likely to say "yes" in the manipulation than in the follow-up phase (mean difference = -.08; 95% CI = -.13, -.03; $p < .0001$). The training group was significantly more likely to say "yes" in the manipulation (mean difference = .30; 95% CI = .22, .38; $p < .0001$) and follow-up phases (mean difference = .22; 95% CI = .15, .30; $p < .0001$) compared to the baseline phase. The tendency to say "yes" was also significantly greater for the training group in the manipulation compared to the follow-up phase (mean difference = -.08; 95% CI = -.13, -.02; $p < .005$).

5.3.1.4 Sensitivity. Mixed ANOVA showed that the main effect of phase was significant, $F(1.84, 134.38) = 6.26$, $p < .005$, $\eta_p^2 = .08$, but the main effect of condition, $F(1, 73) = .22$, p = .64, $\eta_p^2 = .003$, and the phase by condition interaction (Figure 5.3 D), $F(2, 146) = .39$, p = .66, $\eta_p^2 = .01$, were not. Bonferroni post-hoc tests indicate that sensitivity did not change from the baseline to the manipulation phase (mean difference = .004; 95% CI = -.01, .02; p = 1.00), but dropped significantly in the follow-up phase (mean difference = -.02; 95% CI = -.03, -.004; $p < .01$).

Table 5.2

Mean (Standard Deviation) Hit Rate, False Alarm Rate, Sensitivity, and Bias in the Experimental and Control Conditions Across the Baseline, Manipulation, and Follow-up Phases

SSDT responses	Control condition						Experimental condition					
	Baseline		Manipulation		Follow-up		Baseline		Manipulation		Follow-up	
	LP	LA	LP	LA	LP	LA	LP	LA	LP	LA	LP	LA
FA rate[a]	-1.24 (.35)	-1.23 (.45)	-1.03 (.47)	-1.26 (.49)	-1.25 (.50)	-1.34 (.42)	-1.17 (.32)	-1.21 (.42)	-0.67 (.54)	-0.68 (.46)	-0.69 (.44)	-0.72 (.42)
Hit rate[b]	0.73 (.13)	0.71 (.14)	.73 (.13)	0.68 (.13)	0.71 (.14)	0.65 (.11)	0.71 (.13)	0.69 (.14)	.86 (.10)	0.81 (.12)	0.81 (.11)	0.78 (.12)
Sensitivity[a]	0.89 (.05)	0.87 (.05)	.87 (.05)	0.87 (.06)	0.87 (.05)	0.85 (.05)	0.87 (.06)	0.86 (.05)	.87 (.07)	0.86 (.07)	0.86 (.06)	0.85 (.06)
Bias[c]	1.63 (.16)	1.66 (.18)	1.58 (.16)	1.67 (.19)	1.66 (.20)	1.75 (.18)	1.64 (.13)	1.68 (.16)	1.32 (.18)	1.38 (.17)	1.40 (.20)	1.46 (.17)

Note. FA = false alarm; LP = light present trials; LA = light absent trials.

[a] Log transformed data. [b] Reciprocal transformed data. [c] Square root transformed data.

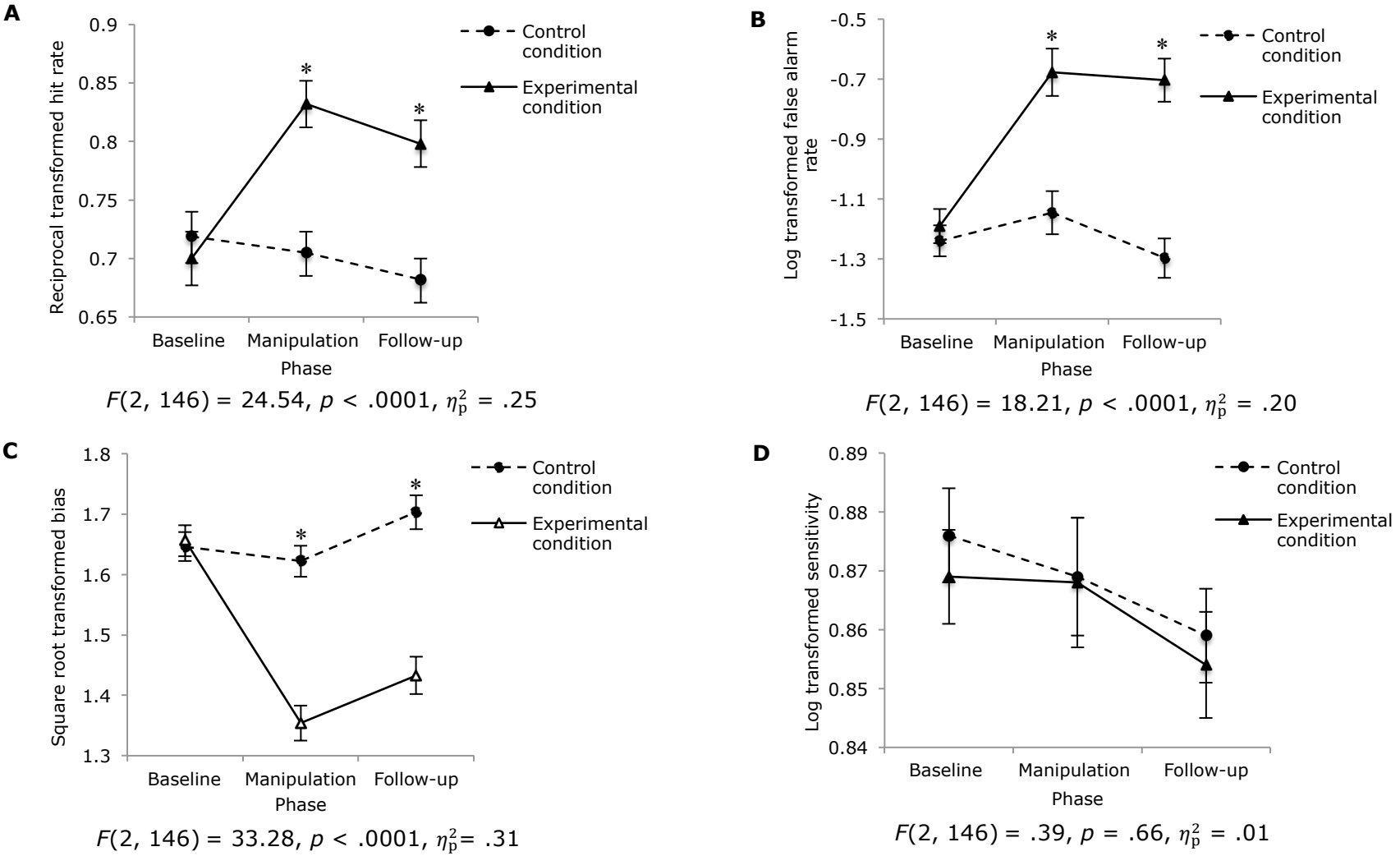


Figure 5.3. Condition by phase interactions for (A) hit rate; (B) false alarm rate; (C) bias; and (D) sensitivity. Error bars are standard errors.

Sensitivity in the manipulation phase was significantly higher than in the follow-up phase (mean difference = .01; 95% CI = .002, .02; $p < .02$).

5.3.2 Hypothesis 2: SSDT Conditioning Will Increase SPS Responses at Follow-up.

Descriptive statistics for the SPS measures are shown in Table 5.3.

5.3.2.1 Total SPSs. The main effects of phase, $F(1, 73) = .39$, $p = .54$, $\eta_p^2 = .01$, condition, $F(1, 73) = 1.56$, $p = .22$, $\eta_p^2 = .02$, and the interaction between phase and condition (see Figure 5.4), $F(1, 73) = .25$, $p = .62$, $\eta_p^2 = .003$, were not significant.

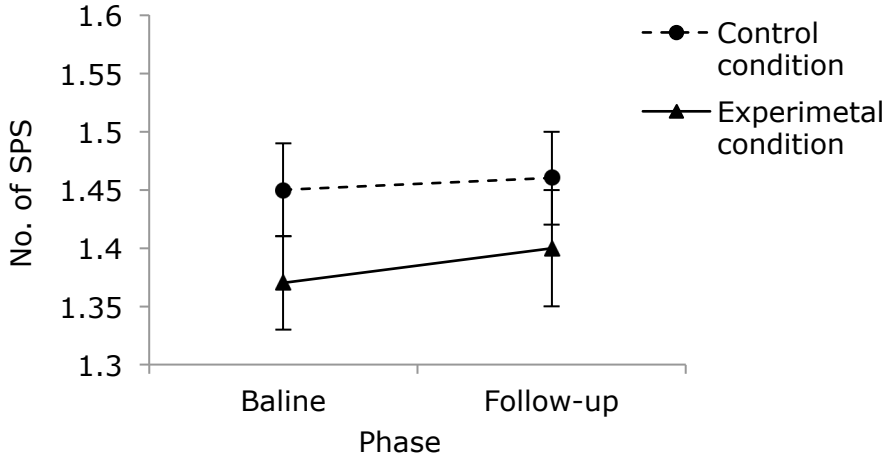
5.3.2.2 Intensity of SPS. The main effects of phase, $F(1, 73) = 4.74$, $p = .03$, $\eta_p^2 = .06$, condition, $F(1, 73) = .88$, $p = .35$, $\eta_p^2 = .01$, and interaction between phase and condition, $F(1, 73) = .77$, $p = .38$, $\eta_p^2 = .01$, were not significant.

Table 5.3

Means and Standard Deviations of the Baseline and Follow-up Responses in the SPS Task for the Control and Training Groups in the False Alarm Increasing Study

	Control group	Training group
Variables and phases	*M* (*SD*)[b]	*M* (*SD*)[b]
Total SPSs[a]		
Baseline	1.45 (.25)	1.37 (.26)
Follow-up	1.46 (.28)	1.40 (.24)
SPS intensity[a]		
Baseline	2.02 (.50)	1.87 (.59)
Follow-up	2.09 (.55)	2.02 (.59)
SPS pleasantness		
Baseline	-.39 (.90)	-.16 (.58)
Follow-up	-.40 (.77)	-.33 (.83)
SPS certainty		
Baseline	3.30 (1.47)	3.13 (1.35)
Follow-up	3.23 (1.41)	3.30 (1.28)

Note. [a] Square root transformed data.



$F(1, 73) = .25$, $p = .62$, $\eta_p^2 = .003$

Figure 5.4. Phase by condition interaction for total SPSs

5.3.2.3 Pleasantness of SPS. There was no significant main effect of phase, $F(1, 63) = .82$, $p = .37$, $\eta_p^2 = .01$, condition, $F(1, 63) = .75$, $p = .39$, $\eta_p^2 = .01$, and interaction between phase and condition, $F(1, 63) = .58$, $p = .45$, $\eta_p^2 = .01$.

5.3.2.4 Certainty of SPS. There was no significant main effects of phase, $F(1, 63) = .07$, $p = .80$, $\eta_p^2 = .001$, condition, $F(1, 63) = .02$, $p = .88$, $\eta_p^2 < .001$, and interaction between phase and condition, $F(1, 63) = .43$, $p = .52$, $\eta_p^2 = .01$.

5.3.3 Hypothesis 3: SSDT Conditioning Will Increase Voice Hearing on the VHT at Follow-up

Descriptive statistics for the VHT responses are shown in Table 5.4.

5.3.3.1 Voice false alarms. The main effect of phase was significant, $F(1, 73) = 57.49$, $p < .001$, $\eta_p^2 = .44$, but the main effect of condition, $F(1, 73) = 3.25$, $p = .08$, $\eta_p^2 = .04$, and interaction between phase and condition, $F(1, 73) = 2.51$, $p = .12$, $\eta_p^2 = .03$, were not significant. The interaction is shown in Figure 5.5. Regardless of condition, participants produced significantly more false alarms in the follow-up than in the baseline phase (mean difference = .92; 95% CI = .68, 1.16; $p < .0001$).

5.3.3.2 Total voices. The main effect of phase was significant, $F(1, 73) = 25.4$, $p < .0001$, $\eta_p^2 = .26$, but the main effect of condition was not significant, $F(1, 73) = 1.58$, $p = .21$, $\eta_p^2 = .02$. Regardless of condition, participants reported significantly

more voices in the follow-up than in the baseline phase (mean difference = .48; 95% CI = .29, .67; $p < .0001$).

The interaction between phase and condition (see Figure 5.6) was close to significant at the Bonferroni corrected alpha value, $F(1, 73) = 4.50$, $p = .037$, $\eta_p^2 = .06$. Bonferroni post hoc test was carried out to explore the interaction further. Participants reported significantly more voices in the follow-up than in the baseline phase both in the control (mean difference = .28; 95% CI = .02, .54; $p < .05$) and experimental conditions (mean difference = .69; 95% CI = .40, .97; $p < .001$). The difference between the experimental and control conditions was not significant in the baseline (mean difference = .05; 95% CI = -.30, .41; $p = .77$) nor in the follow-up phases (mean difference = .46; 95% CI = -.07, .99; $p = .09$).

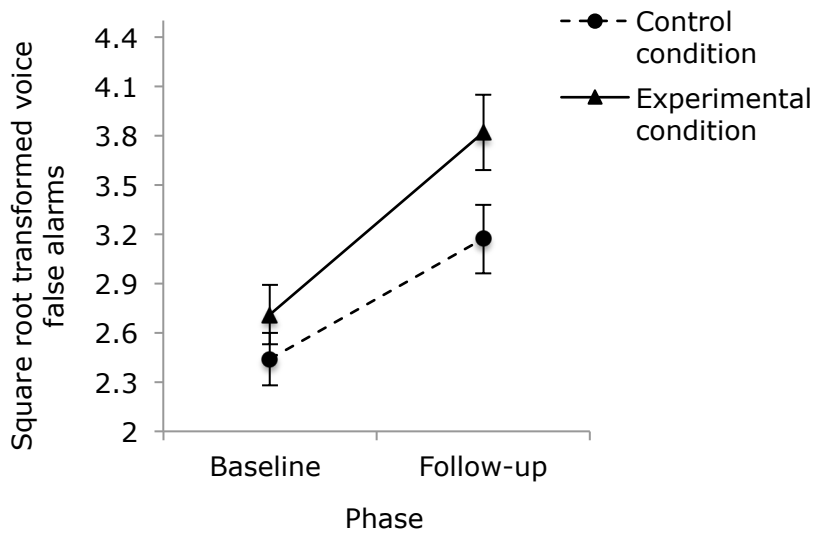
Table 5.4

Means and standard deviations of the baseline and follow-up responses in the voice detection task for the control and training groups in the false alarm increasing study

Variables and phases	Control group *M* (*SD*)	Training group *M* (*SD*)
False alarms [a]		
Baseline	2.44 (.99)	2.71 (1.12)
Follow-up	3.17 (1.24)	3.82 (1.50)
Hits		
Baseline	13.93 (5.82)	12.68 (6.07)
Follow-up	13.15 (6.77)	11.74 (6.81)
Total voices [a]		
Baseline	4.50 (.79)	4.55 (.74)
Follow-up	4.78 (1.19)	5.24 (1.07)

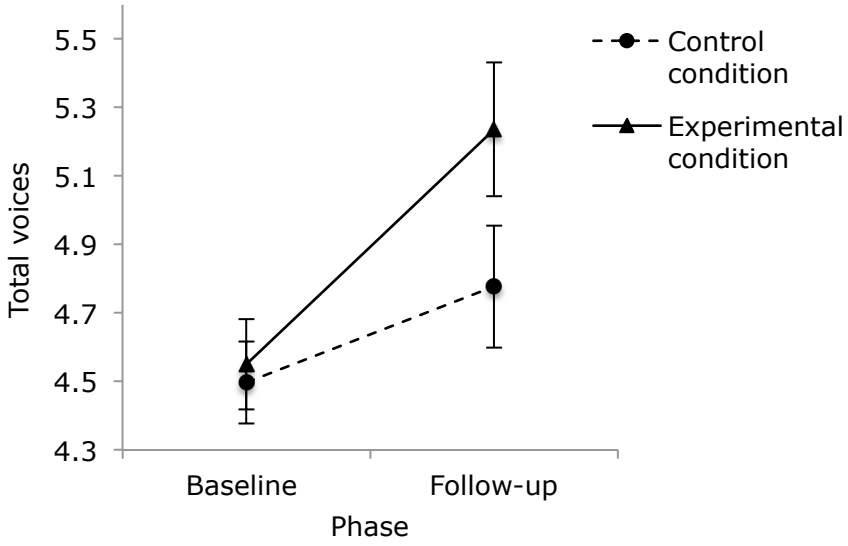
Note. [a] Square root transformed data.

5.3.3.3 Total hits. There was no significant main effects of phase, $F(1, 73) = 2.76$, $p = .10$, $\eta_p^2 = .04$, condition, $F(1, 73) = .92$, $p = .34$, $\eta_p^2 = .01$, and no significant interaction between phase and condition, $F(1, 73) = .02$, $p = .88$, $\eta_p^2 < .001$.



$F(1, 73) = 2.51$, $p = .12$, $\eta_p^2 = .03$

Figure 5.5. Phase by condition interaction for voice false alarms.



$F(1, 73) = 4.50$, $p = .037$, $\eta_p^2 = .06$

Figure 5.6. Phase by condition interaction for total voices.

5.4 Discussion

As predicted, the SSDT training significantly increased the false alarm and hit rates, changed bias (i.e., participants became more likely to say "yes"), but did not change sensitivity between the groups. The SSDT findings of Study 1 were therefore replicated using a modified and more improved study design (where the control condition was better matched to the experimental condition), demonstrating that these effects are reliable. Contrary to our prediction, however, there was no

significant effect of SSDT conditioning on SPS, suggesting that the SPS test may not be appropriate to examine transfer of the SSDT training. An alternative possibility is that there was no transfer of training, which, however, does not fit with the findings of the VHT. Though the training did not affect false alarms in the VHT, a near significant cross-modal effect was found for total voices, that is, experimental participants reported hearing more voices than control participants after the training, having been similar at baseline.

The present study can be viewed as an affirmation of the idea that perceptual distortions can be conditioned using reward and punishment. Like our previous study that used conditioning to increase the false alarm rate (Study 1), the findings confirm that the effects of conditioning are not short-lived, but continue beyond the training period into the follow-up phase when responses were not followed by any consequences. The training effects may be even more long-lasting than thought previously. In study 1, baseline FA rate of the experimental condition was significantly higher than that of the control condition. Similarly, participants were significantly more liberal (in response criterion) in the baseline of the control condition than that of the experimental condition. However, the present study did not find any such differences—control and experimental groups produced nearly the same SSDT response outcomes in the baseline phase. As Study 1 was carried out using a within subjects design, it seems that half of the participants, who had the experimental condition as their first session, still had the influence of the training when they performed the control condition after a week (i.e. the learning effects were carried over to the second session). This increased the overall false alarm rate in the control condition, resulting in a significant difference between the conditions at baseline. The present study, on the other hand, used a between groups design and random allocation of participants to the conditions, which resulted in equivalent group performance at baseline. The findings of the present study thus attest to our decision to use a between groups design to replicate the findings of Study 1 and to control possible carry-over effects.

The findings of the present study also indicate that the same conditioning effects can be obtained with a reduced value of reward and punishment if participants perceive them to be useful (amount of reward and punishment in the present study was half of that in the previous studies). That pseudo-conditioning did not affect control participants suggests the importance of using appropriate schedule of reinforcement and targeting appropriate responses to condition.

SSDT conditioning did not affect SPS responses but participants reported more voices after the training. This, coupled with the SPS findings of Study 1, supports our assumption that the SPS test is not suitable for detecting any potential transfer effects of conditioning on the SSDT. Although the phase by condition interaction was not significant for voice false alarms and total voices, the pattern and effect size for changes in the voice hearing responses (see Figure 5.5 and 5.6) suggest that the difference between control and experimental groups in the follow-up phase might become significant with additional SSDT training. Indeed, our training was brief in comparison to other studies on cross-modal transfer. Nagarajan et al., (1998), for example, trained participants 1 hour each day for 10-16 days on a somatosensory interval discrimination task, with the training generalizing to a similar auditory task. Bratzke et al., (2014) used 5 blocks of 160 trials (i.e. total 800 trials) to train participants on an auditory discrimination task which then transferred to the visual modality. In the present study, there were a maximum of 80 trials that resulted in reward and punishment depending on the responses of participants. It is remarkable how so few training trials resulted in such strong effects on SSDT performance and also seemed to influence responses to the VHT.

The cross-modal transfer of SSDT training could also be detected by testing additional participants. A post hoc power analysis of total voice data (using G*Power 3.1, Faul et al., 2007) indicates that a sample size of 514 (257 participants in each group) is required to detect the transfer effect (d = .25, which was determined from the value of partial eta squared of phase by condition interaction in the preset study) in a two-tailed test with alpha = .05 and power = .8. Similarly, a significant group difference in voice false alarms in the follow-up phase between the conditions could

be obtained in a two-tailed test by testing 920 participants (460 participants in each group) with an effect size of d = .19 (as determined from the partial eta squared value of the present study), alpha = .05 and power = .8.

CHAPTER 6

Study 5: Does Training Decrease Illusory Somatosensory Experiences and Generalise to Separate Perceptual Tasks?

> My pain is qualitatively different from what it was when I was younger. I suffer less because my perception of the pain I feel and the meaning I attach to it have changed. . . . The change in my own pain is psychobiological. My thoughts have been crucial to reducing my pain.
>
> —Siri Hustvedt, The Shaking Woman or A History of My Nerves, pp. 235-237

Study 2 demonstrated that the false alarm rate of high false alarm individuals can be decreased using operant conditioning. However, SSDT conditioning did not generalize to the spontaneous sensation (SPS) test: irrespective of the experimental condition, participants reported more spontaneous sensations (SPSs) in the follow-up than in the baseline. Session (1st or 2nd) determined the number of SPSs reported in different conditions (which was also the case for Study 1, which sought to increase the false alarm rate in low false alarm individuals). When the experimental condition was the first session, participants produced significantly more SPSs than those in the control condition; no such difference was found when the control condition was the first session. Suggestions have been made to replicate the study adopting a between subjects design to control possible carry-over effects. When the findings of both Study 1 and 2 were compared, it was unclear whether the SPS test was an appropriate choice to study perceptual transfer of the SSDT conditioning. Therefore, the voice-hearing task (VHT; see Chapter 4) was developed and the SPS test was modified to examine the proposed transfer effects. The VHT was used in Study 4 (Chapter 5) and showed some indication of cross-modal transfer, while the modified SPS test did not show any sign of perceptual transfer. The findings of the cross-modal transfer would be strengthened if it could be shown that conditioning to decrease SSDT false alarm rate caused a corresponding drop in voices on the VHT. We therefore set the objectives for the present study as follows:

1. To replicate the primary findings of Study 2, such that reward and punishment decreases the SSDT false alarm rate and that this persists over time (i.e. continue into the follow-up phase).

2. To replicate the secondary findings of Study 2 such that SSDT conditioning produces an enduring decrease in hit rate and increase in response bias (i.e. participants become less likely to say "yes"), whilst sensitivity remains relatively unchanged.
3. To examine whether SSDT conditioning transfers to the SPS test, as evidenced by a decrease in SPS in the training group.
4. To investigate whether SSDT conditioning transfers to the hearing of voices on the VHT task (i.e., decreases voice false alarms).

There were two additional objectives in the present study that were also objectives for Study 4 but are examined here as they involve combining the samples from both Studies 4 and 5:

5. To investigate whether SSDT response outcomes correlate with SPS and voice hearing task responses at baseline.

6. To examine whether SSDT and voice false alarms relate to severity of somatic symptoms (as measured by the patient health questionnaire-15) and psychosis proneness (as measure by the mini-psychosis proneness scale) respectively.

6.2 Method

The study design, materials used, and procedure were the same as those of Study 4 (see Section 5.2) except for the baseline false alarm rate of participants and the conditioning procedure in the experimental condition. Secondary analyses involving SSDT light condition, state anxiety, and sleepiness are presented in Appendix AA.

6.2.1 Participants

A total of 76 participants comprising students (*n* = 67, 88.16%) and staff from the University of Manchester participated in the present study (female = 39, 51.31%). They aged between 19 and 37 years (*M* = 22.89, *SD* = 3.66). Nine of the participants were left-handed, as assessed using the Edinburgh Handedness Inventory (Oldfield, 1971). Sample size calculation was based on Study 2 and the procedure was same as that of Study 4 (see Section 5.2.2). With an effect size (d) of .80 and power (1- β) of .80, it was found that 26 participants in each group (52 in total)

would be sufficient to detect the effects of the training on false alarm rate. It was decided to test 35 participants in each group as the data of some participants might not be useable. Since the baseline false alarm rates of participants were unknown and this study was carried out alongside Study 4, we recruited 151 participants to ensure large enough samples for both studies; this resulted in 76 individuals with a high baseline false alarm rate for the purposes of this study.

Inclusion and exclusion criteria and the participant recruitment procedure were the same as those for Study 4 (see Section 5.2.2).

6.2.2 Experimental condition

The structure and procedure of the experimental condition was exactly the same as that of Study 4 except that half of the randomly selected correct rejections were reinforced and half of the randomly selected false alarms were punished (see Figure 5.2, page 134); in other words, participants were trained to say "yes" less rather than more.

6.2.3 Data preparation

6.2.3.1 Exploring outliers and assumptions. Data were examined to identify outliers and to check whether the assumptions of mixed ANOVA were met as described in Chapter 2 (see Section 2.2.5.1). None of the SSDT response outcomes were normal. Total SPSs, SPS certainty, voice false alarms, and total voices were also non-normal. Different techniques (e.g., transforming the data, changing scores, etc.) were tried to determine the best method for correcting deviations from normality. Thus, SSDT false alarm rate and total SPSs data were log transformed, SSDT sensitivity, SSDT bias, SPS certainty, voice false alarms, and total voices were square root transformed, and outliers of SSDT hit rate were changed to one unit above the next highest score in the data set (as suggested in Field, 2009). However, the baseline SSDT false alarm rate in experimental condition light present trials, follow-up SSDT false alarm rate in experimental condition light absent trials, and follow-up bias in experimental condition light present trials remained non-normal even after carrying out the correction procedures, $D(39) = .21$, $p < .001$; $D(39) = .25$, $p < .05$; and $D(39) = .18$, $p < .01$ respectively. As the sample size was big ($N > 30$), ANOVA is

robust to some deviations from normality of distributions (Field, 2009), and the rest of the dependent variable distributions of relevant analyses (e.g. baseline SSDT false alarm rate in light absent condition, manipulation SSDT false alarm rate in light present condition, etc.) were normal, we proceeded with the parametric test.

Mauchly's sphericity test indicated that the assumption of sphericity was violated by the main effect of phase on the SSDT false alarm rate ($\varepsilon = .88$, $p < .01$), hit rate ($\varepsilon = .82$, $p = .001$), sensitivity ($\varepsilon = .84$, $p < .01$), and bias ($\varepsilon = .77$, $p < .001$) and phase by light interaction on the SSDT false alarm rate ($\varepsilon = .92$, $p = .05$), hit rate ($\varepsilon = .83$, $p = .001$), sensitivity ($\varepsilon = .88$, $p < .01$), and bias ($\varepsilon = .68$, $p < .001$). Greenhouse-Geisser correction was applied to the phase by light interaction on SSDT bias as the estimate of its sphericity was less than .75 and Huynh-Feldt correction was applied for the remaining analyses as their sphericity estimates were greater than .75 (Girden as cited in Field, 2009).

Levene's test indicated that variances in the data at baseline met the assumption of homogeneity of variance but the variances were unequal between the groups for the SSDT manipulation phase hit rate in light present condition, $F(1, 74) = 15.21$, $p < .001$, manipulation phase sensitivity in both light present and light absent conditions, $F(1, 74) = 4.74$, $p < .03$, and $F(1, 74) = 7.60$, $p < .01$ respectively, and follow-up phase sensitivity in light present condition, $F(1, 74) = 5.27$, $p < .05$. We expected to see some violations of the assumption in the manipulation and follow-up phases due to probable varying effects of the SSDT conditioning. As the assumption was satisfied with the data in most of the levels of the independent variables (e.g., baseline and follow-up hit rates in both light present and light absent conditions and manipulation phase hit rate in light absent condition satisfied the assumption) and the group sizes were quite similar ($N = 37$ and 39 in the control and experimental conditions respectively), we decided to use a mixed design ANOVA to analyze the data.

Two of the SPS response outcomes, namely SPS intensity and pleasantness did not satisfy the parametric assumptions and therefore non-parametric tests were used to analyze them.

Kolmogorov-Smirnov and Shapiro-Wilk tests indicate that the SSDT false alarm rate in light present and light absent conditions, voice false alarms, and total SPSs, as obtained by combining the data of both Studies 4 and 5, were non-normal in the baseline. None of the available techniques corrected problems in the data. We, therefore, decided to use a nonparametric correlation (i.e. Spearman correlation) to determine the strength and direction of relationships between them (objective 5 of the present study). Likewise, Spearman correlation has been used to investigate the relationships between baseline SSDT false alarms and PHQ-15 scores and between baseline voice false alarms and proneness to psychotic symptoms (objective 6 of the present study).

6.2.3.2 Exclusion of data. Data from all the participants were used to produce the SSDT and VHT results. However, 10 control and 10 experimental participants were excluded from the analyses of SPS pleasantness and certainty as they did not report any SPS either in the baseline or follow-up phase.

6.2.4 Statistical analysis

Statistical techniques used and statistical decisions taken were the same as those for Study 4 (see Section 5.2.5).

6.3 Results

6.3.1 Hypothesis 1: Rewarding SSDT Correct Rejections and Punishing False Alarms Will Decrease False Alarm and Hit Rate and Make Response Criterion Stricter, but Sensitivity Will Remain Comparable to That of the Control Condition (Replication of the Findings of Study 2)

6.3.1.1 False alarm rate. Descriptive statistics on the false alarm rates in different phases and conditions are presented in Table 6.1. Mixed ANOVA showed that the main effects of phase, $F(1.78,131.56) = 75.94$, $p < .0001$, $\eta_p^2 = .51$, and condition, $F(1,74) = 41.76$, $p < .0001$, $\eta_p^2 = .36$, and the phase x condition interaction, $F(2, 148) = 32.62$, $p < .001$, $\eta_p^2 = .31$, were significant. The interaction is shown in Figure 6.1.

Bonferroni corrected post-hoc tests indicate that the baseline false alarm rate did not differ significantly between the control and training conditions (mean difference =.04; 95% CI = -.06, .13; $p = .45$). However, training participants

produced significantly fewer false alarms than control participants both in the manipulation (mean difference =-.51; 95% CI = -.65, -.37; $p < .001$) and the follow-up phases (mean difference =.55; 95% CI = -.72, -.37; $p < .001$). In the control condition, baseline false alarm rate did not differ from the manipulation phase (mean difference =.09; 95% CI = -.01, .20; $p = .09$) but was significantly higher than that in the follow-up phase (mean difference =.15; 95% CI =.01, .30; $p < .05$). In the training condition, the baseline false alarm rate was significantly higher than both the manipulation (mean difference =.57; 95% CI = .47, .67; $p < .001$) and follow-up phases (mean difference =.66; 95% CI = .53, .80; $p < .001$). Training participants, exhibited similar false alarm rates in the manipulation and follow-up phases (mean difference =.09; 95% CI = -.03, .21; $p = .18$).

6.3.1.2 Hit rate. Descriptive statistics on hit rates across the phases and conditions are presented in Table 6.1. The main effect of phase was significant, $F(1.70, 125.44) = 37.58$, $p < .001$, $\eta_p^2 = .33$, as was the interaction between phase and condition, $F(2, 148) = 7.67$, $p < .01$, $\eta_p^2 = .09$. The interaction is shown in Figure 6.1. The main effect of condition was not significant at the Bonferroni corrected alpha of .02, $F(1, 74) = 4.84$, $p = .03$, $\eta_p^2 = .06$.

Bonferroni corrected post-hoc tests indicate that hit rate in the baseline did not differ between the control and experimental groups (mean difference =.02; 95% CI = -.07, .11; $p = .63$). However, the experimental group produced significantly fewer hits than the control group both in the manipulation (mean difference =-.16; 95% CI = -.27, -.04; $p < .01$) and follow-up phases (mean difference =-.15; 95% CI = -.27, -.03; $p < .05$).

The hit rates of control participants did not differ between the baseline and manipulation phases (mean difference =.05; 95% CI = -.02, .12; $p = .31$). Hit rate in the follow-up phase was significantly lower than that in the baseline (mean difference =-.10; 95% CI = -.18, -.02; $p < .01$) and manipulation phases (mean difference = -.06; 95% CI = -.11, -.001; $p = .05$). Hit rates of experimental participants were significantly higher in the baseline than in the manipulation (mean difference =.18; 95% CI =.12, .25; $p < .001$) and follow-up phases (mean difference =.23; 95% CI =

.15, .31; $p < .001$). However, their hit rates did not differ between the manipulation and follow-up phases (mean difference =.05; 95% CI = -.01, .10; $p = .12$).

6.3.1.3 Sensitivity. Descriptive statistics on sensitivity across the phases and conditions are presented in Table 6.1. The main effects of phase, $F(1.72, 127.60) = 2.60$, $p = .09$, $\eta_p^2 = .03$, and condition, $F(1, 74) = .26$, $p = .61$, $\eta_p^2 = .003$, were not significant and nor was the interaction between them, $F(2, 148) = 2.00$, $p = .15$, $\eta_p^2 = .03$. Figure 6.1 shows the interaction.

6.3.1.4 Bias. Descriptive statistics on bias across the phases and conditions are shown in Table 6.1. The main effects of phase, $F(1.62, 120.15) = 68.03$, $p < .001$, $\eta_p^2 = .48$, and condition, $F(1,74) = 21.68$, $p < .001$, $\eta_p^2 = .23$, and the interaction between them, $F(2, 148) = 22.23$, $p < .001$, $\eta_p^2 = .23$, were significant. The interaction is shown in Figure 6.1.

Bonferroni post-hoc tests revealed that bias in the baseline did not differ between the control and training conditions (mean difference = -.03; 95% CI = -.08, .03; $p = .33$) but training participants said "no" (i.e. reported the absence of the vibration) significantly more than the participants in the control group for both the manipulation (mean difference = .19; 95% CI = .12, .26; $p < .001$) and follow-up phases (mean difference = .21; 95% CI = .13, .29; $p < .001$).

In the control condition, bias in the manipulation phase did not differ from that in the baseline (mean difference =.05; 95% CI = -.01, .10; $p = .10$) and follow-up phases (mean difference =-.03; 95% CI = -.07, .01; $p = .21$) but participants were significantly more stringent (i.e., likely to say "no") in the follow-up than in the baseline phase (mean difference = .08; 95% CI = .01, .14; $p < .05$). In the training condition, participants gave the "no" response significantly more in the manipulation and follow-up phases than in the baseline [the mean differences are .21 (95% CI = .16, .27; $p < .001$) and .26 (95% CI = .20, .32; $p < .001$) respectively]. The difference between bias in the follow-up and manipulation phases was also significant, (mean difference = .05; 95% CI = .01, .09; $p = .01$). In other words, training

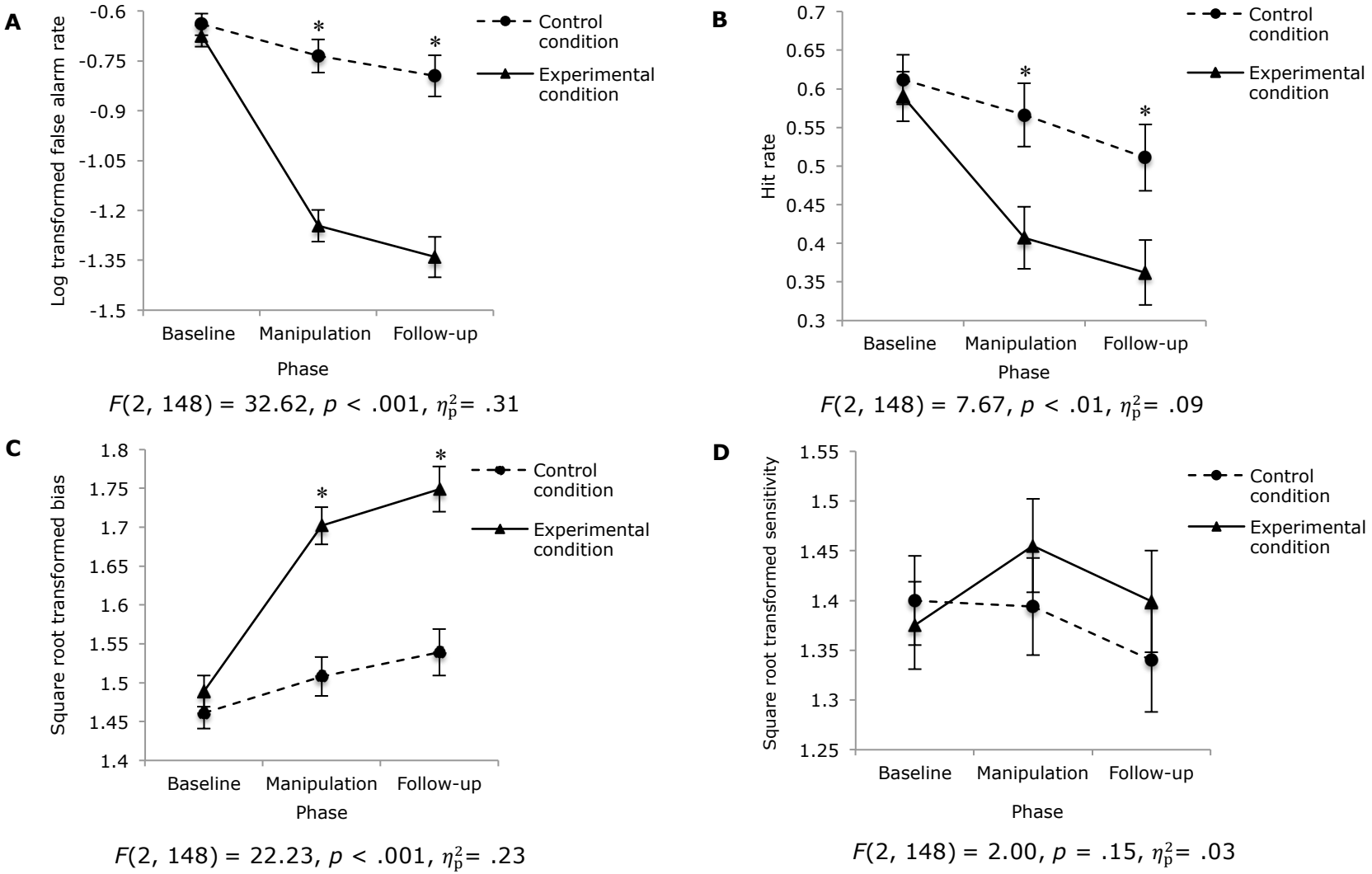
Table 6.1

Mean (Standard Deviation) Hit Rate, False Alarm Rate, Sensitivity, and Bias in the Experimental and Control Conditions Across the Baseline, Manipulation, and Follow-up Phases of Study 5

SSDT responses	Control condition						Experimental condition					
	Baseline		Manipulation		Follow-up		Baseline		Manipulation		Follow-up	
	LP	LA	LP	LA	LP	LA	LP	LA	LP	LA	LP	LA
FA rate[a]	-.54 (.18)	-.74 (.27)	-.60 (.29)	-.87 (.37)	-.63 (.37)	-.96 (.43)	-.57 (.22)	-.78 (.30)	-1.12 (.34)	-1.37 (.38)	-1.23 (.46)	-1.45 (.42)
Hit rate	.68 (.21)	.54 (.24)	.65 (.19)	.48 (.24)	.60 (.24)	.42 (.28)	.65 (.19)	.53 (.21)	.45 (.30)	.36 (.29)	.39 (.28)	.33 (.28)
Sensitivity[b]	1.41 (.31)	1.39 (.24)	1.40 (.27)	1.38 (.23)	1.35 (.29)	1.33 (.31)	1.37 (.28)	1.38 (.29)	1.46 (.35)	1.45 (.33)	1.39 (.37)	1.41 (.35)
Bias[b]	1.39 (.15)	1.53 (.16)	1.43 (.15)	1.59 (.18)	1.45 (.22)	1.63 (.23)	1.43 (.15)	1.55 (.14)	1.66 (.16)	1.75 (.16)	1.72 (.18)	1.78 (.17)

Note. FA = false alarm; LP = light present trials; LA = light absent trials.

[a] Log transformed data. [b] Square root transformed data.



$F(2, 148) = 32.62$, $p < .001$, $\eta_p^2 = .31$

$F(2, 148) = 7.67$, $p < .01$, $\eta_p^2 = .09$

$F(2, 148) = 22.23$, $p < .001$, $\eta_p^2 = .23$

$F(2, 148) = 2.00$, $p = .15$, $\eta_p^2 = .03$

Figure 6.1. Phase by condition interactions for (A) false alarm rate, (B) hit rate, (C) bias and (D) sensitivity. Error bars are standard errors.

Table 6.2

Means and Standard Deviations of the Baseline and Follow-up SPS Responses in the Control and Experimental Conditions of the False Alarm Decreasing Study

	Control condition	Experimental condition
Variables and phases	*M* (*SD*)[b]	*M* (*SD*)[b]
Total number of SPSs[a]		
Baseline	.33 (.17)	.29 (.16)
Follow-up	.31 (.19)	.33 (.15)
SPS intensity		
Baseline	3.17 (2.08 to 4.33)	3.00 (1.33 to 4.17)
Follow-up	3.83 (1.50 to 6.11)	4.33 (2.67 to 5.28)
Certainty of SPS[c]		
Baseline	3.37 (1.18)	3.00 (1.18)
Follow-up	3.26 (1.33)	3.36 (1.16)
Pleasantness of SPS		
Baseline	-.17 (-.67 to .33)	-.11 (-.33 to .00)
Follow-up	-.11 (-.78 to .00)	-.22 (-.67 to .00)

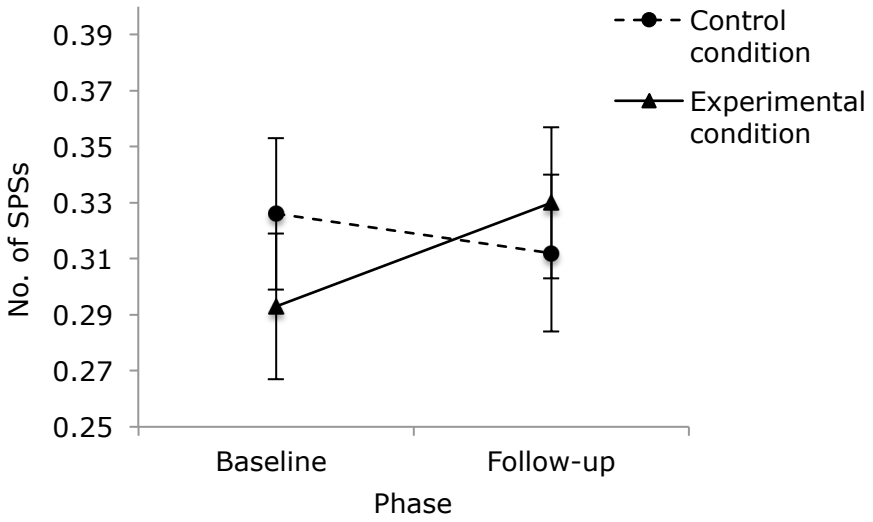
[a] Log transformed score. [b] For SPS intensity and pleasantness, these are median and interquartile range values. [c] Square root transformed score.

participants selected the "no" response significantly more in the follow-up than in the manipulation phase.

6.3.2 Hypothesis 2: SSDT Conditioning Will Decrease SPS Responses at Follow-up

Descriptive statistics for SPS responses are presented in Table 6.2.

6.3.2.1 Total SPSs. The main effects of phase, $F(1, 74) = .46$, $p = .50$, $\eta_p^2 = .01$, and condition, $F(1, 74) = .04$, $p = .83$, $\eta_p^2 = .001$, were not significant, and neither was the interaction between phase and condition, $F(1, 74) = 2.24$, $p = .14$, $\eta_p^2 = .03$. The interaction is shown in Figure 6.2.



$F(1, 74) = 2.24$, $p = .14$, $\eta_p^2 = .03$

Figure 6.2. Phase by condition interaction for total SPSs. Error bars are standard errors.

6.3.2.2 SPS intensity. Mann-Whitney test indicates that baseline SPS intensity did not change between the control (*Mdn* = 3.17) and experimental (*Mdn* = 3.0) conditions, *U* = 625.50, *z* = -1.00, *p* = .32, *r* = -.11. Similarly, the groups (the median values are 3.8 and 4.3 respectively for the control and experimental groups) did not differ in the follow-up phase, *U* = 658.50, *z* = -.66, *p* = .51, *r* = -.08.

Combining both the groups it was found that the intensity of SPS was significantly higher in the follow-up (*Mdn* = 4.17) than in the baseline phase (*Mdn* = 3.14), *Z* = -2.79, *p* < .01, *r* = -.23.

For the experimental condition, SPS intensity was significantly higher in the follow-up (*Mdn* = 4.33) than in the baseline (*Mdn* = 3.00) phase, *Z* = -2.93, *p* < .01, *r* = -.33 (see Figure 6.4). In the control condition, however, baseline (*Mdn* = 3.17) and follow-up (*Mdn* = 3.83) SPS did not differ in intensity, *Z* = -.90, *p* = .37, *r* = -.10 (see Figure 6.3).

6.3.2.3 Pleasantness of SPS. Baseline pleasantness in the control condition (*Mdn* = -.17) was approximately the same as that in the experimental condition (*Mdn* = -.11), *U* = 51, *z* = -.38, *p* = .70, *r* = -.05. Similarly, the follow-up pleasantness did not differ between the conditions (*Mdn* = -.11 and -.22 for the control and experimental groups respectively), *U* = 50, *z* = -.51, *p* = .61, *r* = -.06.

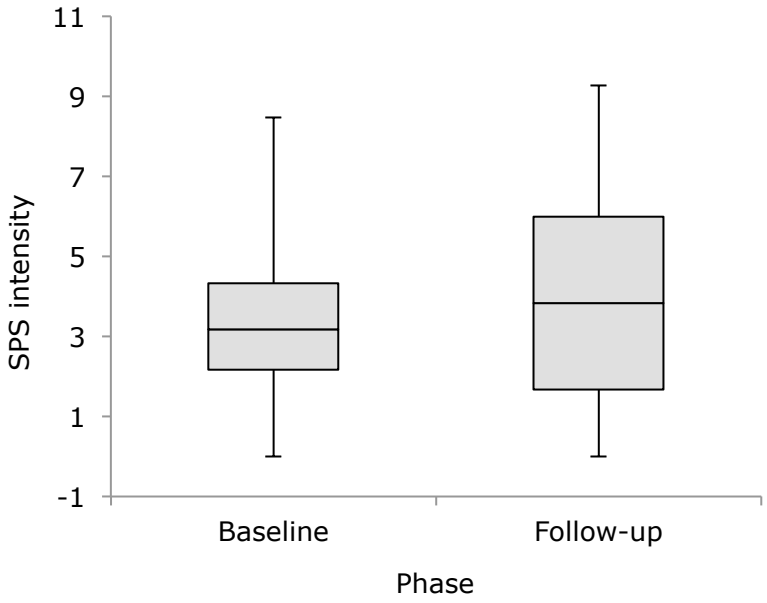


Figure 6.3

SPS intensity in the baseline and follow-up phases of the control condition

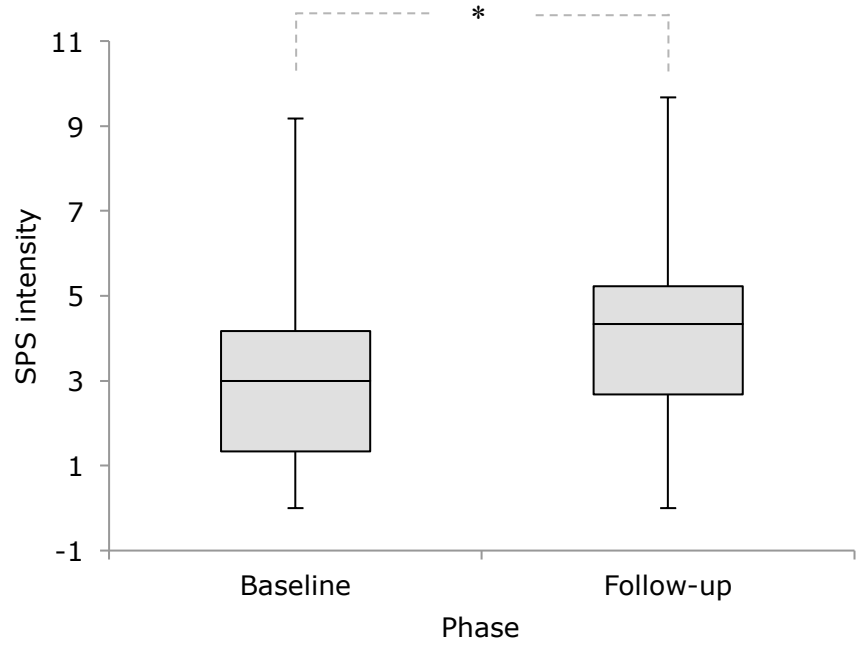


Figure 6.4

SPS intensity in the baseline and follow-up phases of the experimental condition

The difference between the baseline (*Mdn* = -.138) and follow-up (*Mdn* = -.195) SPS pleasantness was not significant, Z = -1.919, p = .06, r = -.17. Further analyses indicate that the baseline (*Mdn* = -.17) and follow- up (*Mdn* = -.11) SPS pleasantness

did not differ in the control condition, Z = -.758, p = .448, r = -.10. Similarly, the difference (*Mdn* = -.11 and -.223 for the baseline and follow-up phases respectively) was not significant for the training condition, Z = -1.906, p = .06, r = -.23.

6.3.2.4 SPS certainty. The main effect of phase, $F(1, 64) = .61$, $p = .44$, $\eta_p^2 = .01$, condition, $F(1, 64) = .22$, $p = .64$, $\eta_p^2 = .003$, and the interaction effect between phase and condition, $F(1, 64) = 2.747$, $p = .10$, $\eta_p^2 = .04$, were not significant.

6.3.3 Hypothesis 3: SSDT Conditioning Will Decrease Voice Hearing at Follow-up

Descriptive statistics for voice-hearing task responses are presented in Table 6.3.

6.3.3.1 Voice false alarms. The main effect of phase was significant, $F(1, 74) = 34.28$, $p < .001$, $\eta_p^2 = .32$. Contrary to prediction, Bonferroni corrected post-hoc tests revealed that participants produced significantly more false alarms in the follow-up than in the baseline phase (mean difference = .82; 95% CI = .54, 1.09; $p < .001$). The main effect of condition was not significant, $F(1, 74) = .24$, $p = .63$, $\eta_p^2 = .003$, nor was the interaction between phase and condition, $F(1, 74) = 2.16$, $p = .15$, $\eta_p^2 = .03$. The interaction is depicted in Figure 6.5.

Table 6.3

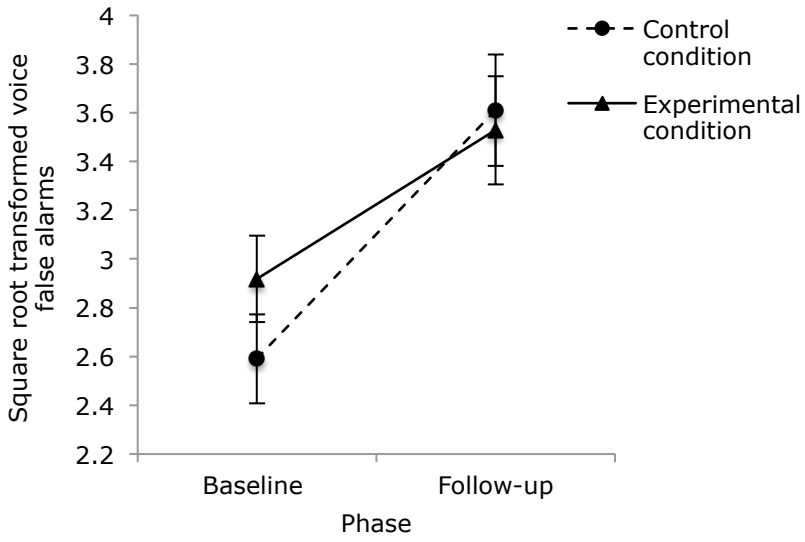
Means and Standard Deviations of the Baseline and Follow-up Responses in the Voice Detection Task for the Control and Training Conditions in the False Alarm Decreasing Study

	Control condition	Training condition
Variables and phases	*M* (*SD*)	*M* (*SD*)
Voice false alarms[a]		
Baseline	2.59 (1.14)	2.92 (1.09)
Follow-up	3.61 (1.53)	3.53 (1.24)
Total voices[a]		
Baseline	4.78 (.85)	4.87 (.82)

(continued)

Variables and phases	Control condition *M* (*SD*)	Training condition *M* (*SD*)
Follow-up	5.15 (1.30)	5.06 (.98)
Voice hits		
Baseline	15.62 (6.38)	14.72 (5.88)
Follow-up	12.84 (6.31)	12.62 (5.60)

Note. [a] Square root transformed score.



$F(1, 74) = 2.16$, $p = .15$, $\eta_p^2 = .03$

Figure 6.5. Phase by condition interaction for voice false alarm. Error bars are standard errors.

6.3.3.2 Total Voices. The main effect of phase was significant, $F(1, 74) = 6.98$, $p = .01$, $\eta_p^2 = .09$. Contrary to prediction, Bonferroni corrected post-hoc test revealed that participants reported more voices in the follow-up than in the baseline phase (mean difference = .28; 95% CI = .07, .49; $p < .05$). The main effect of condition was not significant, $F(1, 74) = .00$, $p = 1.00$, $\eta_p^2 < .001$, nor was the interaction between phase and condition, $F(1, 74) = .28$, $p = .42$, $\eta_p^2 = .01$.

6.3.3.3 Voice hits. The main effect of phase was significant, $F(1, 74) = 23.30$, $p < .001$, $\eta_p^2 = .24$. Bonferroni post-hoc tests revealed that participants made fewer hits in the follow-up than in the baseline phase (mean difference =-2.44; 95% CI =-3.45, -1.44; $p < .001$). The main effect of condition, $F(1, 74) = .19$, $p = .66$, $\eta_p^2 =$

.003, and the phase by condition interaction, $F(1, 74) = .45$, $p = .50$, $\eta_p^2 = .01$, were not significant.

6.3.4 Hypothesis 4: There Will be Significant Correlations Between SSDT False Alarm Rate, Total SPSs, and Voice False Alarms in the Baseline

As mentioned previously, all the participants of Studies 3 and 4 were included in the baseline correlational analyses. Descriptive statistics of the task response outcomes for the entire sample are presented in Table 6.4. Spearman's rho (see Table 6.5) revealed that baseline SSDT false alarm rate both in the light absent and light present conditions had a significant positive correlation with voice false alarms. The correlation coefficients for total SPSs, however, were not significant.

Table 6.4

Median and Interquartile Range of the SSDT, VHT, and SPS Task Response Outcomes in the Baseline Phase (N = 151)

Variables (in the baseline)	Median	IQR
SSDT false alarm rate in light absent condition	.11	.06 to .21
SSDT false alarm rate in light present condition	.16	.09 to .26
Voice false alarm	7	4 to 11
Total SPS responses	1	.33 to 1.67

Note. IQR = Interquartile range.

Table 6.5

Spearman Correlation Coefficients of the Relationships Between Selected SSDT, VHT, and SPS Test Response Outcomes (N = 151)

Variables (in the baseline)	1	2	3
1. SSDT FA rate in light absent condition	-		
2. SSDT FA rate in light present condition	.65**	-	
3. Total SPS	.03	-.002	-
4. Voice FA	**.21****	**.17***	-.05

Note. FA = false alarm. SPS = spontaneous sensation.

*$p < .05$. **$p < .01$.

6.3.5 Hypothesis 5: There Will be a Significant Correlation Between SSDT False Alarm Rate in the Baseline and Somatic Symptom Severity (as Measured by the PHQ-15) and Between Voice False Alarms in the Baseline and Psychosis Proneness (as Measured by the Psychosis Proneness Questionnaire)

Descriptive statistics for the baseline SSDT false alarms, PHQ-15, voice false alarms, and psychosis proneness are presented in Table 6.6. Spearman correlation coefficients of Table 6.7 show that the relationships between the baseline SSDT false alarm rate in light absent and light present conditions and PHQ-15 were not significant. Similarly, there was no significant relationship between voice false alarms and psychosis proneness (Table 6.8).

Table 6.6

Median and Interquartile Range Values of the Baseline SSDT False Alarm Rate, Baseline Voice False Alarms, PHQ-15, and Psychosis Proneness (N = 151)

Variables	*Mdn*	IQR
SSDT false alarm rate in light absent condition	.11	.06 to .21
SSDT false alarm rate in light present condition	.16	.09 to .26
Voice false alarms	7	4 to 11
PHQ-15	7	4 to 10
Psychosis proneness*		
Perceptual aberration-magical ideation	1	1 to 2
Hypomania-impulsivity	2	1 to 3
Social anhedonia	0	0 to 1
Physical anhedonia	1	0 to 1

Note. IQR = Interquartile range.

* The psychosis proneness questionnaire consisted of four scales namely perceptual aberration-magical ideation, hypomania-impulsivity, social anhedonia, and physical anhedonia.

Table 6.7

Spearman Correlation Coefficients of the Relationships Between the Baseline SSDT False Alarm Rates and PHQ-15 Scores (N = 151)

Variables	1	2
1. SSDT false alarm rate in light absent condition	-	
2. SSDT false alarm rate in light present condition	.65*	-
3. PHQ-15	.06	.12

Note. * $p < .01$.

Table 6.8

Spearman Correlation Coefficients of the Relationships Between Baseline Voice False Alarms And Psychosis Proneness (N = 151)

Variables	1	2	3	4
1. Voice false alarms	-			
Psychosis proneness[a]				
2. Perceptual aberration-magical ideation	-.02	-		
3. Hypomania-impulsivity	-.02	.25**	-	
4. Social anhedonia	-.07	.18*	-.12	-
5. Physical anhedonia	.04	.08	-.07	-.08

Note. [a] The psychosis proneness questionnaire consisted of four scales namely perceptual aberration-magical ideation, hypomania-impulsivity, social anhedonia, and physical anhedonia.

*$p < .05$. **$p < .01$.

6.4 Discussion

Consistent with the findings of Study 2, this study demonstrates that the SSDT false alarm rate of high false alarm individuals can be altered with operant conditioning. As before, conditioning decreased the false alarm and hit rates and

made participants less likely to say "yes" (i.e., made their response criterion more stringent), but left sensitivity unchanged. Contrary to the hypothesis, SSDT conditioning did not result in the expected reduction in SPS in the follow-up phase. Rather, intensity of SPS seemed to increase after the training. Also, the hypothesized cross-modal transfer to the VHT did not occur. Irrespective of the condition, participants heard more voices in the follow-up phase, most of which were false alarms (as there were significantly fewer hits at follow-up than at baseline). Correlational analysis found significant relationships between SSDT false alarm rate and voice false alarms in the baseline indicating that both the tactile and auditory modalities may share some common mechanisms responsible for illusory perception (i.e. false alarms). Correlational analysis, however, did not find any significant relationship between SSDT false alarm rate and somatic symptom severity (as measured by the PHQ-15) and between voice false alarms and psychosis proneness (as measured by the psychosis proneness questionnaire of Hay et al., 2001).

Along with corroborating the findings of Studies 1, 2, and 4, the present study addresses the methodological problems of using a repeated measures design in Studies 1 and 2, where training effects were carried over to affect SSDT responses at baseline in the second session for those who were in the experimental group in session one. Using a between groups design in the present study, we found equivalent group performance in the baseline of all the four SSDT response outcomes, allowing for a more clear-cut demonstration of the impact of operant conditioning on SSDT performance. The present study is also consistent with Studies 1, 2 and 4 in suggesting that the SPS test is not suitable to detect any transfer effects of SSDT training. As the SPS test asks participants to look for and report all types of sensations, an alternative approach to the analysis of the data might be to categorize qualitatively similar sensations into fewer groups and to see if reporting of specific category of sensations changes due to the conditioning. It might also be useful to ask participants to detect and report specific sensations if they feel them. These alternative approaches might appear to be more appropriate to study likely transfer effects of the SSDT training as the SSDT uses a very specific stimulation (i.e.

vibration or touch sensation) and therefore the conditioning might transfer to sensations of that sort, and not SPS more generally.

Though the significant correlation coefficient between the SSDT false alarm rate and voice false alarms in the baseline suggests the possibility of cross-modal transfer of SSDT conditioning, the findings of the present study and those of Studies 4 are inconclusive as to whether any such transferred occurred. At the same time, however, the possibility of some transfer cannot be rejected outright. The figures depicting the phase by condition interaction of voice false alarms in Study 4 and 5 (i.e. Figure 5.5 and 6.5 in Chapter 5 and 6, pages 145 and 162 respectively) demonstrate that in both studies, experimental participants produced more false alarms in the baseline than the controls, though the difference was not significant. Also, irrespective of the conditions, participants produced significantly more false alarms in the follow-up than in the baseline phase. The pattern of change was different between the studies, however. In Study 4 (i.e., false alarm increasing study), experimental participants reported more false alarms in the follow-up phase than the controls, whereas the opposite was apparent in the present study (i.e. the experimental participants reported fewer false alarms in the follow-up phase than the controls). There are several possible explanations for this: (i) SSDT conditioning does not transfer to other sensory modalities; (ii) SSDT conditioning does transfer to other sensory modalities but the voice hearing task is not suitable to detect this transfer; and (iii) SSDT conditioning does transfer to other sensory modalities but the voice hearing task did not pick this up because there was a lack of power to detect the effect or insufficient training for significant transfer to take place. The first possibility seems unlikely because previous studies (see e.g., Section 5.1) have shown that perceptual training generalizes to the same and different perceptual modalities; admittedly, however, no other study has used conditioning to change somatosensory distortions like SSDT false alarms. If the second possibility were true then, like the SPS test, there would not be any relationship between the false alarms across the tasks, which was not found here. It therefore seems possible that the non-significant effects were due to inadequate training or a lack of power, the possibilities that were discussed previously

(see Section 5.4). With adequate training or testing more participants, the differences between the groups in the follow-up voice false alarms might become significant. Future studies should consider this possibility. It would be worth mentioning that the general increase in false alarms between phases anyway (regardless of training) makes it hard to measure the result. It should also be noted that other studies (e.g. Bratzke, Schröter, & Ulrich, 2014; Lapid, Ulrich, & Rammsayer, 2009; Nagarajan, Blake, Wright, Byl, & Merzenich, 1998) used (structurally and procedurally) the same task across the modalities to investigate probable transfer effects. However, a potential drawback of this method (i.e. to use the same task in different modalities) is that the obtained relationships might be attributed to common-method variance rather than to common perceptual mechanisms. Thus it is impressive that we picked up a significant relationship between reports of illusory perceptions (i.e. false alarms) in tactile and auditory modalities by administering different perceptual tasks to them, which differ not only in content but also in structure and procedure. Moreover, in spite of using limited number of trials for training, there was a near significant effect on total voices as the findings in the previous chapter demonstrate (see Section 5.3.3.2).

A response pattern common to both the SPS test and voice hearing task in all the studies (i.e. Studies 1, 2, 4, and 5 for the SPS test and studies 4 and 5 for the voice hearing task) was that, regardless of the experimental conditions, participants reported more SPS and voice false alarms in the follow-up than in the baseline phase. The experimental setting and the tasks perhaps affected the perceptual system such that participants experienced progressively more stimulation as time progressed. The testing took place in a relatively quiet and light attenuated room. For most of each testing session, participants were presented with white noise through headphones when they performed the SSDT, which was a repetitive task containing 640 trials. Staying in an artificial environment for a long time (around 1.5 hours in the present studies) while being deprived of normal levels of everyday stimulation perhaps disturbed the equilibrium of the perceptual system, resulting in an increase in illusory percepts over time. Indeed, studies have found that sensory deprivation (e.g., auditory deprivation as achieved by playing white noise through headphones or

auditory and visual deprivation as achieved by using a soundproof dark chamber) increased illusory experiences involving auditory, visual, and bodily sensations (Lloyd, Lewis, Payne, & Wilson, 2012) and psychotic-like experiences involving delusional thinking, perceptual distortions, cognitive disorganization etc. (Daniel & Mason, 2015). That sensory deprivation—a probable artefact of the present studies—produced similar response pattern in both tactile (as measured by the SPS task) and auditory (as measured by the voice-hearing task) modalities can be considered as indirect evidence that there are perceptual processes common to these different modalities.

The absence of any significant relationship between voice false alarms and psychosis proneness (as measured by the questionnaire of Hay et al., 2001) substantiated the conclusion of Chapter 3 (on the validation of the voice hearing task) that the voice hearing task is suitable only to study illusory experiences related to audition but not other psychotic-like experiences, such as body misperception, magical ideation, impulsivity etc.

Consistent with the findings of Studies 1 and 2, the present studies did not find any relationship between the SSDT false alam rate and PHQ-15 scores. This contradicts the findings of previous research suggesting a significant relationship between the two (e.g. Brown et al., 2010, 2012; Katzer, Oberfeld, Hiller, & Witthöft, 2011). It might be that the relationship between the SSDT false alarm rate and somatic symptom severity depends on other factors that were absent in the present studies. It is also possible that some unwanted variables contaminated the relationship. Measuring severity of somatic symptoms in healthy individuals perhaps is problematic because everyday life experiences may influence their responses. For example, it was exam period when some participants took part in the studies and it is possible that the exam related stress had differential effects on somatic symptoms and SSDT performance, thus diminishing the relationship between the two. Future studies should exercise caution about the presence of potential confounding variables while looking into the relationship between SSDT response outcomes and responses on symptom measures.

CHAPTER 7

Study 6: Are Responses in the Somatosensory Signal Detection Task Dependent on Stimuli and Responses in Previous Trials?

> Did the shuddering have something to do with occupying my father's place? . . . Was the sight of that green lawn outside Old Main, where my father once had his office, the image of which is scratched into my memory because I walked there again and again, not only as a child but as a girl, and then as a young woman when I was a student? But it wasn't the vision of the place that started the convulsion; it was the act of speaking. It began with the first word and ended with the last. Was it connected to a memory?
>
> —Siri Hustvedt, The Shaking Woman or A History of My Nerves, pp.135-136

As in previous research, we found (in Studies 1, 2, 4, and 5) that individuals often reported sensations of touch in touch absent trials (i.e. they produced false alarms) on the Somatosensory Signal Detection Task (SSDT; Lloyd et al., 2008). In addition, we demonstrated that training significantly changed false alarm rates and there were some indications that the training might have transferred to another sensory modality. However, we did not find any significant correlation between the SSDT false alarm rate and physical symptom reporting, contrary to past research (Brown et al., 2012; Brown et al., 2010; Katzer et al., 2011).

The findings that false alarms in the SSDT can be conditioned using reward and punishment suggest that the process that brings about false alarms is not fixed, and that its strength and functioning depend on the context and ongoing perceptual and cognitive processes related to the experience of touch. One potentially important factor in these respects is what happened on previous trials of the task. It is known that, if two events occur repeatedly in close temporal succession, the presentation of one event influences the response to the next (Perruchet, Cleeremans, & Destrebecqz, 2006). A number of studies have demonstrated this experimentally (see Appendix AB for a summary of the studies). For example, one of the earliest studies on weight judgment showed that a contrast effect exists between the weight of the preceding stimulus and judgment of the current stimulus; that is, the same current stimulus was judged lighter if the preceding stimulus was heavier but heavier if the preceding stimulus was lighter (Fernberger, 1920). Using a different (absolute

judgment) task with auditory stimuli and feedback, Holland and Lockhead (1968) later found that judgments in the current trial tended to be similar to stimuli in the immediately prior trial (so-called assimilation) but different from the stimuli in earlier trials (trials two to five; so-called contrast). Assimilation and contrast are also reported in other studies and are discussed under the term sequential effects (Lockhead, 1992; Podlesek, 2010). According to Lockhead (1992), sequential effects are found in every psychophysical judgment task no matter what procedures are used. According to Jones, Curran, Mozer, and Wilder (2013), sequential effects are ubiquitous features of human and animal behaviour.

Two alternative interpretations have been proposed to account for sequential effects (i.e. sequential dependency of the current response on previous events): conscious expectancy and automatic activation (Perruchet et al., 2006). Expectancy is the conscious anticipation of what might come next, given what we know about previous events. With regard to the SSDT, if a vibration was present on the previous trial, our knowledge of that might influence whether we expect a vibration in the current trial. The Gambler's Fallacy is a well-known conscious expectancy (Burns & Corpus, 2004), which is the belief that an event is less likely to be repeated if other events are equiprobable. Participants guided by the Gambler's Fallacy in the SSDT are more likely to expect vibration in the current trial (and thus is more likely to respond "yes") if they did not perceive a vibration in previous trials (and responded "no"). Automatic activation, on the other hand, is independent of conscious expectancy and is influenced by previous associations between events and the activation of their underlying representations in memory (Perruchet et al., 2006). A good example of this process is priming, which is a form of nonconscious memory that influences identification, production, or classification of an event due to previous experiences with the same or related events (Schacter, Dobbins, & Schnyer, 2004). If automatic activation influences responses on the SSDT, participants are more likely to expect and report vibration in the current trial if they felt the vibration (and therefore the relevant memory was activated) in previous trials. This is similar to the interpretation of SSDT false alarms, which are understood as misinterpretations of unrelated

sensory information due to excessive activation of touch representations in memory (i.e. unrelated sensory information is perceived as vibration due to its erroneous association with the activation of memory related to touch, Brown et al., 2012).

If the Gambler's Fallacy guides SSDT responses, the conscious expectancy for the presence of vibration in the current trial would be highest (and thus the possibility of responding "yes") after a long run of vibration absent trials, and lowest after a long run of vibration present trials. On the other hand, if automatic activation guides SSDT responses, the likelihood of responding "yes" in the current trial would be highest after a long run of vibration present trials and lowest after a long run of vibration absent trials.

The presence of such sequential effects on the SSDT would have several implications for signal detection research. First, it would replicate the evidence that context, such as task characteristics and previous events, affect detection of signals. Second, it would confirm a relationship between sequential effects and response bias and its effect on task response. Third, it would demonstrate stability and change in sequential effects under training and feedback conditions (i.e., it would address the question of how our perceptual system learns to give specific responses). Fourth, psychophysical studies in general have provided scant attention to sequential effects in response errors (e.g. false alarms) and whether they are related to psychological abnormalities. This possibility can be investigated by studying the relationship between sequential effects on the SSDT and appropriate measures of mental health, such as the Patient Health Questionnaire-15 (PHQ-15). Fifth, comparison between the sequential effects for the SSDT responses (i.e. hits, false alarms, misses, and correct rejections) would allow us to better understand the bottom-up (which depend on stimulus input) and top-down (which involve memory systems and expectation) processes underlying perceptual decision making and experiences.

Each SSDT trial consists of three events, namely vibration, light, and response. The present study investigated sequential effects (conscious expectation vs. automatic activation) of each of these events on responses in current trials. Given the complexity of the SSDT, and the additional complexity added by the training

procedure, we focused on effects across two successive trials (i.e. response in the current trial and events in the immediately preceding [N-1] trial) here. This chapter therefore looks at whether:

1. SSDT events (i.e. presence of vibration, presence of light, and responding "yes") in the N-1 trial affect responses (i.e. "yes" vs. "no") in the current trial.
2. Individuals differ in sequential effects on the SSDT.
3. Sequential effects in the SSDT are related to symptom reporting.
4. SSDT training (as described in Chapters 2, 3, 5, and 6) changes any baseline sequential effects.

7.2 Method

7.2.1 Participants

The second session SSDT data of Studies 1 and 2 had been excluded due to the possibility that experiences in the first session might have been carried over and influenced the performance in second session. Thus in the present study, we examined sequential dependency in the SSDT data of 227 participants who participated in Studies 1 (first session), 2 (first session), 4, and 5 (as presented in Chapters 2, 3, 5, and 6 respectively). Of these, 121 were low false alarm participants (false alarm rate [FAR] < .16 in Study 1 and < .15 in Study 4) and 106 were high false alarm participants (FAR ≥ .16 in Study 1 and ≥.15 in Study 4) at baseline in the light present condition; 131 were female and 96 were male and their mean age was 23.09 years (age range = 18 to 39 years, *SD* = 4.45). Sixty-five of the low false alarm participants and 50 of the high false alarm participants had been allocated to the control condition and 56 participants in both the false alarm groups to the experimental condition.

7.2.2 Measures

7.2.2.1 The patient health questionnaire-15 (PHQ-15). The PHQ-15 (for details, see Section 2.2.3.1.1) scores of the participants obtained during Studies 1, 2, 4 and 5 were used to investigate their relationships with sequential dependencies between SSDT events.

7.2.2.2 The somatosensory signal detection task (SSDT). Current "yes" and "no" responses that have been sequentially analyzed represent hits (responding "yes" when vibration was present), false alarms (responding "yes" when vibration was absent), misses (responding "no" when vibration was present), and correct rejections (responding "no" when vibration was absent) on the SSDT.

7.2.3 Procedure

7.2.3.1 Extraction and synthesis of the SSDT data. For each type of outcome on the current trial (i.e. hits, false alarms, misses, and correct rejections), we used a software package (MATLAB, Version 2015a) to extract information concerning the response ("yes" vs. "no"), vibration (present vs. absent) and light (present vs. absent) on the N - 1 trial, as shown in Figure 7.1. The MATLAB script looked for and counted the frequency of occurrences of each pair of events (e.g., sequences constituting a hit in the current trial and a "yes" response in the N-1 trial) across all of the SSDT trials.

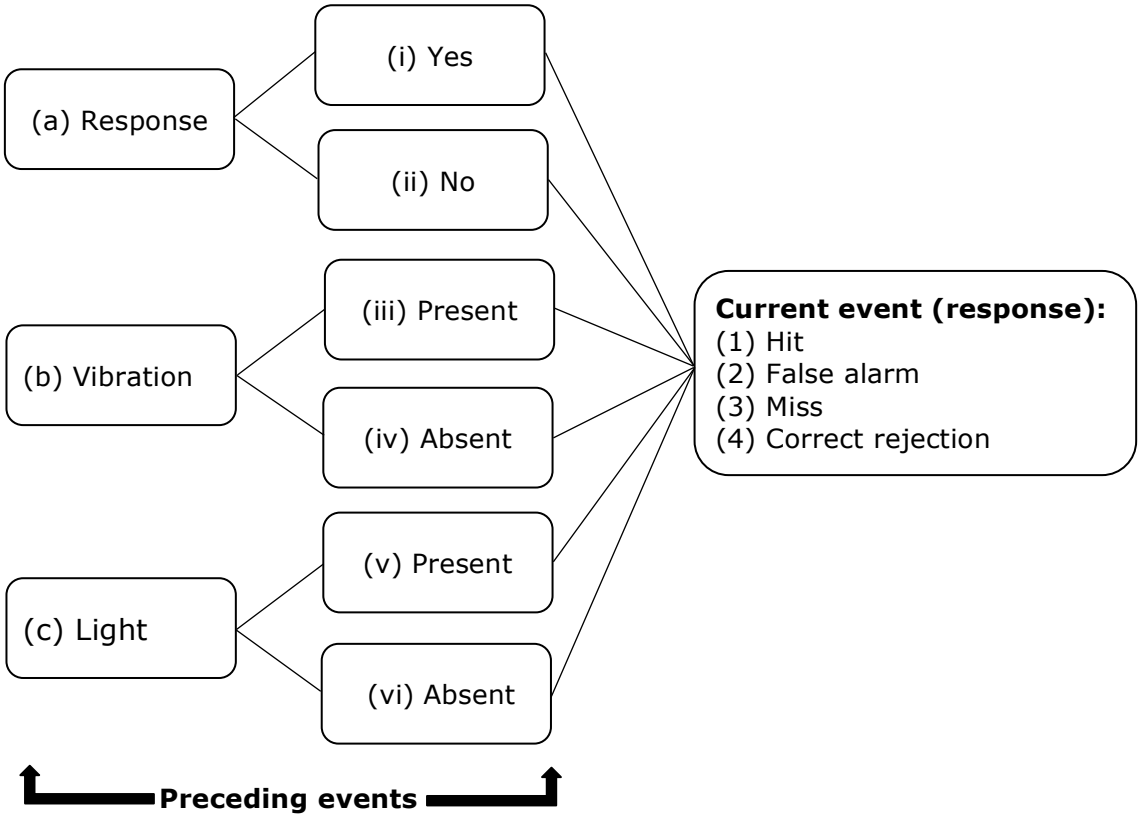


Figure 7.1. Pairs of preceding ("yes" or "no" response, presence or absence of vibration, presence or absence of light) and current (hit, false alarm, miss, and correct rejection) events investigated for sequential effects.

7.2.3.2 Exclusion of data and participants. Participants with very high (>.90) and low (<.10) hit rates (average hit rate combining the light present and light absent trials) in the baseline phase were excluded from the analysis. This was to ensure that the number of hits and false alarms for each participant was adequate to carry out the sequential analysis. A total of 22 participants were excluded on this basis as shown in Table 7.1. This left 105 low and 100 high false alarm participants for the sequential analysis.

Table 7.1

Number of Participants Excluded from the Studies Due to Excessively High or Low Hit Rate in the Baseline Phase

Study	Control group	Experimental group	*n*
1 (LFA)	4	2	6
2 (HFA)	0	2	2
4 (LFA)	6	4	10
5 (HFA)	3	1	4
n	13	9	22

Note. LFA = low false alarm participants; HFA = high false alarm participants.

There were five high and one low false alarm participants who, in the follow-up phase, did not produce any "yes" responses (resulting in zero relevant sequential data) who were therefore excluded from the analyses. Thus there were 104 low false alarm and 95 high false alarm participants eligible for the analyses involving both the baseline and follow-up phases.

In the analyses of training effects on the current "yes" (combining hits and false alarms) and "no" (combining misses and correct rejections) responses, there were 54 control and 50 experimental participants in the low false alarm group and 47 control and 48 experimental participants in the high false alarm group. To take advantage of the fact that the *F*-statistic is quite robust to violation of normality and homogeneity of variance when group sizes are equal (Field, 2009), four of the control participants with low false alarm rates and one experimental participant with a high false alarm rate were randomly excluded to ensure that the groups were equal in size. Thus these

ANOVAs have been carried out with 100 low and 94 high false alarm participants divided equally between the groups.

There were 13 high false alarm participants who produced zero false alarms in the follow-up phase and therefore they were excluded from the sequential analysis that examined SSDT training effects on the current false alarms of high false alarm participants. Thus 47 control and 40 experimental high false alarm participants remained for those analyses. To make the group size equal, seven control participants were randomly excluded.

Participants were not excluded from *t*-tests (that examined sequential effects of N-1 events on current responses) and correlational analysis (that examined relationships between sequential effects and PHQ-15 scores).

7.2.3.3 Data analysis. SSDT data were processed and analysed in accordance with the research questions described below.

7.2.3.3.1 Question 1: Did N-1 event (i.e., response, presence or absence of vibration, presence or absence of light in the preceding trial) affect the response (i.e. "yes" or "no") in the current trial? To answer this question, paired sample *t*-tests were carried out to analyze baseline data of the SSDT. This determined whether the proportion of "yes" (including both hits and false alarms) and "no" (including both misses and correct rejections) responses in current trials differed significantly in relation to the preceding event (i.e. response, vibration, or light). Equations 1 and 2 were used to calculate the proportions:

Equation 1: Proportion (calculated as a percentage) of current "yes" responses (hits plus false alarms) preceded by an SSDT event =

$$\frac{Frequency\ of\ (hits + false\ alarms)\ preceded\ by\ an\ SSDT\ event}{Total\ number\ of\ hits\ and\ false\ alarms} \times 100$$

Equation 2: Proportion (calculated as a percentage) of the current "no" response (misses plus correct rejections) preceded by an SSDT event =

$$\frac{Frequency\ of\ (misses\ +\ correct\ rejection\ )\ preceded\ by\ an\ SSDT\ event}{Total\ number\ of\ misses\ and\ correct\ rejections} \times 100$$

To test the assumption of normality, differences between the pairs of percentages (as equations 1 and 2 calculated) were computed to see if this new variable (i.e. percentage of hits and false alarms preceded by an SSDT event – percentage of misses and correct rejections preceded by the same SSDT event) was normally distributed. All the distributions were found to be normal, $D(205) = .06$, $p = .06$, when the preceding event was the "yes" response; $D(205) = .06$, $p = .20$, when vibration was present in the preceding trial; and $D(205) = .04$, $p = .20$, when light was present in the preceding trial.

7.2.3.3.2 Question 2: Is there any relationship between the N-1 effect and symptom reporting? The distribution of PHQ-15 scores was non-normal, $D(205) = .12$, $p < .001$ and transformations did not correct the problem. Therefore, Spearman's rank correlation coefficients were calculated between PHQ-15 scores and measures of the N-1 effect in the baseline phase.

N-1 responses are likely to be affected by response bias. In other words, response criterion might confound the sequential relationship between consecutive responses. For example, for both "yes" and "no" responses in current trials, N-1 responses are likely to be "yes" if the response criterion is liberal. The opposite might be obtained for a stringent response criterion. Also, for some participants, vibration or light might more often be present in N-1 trials merely by chance (though they were randomly presented). Therefore, to carry out the correlational analysis, sequential effect scores in the baseline (equations 1 and 2) were corrected for response bias and the presence of vibration and light in preceding trials. Equation 3 was used to determine the overall presence of an event.

Equation 3: Proportion (calculated as a percentage) of all the trials preceded by an event (response, presence of vibration, or presence of light) =

$$\frac{Frequency\ of\ (hits\ +\ false\ alarms\ +\ misses\ +correct\ rejections)\ preceded\ by\ an\ event}{Total\ number\ of\ trials} \times 100$$

Corrected scores were the differences between the corresponding scores that equation 1 (or 2) and 3 produced. For example, the corrected N-1 effect of "yes"

response on the current "yes" response = percentage of current "yes" responses preceded by another "yes" response (equation 1) – percentage of all the trials preceded by the "yes" response (equation 3).

As all the participants produced sufficient numbers of (i.e. at least one) misses and correct rejections necessary to calculate the percentages, they were not combined for correlational analysis (which was not the case for hits and false alarms). Thus, nine corrected sequential effects were obtained for this analysis:

(i) Effect of N-1 "yes" response on hits and false alarms (i.e., proportion of hits and false alarms on the current trial was the dependent variable and N - 1 "yes" response was the independent variable)

(ii) Effect of N-1 "yes" response on misses.

(iii) Effect of N-1 "yes" response on correct rejections.

(iv) Effect of N-1 presence of vibration on hits and false alarms.

(v) Effect of N-1 presence of vibration on misses.

(vi) Effect of N-1 presence of vibration on correct rejections.

(vii) Effect of N-1 presence of light on hits and false alarms.

(viii) Effect of N-1 presence of light on misses.

(ix) Effect of N-1 presence of light on correct rejections.

7.2.3.3.3a Question 3a: Did the SSDT training change sequential effects?

Mixed ANOVAs were carried out with corrected scores (computed using the equations described above) to determine whether the baseline sequential effects changed at follow-up due to the SSDT training. In this analysis, condition (control vs. training) was the between-group independent variable and phase was the within-group independent variable. Phase included baseline and follow-up sequential effects of

(i) N-1 "yes" response on responding "yes" (hits plus false alarms).

(ii) N-1 "yes" response on responding "no" (misses plus correct rejections).

(iii) N-1 presence of vibration on responding "yes" (hits plus false alarms).

(iv) N-1 presence of vibration on responding "no" (misses plus correct rejections).

(v) N-1 presence of light on responding "yes" (hits plus false alarms).

(vi) N-1 presence of light on responding "no" (misses plus correct rejections).

Baseline and follow-up sequential effects were compared independently in the studies in which participants were trained to produce more (i.e. Studies 1 and 4) or fewer (i.e. Studies 2 and 5) false alarms.

7.2.3.3.3b Question 3b: Did SSDT training change sequential effects for false alarms of high false alarm participants? Mixed ANOVAs were carried out on corrected percentages of false alarms (using equation 1 and 3 but including only the frequency of false alarms in the numerator of equation 1) of high false alarm participants, with condition (control vs. training) as the between-group independent variable and phase (baseline vs. follow-up) as the within-group independent variable. Phase included baseline and follow-up sequential effect of

(i) N-1 "yes" responses on false alarms.

(ii) N-1 presence of vibration on false alarms.

(iii) N-1 presence of light on false alarms.

7.2.3.4 Examining the assumptions of mixed ANOVA. Most of the distributions (there were 24 distributions in each study) satisfied the assumptions of mixed ANOVA. Where problems remained, to satisfy the assumption of normality, normalizing transformations were carried out with the following distributions:

(i) Effects of N-1 "yes" responses on responding "yes": both false alarm increasing and decreasing studies were transformed to their square roots.

(ii) Effects of N-1 "yes" responses on responding "no": data of false alarm decreasing studies corrected by reverse score square root transformation.

Standard correction techniques could not fix all deviations from normality (see Appendix AC for a detailed list of the distributions that violated the assumptions of mixed ANOVA). It is to be noted that the assumptions were violated mostly in the follow-up phase, which was expected because the SSDT training and experiences of working in the lab for a long period of time might have differential effects on participants, thereby influencing their performance on the SSDT and sequential dependency between the SSDT events. As the *F*-statistic is quite robust to violations of the assumptions (Field, 2009), the sample size was large, and the assumptions were satisfied in most of the cases, we decided to persist with mixed ANOVAs to

analyze the data. However, non-parametric tests were used to analyze the effect of N-1 light on responding "no" in studies 1 and 4 (where participants were trained to produce more false alarms) because in this case both the assumptions of normality and homogeneity of variance were violated. Mann-Whitney test was used to test differences between the control and experimental conditions and Wilcoxon signed-rank test was used to compare the baseline and follow-up phases.

7.3 Results

7.3.1a Hypothesis 1a: "Yes" Responses Will More Often be Preceded by Another "yes" Response Than Will "no" Responses

7.3.1b Hypothesis 1b: "Yes" Responses Will More Often be Preceded by a Vibration in the N-1 Trial Than Will "no" Responses

7.3.1c Hypothesis 1c: "yes" Responses Will More Often be Preceded by Light in the N-1 Trial Than Will "no" Responses

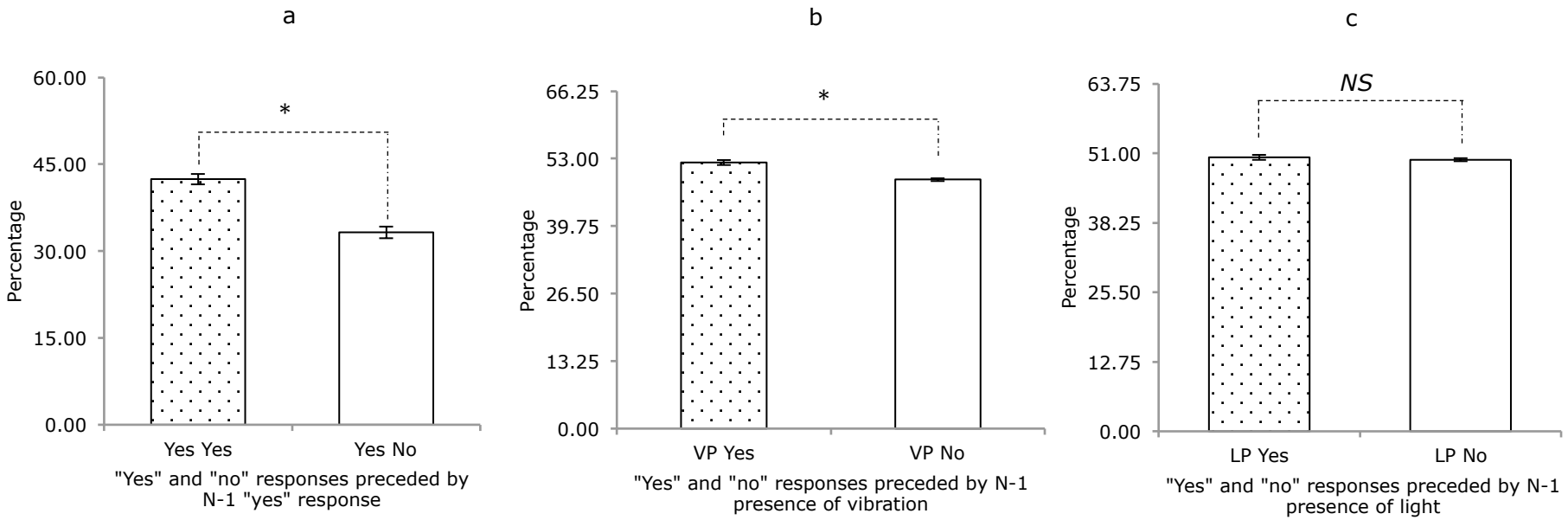
As hypothesized (1a), a paired sample *t*-test indicates that "yes" responses (hits and false alarms) on the current trial were preceded more often by "yes" responses on the N - 1 trial (M = 42.44, SE = .91) than were "no" responses (misses and correct rejections, M = 33.24, SE = 1.02), $t(204) = 10.93$, $p < .001$, $r = .61$ (see Figure 7.2a).

In accordance with hypothesis 1b, it was found that the proportion of vibrations preceding "yes" responses on the current trial (M = 52.25, SE = .50) was significantly higher than that preceding "no" responses (M = 48.87, SE = .29), $t(204) = 4.58$, $p < .001$, $r = .31$ (see Figure 7.2b).

Contrary to our prediction (1c), there was no difference between "yes" (M = 50.26, SE = .49) and "no" responses (M = 49.80, SE = .26) in terms of whether they were preceded by the light, $t(204) = .67$, $p = .50$, $r = .05$ (see Figure 7.2c).

7.3.2 Hypothesis 2: Sequential Effects Will Be Positively Correlated with Symptom Reporting

In order to evaluate whether there is a correlation between sequential effects and symptom reporting, we need to know the features of the distributions of sequential data. To this aim, histograms were constructed for scores obtained using



$t(204) = 10.93$, $p < .001$, $r = .61$ $t(204) = 4.58$, $p < .001$, $r = .31$ $t(204) = .67$, $p = .50$, $r = .05$

Figure 7.2. Difference between the occurrences of "yes" and "no" responses as they were preceded by N-1 "yes" response (a), presence of vibration (b), and presence of light (c). Error bars are standard errors.

equations 1 and 2 and the differences between them (Figure 7.3). All the graphs are symmetric and unimodal with few outliers in the effects of N-1 "yes" response. The spread of the graphs portrays individual differences in sequential effects that approximate a normal curve.

Descriptive statistics of the variables are presented in Table 7.2. Spearman correlation coefficients are presented in Table 7.3. The severity of reported somatic symptoms had a significant positive correlation with the N-1 effect of the light on miss trials, r (205) = .15, p < .05. None of the other effects were significant.

Table 7.2

Median and Interquartile Range of the PHQ-15 Scores and sequential effects in the baseline phase (N = 205)

Variables	*Mdn*	IQR
PHQ-15	6	4 to 9.5
Effect of N-1 "yes" responses on		
hits and false alarms	5.10	1 to 11.32
misses	-5.72	-11.48 to -1
correct rejections	-1.01	-4.47 to 1.77
Effect of N-1 presence of vibration on		
hits and false alarms	2.10	-3.05 to 6.91
misses	-2.84	-8.41 to 2.19
correct rejections	-.56	-4.02 to 4
Effect of N-1 presence of light on		
hits and false alarms	0	-4.35 to 5.09
misses	0	-5.75 to 4.54
correct rejections	0	-3.60 to 3.39

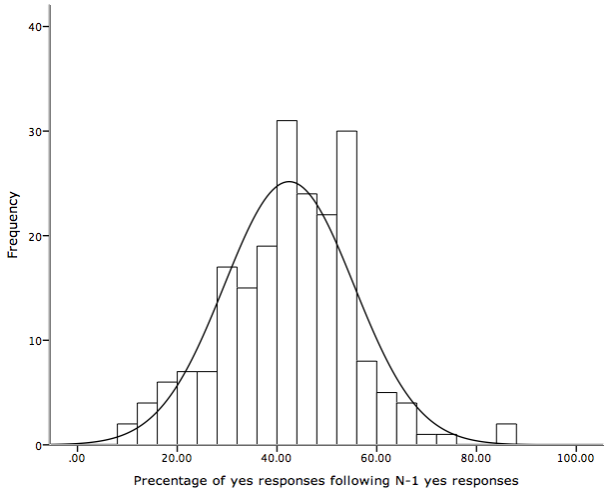
Note. IQR = Interquartile range.

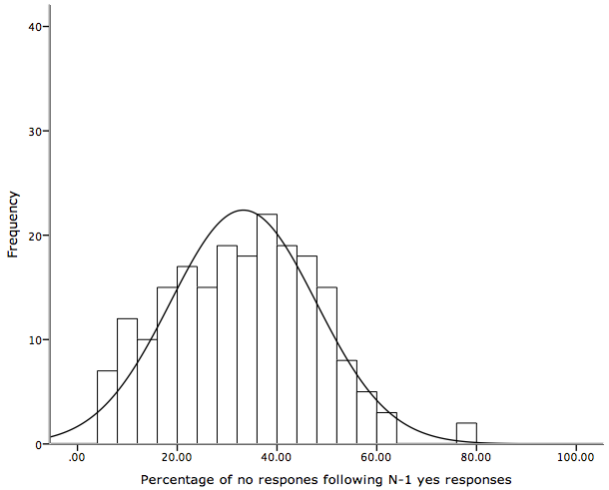
7.3.3 Hypothesis 3: The SSDT Training Will Change the N-1 Effects

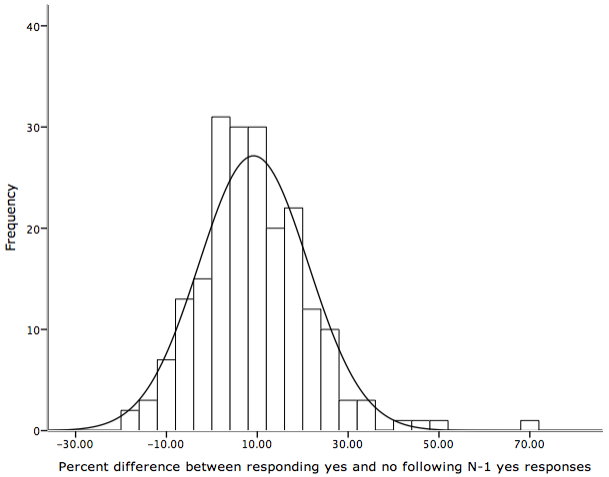
7.3.3.1 Studies on training participants to produce more false alarms (Studies 1 and 4)

Descriptive statistics on the sequential effects are presented in Table 7.4.

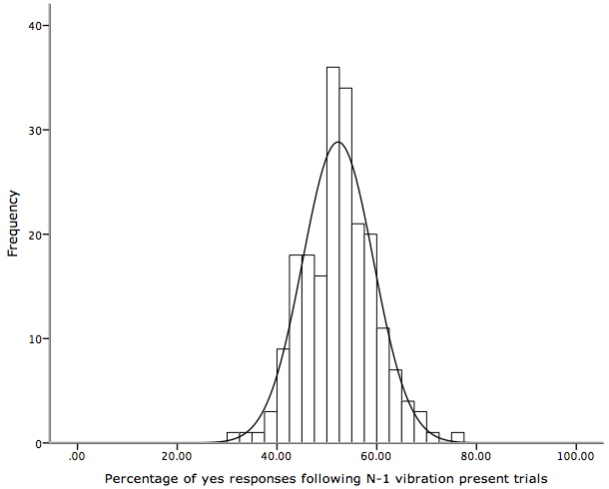
a



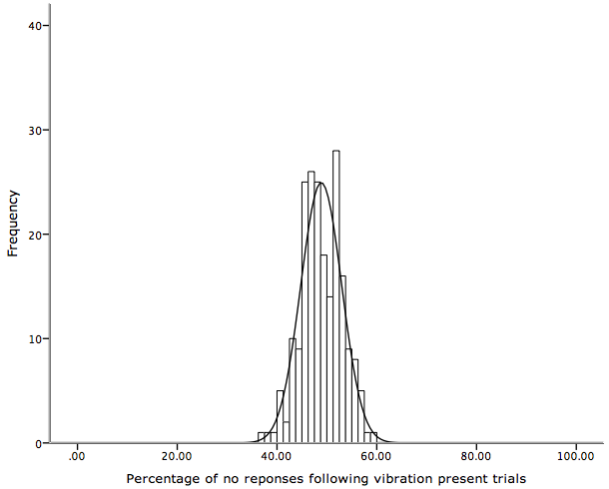




b



1



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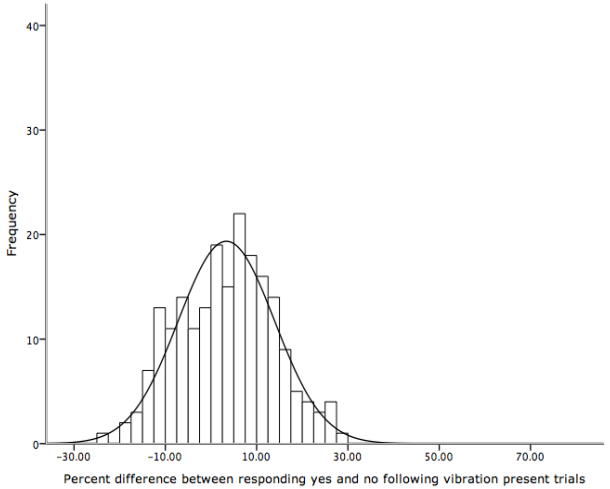
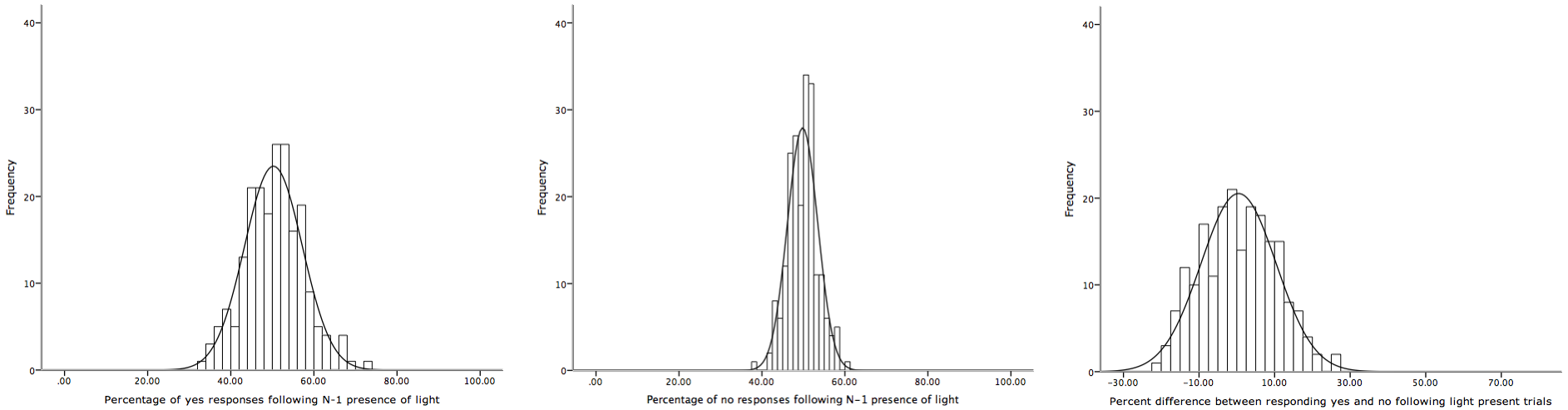


3

(continued)

c



1 2 3

Figure 7.3. Distributions of "yes" (1) and "no" (2) responses and differences between them (3) as they were preceded by N-1 "yes" response (a), presence of vibration (b), and presence of light (c).

Table 7.3

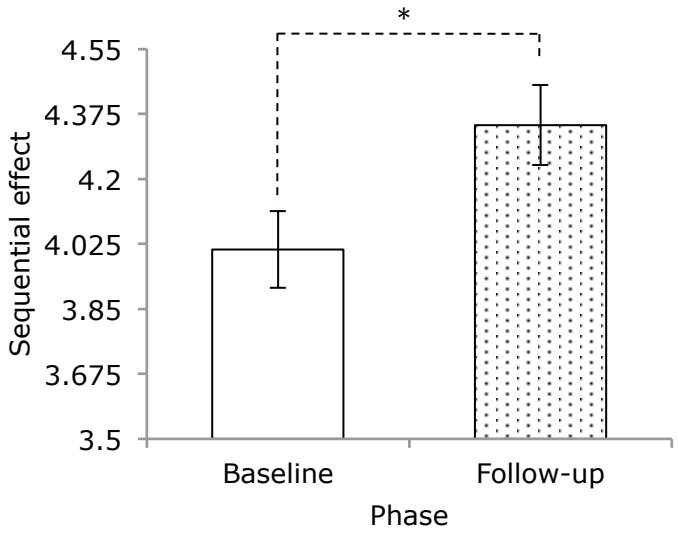
Spearman Correlation Coefficients of the Relationships Between the PHQ-15 Scores and Sequential Effects in the Baseline (N = 205)

Variables	1	2	3	4	5	6	7	8	9
1. PHQ-15	--								
Effect of N-1 "yes" responses on									
2. hits and false alarms in current trials	-.01	--							
3. misses in current trials	.07	-.35**	--						
4. correct rejections in current trials	-.01	-.57**	-.14*	--					
Effect of N-1 presence of vibration on									
5. hits and false alarms in current trials	-.03	.44**	-.21**	-.29**	--				
6. misses in current trials	.05	-.15*	.49**	-.05	-.33**	--			
7. correct rejections in current trials	.02	-.24**	-.04	.30**	-.64**	-.30**	--		
Effect of N-1 presence of light on									
8. hits and false alarms in current trials	-.01	.17*	-.03	-.15*	.04	.02	-.11	--	
9. misses in current trials	.15*	-.06	.17*	-.07	-.08	.06	.11	-.35**	--
10. correct rejections in current trials	-.10	-.05	-.13	.22**	.02	-.07	.03	-.61**	-.35**

Note. $^{*}p < .05$. $^{**}p < .01$.

7.3.3.1.1 Effect of N-1 "yes" responses on responding "yes" (hits and false alarms). There was a significant main effect of phase, F (1, 98) = 5.39, p < .05, η_p^2 = .05. Bonferroni corrected post hoc test indicates that the sequential effect was significantly greater in the follow-up than in the baseline phase, mean difference = .34, p < .05, 95% CI [.05, .62] (see Figure 7.4).

The main effect of condition was not significant, F(1, 98) = .93, p = .34, η_p^2 = .01, and neither was the interaction between phase and condition, F(1, 98) = .07, p = .80, η_p^2 = .001.



F (1, 98) = 5.39, p < .05, η_p^2 = .05

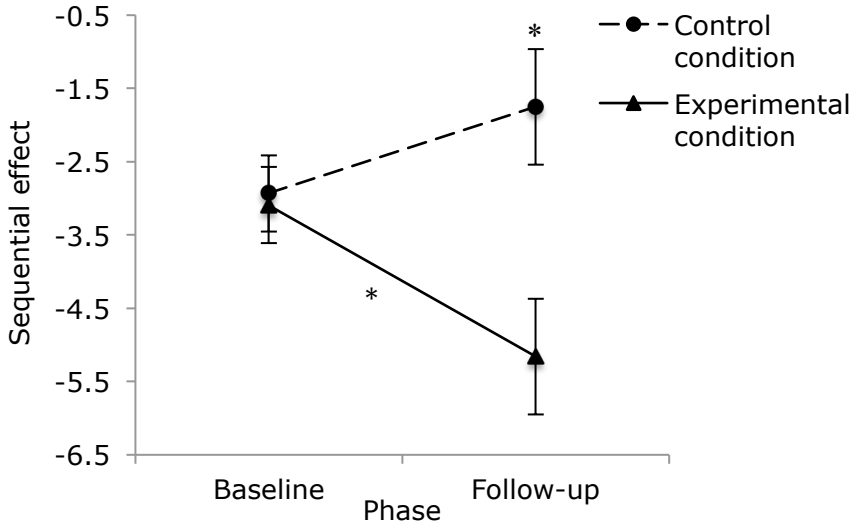
Figure 7.4. Effect of N-1 "yes" responses on responding "yes" in the baseline and follow-up phases of the studies that trained participants to increase false alarm rates.

7.3.3.1.2 Effect of N-1 "yes" responses on responding "no" (misses and correct rejections). The main effect of phase was not significant, F(1, 98) = .51, p = .48, η_p^2 = .01. The main effect of condition was significant, F(1, 98) = 6.27, p < .05, η_p^2 = .06, as was the interaction between phase and condition, F(1, 98) = 6.82, p < .05, η_p^2 = .07 (see Figure 7.5).

Bonferroni corrected post hoc test indicated that the baseline sequential effect did not differ between the conditions, mean difference = .16, p = .74, 95% CI [-1.31, 1.63], but in the follow-up phase the effect was stronger in the control condition (i.e.

the proportion of "yes" responses prior to a "no" were higher in the control than the experimental condition), mean difference = 3.41, $p < .01$, 95% CI [1.20, 5.62].

There was no significant difference between the phases in the control condition, mean difference = -1.18, $p = .18$, 95% CI [-2.93, .57]. In the experimental condition, the sequential effect was stronger in the baseline than in the follow-up phase, mean difference = 2.07, $p < .05$, 95% CI [.32, 3.82].



F (1, 98) = 6.82, $p < .05$, $\eta_p^2 = .07$

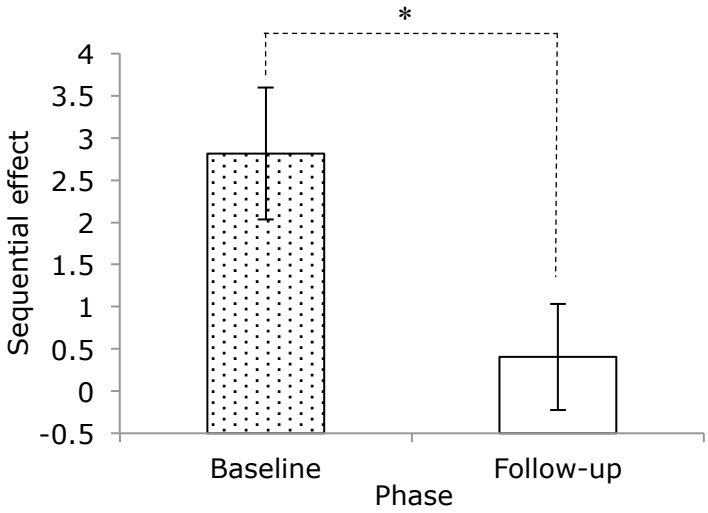
Figure 7.5. Sequential effect of N-1 "yes" responses on responding "no" in the baseline and follow-up phases of the control and experimental conditions in the studies that trained participants to increase false alarm rates.

7.3.3.1.3 Effect of N-1 vibration on responding "yes" (hits and false alarms). There was a significant main effect of phase, $F(1, 98) = 6.93$, $p < .05$, $\eta_p^2 = .07$. Bonferroni corrected post hoc test indicated that "yes" responses were preceded more by a vibration in the baseline than in the follow-up phases, mean difference = 2.41, $p < .05$, 95% CI [.59, 4.23] (see Figure 7.6).

The main effect of condition and the phase by condition interaction were not significant, $F(1, 98) = .08$, $p = .78$, $\eta_p^2 = .001$, and $F(1, 98) = 1.01$, $p = .32$, $\eta_p^2 = .01$, respectively.

7.3.3.1.4 Effect of N-1 vibration on responding "no" (misses and correct rejections). The main effect of phase was not significant, $F(1, 98) = .56$, $p = .46$, η_p^2

= .01, and neither was the main effect of condition, $F(1, 98) = 1.05$, $p = .31$, $\eta_p^2 = .01$. The interaction between phase and condition was nearly significant, $F(1, 98) = 3.53$, $p = .06$, $\eta_p^2 = .04$.



$F(1, 98) = 6.93$, $p < .05$, $\eta_p^2 = .07$

Figure 7.6. Sequential effect of N-1 presence of vibration on responding "yes" in the baseline and follow-up phases of the studies that trained participants to increase false alarm rates.

7.3.3.1.5 Effect of N-1 light on responding "yes" (hits and false alarms). The main effect of phase, $F(1, 98) = .09$, $p = .76$, $\eta_p^2 = .001$, condition, $F(1, 98) = .35$, $p = .56$, $\eta_p^2 = .004$, and interaction between them, $F(1, 98) = .01$, $p = .94$, $\eta_p^2 < .001$, were not significant.

7.3.3.1.6 Effect of N-1 light on responding "no" (misses or correct rejections). A Mann-Whitney U test indicated that the sequential effect in the control group did not differ from that in the experimental group both in the baseline, $U = 1211$, $z = -.27$, $p = .79$, $r = -.03$, and follow-up, $U = 1238$, $z = -.08$, $p = .93$, $r = -.01$, phases.

A Wilcoxon signed-rank test showed that the baseline (*Mdn* = .14 and -.35 in the control and experimental conditions respectively) and follow-up (*Mdn* = .15 and .07 in the control and experimental conditions respectively) effects did not differ in

Table 7.4

Means (and Standard Deviations)[a] of the Sequential Effects of N-1 Events ("Yes" Response, Presence of Vibration, and Presence of Light) on Responding "Yes" or "No" in the Baseline and Follow-up Phases of the Studies that Trained Participants to Increase Their False Alarms (Studies 1 and 4)

	Control group		Training group	
Sequential effects	Baseline	Follow-up	Baseline	Follow-up
N-1 "yes" responses and current responses were				
"yes" (hits or false alarms)[b]	3.95 (.87)	4.25 (.85)	4.07 (1.16)	4.43 (1.27)
"no" (misses or correct rejections)	-2.93 (3.14)	-1.75 (2.51)	-3.09 (4.18)	-5.16 (7.48)
N-1 presence of vibration and current responses were				
"yes" (hits or false alarms)	3.13 (8.18)	-.21 (6.86)	2.51 (7.43)	1.02 (5.68)
"no" (misses or correct rejections)	-1.19 (3.06)	.21 (2.42)	-.78 (3.41)	-1.38 (5.89)
N-1 presence of light and current responses were				
"yes" (hits or false alarms)	.15 (5.95)	-.09 (6.48)	.82 (9.63)	.42 (6.16)
"no" (misses or correct rejections)[c]	.14 (-1.82 to 3.33)	.15 (-1.59 to 1.08)	-.34 (-2.93 to 2.03)	.07 (-2.32 to 1.38)

Note. [a] Means and standard deviations are computed from the corrected sequential effects measured in percentage. [b]Square root transformed score. [c]Statistics are median with interquartile range scores in parentheses.

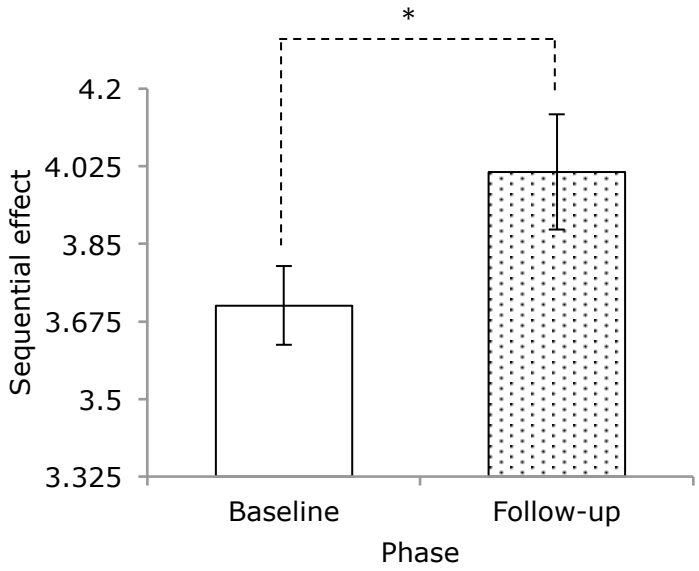
either the control, z = -.61, p = .55, r = -.01; or the experimental, z = -.02, p = .98, r = -.002, conditions.

7.3.3.2 Studies on training participants to decrease false alarms (Studies 2 and 5). Descriptive statistics on the sequential effects are presented in Table 7.5.

7.3.3.2.1 Effect of N-1 "yes" responses on responding "yes" (hits and false alarms). There was a significant main effect of phase, $F(1, 92) = 5.35$, $p < .05$, $\eta_p^2 = .06$. Bonferroni corrected post hoc test indicates that the effect was stronger in

the follow-up than in the baseline phase, mean difference = .30, $p < .05$, 95% CI [.04, .56] (see Figure 7.7).

The main effect of condition was not significant, $F(1, 92) = 2.22$, $p = .14$, $\eta_p^2 = .02$; and neither was the phase by condition interaction, $F(1, 92) = .01$, $p = .93$, $\eta_p^2 < .001$.



$F(1, 92) = 5.35$, $p < .05$, $\eta_p^2 = .06$

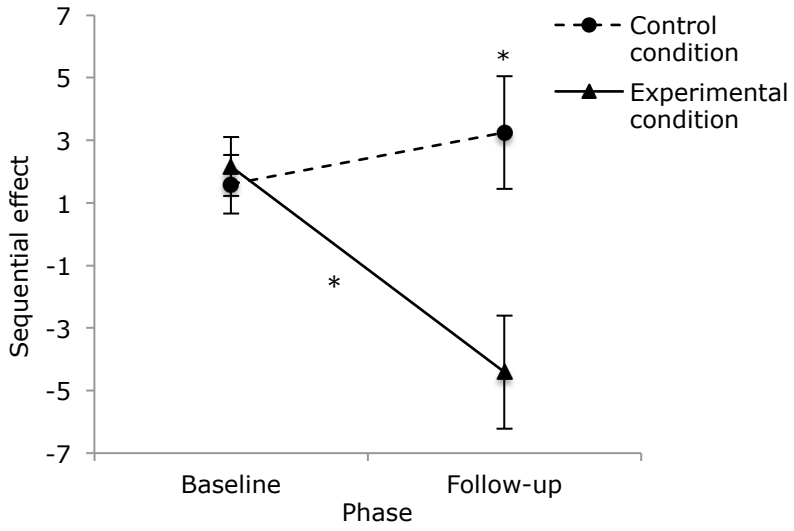
Figure 7.7. Effect of N-1 "yes" responses on responding "yes" in the baseline and follow-up phases of the studies that trained participants to decrease false alarms

7.3.3.2.2 Effect of N-1 "yes" responses on responding "no" (misses and correct rejection). The main effect of phase was not significant, $F(1, 92) = .97$, $p = .33$, $\eta_p^2 = .01$, and neither were the main effect of condition, $F(1, 92) = .58$, $p = .45$, $\eta_p^2 = .01$, and interaction between them, $F(1, 92) = .97$, $p = .33$, $\eta_p^2 = .01$.

7.3.3.2.3 Effect of N-1 vibration on responding "yes" (hits and false alarms). The main effect of phase was not significant, $F(1, 92) = 2.90$, $p = .09$, $\eta_p^2 = .03$. The main effect of condition was significant, $F(1, 92) = 6.17$, $p < .05$, $\eta_p^2 = .06$; the phase by condition interaction was also significant, $F(1, 92) = 8.07$, $p < .01$, $\eta_p^2 = .08$ (see Figure 7.8).

Bonferroni corrected post hoc test indicated that the effect did not differ between control and experimental groups in the baseline phase, mean difference = -

.56, p = .67, 95% CI [-3.20, 2.08]. But after training (i.e. in the follow-up phase), experimental participants had a lower proportion of vibration-present trials preceding a "yes" response than control participants, mean difference = -7.67, p < .01, 95% CI [-12.73, -2.60]. The sequential effect did not differ between the two phases in the control condition, mean difference = -1.65, p = .42, 95% CI [-5.72, 2.42]; in the experimental condition, the effect dropped significantly in the follow-up phase (i.e. after training), mean difference = -6.58, p < .01, 95% CI [-10.64, -2.51].



$F(1, 92) = 8.07$, $p < .01$, $\eta_p^2 = .08$

Figure 7.8. Effect of N-1 vibration on responding "yes" in the baseline and follow-up phases of the control and experimental conditions in the studies that trained participants to decrease false alarms

7.3.3.2.4 Effect of N-1 vibration on responding "no" (misses and correct rejections). The main effect of phase was not significant, $F(1, 92) = .37$, $p = .54$, $\eta_p^2 = .004$; the main effect of condition was not significant, $F(1, 92) = 1.68$, $p = .20$, $\eta_p^2 = .02$; also the interaction between them was not significant, $F(1, 92) = 1.20$, $p = .28$, $\eta_p^2 = .01$.

7.3.3.2.5 Effect of N-1 light on responding "yes" (hits and false alarms). Main effect of phase, $F(1, 92) = .37$, $p = .54$, $\eta_p^2 = .004$, main effect of condition, $F(1, 92) = .43$, $p = .51$, $\eta_p^2 = .005$, and interaction between them, $F(1, 92) = .02$, $p = .88$, $\eta_p^2 < .001$, were not significant.

Table 7.5

Means (and Standard Deviations)[a] of the Sequential Effects of N-1 Events ("Yes" Response, Presence of Vibration, and Presence of Light) on Responding "Yes" and "No" in the Baseline and Follow-up Phases of the Studies that trained participants to decrease false alarms

Sequential effects	Control group		Training group	
	Baseline	Follow-up	Baseline	Follow-up
N-1 "yes" response and the current response was				
"yes" (hits and false alarms)[b]	3.58 (.78)	3.87 (1.15)	3.84 (.93)	4.15 (1.34)
"no" (misses and correct rejections)[c]	3.21 (.73)	3.21 (.83)	3.39 (.79)	3.20 (.67)
N-1 vibration and the current response was				
"yes" (hits and false alarms)	1.60 (5.94)	3.25 (7.36)	2.16 (6.92)	-4.41 (15.88)
"no" (misses and correct rejections)	-1.22 (4.18)	-1.46 (3.68)	-1.04 (4.45)	-.19 (1.38)
N-1 light and the current response was				
"yes" (hits and false alarms)	.57 (5.08)	-.01 (8.33)	-.17 (5.60)	-1.12 (14.44)
"no" (misses and correct rejections)[c]	-.42 (3.92)	-.12 (4.08)	.23 (4.01)	.52 (1.35)

Note. [a] Means and standard deviations are computed from the corrected sequential effects measured in percentage. [b]Square root transformed statistics. [c]Reverse square root transformed statistics.

7.3.3.2.6 Effect of N-1 light on responding "no" (misses and correct rejections). None of the main or interaction effects was significant: main effect of phase, $F(1, 92) = .40$, $p = .53$, $\eta_p^2 = .004$; main effect of condition, $F(1, 92) = 1.35$, $p = .25$, $\eta_p^2 = .02$; interaction between phase and condition, $F(1, 92) = .00$, $p = .99$, $\eta_p^2 < .001$.

7.3.4 Hypothesis 4: SSDT Training Will Change Sequential Effects of N-1 Events (i.e. the "yes" Response, Presence of Vibration, and Presence of Light) on Current False Alarms of High False Alarm Participants

To carry out the preceding analyses (i.e. those captured by hypothesis 3), hits and false alarms were combined to obtain the dependent variable (i.e. the current "yes" response). This was done because the objective was to determine whether training changed sequential effects on the current "yes" and "no" responses of high and low false alarm participants. The present analyses, on the other hand, were carried out only with current false alarms of high false alarm participants. Descriptive statistics on the effects are presented in Table 7.6.

Table 7.6

Means (and Standard Deviations)[a] of the Sequential Effects of N-1 Events (Responding "Yes", Presence of Vibration, and Presence of Light) on the Baseline and Follow-up False Alarms in the Studies That Trained Participants to Decrease False Alarms

N-1 event	Control group		Training group	
	Baseline	Follow-up	Baseline	Follow-up
"Yes" response	3.30 (12.01)	4.57 (11.99)	7.94 (16.26)	10.34 (31.51)
Presence of vibration	3.88 (11.61)	4.98 (9.68)	4.07 (12.46)	3.90 (29.75)
Presence of light	-1.19 (10.16)	-.88 (12.18)	.10 (10.72)	-8.62 (26.44)

Note. [a] Means and standard deviations are computed from the corrected sequential effects measured in percentage.

7.3.4.1 Effect of N-1 "yes" responses. In contrast to our hypothesis, the main effects of phase, $F(1, 78) = .48$, $p = .49$, $\eta_p^2 = .01$, condition, $F(1, 78) = 2.20$, $p = .14$, $\eta_p^2 = .03$, and the interaction between them, $F(1, 78) = .05$, $p = .83$, $\eta_p^2 = .001$, were not significant.

7.3.4.2 Effect of N-1 vibration. Contrary to the hypothesis, none of the effects was significant: main effect of phase, $F(1, 78) = .03$, $p = .87$, $\eta_p^2 < .001$; main

effect of condition, $F(1, 78) = .03$, $p = .88$, $\eta_p^2 < .001$; interaction between phase and condition, $F(1, 78) = .05$, $p = .82$, $\eta_p^2 = .001$.

7.3.4.3 Effect of N-1 light. The main effect of phase was not significant, $F(1, 78) = 2.95$, $p = .09$, $\eta_p^2 = .04$. The main effect of condition was not significant, $F(1, 78) = 1.42$, $p = .24$, $\eta_p^2 = .02$. The interaction between phase and condition was nearly significant, $F(1, 78) = 3.40$, $p = .07$, $\eta_p^2 = .04$.

7.4 Discussion

The aim of the present study was to investigate whether SSDT events in preceding trials affect what response is given in current trials. Data from the SSDT studies reported earlier were processed to determine the sequence of events including responses, vibration, and light that took place prior to the current trial. The present study used information only about the immediately preceding (i.e. N-1) events leading to the current response.

As predicted, the N-1 trials for "yes" responses consisted of a higher proportion of "yes" responses and vibrations than for "no" trials, but no effect was found for the presence of the light. These findings are consistent with the results of previous studies (Podlesek, 2010; Staddon, King, & Lockhead, 1980; Ward & Lockhead, 1971) and suggest that automatic activation underlies sequential effects; previous studies on absolute judgment tasks have called this an assimilation effect (Podlesek, 2010). It is likely that N-1 "yes" responses and vibration activated mental representations related to the sensation of touch which persisted to affect current trials and thus more often produced the "yes" response. This explanation corresponds with Brown's (2004) view that excessive activation and perceptual selection of inappropriate symptom representations underlie the development and maintenance of the illusory phenomena that constitute medically unexplained symptoms. We also found that individuals differed in how strongly and in which direction (i.e. responding "yes" or "no") their current responses were affected by previous events and the effects were approximately normally distributed (as Figure 7.3 indicates).

We also investigated whether individual differences in sequential effects were related to experiencing somatic symptoms as measured by the PHQ-15. It was found

that the effect of N-1 light on current miss responses had a significant positive correlation with PHQ-15 scores. High PHQ-15 participants more often failed to detect vibration in current trials when light was present in preceding trials. This finding should be interpreted with caution, because (i) it accounted for only two per cent (r = .15) of the effect and (ii) PHQ-15 scores were unrelated to the other sequential effects. Nevertheless, the significant correlation conforms well to the idea that patients with medically unexplained symptoms misinterpret benign sensations as illness symptoms (Brown, 2004; Kirmayer & Looper, 2007), which, can be interpreted as their failure to distinguish between the significance of different somatic experiences. Though the light is a neutral stimulus in the SSDT, studies (Katzer et al., 2011; Lloyd et al., 2008; McKenzie et al., 2010; and also see appendices C, N, X, and AA) consistently found that participants misinterpret the sensation of light as that of vibration, prompting them to report the presence of touch when actually it was absent. In case of current miss responses with light present on preceding trials, it might be that participants misattribute the sensation of vibration on the current trial as coming from the light on the preceding trial and therefore respond "no" (i.e. miss the vibration). Also, on some of the trials, participants experience the light and the touch together, which might give a stronger experience of the vibration, making the vibration alone on the current trial seem weaker and thus misguiding the perceptual system to miss the vibration. In the present studies, this perceptual failure to discriminate between sensory stimulations increased, as would be expected, as the reported severity of somatic symptoms increased. Indeed, recalculation of the correlation coefficients between the variables (i.e. effect of N-1 light on misses in current trials and PHQ-15 scores) found a significant relationship only for high false alarm participants [$r(100) = .21$, $p < .05$] but not for their counterparts [i.e. low false alarm participants, $r(105) = .09$, $p = .39$]. A recent study also found that interoceptive accuracy training with heartbeat perception (which aimed to improve objective assessment of bodily sensations) significantly reduced state symptoms of somatoform patients (Schaefer, Egloff, Gerlach, & Witthöft, 2014). However, this probable interpretation of the significant correlation (between the effect of N-1 light

on current misses and PHQ-15) becomes weak if both the presence of vibration and presence of light in previous trials are considered equivalent in their effects on current miss responses, because the correlation coefficient between the effect of N-1 vibration on current misses and PHQ-15 scores (r = .05) was not significant.

An additional finding of the correlational analysis is that there were significant relationships between the sequential effects, specifically between the effects of N-1 response and N-1 vibration (see Table 7.3). In other words, those participants susceptible to the effects of N-1 responses also appear to be susceptible to the N-1 stimulus effects. This suggests that sequential dependency in perceptual performance includes both bottom-up (related to the presence of vibration in N-1 trials) and top-down (related to responses in N-1 trials) processes, which are likely to work together. An idea for future studies is to consider both stimulus and response in N-1 trials to examine interactive effects of both N-1 bottom-up and top-down processes on the current response.

A limitation of the correlation coefficients as obtained in this study is related to the finding that there were two opposite sequential dependencies between successive SSDT events. In other words, as Figure 7.3 demonstrates, the sequential effects were bidirectional. For example, the current "yes" response of most of the participants was preceded by another "yes" response, but for some the preceding response was "no". As we considered both the groups in the same analysis, it is likely that the opposite sequential effects weakened its strength of association with PHQ-15. Future studies, therefore, should consider carrying out correlational analysis separately for these groups.

Findings are mixed with regard to the effects of the SSDT training on sequential effects. Training low false alarm participants to produce more false alarms significantly decreased their responding "no" when preceded by the N-1 "yes" response, whereas training high false alarm participants to produce fewer false alarms significantly decreased their responding "yes" when preceded by the vibration in the N - 1 trial. These changes correspond to the perceptual training that participants went through—low false alarm participants were trained to respond "yes" (which was

expected to decrease their post-training "no" responses) whereas high false alarm participants were trained to respond "no" (which was expected to decrease their post-training "yes" responses). The change in high false alarm participants seems particularly interesting—it suggests that they might experience a particularly pronounced carry-over from experiencing a vibration in the previous trial, which was reduced in the follow-up phase due to the training. It can be argued, in accordance with the explanatory framework of the integrative cognitive model of MUS (Brown, 2004), that the training decreased the activation of touch related schema and thus its carry over to the following response. However, the training did not change other sequential effects, even though some of them significantly changed with time (i.e. between the baseline and follow-up phases) in both the control and experimental conditions. It is interesting that the effect of N-1 "yes" response on responding "yes" in the current trial significantly increased in the follow-up phase (irrespective of condition) in both the studies. This suggests that (extraneous) variables other than the training can also change sequential dependency between the SSDT events. Presumably, they might be related to fatigue or sensory deprivation as explained in Chapter 6 (see Section 6.4).

The SSDT training did not change the effects of N-1 events on false alarms of high false alarm participants in current trials. It might be that the training was not sufficient to change sequential dependency of false alarms. This is unlikely because training in all the SSDT conditioning studies (i.e. Studies 1, 2, 4, and 5) had a strong effect on response bias, which suggests that follow-up false alarms (in comparison to false alarms in the baseline phase) are more likely to be preceded by a N-1 "yes" response in false alarm increasing studies (i.e. Studies 1 and 4) and a N-1 "no" response in false alarm decreasing studies (i.e. Studies 2 and 5). A more probable explanation is that the presence of only a small number of data points for false alarms caused this null effect. In the light present condition of Study 5, for example, high false alarm participants produced between 6 and 36 false alarms in the baseline and between 1 and 33 false alarms in the follow-up phases. In the light absent condition, they produced between 1 and 30 false alarms in the baseline and between 1 and 25

false alarms in the follow-up phases. This suggests that the number of false alarms for some participants was not adequate to detect possible training effects (i.e. they decreased the power of the test). Therefore, a better approach to the analysis would be to include only those participants who have sufficiently large number of false alarms in the baseline (e.g. at least 30 false alarms combining both light present and light absent conditions). This can be tested in future studies.

It should be noted that we have only analyzed baseline and follow-up SSDT data in this study, because sequential effects are more likely to be stable in these phases than in the manipulation phase which involved training. Therefore, we do not know whether the baseline sequential relationships changed in the manipulation phase when experimental participants were conditioned with reward and punishment and whether any such sequential effects differed from control participants. Future studies should include all the phases (or individual blocks) to carry out additional analyses separately for the SSDT response outcomes (i.e. hits, false alarms, misses, and correct rejections) which would broaden our understanding of training effects as well as perceptual and cognitive processes underlying the responses in the SSDT. It would also be interesting to analyze baseline data of the participants who had the control condition in the second session (in Studies 1 and 2) to see whether the changes in sequential effects persisted over time (which was an average of seven days).

In the present study, we only considered two successive trials. Perruchet's paradigm (Perruchet, 1985) as well as previous research suggest that the current response in a psychophysical task might be the result of a sequence of multiple previous trials (Staddon et al., 1980). It is therefore necessary to include more than one preceding trial in the analysis. This would give us a more elaborate picture of whether and how sequential dependency between the SSDT events changes over time.

In the present study, we calculated sequential effects separately for response, presence of vibration and presence of light in the preceding trial. A potential limitation of this approach is that segregating N-1 SSDT events, when actually participants experienced them simultaneously, cannot unravel their interactive influence on the

current response. Future studies should consider combining the effects of the preceding events, although formulating a valid procedure to standardize the events and combine them into a composite measure would be challenging as they have different distributions (as evident in Figure 7.3) and their importance in affecting the current response is not the same (as the findings of *t*-tests indicate). A possible solution would be to carry out multiple regressions of the current yes and no responses with previous events as the predictors. Another issue that future studies should consider is the contribution of response bias (a general tendency to respond "yes" or "no") to sequential effects, which was excluded in the present study by using corrected scores to keep sequential effects from being confounded by response bias. The correlation coefficients between response bias and sequential effects could be used to investigate this possibility.

To the best of our knowledge, this is the first study to examine sequential effects in the SSDT and similar experimental paradigms. It is evident from our studies that current SSDT responses not only depend on experimental manipulations but also on what responses were given or what stimuli were present in previous trials. Individuals differed with regard to these effects and findings provide tentative evidence that some sequential effects on the SSDT might have clinical relevance. Further studies should look deeper into these sequential relationships by including and combining data of more than one previous trial and considering other possibilities to process and analyze data.

CHAPTER EIGHT: GENERAL DISCUSSION

> Whatever the case may be, on a more pedestrian "level," there is no simple identifiable cause and effect to illuminate what exactly is wrong with me, no linear motion from one thing to another, but a number of factors that may or may not play a role in the vagaries of the shaking woman's path.
>
> —Siri Hustvedt, The Shaking Woman or A History of My Nerves, p. 246

The primary aim of this thesis was to investigate whether participants can be trained to change somatosensory misperceptions (so-called false alarms) in the somatosensory signal detection task (SSDT; Lloyd, Mason, Brown, & Poliakoff, 2008). Previous research found that false alarms in this task vary between individuals, are stable over testing sessions (McKenzie et al., 2010), resistant to change (McKenzie et al., 2012; Mirams et al., 2012, 2013), and correlate with severity of somatic symptoms (Brown et al., 2012; Katzer, Oberfeld, Hiller, & Witthöft, 2011). Thus, there are both empirical and theoretical grounds (e.g., Brown, 2004) to regard SSDT false alarms as a laboratory analogue of medically unexplained symptoms (MUS). Finding ways to change the SSDT false alarm rate was considered potentially useful because this might translate into interventions to help MUS patients to deal with their symptom experiences. This inspired our studies. Though our main objective was to investigate the false alarm responses and their relationships with somatic symptoms, the work also has implications for broader topics such as transfer of perceptual training, anomalous experience more generally (i.e. hearing of voices), sequential effects, and different methodological issues (e.g. selecting a suitable study design, designing a viable training protocol, dealing with potential extraneous variables, etc.). These are discussed in this chapter, which is organized into six sections. The first section summarizes the findings of this PhD project, the second and third sections interpret the findings in relation to current models of MUS and previous research on SSDT training, the fourth section describes potential uses of the voice-hearing task paradigm, the fifth section gives directions for future studies, and the final section contains a concluding remark.

8.1 A Summary of the Studies and Their Findings

We have carried out four SSDT studies, developed and validated a voice detection task, and established a method for carrying out sequential analyses on SSDT data. The SSDT studies gave us reliable and conclusive findings that the false alarm rate can be changed with operant conditioning (by rewarding certain response while punishing others). The findings are reliable because we obtained similar results in separate studies testing different groups of participants. Our initial findings were fully replicated — conditioning significantly increased the false alarm rate of low false alarm participants in Studies 1 and 4 (presented in Chapters 2 and 5 respectively) and significantly decreased the false alarm rate of high false alarm participants in Studies 2 and 5 (presented in Chapters 3 and 6 respectively). We regard the SSDT findings as conclusive because the study design was changed considerably before replicating the studies, yet the findings were in accordance with our predictions. Results of the replication studies justified adopting a between-groups design instead of the initial within-subjects design, which seemed to contaminate the baseline data with carry-over effects (although this indicated how persistent the effects of the conditioning could be on the SSDT false alarm rate). Confidence about the effectiveness of conditioning in changing false alarms also derives from the finding that reducing the amount of reward by half (from 10p to 5p) did not reduce the effect. Use of a sham reinforcement and punishment schedule in the control condition provided further evidence to support the training paradigm, as pseudo-conditioning had no effect on SSDT response outcomes. Though operant conditioning changed the SSDT response outcomes (i.e. false alarm rate, hit rate, and response bias), we do not know whether the training also modified the underlying perceptual processes. This is a shortcoming of psychophysical tasks, such as the SSDT as it measures only behavioural responses. To address this limitation, we need to measure neurological activity which would provide more direct evidence for the involvement of perceptual processes in the issue. Future studies therefore should employ techniques to measure brain functioning while conditioning the SSDT.

Though operant conditioning brought about the desired change in SSDT false alarm rates, it was not found to transfer to another somatosensory task (i.e.

spontaneous sensation test). Similarly, transfer of the training to a different modality (as investigated by the voice-hearing task) was not as convincing as the SSDT findings, although the results suggested that the modalities (tactile and auditory) share common properties and with additional training a significant transfer between them might be obtained.

Contrary to previous studies (e.g. Brown et al., 2012; Brown, Brunt, Poliakoff, & Lloyd, 2010; Katzer et al., 2011), we did not find a significant relationship between somatic symptoms (as measured by the PHQ-15) and false alarm rates on the SSDT. However, one of the sequential effects (i.e. effect of N-1 light on misses in current trials) correlated significantly with PHQ-15 scores. Further analysis demonstrated that the relationship was significant only for high false alarm participants (though there were statistical issues, such as bi-directionality in sequential dependency, which need to be addressed to be more certain about the relationship between reporting of symptoms and sequential effects). Brown et al. (2010) and Katzer et al. (2012) found significant positive correlations between SSDT false alarm rates and the tendency to experience pseudoneurological symptoms (as measured by the Somatoform Dissociation Qiestionnaire-20 and Somatoform Symptoms-2 respectively in the studies, which we did not use in our studies). Katzer et al. (2012), however, did not obtain any significant relationship between false alarms and PHQ-15 scores (which agrees with our findings). It would be premature to draw any conclusion regarding the relationship between the variables at this stage. We recruited healthy volunteers who were predominantly university students. Collecting clinically relevant information from non-clinical participants increases the risk of range problem (i.e. when data resides within a restricted range; Katzer et al., 2011), which may reduce the strength of correlation between variables. Some of the participants also took part in our studies during the period of their semester final exams. It is likely that some of their reported symptoms were due to exam-related stress and thus were temporary and unreliable, which might have confounded the correlation. However, it is also possible that the SSDT false alarm rate is too rudimentary to be a reliable correlate of a complex psychological phenomenon such as symptom reporting, which is considered

to be the manifestation of multiple intricate psychological processes involving affective (e.g. negative affect; Thompson, Waltz, Croyle, & Pepper, 2007), cognitive (e.g. negative interpretation of body sensations; Witthöft, Basfeld, Steinhoff, & Gerlach, 2012), perceptual (e.g. increased symptom-focused attention; Witthöft, Gerlach, & Bailer, 2006) and behavioural (e.g. excessive health care utilization; Barsky, Ettner, Horsky, & Bates, 2001) components. It would be worth investigating whether SSDT false alarms are associated with specific components of symptom reporting rather than with the entirety of it.

In the sequential analyses, we found that responses in current SSDT trials were affected both by responses and the presence of the vibration on immediately preceding (N-1) trials, but not by prior presentation of the light. More specifically, participants said "yes" more often on current trials if they responded "yes" or the vibration was present in the immediately preceding trial. This is known as assimilation (Lockhead, 1992) in the literature on sequential dependency. Studies have reported another effect called contrast (Lockhead, 1992), whereby responses in current trials tend to be different from the events that occurred earlier in the sequence. In our study, we only analyzed immediately preceding events (i.e. N-1 responses, vibration, and light). This requires that future studies should examine whether earlier SSDT events (i.e. responses, vibration, and light in N-2, N-3, N-4 trials and so on) affect responses in current trials and whether any such effects are altered by training or have any clinical relevance (e.g. whether they are related to somatization).

Results of sequential analysis also demonstrated that the distribution of sequential effects were approximately normal, suggesting meaningful individual differences in how strongly responses were sequentially dependent. Training and experiences related to performing in lab changed sequential dependencies of both low and high false alarm participants. The training effect, however, was not very strong as some of the sequential relationships remained unchanged.

We conducted secondary analyses on data pertaining to the presence of the light and other variables, such as sleepiness and state anxiety. These were not the focus of this thesis and are therefore presented in appendices, but are discussed briefly below.

Like previous SSDT studies (Brown et al., 2012; Brown, Brunt, Poliakoff, & Lloyd, 2010; Katzer, Oberfeld, Hiller, & Witthöft, 2011; Lloyd, Mason, Brown, & Poliakoff, 2008; Lloyd, McKenzie, Brown, & Poliakoff, 2011; McKenzie, Lloyd, Brown, Plummer, & Poliakoff, 2012; McKenzie, Poliakoff, Brown, & Lloyd, 2010; Mirams, Poliakoff, Brown, & Lloyd, 2010; Treshi-marie Perera, Newport, & McKenzie, 2015), we found that light (see the secondary analyses presented in Appendices C, N, X, and AA) significantly increased hit rate, false alarm rate, and liberal response criterion. Light also increased sensitivity in Studies 2 and 5 (see Appendices N and AA) which is consistent with the findings of McKenzie et al. (2012); Katzer, Oberfeld, Hiller, Gerlach, and Witthöft (2012); Brown et al. (2010); Lloyd et al. (2011); and Mirams et al. (2010). It is interesting to note that light did not increase sensitivity of high false alarm participants in Studies 1 and 4 (see Appendices C and X). Previous studies explained light-induced increases in sensitivity as resulting from the integration of multisensory information (McKenzie et al., 2012; Mirams et al., 2010). This brings up the question of whether multisensory integration is impaired among people who produce many false alarms in the SSDT or experience unusually high number of illusory sensations in general. Correlation studies could be carried out involving the SSDT and other multisensory integration tasks (such as the cross-modal congruency task; Spence, Pavani, & Driver, 2004) to see if there is any relationship between performances on them.

Sleepiness and state anxiety did not affect SSDT outcomes, although there were some indications that participants became more alert in the manipulation phase that involved winning and losing of points (e.g. see Section N.3).

8.2 What Theories/Models Explain the SSDT Findings?

A number of models can account for the SSDT findings. As the focus of this thesis is on the implications of SSDT training for MUS, I shall consider these effects from the perspective of the integrative cognitive model (Brown, 2004), the signal-filtering model (Rief & Barsky, 2005), the somatosensory amplification model (Barsky, 1992), the "off line" body image and "as if" loop model (Damasio, 1994), as well as operant conditioning theory (Skinner, 1953). It is to be noted that healthy individuals

were recruited for the SSDT studies. Their false alarm rates were divided into high and low for the purpose of the studies but this division has no clinical significance according to current knowledge. Nevertheless, they allowed us to test the basic principles, such as experiences and learning that most of the MUS theories regard as important for the development and maintenance of MUS.

According to the signal-filtering model, amplification of sensory signals or reduced functioning of the neuronal filtering system may bring about misperceptions. The integrative cognitive model suggests that felt sensations might be caused by excessive and inappropriate activation of mental representations even in the absence of relevant stimulation. The somatosensory amplification model proposes that individuals differ in perceptual sensitivity, which affects our interpretation of bodily sensations. If sensitivity is increased (i.e. amplified), benign sensations might be perceived as intense and disturbing. Damasio (1994) believes that repeated association between a signal and its corresponding mental representation develops into an "off line" body image, which may cause the signal to be perceived in its absence, that is, "as if" the signal is present. Such perceptual distortions, according to operant conditioning theory, can be strengthened with reinforcement. These theories are considered in relation to the different phases of the SSDT studies.

Baseline SSDT performance can be considered a measure of the participant's usual perceptual processing. According to the integrative cognitive model (Brown, 2004), responses at this stage will have been largely regulated by the primary attentional system (PAS), which uses memory representations to respond automatically to sensory information. With respect to the signal-filtering model (Rief & Barsky, 2005), baseline responses can be regarded as the outcomes of normal functioning of the neural filtering processes.

The manipulation phase was a novel situation for participants because their SSDT responses started to win and lose points (i.e. money). According to the integrative cognitive model, the Secondary Attentional System (SAS) should intervene at this point to bias the PAS toward a particular response schema to ensure winning the maximum points possible. In doing so, it will have instructed the PAS to adopt a

more liberal stance in relation to decisions regarding the vibration in Studies 1 and 4 and a more conservative stance in Studies 2 and 5. This increased both the hit and false alarm rate in Studies 1 and 4 and decreased them in Studies 2 and 5.

In terms of the signal-filtering model, the motivation to win points will have influenced the functioning of the signal-filtering system to either allow more signals to enter the perceptual system in Studies 1 and 4 or fewer in Studies 2 and 5. Although this means signal was more likely to be correctly identified in the former (hence the hit rate going up), it also resulted in noise (either coming from the body, or from the activation of mental representations) sometimes being mistaken for signal, hence the increase in false alarms. In Studies 2 and 5, in contrast, the response criterion became more stringent in this phase. This not only decreased false alarms but also hits (as some of the relevant signals were identified as noise). Though hit rates and false alarm rates changed in all the studies, sensitivity remained unchanged. This is probably false alarms and correct rejections in Studies 1 and 4 and hits and misses in Studies 2 and 5 were not conditioned (i.e. did not win or lose any points) and therefore did not produce any consequences.

In view of the somatosensory amplification model, the experience of reinforcement and punishment with feedback provided information and ideas about what might increase the chance of winning the most points. In Studies 1 and 4, participants learned that detecting signals (vibrations) was rewarding. This experience increased (i.e. amplified) participants' sensitivity to somatic signals, resulting in a surge in perceiving vibration when it was present (i.e. hits increased) and also when it was not present (i.e. false alarms increased, due to misperceiving irrelevant somatic signals as vibration). The circumstances were different in Studies 2 and 5. Here participants learned that rejecting (irrelevant) signals was rewarding. This experience reduced (i.e. minimized) their sensitivity to somatic signals and thereby their hit and false alarm rates.

According to the integrative cognitive model, mental representations (i.e. schemata) develop a lower activation threshold if they remain active for a long period of time. Following this proposition, it is likely that extended use of a liberal decision

criterion in Studies 1 and 4 and a conservative criterion in Studies 2 and 5 will have decreased the threshold of underlying perceptual representation and heightened their activation level. As a result, the PAS will have continued to use these criteria to make perceptual decisions in the follow-up phase, even though there was no benefit in terms of winning or losing points. This explanation parallels Damasio's (1994) propositions that prolonged experience with particular stimulation forms "off-line" body images and "as if" loops in the brain, which subsequently can function independently of sensory information. The same can be argued regarding the functioning of the filtering system or amplification of somatic signals. It is apparent that experiences and learning in the manipulation phase continued to influence sensory filters and sensitivity to somatic sensation and therefore the hit and false alarm rates did not return to their baseline levels.

In case of sequential dependency, it seems that the presence of vibration or responding "yes" in N-1 trials will have increased the activation of touch schema which carried over to produce "yes" responses in the following trials. N-1 vibration and "yes" responses perhaps also influenced the filter system to remain liberal and to amplify somatic signals to produce more "yes" responses. High false alarm participants were trained to restrain the activation of touch schema (or stringent the filter system), which significantly decreased current "yes" responses following N-1 vibration (see Section 7.3.3.1.3). The light on the preceding trial did not have sequential effect because it will have activated a schema irrelevant to touch. However, it is possible that the sequential effects of N-1 light were through its effect on the response ("yes") on a previous trial (i.e. if the light did have an effect it was through increasing the N-1 "yes" responses). This is consistent with the finding that light significantly increased "yes" responses in all the SSDT studies (see Appendices C, N, X, and AA).

8.3 Findings of the Present Studies With Reference to Training in Previous SSDT Studies

My studies supplemented and expanded the findings of the SSDT studies that attempted to train participants to change their false alarm rate. The training protocol

of McKenzie et al. (2012) involved concurrent presentation of supra-threshold tactile and light stimuli. Low association participants received light-only trials three times more frequently than high association participants. High association participants, on the other hand, received bimodal trials three times more frequently than low association participants. The training significantly decreased the false alarm rate of the low association group but it did not increase the false alarm rate of the high association group. The study has some methodological limitations (e.g., incomparable control group) and findings were not consistent with predictions about the effect of manipulating the association between the light and touch stimuli on SSDT false alarm rates. Brown et al (2010) sought to manipulate the activation of memory representations of touch and subsequent reporting of false alarms. To this aim, they presented pictures and associated vibrations. In the ensuing recognition task, they asked participants to identify pictures and to recall and assess the vibration that was paired with it. This procedure did not result in the predicted changes in SSDT false alarm rates, perhaps because the task did not activate memory representations that were sufficiently related to the SSDT stimulus.

The training procedure used by Mirams, Poliakoff, Brown, and Lloyd (2012) was more successful at changing false alarm rates in the SSDT. They used interoceptive and exteroceptive attention tasks with feedback. During the interoceptive attention training, participants attended to their left index fingertip, counted pulse sensations in it for a brief period of time and received feedback about the accuracy of their counting. In the exteroceptive attention training, a haptic perception task was used in which participants reported how many times grating domes were presented vertically or horizontally to the fingertip, while vision of the hand was blocked using a screen (threshold level grating orientation was determined for each participant before the training). It was found that the interoceptive attention task increased the false alarm rate whereas the exteroceptive attention task decreased it. Though the study did not report inferential statistics concerning the changes in the false alarm rate or how efficient the training was, it strongly suggests that manipulation of somatic attention is a potential way to change somatosensory misperceptions. In line with this

possibility, more compelling findings come from the study of Mirams, Poliakoff, Brown, and Lloyd (2013). In this study, experimental participants practiced body-scan meditation over a period of seven days (two sessions in a lab and in-between five sessions at home), whereas control participants listened to stories over the same period of time. It was found that the false alarm rate decreased significantly in the meditation group from time 1 (i.e. the first session in the lab) to time 2 (i.e. the second session in the lab). Though this procedure has problems with regard to controlling extraneous variables (e.g., the majority of the training sessions took place at home and the effectiveness of such training depends largely on the ability of participants to follow instructions and act accordingly), it is a pragmatic paradigm in that it focuses on perception and might be useful for interventions.

Our conditioning paradigm took a different approach to train participants. Rather than depending on participants' perceptual and cognitive abilities, this paradigm aims to modify responses on the SSDT using reinforcement and punishment. The training procedure is very specific, well defined, and depends on experimental manipulations, which is likely to control the effects of confounding variables. The researcher defines when, how, and which responses are to be conditioned. In our studies, these precise and highly controlled manipulations changed SSDT responses as predicted. In addition, it is very reliable—we replicated the findings in separate studies carried out with different groups of participants in different settings (i.e. Studies 4 and 5 were carried out in a lab different from that of Studies 1 and 2) and different study designs and procedures.

8.4 Broader Potential of the Voice-Hearing Task

We have developed and validated the voice-hearing task (see Chapter 4) to study cross-modal transfer of the SSDT training. But it has potential uses in the area of psychosis as a paradigm for studying auditory hallucination. The majority of the existing auditory tasks (see Chapter 4 and Appendix P for a review), used to study hallucination proneness, are composed of meaningful stimuli which increases the risks that responses might become confounded by familiarity (e.g. the tasks of Hoskin, Hunter, & Woodruff, 2014; Merckelbach & van de Ven, 2001; Moseley, Fernyhough, &

Ellison, 2014; Vercammen, de Haan, & Aleman, 2008), some tasks are too long (e.g. the tasks of Barkus, Stirling, Hopkins, McKie, & Lewis, 2007; Galdos et al., 2011; Hoskin et al., 2014), some are restricted in terms of how many responses participants can produce (e.g. the tasks of Barkus et al., 2007; Bentall & Slade, 1985; Hoskin et al., 2014; Vercammen et al., 2008), and some have problems with task structure (e.g. the tasks of Barkus et al., 2007; Bentall & Slade, 1985; Hoffman et al., 1995; Vercammen et al., 2008). Our task, on the other hand, is well constructed, well structured, psychometrically sound (i.e. reliable and valid to study auditory hallucination), flexible, and brief (making it very convenient for using in long studies), which address the limitations of existing auditory tasks.

The structure of the voice-hearing task could be adopted to develop a new version of the SSDT. In its present version, the number of false alarms that one can produce depends on how many trials the SSDT entails. For example, there can be a maximum 40 false alarms in a block of 80 SSDT trials. Thus its sensitivity to individual differences in reporting somatosensory illusions is restricted. Also, participants must respond in every trial, which increases the possibility that some of their hits and false alarms actually are guesses but proper perception. Participants, on the other hand, would have absolute freedom, as they have in the voice-hearing task, to respond to a one-trial open-ended SSDT and there would not be any restriction on when and how many times they perceive vibration. Such a task, therefore, would be more sensitive to and facilitate top-down processes, which are the salient features of the mechanisms proposed to underlie MUS (e.g. see the Integrative Cognitive Model of MUS by Brown, 2004). Moreover, the one-trial paradigm is more efficient than discrete trials. For example, if consistent with the voice-hearing task data, such a task could produce more false alarms in 10 minutes than the SSDT can produce in 30 minutes. The paradigm, however, has the drawback that its data are not suitable to compute signal detection test statistics (i.e. hit rate, false alarm rate, response bias, and sensitivity) as can be done with the present SSDT data. In spite of this limitation, this approach would be very convenient and efficient for perceptual studies aiming to investigate changes in total responses (including hits and false alarms).

In the modified version of the SSDT, the mean and standard deviation of the thresholds obtained from a pilot sample could be used to decide on the range of values for the intensity of vibration in the main task. Like the voice-hearing task, the vibration would be presented randomly in a single continuous trial and participants would be asked to respond whenever they thought they felt a touch. It would be interesting to test participants on both the conventional and proposed new version of the SSDT to see how strongly the response outcomes (i.e. hit and false alarm rates in the SSDT and total hits and false alarms in the new task) correlate both together, and with symptom reporting on the PHQ-15. Training cannot be carried out in this new task in the same way as was done in the manipulation phase of the SSDT (in Studies 1, 2, 4, and 5), but a series of brief open-ended trials (each would be a one-trial task in which participants can give as many responses as they want) can be arranged with feedback including reinforcement and punishment after each trial.

8.5 Future Studies

A number of potential studies have already been proposed in the previous sections. The findings of our studies also pose more questions.

Conditioning the false alarm rates in the present studies did not change sensitivity between the groups. It would be interesting to see whether it is also possible to condition (i.e. train) sensitivity, for example by employing a continuous reinforcement and punishment schedule for all the SSDT response outcomes, that is, by rewarding all the hits and correct rejections and punishing all the false alarms and misses. This should not increase but decrease false alarm rates, as participants become more able to discriminate signal from noise due to their enhanced sensitivity.

In our studies, we only tested healthy participants. Thus, we do not know how patients with somatic symptoms or conversion disorders would respond to conditioning while performing the SSDT (i.e. to what extent reinforcement and punishment would change their false alarm rates and how long that would persist). To answer this, future studies should recruit patient groups along with healthy volunteers.

We have not processed all of the available sequence data (Chapter 7) due to time limitation and to keep the analysis simple, so definitive conclusions regarding training effects on sequential relationships between SSDT events cannot be drawn at this stage (but we have plans to conduct these analyses down the line).

Our formulated operant conditioning paradigm can be adopted to augment some of the training effects observed previously in research with the SSDT. For example, the procedure used by Mirams et al. (2012) could be modified to condition the accuracy of interoceptive and exteroceptive perception of participants (by rewarding certain responses, such as when participants report approximately correct number of pulse sensations, while punishing others, such as when the reported pulse sensations deviate too much from what was actually experienced).

The conditioning procedure designed here can also be used independently of the SSDT to investigate somatic symptoms. For example, it would be instructive to investigate whether operant conditioning can be used to change the perception of (ethically appropriate) experimental pain stimuli, such as laser or electric pulses. In a typical mixed-design study, the experimental group could be conditioned to overestimate or underestimate the intensity of pain (or warmth) of a range of painful laser pulses (the intensity of the pulses would be determined beforehand for each participant according to their pain threshold). In the baseline and follow-up phases they might be presented with the same series of pulses to see if their judgment of pain or warmth had changed due to the training. Control participants would also go through the same three phases but would receive random reinforcement and punishment in the manipulation phase. We could also include additional tasks and variables, such as measures of state somatic symptoms to see if the conditioning had any effect on them. Our operant conditioning paradigm could also be used with other senses, for example, to study auditory, visual, or olfactory perception.

An important finding of our studies is that illusory perception (i.e. false alarms) in different sensory modalities (tactile and auditory as investigated in our studies) were correlated. This suggests that different senses share a common mechanism that underlies false perception. Though the SSDT training did not significantly change

responses in the voice-hearing task, changes in the total number of voices in Study 4 (see Section 5.3.3.2) suggest that, with adequate training (e.g. by increasing the number of training trials), it might be possible to transfer the effect to a different perceptual modality. Further studies need to be carried out including additional senses (e.g. visual) to test these possibilities along with determining their clinical implications. For example, studies on cross-modal illusory experiences might answer the question of why some patients experience MUS in multiple physiological systems (Kirmayer & Robbins, 1991) which are not caused by anxiety, depression, or other psychiatric disorders.

As mentioned previously, integrated use of the SSDT and electrophysiological and brain imaging techniques would answer the question of whether the SSDT training modified the underlying perceptual processes. Such investigations will not only inform us how different areas of the brain function to produce somatosensory distortions (and thus will validate the comparative influence of perception and response bias) but will also indicate how operant conditioning brings about or modifies that illusory perception. In an electroencephalogram (EEG) study, for example, one could see earlier post-stimulus differences in responses to vibrations if the effect of SSDT training is perceptual. It will be interesting to see how different brain areas known to relate to false alarm responses in the SSDT (e.g. medial parietal and medial prefrontal cortex [Lloyd et al., 2011]) and operant conditioning (e.g. striatal and mesial forebrain, anterior cingulate, and thalamus [Knutson et al., 2000]) interact to change illusory tactile experiences and other perceptual distortions.

Interestingly, the SSDT has never been used with children and adolescents perhaps due to the complexity of the task (i.e. children may not understand the procedure and instructions and also it takes considerable time to complete the blocks of trials, which may not be child appropriate) and due to the fact that MUS in children are not like those reported by adults though there are similarities between them (Eminson, 2007). However, the one-trial version of the SSDT (that we have proposed pointing out the advantages of the voice-hearing task) can easily be used with older children and adolescents. It would be revealing to see how children respond to the

SSDT (and also to the voice-hearing task) and whether their performance would relate to symptom reporting. Testing both children and adults on the SSDT would suggest whether somatosensory distortions are modulated by age (and thus by experiences) or are universal phenomena found in all age groups.

There are a number of potential clinical uses for our training paradigm. For example, it might be integrated into a broader intervention programme to train patients to distinguish between real and illusory somatosensation and thus to reduce somatic false alarms. Since the training depends less on cognitive and attentional resources, it might be useful in cases where MUS patients are incapable of complying with cognitively demanding psychotherapy. The training protocol might prove useful for reducing illusory perceptions in other sensory modalities too. Different versions of the training can be tried out (e.g. rewarding and punishing all the response outcomes instead of selected ones) to determine their relative effectiveness. The training paradigm could also be effectively integrated into wearable tech (e.g. health-wear, smart watch etc.) which are capable of providing real-time feedback about bodily conditions, such as heart rate, breathing, perspiration, temperature, muscle activity, etc. Conditioning the subjective experience of somatic symptoms based on objective feedback from physiological measures might help patients to deal with MUS better. Further research is needed to examine these possibilities.

8.6 Concluding Remarks: Characterizing Response Outcomes on the SSDT

In Chapter 1 (see Figure 1.3), we proposed a model to account for the SSDT response outcomes. We started asking the question of whether false alarm rates on the SSDT could be changed with training and whether that would transfer to other perceptual experiences. Our studies (Studies 1, 2, 4, and 5 as presented in Chapters 2, 3, 5, and 6 respectively) provided strong evidence that operant conditioning can change the SSDT false alarm rate. There was some indication that the training transferred to the voice-hearing task responses (Studies 3, 4, and 5 as presented in Chapters 4, 5, and 6 respectively), although it was not strong enough to be statistically significant. Sequential analysis (Study 6 as presented in Chapter 7) indicated that responses in current SSDT trials were affected by N-1 (i.e. immediately

preceding) responses and vibration. Severity of somatic symptoms had significant positive correlation with the effect of N-1 light on misses in current trials, though any such relationship was not found for the SSDT false alarm rates. With these findings in mind, we have characterized the model on the SSDT response outcomes as presented in Figure 8.1 (page 216). In sum, our studies strongly suggest that illusory perceptual experiences are trainable which might have important implications for the treatment of MUS patients.

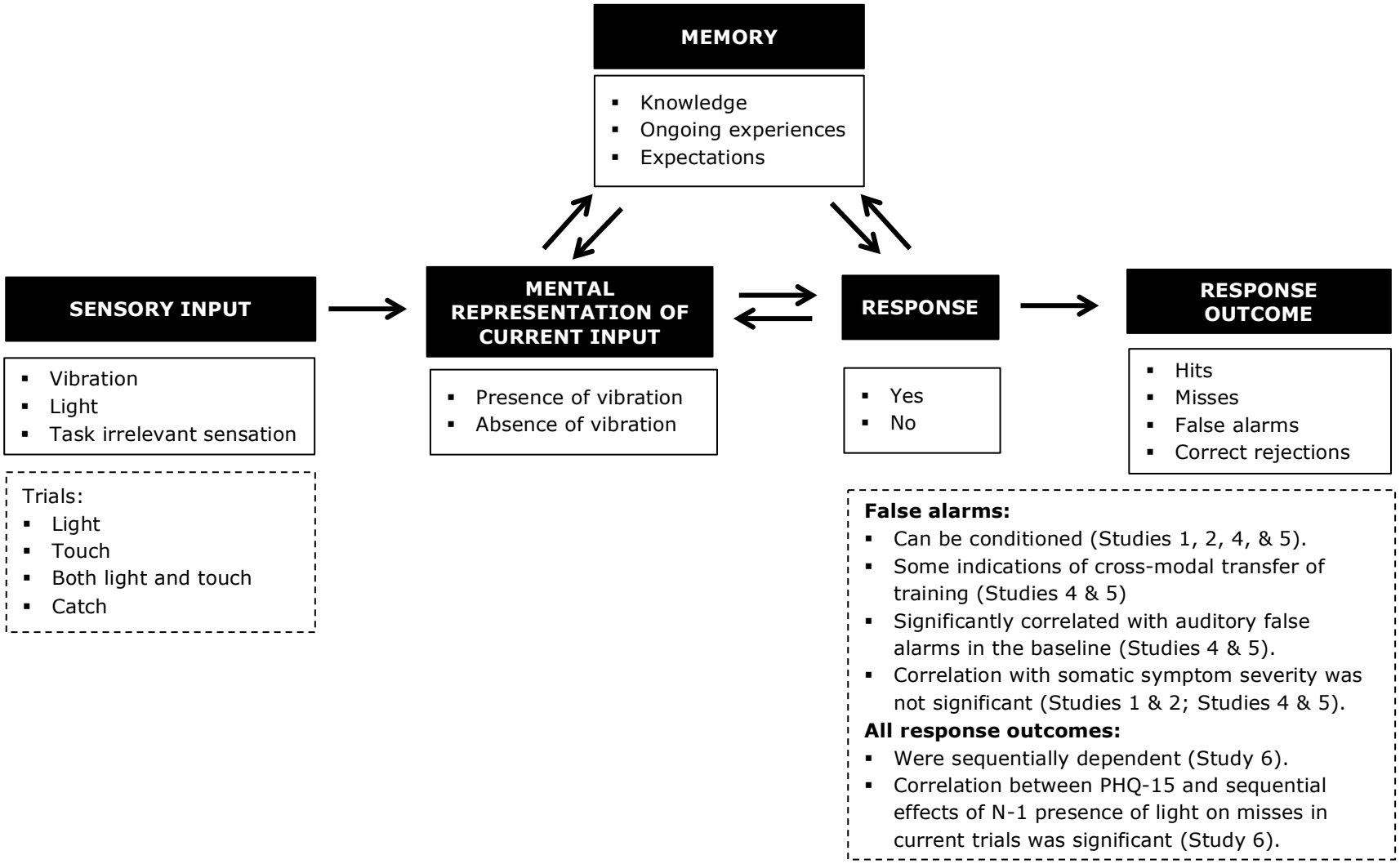


Figure 8.1. SSDT events and underlying cognitive and perceptual processes. Findings presented in the right doted box characterize the response outcomes.

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Appendix A: Summary of the SSDT studies

Study	Design/Procedure	Participants	Results
Lloyd, Mason, Brown, and Poliakoff (2008)	• 50% tactile threshold was determined in blocks of 13 trials with 10 vibration-present trials and 3 vibration-absent trials in each block. • The SSDT had two blocks of 80 trials.	• 19 right-handed participants (10 female). • 21 to 34 years of age (mean = 24.6, *SD* = 2.54).	• Light increased the hit rate. • Light increased the false alarm rate. • Light did not change sensitivity. • Liberal response criterion was found in light present condition.
McKenzie, Poliakoff, Brown, and Lloyd (2010)	**Experiment 1:** • It was a mixed design with light (present vs. absent) x cue modality (visual vs. auditory) x session (session 1 vs. session 2) as within-subjects factors and modality running order was a between-subjects factor. • 50% threshold for the detection of tactile stimulation was determined using staircase method in a series of blocks, each containing 13 trials (10 vibration-present and 3 vibration-absent trials). Some of the blocks were visually cued and others were auditorily cued. • There were two experimental sessions with a gap of seven days between them. • There were four blocks of 80 SSDT trials in each block: two blocks with visual cues and two blocks with auditory cues. • Threshold was determined again after the first two blocks of the SSDT. • Participants responded either "yes" or "no" after every trial in the threshold phase and the SSDT. **Experiment 2:** • It was a mixed design with light (present vs. absent) x cue modality (visual vs. tactile) x session (session 1 vs. session 2 vs. session 3) as within-subjects factors and modality running order was a between-subjects factor. • Threshold task was same as the previous experiment. • An orange LED and a bone conductor fixed to the first dorsal interosseous muscle of the left hand replaced the visual and auditory cue stimuli.	**Experiment 1:** • 18 right handed (four males) • 22-32 years of age (mean = 26.5, *SD* = 2.96) **Experiment 2:** • 18 right handed (five males) • 19-35 years of age (mean = 25.5, *SD* = 4.9)	**Experiment 1:** • Light increased sensitivity. hit rate. liberal (yes) responses. false alarm rate. • Cue modality (visual vs. auditory) did not affect any of the SSDT response outcomes. • The main effect of session was not significant. **Test-retest correlation between the SSDT parameters:** • False alarm rates correlated significantly both in light-preset and light-absent conditions. • Response bias correlated significantly both in light-preset and light-absent conditions. • Hit rates correlated significantly in light-absent conditions. • Sensitivity did not correlate. **Conclusion:** • Significant correlation coefficient between false alarms in two sessions was explained as stable, trait-like characteristic of false alarms. • False alarms seemed to be more influenced by response criterion than threshold level or tactile sensitivity. **Experiment 1:** • The main effect of cue modality was not significant. • Light increased hit rate, false alarm rate, and liberal response bias.

Study	Design/Procedure	Participants	Results
	• There were three session: one week gap between session 1 and 2, four weeks gap between session 1 and 3. • Four blocks of 80 SSDT trials in each session—two blocks with visual cue and two blocks with tactile cue.		• Sensitivity was not affected by light • The main effect of session was not significant. **Test-retest correlation between the SSDT parameters:** • Hit rates were correlated between the sessions. • Bias was correlated between the sessions. • Both light-present and light-absent false alarms were correlated between the sessions.
Katzer, Oberfeld, Hiller, and Witthöft (2011)	**Thresholding**: • Modified the thresholding procedure used in the previous SSDT studies. • Instead of a single-interval, "yes-no" task, a two-alternative forced choice task was used. • Transformed up-down adaptive procedure was used to determine tactile threshold of each individual. • Threshold was defined as the intensity of vibration correctly identified in 70.7% trials. • Acoustic start cue was used to signal observation intervals. • There were three blocks: practice blocks (40 trials), first measurement of tactile threshold (eight reversals), second measurement block (eight reversals). **SSDT** • Four blocks of 40 trials • Tactile pulse was delivered to the dominant hand. **Finger pulse interoceptive task** • Participants reported if they felt finger pulse in general during the experiment on an ordinal scale ranging from 0 (*never*) to 2 (*all the time*). **Questionnaires used:** • The PHQ-15 • The Whitley Index measured health anxiety. • The Multidimensional Inventory of Hypochondriacal Traits assessed cognitive, behavioural, affective, and perceptual aspects of health anxiety.	• 67 participants (14 males), mean age = 23.2, *SD* = 4.8.	**Hit rate** • Light increased the hit rate. **False alarm rate** • Light increased the false alarm rate. **Response bias** • Liberal response bias in light present trials. **Correlational analysis** **False alarms:** • The PHQ-15 had significant positive relationship with the false alarm rate (averaged across light-present and light-absent trials). • Interoceptive perception of finger pulse did not correlate with false alarms. • Health anxiety was correlated with false alarms when controlled for trait anxiety. **Response bias:** • Significant negative correlation with PHQ-15. Negative value of bias indicates liberal response criterion. • Negative correlation with total health anxiety score (also with the subscale scores representing health anxiety and health beliefs and complaints). • Affective scale of MIHT had significant negative correlation. **Tactile sensitivity:** • Significant positive correlation with trait anxiety. **Tactile perception threshold:** • Reliable and stable.

Study	Design/Procedure	Participants	Results
	• The State Trait Anxiety Inventory to measure trait anxiety.		• Not related with measures of MUS or health anxiety. **Interoceptive finger pulse perception:** • Significant positive correlation with the PHQ-15. • Significant positive correlation with WI total score (health anxiety measure), WI anxiety scale, and WI somatic score. **Conclusion:** • Replicated bias and sensitivity related findings of previous SSDT studies. • Findings support the use of the SSDT to study somatoform disorders in laboratory settings.
Perera, Newport, and McKenzie (2015)	• A reality device called the MIRAGE system was used. Participants see real-time video of their own had in its actual location. • Three illusory conditions were created: stretched finger, shrunken finger, and detached finger. • A veridical baseline condition. • Staircase procedure was used to determine the 50% threshold of each participant in detecting vibration. • Four blocks of 80 trials; a block for a condition. 20 trials for each of the stimulus conditions. Two response options after a trial: "yes" and "no". • Task sequence: Acclimatization questionnaire (measured sense of ownership of the hand seen through the MIRAGE in its actual location prior to the illusion) -> first block of the SSDT -> one of the three illusory conditions in a counterbalanced order -> answering the ownership questionnaire -> second block of the SSDT -> finger was brought back to its original length/appearance -> Next condition started • No feedback was given on performance.	• 31 right-handed individuals (10 male). • 18-26 years of age	**Hit rate:** • Light increased hit rate. • Hit rate was significantly higher in the stretched finger condition compared to the veridical baseline condition. • Hit rate was significantly higher in the shrunken finger condition compared to the veridical baseline condition. **False alarm rate:** • Light increased the false alarm rate. • The false alarm rate was significantly lower in the detached finger condition compared to the veridical baseline condition. It has been explained stating that participants probably focused more on the detached tip of the finger (not the whole finger) and this reduced tactile noise (internal bodily sensation), which are usually confused with tactile vibration producing false alarms. **Sensitivity:** • Sensitivity was significantly higher in the shrunken finger condition compared to the veridical baseline condition. **Response criterion:** • Participants more often reported the presence of vibration when light was present. • Participants reported the vibration more often in the stretched finger condition compared to the veridical baseline condition.

Study	Design/Procedure	Participants	Results
			• Participants reported the vibration less often in the detached finger condition compared to the veridical baseline condition. **Summary:** • Both shrinking and stretching finger illusions improved detection of touch. The previous effect was explained resulting from sensitivity and later from bias (i.e. from different underlying mechanisms). • The findings remained the same when controlled for trait negative affect (as measured by the trait anxiety scale of the state-trait anxiety inventory or STAIT-T) and somatosensory amplification (as measured by the somatosensory amplification scale).
Brown et al. (2012)	**Study 1** • Questionnaires used: PHQ-15 measured somatization, 9-item Patient Health Questionnaire (PHQ-9) assessed depression, STAIT-T measured trait anxiety, STAIT-S measured current anxiety. **Thresholding:** • 50% threshold (defined as the vibration intensity that the participant could detect between 40% to 60% of the trials) for tactile stimulus was determined for each of the participants. • There were 10 stimulus trials and 3 empty trials in each block of trials during the thresholding. • Participants responded "yes" and "no" to indicate if they had felt any vibration. • Vibration detection threshold was carried out again after the first two experimental blocks. **SSDT:** • Four blocks of 80 trials. • Four response options after a trial: "definitely yes", "maybe yes", "definitely no", "maybe no". **Study 2** **Questionnaires used:**	**Study 1** • 35 right handed university students and staff (25 female participants) • Mean age 24 (*SD* = 3.59) **Study 2** • 55 patients • Medically unexplained patients = 30 (17 females; mean age = 38.8, *SD* = 8.19). • Medically explained patients = 25 (10 females; mean age = 38.2, *SD* = 9.04).	**Study 1** **Hit rate:** • Light increased hit rate. **False alarm (FA) rate:** • Light increased false alarm rate. **Response criterion:** • Lower in light present condition (participants more likely responded "yes" when light was present). **Sensitivity** • Did not differ between the light conditions. **PHQ-15 and the FA rate:** • Significant correlation between the two in light-present condition. • Correlation was not significant in light absent condition. • The FA rate in light present condition was a significant predictor of PHQ-15. **Study 2** **Hit rate:** • Light increased hit rate. **Sensitivity:** • Light increased sensitivity.

Study	Design/Procedure	Participants	Results
	• Hospital Anxiety and Depression Scale (HADS) to measure anxiety and depression. • The short form of the Health Anxiety Inventory (HAI) to measure hypochondriacal worry. • The SSAS measured the tendency to experience physical sensations as aversive and interpret them as potential indication of illness. • PHQ-15 to measure somatization. • The SSDT procedure was the same as Study 1.		**Response criterion:** • Lower in light present condition. **FA rate** • Did not differ between the light conditions. • There was no significant difference in false alarm rates between the groups. **PHQ-15 and FA rate:** • Significant positive correlation between the two both in light-present and light absent conditions. • The FA rate in light absent condition (but not in light present condition) was a significant predictor of PHQ-15. **Conclusion:** • SSDT false alarms were related to symptom reporting (i.e. functional somatization). • SSDT false alarms were not related to trait anxiety, hypochondriasis, somatosensory amplification, or the tendency to express psychopathology in the form of somatic symptoms.
McKenzie, Lloyd, Brown, Plummer, and Poliakoff (2012)	**Study 1** **Thresholding**: • 50% threshold (defined as the vibration intensity that a participant could detect between 40% to 60% of the trials in three consecutive blocks) for tactile stimulus was determined for all the participants. • There were 10 stimulus trials and 3 empty trials in each block of trials during the thresholding. • Participants responded yes or no to indicate if they had felt any vibration. **SSDT:** • Four blocks of 80 trials • Blocks two and three: 20 touch trials, 20 light only trials, 20 both light and touch trials, 20 catch trials. • Blocks one and four: 40 touch trials, 20 light only trials, 20 catch trials. • Trials were presented randomly.	**Study 1** • 22 right-handed participants (4 males) • 18 to 31 years of age (mean = 21.1 years; *SD* = 4.41) **Study 2** **Experimental group:** • 50 right-handed participants (5males). • 18 to 35 year of age (mean age = 21.8, *SD* = 3.55) • Participants were	**Study 1** **False alarm rate (all four blocks)** • Significantly increased in light- present condition. • Main effect of block was not significant. • Light x block interaction was not significant. • Participants were more confident (i.e. "definite") about their response when light was present. **Effect of light in Blocks 2 & 3:** • The hit rate was significantly higher. • Tactile sensitivity was significantly higher. • Response bias was significantly lower. **Performance on light-absent trials in all four blocks:** • Hit rate in Block 4 was significantly lower than all other blocks. • Sensitivity in Block 4 was significantly lower than Blocks 2 and 3, but not Block 1. • Significantly higher bias in Block 4 than the others.

Study	Design/Procedure	Participants	Results
	• Four response options after a trial. **Study 2** • Task sequence: Training -> Thresholding -> Two blocks of the experimental task (SSDT) -> Thresholding -> Two blocks of the experimental task (SSDT) **Training:** • 2 blocks of 80 trials • Supra-threshold tactile stimuli were used • Feedback was provided on every trial. • Low association group received the light alone trials three times more than the high groups. • High association group received the bimodal trials three times more than the high groups. • Both groups received equal number of tactile pulses. **SSDT** • 4 blocks of 80 trials • Threshold was determined again after the first two blocks. • At the end, participants were asked if the proportion of bimodal presentations differed between the training and SSDT tasks.	assigned randomly to low and high association groups. **Control group:** • 25 participants (9 males) • 20 to 35 years of age (mean age = 25.8, *SD* = 4.63).	• Fatigue or change in threshold seemed to affect performance (hit rate) in Block 4. **Conclusion:** • False alarms in light present trials are not due to any association or illusory correlation experienced or learned while performing in the experiment. • Authors suggested that false alarms in light present trials result from automatic association between light and vibration due to their close proximity in space. This association probably is cause by strategic encoding processes that are developed through our everyday experiences involving multisensory events. **Study 2** • Awareness of the difference between the training and SSDT tasks (used as a covariate) in terms of bimodal presentation did not affect the dependent variables. **Thresholding** • There was no significant difference between the two groups with regard to how many blocks they required to reach the 50% threshold, either at the start or after the first two SSDT blocks. • The intensity of the threshold level vibration did not differ between the groups at either time point. **False alarm rate** • Light increased false alarm responses in both the groups. • The low association group produced fewer false alarms (in both light present and absent conditions) than the high association group. • Low group made significantly fewer false alarms than the control group. • There was no significant difference between the control and high association groups. • Percentages of false alarms in light-present and light-absent conditions were highly correlated for both the groups.

Study	Design/Procedure	Participants	Results
			Hit rate • Light increased the hit rate in both the groups. • Light related increase in the hit rate was not correlated with light related increase in the false alarm rate. **Sensitivity** • Light increased sensitivity to touch. • Sensitivity of low association group was significantly higher than high association group. **Bias** • In both the groups, light increased the tendency to report the presence of touch than when it was absent. **Confidence rating** • For both the groups light increased confidence about hits and false alarms. • Presence of light decreased confidence in misses and correct rejections. • The low association group was more confident about correct rejections than the high association group. **Conclusion:** • The association training decreased false alarm rate of low association group relative to both the control and high association groups. • The training did not increase the false alarm rate in high association group. • The authors suggested that the association between light and touch was established through our life-long experience and perhaps was already at ceiling and therefore the training was not effective in increasing the false alarm rate. • Integration of visual and tactile modality (i.e. multimodal integration) influences SSDT performance, not just bias. • Effect of light in the absence of tactile stimulus is not the same when tactile stimulus is present. • Authors concluded from the confidence rating that false alarms were not merely guessing, rather they correspond to subjective experience.

Study	Design/Procedure	Participants	Results
			• Authors explained the null effect of training on hit rates suggesting that enhancement in perceptual experience (i.e. cross-modal enhancement) by the simultaneous presentation of bimodal stimuli (i.e. light and touch) took place at an early processing stage, rather than being affected by later decisional processes.
Katzer, Oberfeld, Hiller, Gerlach, and Witthöft (2012)	**Thresholding**: • Dominant hand was used to detect tactile detection threshold. • Thresholding procedure was similar to that in Katzer, Oberfeld, Hiller, and Witthöft (2011). • Practice blocks included 50 trials. • Threshold was determined before and after the SSDT phase. **The SSDT**: • Same as that described in Katzer, Oberfeld, Hiller, and Witthöft (2011). • The four blocks were divided into two test halves. **Questionnaires**: • PHQ-15. • Somatoform Symptoms–2 (SOMS-2) measured medically unexplained somatic complaints during the last two years. • Whitley Index to measure health anxiety. • State-Trait Anxiety Inventory (STAI) to measure trait anxiety. • The Beck Depression Inventory-II (BDI-II) assessed severity of depressive symptoms.	• 65 participants (18 to 65 years of age) • 33 SFD patients (24 women, mean age = 43.42 years, *SD* = 9.87) • 32 healthy individuals (22 women, mean age = 41.72 years, *SD* = 11.53).	**Response bias**: • Light (present vs. absent) x group (SFD vs. control participants) x SSDT phase (first half vs. second half) was significant. • SFD group was more liberal than the control group in the first half (post hoc test revealed the effect in the first block, not in the second block) of the SSDT trials in light absent condition. **Tactile sensitivity**: • Light increased tactile sensitivity. **False alarms**: • Occurred significantly more in the first half than in the second half of the SSDT trials. • Light did not change the false alarm rate. • There was no group difference (SFD vs. control participants). **Tactile detection threshold**: • Determined reliably in both the groups. • SFD group had significantly lower threshold that the control group. • Increased significantly in the second part of the SSDT. **Correlational analysis**: • SOMS-2 subscale that assesses pseudoneurological symptoms had significant correlation with false alarms, light modulated false alarms, and light modulated bias in the SFD group but not with the control group. • Threshold of SDF group had significant positive correlations with the PHQ-15 score, BDI-II scores, and Whitley Index scores (that measures health anxiety) • Difference score of sensitivity (increase in sensitivity

Study	Design/Procedure	Participants	Results
			caused by light) had significantly negative correlation with SOMS-2 total score and SOMS-2 vegetative scores. • PHQ-15 did not have significant correlation with false alarms.
Brown, Brunt, Poliakoff, and Lloyd (2010)	• A 2 (low vs. high SDQ-20 groups) x 2 (minimal recall vs. maximum recall groups) x 2 (light present vs. light absent) mixed design was used. **Questionnaires:** • The somatoform dissociation questionnaire -20 (SDQ-20) measured the tendency to experience pseudoneurological symptoms. • The trait scale from the State-Trait Anxiety Inventory. • The Somatosensory Amplification Scale (SSAS) to measure the tendency to notice and experience ambiguous sensory events as unpleasant. • Depression subscale of the Depression Anxiety Stress Scale (DASS-21). **SSDT** • Two 80 trials block. **Memory task** • Two phases: training and recall • Training phase was identical for all the participants. In this phase, 18 pictures were presented twice to look at. • Nine of the pictures were accompanied by nine different vibrations and nine were presented alone. • 12 pictures were presented during the recall phase. • Maximal recall group were presented 75% (9 out of 12) of the same pictures used during the training phase. • Minimal recall group were presented 25% (3 out of 12) of the same pictures used during the training phase. • When a picture was identified as accompanied by vibration, participants were asked to recall and assess the vibration. • When a picture was identified as not being accompanied by vibration, participants were asked to assess the picture.	• 40 low SDQ and 40 high SDQ participants without a history of pseudoneurological symptoms. • 19 male (mean age =20.61, *SD* = 3.96).	**Comparability of groups:** • High SDQ-20 participants scored higher on all of the questionnaires than the low SDQ-20 participants. **Tactile sensitivity**: • Light increased tactile sensitivity. **Response criterion**: • Light increased liberal response. • Maximal recall group had liberal response criterion (more likely to respond "yes"). • High SDQ-20 group had liberal response criterion when covariates (depression, trait anxiety, and somatosensory amplification) were controlled. **Hit rate** • Light increased the hit rate. **False alarm rate** • Light increased the FA rate. • When covariates (depression, trait anxiety, and somatosensory amplification) were controlled, high SDQ-20 participants produced more false alarms than the low SDQ-20 participants. **Training effect** • The false alarm rate did not differ between the high and low association groups. **Conclusion:** 1. Results suggest an association between illusory touch and experiencing pseudoneurological symptoms (i.e., somatoform dissociation) in nonclinical participants. 2. Interoceptive perception of unrelated bodily sensations (such as heartbeats) cannot account for false alarms. 3. False alarms represent distortions in somatosensory awareness, not just response bias. 4. Conscious recollection of vibrotactile events has little or

Study	Design/Procedure	Participants	Results
	• This recall process was expected to activate vibration-related memory representations. • Participants of each SDQ groups were randomly allocated to maximal recall and minimal recall groups.		no impact on the tendency to experience illusory touch.
Lloyd, McKenzie, Brown, and Poliakoff (2011)	• This study used fMRI to investigate neural correlates of false alarms. • Participants could observe their non-dominant hand and instructions on a projection screen looking at a mirror placed on top of the head-cage of the fMRI scanner. • The thresholding procedure was the same as that used by Lloyd, Mason, Brown, and Poliakoff (2008). fMRI data was not collected during the thresholding task. • There were two blocks of 80 SSDT trials.	• 18 right-handed participants (10 female). • 20-40 years of age (mean = 27, *SD* = 4.8).	**Behavioural results** • Light increased the hit rate. • Light increased sensitivity. • Light produced more liberal responses. • No difference in the false alarm rates between light-present and light-absent conditions. **fMRI results** • False alarm vs. correct rejection in the presence of light: activation was seen in bilateral posterior cingulate cortex extending into primary and secondary visual cortex. • False alarm vs. correct rejection in the absence of light: activation in (a) bilateral medial frontal cortex extending into paracingulate cortex and (b) bilateral precuneus extending into bilateral posterior cingulate cortex. • Collapsing light present and light absent data revealed activation in medial frontal cortex in response to false alarms to touch. **Conclusion:** • False alarms in both light-present and light-absent trials activated a network of regions that involves medial parietal and medial prefrontal cortex including precuneus, posterior cingulate and paracingulate cortex. • Top-down regions are responsible for somatic misperception.
Mirams, Poliakoff, Brown, and Lloyd (2010)	• A mixed design was used with light (present vs. absent) x tactile pulse (present vs. absent) x vision condition (non-informative vision of the hand vs. no vision of the hand) as within-subjects factors and response key order and condition order (vision or no-vision condition first) were the between-subjects factors. • 50% threshold for detecting vibration was determined using staircase procedure in a series of blocks, each	• 37 right handed (6 males) • 19-48 years of age (mean = 22, *SD* = 5.86)	**False alarms:** • Viewing the hand significantly increased the false alarm rate. • In vision condition, light (when compared with no-light trials) significantly increased the false alarm rate. • In light trials, vision (when compared with no-vision) significantly increased the false alarm rate. • The main effects of response key order and condition

Study	Design/Procedure	Participants	Results
	containing 10 vibration-present trials and 3 vibration-absent trials. • "Yes"/"no" response options were used in the threshold trials and "definitely yes", "maybe yes", "maybe no", "definitely no" response options were used in the SSDT. • Participants could see their hand in the non-informative vision condition. • In the no-vision condition, the left arm, hand, and finger were covered with a black cape, but the LED was visible. • A luminous orange dot was placed on the nail of the finger attached to the bone conductor (or on the black cape). • SSDT contained two blocks of 80 trials for each vision condition.		order were not significant. **Hit rates** • Light increased hit rates. • Main effect of vision condition was not significant. **Sensitivity** • Light increased sensitivity. • Main effect of vision condition was not significant. **Response criterion** • Light increase liberal response criterion. • Main effect of response key order was significant. Participants were more likely to respond 'yes' when the key "1" meant "definitely yes" than when it meant "definitely no". • Main effect of vision condition was not significant. **Certainty ratings:** • Participants were more certain about hits than false alarms. • Light increased certainly ratings. • Vision did not affect certainty rating.
Mirams, Poliakoff, Brown, and Lloyd (2012)	**Experiment 1:** • A repeated measures design was used where heartbeat perception (HBP task vs. no HBP task), and SSDT light condition (present vs. absent) were the within-subjects variable. • There was gap of seven days between the sessions. • 50% threshold for detecting vibration was determined using staircase procedure in a series of blocks, each containing 10 vibration-present trials and 3 vibration-absent trials. • There were two blocks of 80 SSDT trials. • The HBP task was altered in four ways: (i) participants were instructed to attended to their left index fingertip and count pulse sensation in it, (ii) a pulse monitor was used to apply pressure to the fingertip so that pulses were more noticeable, (iii) performance feedback was given after each interval, and (iv) the task was repeated	**Experiment 1:** • 37 right-handed participants (30 female) • Aged 19 to 48 years (mean = 21.97 years, *SD* = 4). **Experiment 2:** • 39 right-handed participants (22 female) • Aged 19 to 45 years (mean=23.94 years, *SD*=5.28)	**Experiment 1** • Light produced liberal response in both with or without HBP task. • In light-present trials, the two HBP conditions did not differentially affect the response criterion. • In light-absent trials, HBP task (in comparison to no HBP task) brought about more "yes" responses. • Light's effect on response criterion reduced after performing in the HBP task. • The HBP task did not affect confidence about hits and false alarm responses. • Light and HBP task and their interaction did not affect sensitivity. **Experiment 2** Sensitivity: • Main effect of the grating orientation task was not significant.

Study	Design/Procedure	Participants	Results
	over six intervals. • ECG was recorded to count the number of heartbeats that occurred during each interval. • Time duration that was used for the intervals: 25, 35, and 45 seconds. **Experiment 2:** • A repeated measures design was used where grating orientation (before vs. after the grating orientation task), and SSDT light condition (present vs. absent) were the within-subjects variable. • One experimental session **The grating orientation task:** • 10 plastic grating domes with grooves of a different width were used. • Threshold for the task was defined as the grating width that the participant could correctly identified the orientation (vertical or horizontal presentation) 80% of the time. • The threshold level grating orientation was used in the counting task. Participants kept a mental count of how many times the grating domes were presented vertically or horizontally. They were given accuracy feedback. • During the grating orientation task a wooded screen prevented vision of the hand.		• Grating orientation x light was not significant. • Light increased sensitivity. Confidence: • The grating orientation task did not affect confidence for hits and false alarm responses. Response criterion: • The grating orientation task made the response criterion more stringent (resulting in fewer yes response). • Light brought about more liberal response criterion.
Mirams, Poliakoff, Brown, and Lloyd (2013)	• Participants were randomly allocated to control and experimental groups. • There were two sessions in both the control and experimental conditions with a gap of seven days in between them. • In the first session, control group performed in the SSDT followed by listening to stories for 15 minutes. Experimental group did 15-minute body-scan meditation after the SSDT. • In the second session, control group listened to stories for 15 minutes followed by responding to the SSDT trials. Experimental group did 15-minute body-scan	• 62 right-handed participants (6 male). • Mean age = 19.21 years, *SD* = .75.	• Both control and experimental groups reported a reduction in attentional control and mindfulness over time as measured with the questionnaires. • Trait mindfulness had significant negative correlation with the PHQ-15 score at session 1. **Sensitivity:** • Time x group interaction was significant: sensitivity of the experimental group was significantly higher in the second session (than in the first session). **False alarm rate:** • For the meditation group, the increase in sensitivity in time 2 was due to significant decrease in false alarm

Study	Design/Procedure	Participants	Results
	meditation followed by the SSDT. • In between the two sessions, control participants listened to stories for 15 minutes everyday (days 2-6). During this time, experimental participants practiced body-focused meditation for 15 minutes. • 50% threshold for detecting vibration was determined using staircase procedure in a series of blocks, each containing 10 vibration-present trials and 3 vibration-absent trials. • There were two blocks of 80 trials in the SSDT. Half of the participants were instructed to press keyboard buttons labeled 1 for *definitely yes*, 2 for *maybe yes*, 3 for *maybe no*, or 4 for *definitely no*. The other half received the reverse instructions (i.e., 1 for *definitely no*, 2 for *maybe no* etc.). **Questionnaires:** • The Mindful Attention Awareness Scale (MAAS) measured trait mindfulness. • The observe and act aware subscales of the Five Facets of Mindfulness Questionnaire (FFMQ) measured the effect of the meditation intervention on mindfulness. • The Attentional Control Scale (ACS) measured perceived attentional control. • The PHQ-15 measured severity of physical symptoms experienced over the previous week. • State Trait Anxiety Inventory (STAI) to measure state and trait anxiety.		from time 1 to time 2. • Main and interaction effects for hits and response criterion were not significant.

Appendix B: Summary of the studies on operant conditioning and signal detection tasks

Study	Sensory modality and task	Independent Variable	Dependent Variable	Study Design	No. of participants	Procedure	Main findings
Lie and Alsop (2007, 2009)	Visual perceptual discrimination task (Two alternative signal detection task): stimulus arrays consisted of blue and red alien cartoon character	**a) Response-consequence:** i) Reinforcer (point gains) for correct response ii) Punisher (response cost/point loses) for errors **b. Patterns of Reinforcement (R) and Punishment (P) ratio:** i) Four patterns of R & P ratio counterbalanced (5:1, 2:1, 1:2, 1:5) in Experiment 1 ii) Two patterns of R & P ratio counterbalanced in Experiment 2 (5:1 or 1:5) iii) In Study 3**,** R and P ratios were like study 1	1. Bias 2. Sensitivity/ Discriminability	**> Experiment 1:** Mixed: repeated-measures & between groups (Group A: R Group B: R & P) **>Experiment 2:** Like experiment 1 with changes in R/P ratio, order of R and R+P trials, and stimuli (Group A: order 1 Group B: order 2) **> Experiment 3:** Repeated measures: it had different stimulus difficulty levels and stimulus presentation (4 conditions: R, and other 4 conditions: P+R)	**Exp 1:** 6 (18-19yr) **Exp 2:** 16 (18-35yr) **Exp 3:** 8 (19-24yr)	**>** Controlled reinforcement procedure **>** Overall R = VI 10-s (all studies). Overall P = VI 20-s (3rd study: VI 10-s) **>** Feedback about total no. of points won	**Experiment 1:** 1. Discriminability: R & P > only R 2. Both R and P influenced biasness. **Experiment 2:** 1. R+P increased sensitivity 2R+P > 1R+P 2. Bias: R+P > R **Experiment 3:** 1. Discriminability did not depend on relative reinforcer frequency. 2. Discriminability /sensitivity: R+P > R 3. Bias: Higher R > lower R 4. Order of R only and R+P conditions affected sensitivity
Lie and Alsop (2010)	Visual perceptual	**a) Response-consequence:**	1. Correct response	Mixed design (repeated-	24 (18-24yr)	> controlled R and P ratio	1. Participants made more correct response when

Study	Sensory modality and task	Independent Variable	Dependent Variable	Study Design	No. of participants	Procedure	Main findings
	discrimination task (Two alternative signal detection task): stimulus arrays consisted of blue and red alien cartoon character	i) Reinforcer (Point gains) for correct response and "ta da" sound ii) Punishment (point loses) for errors and "argh!" sound. **b) Disparity of stimuli:** i) High ii) Medium iii) Low	2. Bias 3. Sensitivity	measures and between groups)		**>** Feedback about total no. of points won > **High disparity:** VI 15-s for R and VI 10-s for P. **> Med. disparity:** VI 10-s for R and VI 15-s for P. **> Low disparity:** VI 10-s for R and VI 40-s schedule.	stimuli were more disparate. 2. P biased away respondents from the response option associated with higher frequency of P. 3. P affected sensitivity. 4. No interaction between stimulus control and P control and between bias and stimulus disparity.
Johnstone and Alsop (2000)	Visual perceptual discrimination task (Two alternative signal detection task): Arrays of squares and circles with different levels of discrimination were used as stimuli	**a) Reinforcement Procedure:** i) Controlled reinforcement procedure ii) Uncontrolled reinforcement procedure (VR 3) **b) Reinforcer ratio:** i) 4:1 ii) 1:4	i) Bias ii) Sensitivity	Mixed (repeated measures and between groups design)	8 (19-24yr)	**>** R was point gain and brief 1000 Hz tone **>** Feedback about total no. of points won	1. Reinforcer ratio affected bias pattern and ROC plot. 2. Controlled reinforcer ratio resulted in a general pattern of bias and ROC plot. 3. Uncontrolled reinforcer ratio led to variable pattern of bias and ROC points.
Mattke, Wylie, Woods, Tuma, and Layng (1989)	Schedule sensitivity task (visual)	Mixed fixed-ratio schedules. The larger one was the signal.	Human schedule sensitivity				Sensitivity was high when the difference between the schedules was high. However, sensitive

Study	Sensory modality and task	Independent Variable	Dependent Variable	Study Design	No. of participants	Procedure	Main findings
							performance was observed even under similar mixed fixed-ratio conditions.
Woods, Wylie, Mattke, Tuma, and Layng (1989)	Schedule sensitivity task (visual)	Mixed variable-ratio schedules. The larger one was the signal.	Human schedule sensitivity				Sensitivity had a positive correlation with stimulus disparity.
Tuma, Wylie, Mattke, Woods, and Layng (1989)	Schedule sensitivity task (visual)	VR 45 and VR 25 schedule of reinforcement. The larger one was the signal.	Human schedule sensitivity				Manipulation of the probability of schedule presentation resulted in bias development.
Johnstone and Alsop (1996)	> Two-choice signal detection task	**Experiment 1:** i) Signal presentation probability: three conditions ii) Feedback: present, absent iii) Response consequence: R for correct response and P for incorrect response **Experiment 2:** i) Signal presentation probability: three conditions ii) Outcome for correct responses: money, the word 'correct', and pixel stars/only the word 'correct' **Experiment 3:** i) Circle: square ratio: 77:67 and vice versa. ii) Rate of reinforcement iii) Signal presentation	1. Bias 2. Discriminability	**Experiment 1 and 2:** 2x3 repeated measures **Experiment 3:** 2x2x2 repeated measures **Experiment 4:** Between groups design	**Experiment 1** 6 (18-23 yrs.) **Experiment 2** 6 (22-28 yrs.) **Experiment 3:** 4 (18-21 yrs.) **Experiment 4:** 4 (18-20 yrs.)	**Experiment 1:** > In half of the trials participants received only R and in the other half both R and P. > Controlled R and P procedure was used. **Experiment 2:** All correct responses resulted in either R or feedback **Experiment 3:** Participants did not receive feedback for incorrect response.	> Participants were biased towards the stimulus presented least often when reinforcement distribution was constant and equal. > The above effect was reliable with extended training and monetary, rather than point, reinforcement. > When each correct response followed by reward or feedback, participants became biased toward the stimulus presented most often. > Deducting money (intended as punishment) for equal numbers of incorrect responses on each alternative, or varying the overall rate of reinforcement, produced no

Study	Sensory modality and task	Independent Variable	Dependent Variable	Study Design	No. of participants	Procedure	Main findings
		probability: two conditions **Experiment 4:** I) Signal presentation probability: two conditions ii) No. of trials: 200-500, 700-1000, 1200-1500, 1700-2000					clear change in response bias. > In experiment 4, over successive trials discriminability tended to increase.

Appendix C: Secondary analysis of Study 1

C.1 Effects of Light

Non-significant main and interaction effects of light on the SSDT response outcomes are shown in Table C1.

C.1.1 False Alarm Rate

There was a significant interaction between light and phase, $F(2, 88) = 8.83$, $\eta^2_p = .17$, $p < .0001$. Bonferroni corrected post-hoc tests revealed that the false alarm rate was the same at baseline for both light present and light absent trials (mean difference = .01; 95% CI = -.02, .04; $p = .57$). However, the false alarm rate was significantly higher in the presence of light both in the manipulation (mean difference = .03; 95% CI = .01, .05; $p < .01$) and follow-up phases (mean difference = .04; 95% CI = .01, .07; $p < .01$).

The main effect of session, was significant, $F(1, 44) = 4.30$, $\eta^2_p = .09$, $p < .05$, as was the interaction between light, condition, and session, $F(1, 44) = 4.92$, $\eta^2_p = .10$, $p < .05$. Bonferroni corrected post-hoc tests indicated that when the control condition was the first session, participants produced significantly more false alarms in the experimental than in the control condition both in the presence (mean difference = .08; 95% CI = .02, .15; $p < .05$) and absence of light (mean difference = .06; 95% CI = .01, .12; $p < .05$). However, such differences in false alarm rates were not found between the conditions both in the presence (mean difference = -.01; 95% CI = -.08, .06; $p = .80$) and absence of light (mean difference = .04; 95% CI = -.02, .10; $p = .18$) when the experimental condition was the first session. When the control condition was the first session, false alarm rates did not differ between light present and light absent trials both in the control (mean difference = -.01; 95% CI = -.05, .04; $p = .81$) and experimental conditions (mean difference = .01; 95% CI = -.02, .05; $p = .34$). When the experimental condition was the first session, false alarm rates did not differ between the presence and absence of light in the experimental condition (mean difference = .02; 95% CI = -.02, .05; $p = .35$), but in the control condition, participants produced significantly more false alarms in the presence of the light than when the light was absent (mean difference = .07; 95% CI = .02, .11; $p <$

.01). In the experimental condition, the groups (i.e. control vs. experimental condition in the first session) did not differ both in the presence (mean difference = .04; 95% CI = -.04, .13; p = .33) and absence of the light (mean difference = .04; 95% CI = -.05, .13; p = .37). In the experimental condition, the groups did not differ in the absence of the light (mean difference = .06; 95% CI = -.01, .14; p = .11) but in light present trials, participants who had the experimental condition as the first session produced significantly more false alarms than their counterparts (mean difference = .13; 95% CI = .05, .22; p < .01).

C.1.2 Hit Rate

There was a main effect for light, such that the hit rate was higher on light present trials, $F(1, 44) = 44.31$, $\eta^2_p = .50$, $p < .0001$.

C.1.3 Response Bias

There was a significant main effect of light on response bias, $F(1, 44) = 37.37$, $\eta^2_p = .46$, $p < .0001$. There were significant interactions between light and phase, $F(2, 88) = 9.85$, $\eta^2_p = .18$, $p < .0001$, and between light, phase, and condition, $F(2, 88) = 4.60$, $\eta^2_p = .10$, $p = .01$.

Bonferroni corrected post-hoc tests revealed that when light was absent in the experimental condition, participants were more biased towards responding "no" in the baseline than in the manipulation (mean difference = .20; 95% CI = .13, .27; p < .01) and follow-up phases (mean difference = .09; 95% CI = .02, .15; p < .01), with lower bias in the manipulation than in the follow-up phase (mean difference = -.11; 95% CI = -.18, -.04; p < .01). When light was absent in the control condition, bias towards responding "no" in the follow-up phase was significantly greater than that in the baseline (mean difference = .15; 95% CI = .09, .22; p < .01) and manipulation phases (mean difference = .06; 95% CI = .02, .11; p < .01), with bias in the manipulation phase greater than that in the baseline phase, (mean difference = .09; 95% CI = .04, .14; p < .01). In light present trials of the experimental condition, bias towards responding "no" in the baseline phase was significantly greater than both the manipulation (mean difference = .26; 95% CI = .17, .34; p < .01) and follow-up phases (mean difference = .11; 95% CI = .04, .19; p < .01), with higher response

bias in the follow-up than in the manipulation phase (mean difference = .14; 95% CI = .08, .21; $p < .01$). In light present trials of the control condition, bias towards responding "no" was significantly less in the baseline phase than both the manipulation (mean difference = -.06; 95% CI = -.10, -.01; $p < .01$) and follow-up phases (mean difference = -.07; 95% CI = -.12, -.02; $p < .01$), with no difference between the manipulation and follow-up phases (mean difference = -.02; 95% CI = -.07, .04; $p = 1.00$).

In light absent trials of the baseline phase, bias towards responding "no" was greater in the experimental than in the control condition (mean difference = .09; 95% CI = .03, .15; $p < .01$), with higher response bias in the control than in the experimental condition both in the manipulation (mean difference = .20; 95% CI = .12, .27; $p < .01$) and follow-up phases (mean difference = .15; 95% CI = .08, .22; $p < .01$). Similarly, in light present trials of the baseline phase, bias towards responding "no" was greater in the experimental than in the control condition (mean difference = .10; 95% CI = .04, .16; $p < .01$), with higher response bias in the control than in the experimental condition both in the manipulation (mean difference = .21; 95% CI = .13, .29; $p < .01$) and follow-up phases (mean difference = .09; 95% CI = .02, .15; $p < .01$).

In the experimental condition, bias towards responding "no" in all three phases was greater in the absence of light than when the light was present (mean difference in the baseline phase = .03; 95% CI = .004, .06; $p < .05$; mean difference in the follow-up phase = .09; 95% CI = .06, .12; $p < .01$; mean difference in the follow-up phase = .06; 95% CI = .03, .09; $p < .05$). In the baseline phase of the control condition, there was no significant difference in response bias between the light conditions (mean difference in the baseline phase = .04; 95% CI = -.01, .09; $p = .10$). However the bias was significantly greater in the absence of light than when the light was present both in the manipulation (mean difference in the baseline phase = .08; 95% CI = .05, .10; $p < .01$) and follow-up phases (mean difference in the baseline phase = .12; 95% CI = .09, .16; $p < .01$) of the control condition.

C.1.4 Sensitivity

Light had a significant effect on sensitivity, $F(1, 44) = 13.82$, $\eta^2_p = .24$, $p < .005$, such that participants were better able to detect the vibration in light present trials (mean difference between light present and light absent trials = .26; 95% CI = .12, .39; $p < .005$).

C.2 Sleepiness

Data on sleepiness satisfied the assumptions of mixed ANOVA, except that time of assessment (i.e., sleepiness before the baseline, manipulation, and follow-up phases) violated the assumption of sphericity ($\varepsilon = .79$, $p < .01$). As the estimate of sphericity was greater than 0.75, the Huynh-Feldt correction was used to produce a valid *F*-ratio for this variable (Girden as cited in Field, 2009).

In the mixed ANOVA, time of assessment and condition (control vs. experimental) was the within-group independent variables and session (control vs. experimental condition in the first session) was the between-groups independent variable. It was found that the main effect of condition was significant, $F(1, 44) = 7.33$, $\eta^2_p = .14$, $p = .01$, as was the interaction between condition and session, $F(1, 44) = 8.35$, $\eta^2_p = .16$, $p < .01$. Bonferroni corrected post-hoc tests revealed that participants were sleepier in the control than in the experimental condition when the control condition was the first session (mean difference = 1.39; 95% CI = .70, 2.08; $p < .01$). However, sleepiness did not differ between the conditions when experimental condition was the first session.

The main effect of time (i.e. sleepiness prior to the baseline vs. manipulation vs. follow-up phases) was significant, $F(1.75, 76.83) = 16.77$, $\eta^2_p = .28$, $p < .01$, so was the interaction between time and condition, $F(2, 88) = 12.31$, $\eta^2_p = .22$, $p < .01$. Bonferroni corrected post-hoc tests revealed that participants were sleepiest at the follow-up followed by the manipulation and baseline phases of the control condition (mean difference between the follow-up and baseline phases = 1.81; 95% CI = 1.11, 2.50; $p < .01$; between the follow-up and manipulation phases = .67; 95% CI = .21, 1.12; $p < .01$; and between the manipulation and baseline phases = 1.14; 95% CI = .66, 1.63; $p < .01$). In the experimental condition, sleepiness was significantly higher at the manipulation than at the baseline phase (mean difference = .89; 95% CI =

.24, 1.54; $p < .01$). However, sleepiness did not differ between the baseline and follow-up (mean difference = -.31; 95% CI = -1.16, .53; $p = 1.00$) and between the manipulation and follow-up phases (mean difference = .58; 95% CI = -.13, 1.29; $p = .15$).

Non-significant main and interaction effects for sleepiness are presented in Table C1.

C.3 Anxiety

Data on state anxiety satisfied the assumptions of mixed ANOVA. In this analysis, time (state anxiety before vs. after the manipulation phase), and condition (control vs. experimental) were the within-group independent variables and session (control vs. experimental condition in the first session) was the between-groups independent variable. It was found that the interaction between time, condition, and session was significant, $F(1, 44) = 6.31$, $\eta^2_p = .13$, $p < .05$. Bonferroni corrected post-hoc tests revealed that in the experimental condition when it was the first session, participants were more anxious after the manipulation phase than before (mean difference = 1.5; 95% CI = .44, 2.56; $p < .01$), but state anxiety did not differ between the assessment times (i.e. before and after the manipulation phase) if the experimental condition was the second session (mean difference = -.13; 95% CI = -1.14, .89; $p = .81$). In the control condition, state anxiety did not change between the assessment times both when the control condition was the first (mean difference = -.79; 95% CI = -1.79, .21; $p = .12$) and second session (mean difference = .55; 95% CI = -.50, 1.59; $p = .30$).

Non-significant main and interaction effects for state anxiety are presented in Table C1.

Table C1

Non-Significant Main and Interaction Effects Found for Light on the SSDT Response Outcomes, and State Anxiety and Sleepiness

Variables	*F*	*df*	*p*	η^2_p
False alarm rate				

(continued)

Variables	*F*	*df*	*p*	η^2_p
Light	3.69	1, 44	.06	.08
Light X Condition	.90	1, 44	.35	.02
Light X Session	2.34	1, 44	.13	.05
Light X Phase X Condition	2.88	2, 88	.06	.06
Light X Phase X Session	.36	2, 88	.70	.01
Light X Phase X Condition X Session	1.09	2, 88	.34	.02
Hit rate				
Light X Phase	1.90	1.81, 79.77	.16	.04
Light X Condition	3.82	1, 44	.06	.08
Light X Session	.07	1, 44	.79	.002
Light X Phase X Condition	1.42	2, 88	.25	.03
Light X Phase X Session	1.60	2, 88	.21	.04
Light X Condition X Session	.003	1, 44	.96	< .001
Light X Phase X Condition X Session	1.48	2, 88	.23	.03
Response bias				
Session	.61	1, 44	.44	.01
Light X Condition	2.33	1, 44	.13	.05
Light X Session	.17	1, 44	.69	.004
Light X Phase X Session	1.12	2, 88	.33	.03
Light X Condition X Session	4.24	1, 44	.05	.09
Light X Phase X Condition X Session	.81	2, 88	.45	.02
Sensitivity				
Light X Phase	1.53	2, 88	.22	.03
Light X Condition	.13	1, 44	.73	.003
Light X Session	1.93	1, 44	.17	.04
Light X Phase X Condition	.93	1.81, 79.60	.39	.02
Light X Phase X Session	2.06	2, 88	.13	.05
Light X Condition X Session	1.63	1, 44	.21	.04

(continued)

Variables	*F*	*df*	*p*	η^2_p
Light X Phase X Condition X Session	.001	2, 88	1.00	< .001
State anxiety				
Main effect of time	3.77	1, 44	.06	.08
Main effect of condition	.48	1, 44	.49	.01
Main effect of session	.76	1, 44	.39	.02
Time X Condition	.92	1, 44	.34	.02
Time X Session	.12	1, 44	.73	.003
Condition X Session	.04	1, 44	.84	.001
Sleepiness				
Main effect of session	.19	1, 44	.66	.004
Time X Session	1.82	2, 88	.17	.04
Time X Condition X Session	.87	2, 88	.42	.02

Correlation coefficients were computed to determine whether performance on the SSDT and SPS task was related to state anxiety and sleepiness in the control (see Table C2) and experimental conditions (see Table C3). As a number of variables were non-normal, non-parametric correlation (i.e. Spearman rank-order correlation) was used. To avoid the possibility of Type I error due to family-wise error, the level of significance was corrected (i.e. Bonferroni correction) to .002 for the SSDT response outcomes and .005 for the SPS variables.

There were 12 and 13 participants in the control and experimental conditions respectively who did not report any SPS in the baseline or follow-up phases and thus were excluded from the analysis of SPS pleasantness and certainty. Remaining correlation coefficients in both he conditions were calculated for 46 participants.

A significant negative correlation was found in the control condition between sleepiness prior to the baseline phase and certainty of SPSs in the follow-up phase, indicating that the sleepier participants were at the baseline phase the less certain they were about SPSs in the follow-up phase. The other correlation coefficients in both the conditions were not significant.

Overall, statistical analyses of sleepiness and state anxiety indicate that responses in the SSDT and SPS test were not affected by these variables.

Table C2

Summary of Medians and Interquartile Ranges (IQR) for Sleepiness and State Anxiety and Their Correlations With SPS Measures and SSDT Response Outcomes in the Control Condition

Variables	Sleepiness			State anxiety	
	Before the baseline phase	Before the manipulation phase	Before the follow-up phase	Before the baseline phase	Before the follow-up phase
Baseline SPS					
Total number	.10	.04	-.08	.24	-.05
Intensity	.14	.05	-.05	.34	.08
Pleasantness	.24	.03	-.08	.04	.03
Certainty	-.13	-.07	-.02	.31	.10
Extent	.21	.12	.06	.17	-.04
Follow-up SPS					
Total number	-.23	-.28	-.18	.09	-.09
Intensity	-.29	-.22	-.16	.11	-.05
Pleasantness	.29	.20	.03	-.32	-.24
Certainty	**-.51***	-.40	-.16	.02	-.28
Extent	-.03	-.11	.00	.01	-.05
SSDT response outcomes in the baseline phase					
Hit rate in light absent trials	-.06	-.21	-.22	.02	-.03
Hit rate in light present trials	-.19	-.21	-.23	.11	.03
False alarm rate in light absent trials	-.14	-.13	-.05	.31	.11
False alarm rate in light present trials	-.10	-.14	-.12	.19	.03
Sensitivity in light absent trials	.00	-.11	-.14	-.11	-.07
Sensitivity in light present trials	-.03	-.08	-.14	-.01	.02

(continued)

Variables	Sleepiness			State anxiety	
	Before the baseline phase	Before the manipulation phase	Before the follow-up phase	Before the baseline phase	Before the follow-up phase
Bias in light absent trials	.16	.24	.19	-.20	-.07
Bias in light present trials	.25	.25	.23	-.14	-.02
SSDT response outcomes in the manipulation phase					
Hit rate in light absent trials	-.06	-.10	-.09	-.15	-.14
Hit rate in light present trials	-.15	-.13	-.11	-.09	-0.14
False alarm rate in light absent trials	-.10	-.05	.01	-.02	.004
False alarm rate in light present trials	-.13	-.14	-.14	.14	.17
Sensitivity in light absent trials	.03	-.03	-.07	-.14	-.12
Sensitivity in light present trials	-.03	-.03	-.04	-.17	-.18
Bias in light absent trials	.07	.08	.06	.13	.08
Bias in light present trials	.18	.15	.17	.00	-.02
SSDT response outcomes in the follow-up phase					
Hit rate in light absent trials	.02	-.12	-.18	-.09	-.11
Hit rate in light present trials	-.10	-.10	-.08	-.10	-.22
False alarm rate in light absent trials	-.02	-.05	-.08	-.08	-.26
False alarm rate in light present trials	-.16	-.22	-.21	.22	.13
Sensitivity in light absent trials	.05	-.07	-.12	-.01	.09
Sensitivity in light present trials	.03	.02	.01	-.16	-.17
Bias in light absent trials	.01	.12	.19	.14	.23
Bias in light present trials	.12	.11	.07	-.04	.10
Median	5	6	7	9	9
IQR	3 to 7	5 to 8	6 to 8	7 to 11	7 to 11

Note. SPS = spontaneous sensation; SSDT = Somatosensory Signal Detection Task.
$^{*}p < .005$.

Table C3

Summary of Medians and Interquartile Ranges (IQR) for Sleepiness and State Anxiety and Their Correlations With SPS Measures and SSDT Response Outcomes in the Experimental Condition

Variables	Sleepiness			State anxiety	
	Before the baseline phase	Before the manipulation phase	Before the follow-up phase	Before the baseline phase	Before the follow-up phase
Baseline SPS					
Total number	.25	.03	-.30	.11	.04
Intensity	.30	.11	-.14	.10	.05
Pleasantness	-.10	.11	-.23	.09	.00
Certainty	.11	.07	-.05	-.03	-.14
Extent	.37	.16	-.21	.07	.12
Follow-up SPS					
Total number	.14	.09	-.23	.09	.27
Intensity	.14	.00	-.14	-.02	.19
Pleasantness	.01	.38	.07	-.10	-.17
Certainty	-.07	-.28	-.21	-.23	.13
Extent	.19	.12	-.29	.09	.22
SSDT response outcomes in the baseline phase					
Hit rate in light absent trials	-.05	-.24	-.38	-.02	-.30
Hit rate in light present trials	-.06	-.25	-.39	.04	-.20
False alarm rate in light absent trials	.29	.11	-.12	-.05	-.04
False alarm rate in light present trials	.08	.14	-.08	.20	.15
Sensitivity in light absent trials	-.24	-.31	-.34	-.02	-.25
Sensitivity in light present trials	-.05	-.23	-.31	-.04	-.21
Bias in light absent trials	-.04	.14	.31	.04	.23
Bias in light present trials	.06	.24	.41	-.08	.16

(continued)

Variables	Sleepiness			State anxiety	
	Before the baseline phase	Before the manipulation phase	Before the follow-up phase	Before the baseline phase	Before the follow-up phase
SSDT response outcomes in the manipulation phase					
Hit rate in light absent trials	.07	.02	-.16	-.12	-.41
Hit rate in light present trials	.09	.04	-.08	-.11	-.36
False alarm rate in light absent trials	.07	.22	-.03	-.02	-.08
False alarm rate in light present trials	.12	.38	.18	.10	.02
Sensitivity in light absent trials	-.07	-.16	-.15	-.03	-.20
Sensitivity in light present trials	-.01	-.17	-.16	-.11	-.24
Bias in light absent trials	.00	-.11	.25	.04	.27
Bias in light present trials	-.18	-.30	-.03	.02	.21
SSDT response outcomes in the follow-up phase					
Hit rate in light absent trials	-.13	-.06	-.16	-.08	-.29
Hit rate in light present trials	-.07	.02	-.12	-.03	-.27
False alarm rate in light absent trials	-.02	.17	-.15	-.07	-.05
False alarm rate in light present trials	.06	.19	-.02	.11	.06
Sensitivity in light absent trials	-.02	-.06	-.02	.04	-.19
Sensitivity in light present trials	-.02	-.09	-.08	-.09	-.26
Bias in light absent trials	.15	-.06	.18	.09	.18
Bias in light present trials	.02	-.12	.10	-.04	.13
Median	5	6	5	9	9
IQR	3 to 6	4 to 7	3 to 7	8 to 10	8 to 12

Note. SPS = spontaneous sensation; SSDT = Somatosensory Signal Detection Task.

Appendix D: Advert for Studies 1 and 2

MANCHESTER 1824
The University of Manchester

RESEARCH PARTICIPANTS NEEDED

School of Psychological Sciences

Title of the Research: Learning and tactile perception study

About the Experiment

The experiment will be carried out in two 1.5-hour sessions, with a gap of at least seven days in between them. As part of the experiment, you will be asked to fill in some questionnaires and complete some simple tactile (i.e. touch) perception tasks.

To take part in this study you must

- Be aged between 18 to 4
- Understand instructions in English well
- Not have any medical conditions that might affect your sense of touch

Compensation

Participants will be compensated for the inconvenience of taking part. Undergraduate students in psychology are eligible to receive 1 credit for each full 15-minute time slot completed, up to a maximum of 12 credits. Credits or compensation will be available at the end of the second testing session.

Name of the research ethics committee

University of Manchester Ethics Committee

Ethics Committee Number: 1

If you are interested to take part in the experiment, please visit **http://goo.gl/KMFtl** or email mdakibul.huque@postgrad.manchester.ac.uk

MANCHESTER 1824
The University of Manchester

Appendix E: Participant information sheet for Studies 1 and 2

Title of the Research: Learning and tactile perception study

Introduction

You are invited to take part in a research study about learning and tactile perception. Please read this information sheet carefully so that you are able to make an informed decision about whether or not to participate in this study. If anything is unclear or you have any questions about the research, you are very welcome to contact me (please see the contact details at the end of this form) and I will do my best to provide the information you need.

What is the aim of the research?

The present study is part of my PhD research. The main objective is to investigate how past experience affects subsequent touch perception.

Why have I been chosen?

Anyone from the University of Manchester aged between 18-40 can participate in this study. Our aim is to collect data from at least 88 individuals.

What would I be asked to do if I took part?

If you decide to take part in this study, you will be asked to complete a short questionnaire about physical symptoms (e.g. stomach pain) that you might have experienced in the past month. You will then be asked to attend two testing sessions, each lasting 1.5 hours, about a week apart. During the testing sessions, you will be asked to complete a simple tactile (i.e., touch) perception task, which involves using the index finger of your non-dominant hand to detect gentle vibrations. In another task, you will be asked to focus on your non-dominant hand and report any sensations that you feel in it. You will also be asked to complete three short questionnaires, one about how anxious you feel, one about what hand you use to perform certain tasks, and the other one about your current state of tiredness.

During the experiment, the testing room will be dimly lit and you will be presented white noise through headphones to mask outside sound. You will also be asked to sit in the same place for much of the testing session, although you will have the opportunity to take breaks if you wish.

Will my data be confidential?

Yes, we will manage all of your data in a secure way to ensure that your confidentiality is protected. We will do this by ensuring that:

- Any printed copies of your personal information (such as your name, contact address, and date of birth) will be kept in a locked file cabinet on University premises.
- None of your experimental data will be stored in the same place as any personal information. We do need to know who provided what data (to ensure that data from the two testing sessions are identified as coming from the same person), but we will do this by using a unique identification code rather than personal information. Any documents linking personal information to identification codes will be kept separate from the data itself and stored in a locked file cabinet on University premises.
- Any electronic files containing personal information will be encrypted.
- Only the research team will have access to data.

What happens if I do not want to take part or if I change my mind?

It is entirely up to you whether you take part or not. If you do decide to take part, you are free to withdraw at any point without having to give a reason.

What are the benefits and risks to taking part in the study?

The potential benefit of participating in the study is that you will be compensated for the inconvenience of taking part. If you are a student of psychology, you will be eligible to receive 1 credit for each full 15-minute time slot completed, up to a maximum of 12 credits. Credits or compensation will be available at the end of the second testing session.

It is very unlikely that the present research will cause any physical or psychological harm to you. The experimental procedures are simple and benign in nature and have been used in a number of studies with ethical approval from the university.

The questionnaires that will be used in the present experiment are highly regarded by both clinicians and researchers around the world. There is no known potential risk in using them, although there is a very small chance that some participants will be mildly upset answering questions about their physical health and anxiety.

The only real concern in the present study is the fatigue or tiredness that might result from working in a light attenuated room for 90 minutes long experimental session. However, if you are tired, you can take rest at any point of the experiment.

Where will the research be conducted?

The research will be carried out in a lab at Zochonis Building of the University of Manchester.

Will the outcomes of the research be published?

The outcomes of the research will be submitted for publication in peer-reviewed journals. However, the papers will not contain your name or address. If you want, we will send you a summary of the findings after the final data analysis.

What if something goes wrong?

We do not expect anything to go wrong. In the very unlikely event that something untoward does happen, you can contact me in the first instance. You can also contact my supervisor (richard.j.brown@manchester.ac.uk). If you wish to make a formal complaint about the conduct of the research please contact the head of the Research Office, Christie Building, University of Manchester, Oxford Road, Manchester, M13 9PL.

Contact details

Akib Ul Huque, PhD Student
School of Psychological Sciences, University of Manchester
Room S42, 2nd Floor Zochonis Building, Brunswick Street
Manchester, M13 9PL
Email: mdakibul.huque@postgrad.manchester.ac.uk

This project has been approved by the

University of Manchester Research Ethics Committee

MANCHESTER 1824
The University of Manchester

Appendix F: Consent form for Studies 1 and 2

SCHOOL OF PSYCHOLOGICAL SCIENCES

Title of Project: Learning and Tactile Perception Study

If you are happy to participate please complete the consent form below

Please write "Yes" in the box

1. I confirm that I have read the attached information sheet on the above project and have had the opportunity to consider the information and ask questions and had these answered satisfactorily. | |

2. I understand that my participation in the study is voluntary and that I am free to withdraw at any time without giving a reason and without detriment to any treatment/service. | |

3. I would be happy to be contacted to take part in further research on this topic on this email address (please note that you do not need to provide your e-mail address here if you do not want to be contacted in the future):

4. I would like to receive a summary copy of the results and my preferred method of contact and details are:

5. Did you participate or have you registered to participate in the study entitled **"Mood and Bodily Symptoms"** which is being carried out by **Anna Chapman**? If your answer is "Yes", please write the date of your participation in the following box:

I agree to take part in the above project

Name: _____

Signature: _____

Date: _____

Appendix G: The patient health questionnaire-15 (PHQ-15)

During the past 4 weeks, how much have you been bothered by any of the following problems?	Not bothered at all (0)	Bothered a little (1)	Bothered a lot (2)
a. Stomach pain ……	☐	☐	☐
b. Back pain ……	☐	☐	☐
c. Pain in your arms, legs, or joints (knees, hips, etc.)	☐	☐	☐
d. Menstrual cramps or other problems with your period **[Women only]**……	☐	☐	☐
e. Headaches ……	☐	☐	☐
f. Chest pain ……	☐	☐	☐
g. Dizziness ……	☐	☐	☐
h. Fainting spells ……	☐	☐	☐
i. Feeling your heart pound or race ……	☐	☐	☐
j. Shortness of breath ……	☐	☐	☐
k. Pain or problems during sexual intercourse ……..	☐	☐	☐
l. Constipation, loose bowels, or diarrhea ……	☐	☐	☐
m. Nausea, gas, or indigestion ……	☐	☐	☐
n. Feeling tired or having little energy ……	☐	☐	☐
o. Trouble sleeping ……	☐	☐	☐

Appendix H: Short-form of the state scale of the Spielberger state-trait anxiety inventory

A number of statements which people have used to describe themselves are given below. Read each statement and then circle the most appropriate number to the right of the statement to indicate how you feel right now, at this moment. There are no right or wrong answers. Do not spend too much time on any one statement but give the answer which seems to describe your present feelings best.

	Not at all	Somewhat	Moderately	Very much
1. I feel calm	1	2	3	4
2. I am tense	1	2	3	4
3. I feel upset	1	2	3	4
4. I am relaxed	1	2	3	4
5. I feel content	1	2	3	4
6. I am worried	1	2	3	4

Appendix I: Edinburgh handedness inventory

Surname …… Given Names……

Date of Birth …….. Sex…….

Please indicate your preferences in the use of hands in the following activities *by putting +in the appropriate column*. Where the preference is so strong that you would never try to use the other hand unless absolutely forced to, *put* + +. If in any case you are really indifferent *put* + *in both columns*.

Some of the activities require both hands. In these cases the part of the task, or object, for which hand preference is wanted is indicated in brackets.

Please try to answer all the questions, and only leave a blank if you have no experience at all of the object or task.

		LEFT	RIGHT
1	Writing		
2	Drawing		
3	Throwing		
4	Scissors		
5	Toothbrush		
6	Knife (without fork)		
7	Spoon		
8	Broom (Upper hand)		
9	Striking Match (match)		
10	Opening box (lid)		

L.Q.	

DECILE	

Appendix J: The Karolinska sleepiness scale

Please indicate your sleepiness during the last 5 minutes. Give yourself a rating by placing an "X" in the appropriate box. Remember that you may also use intermediate steps.

1 – Very Alert ☐

2 – ☐

3 – Alert-normal level ☐

4 – ☐

5 – Neither alert nor sleepy ☐

6 – ☐

7 – Sleepy but no effort to stay awake ☐

8 – ☐

9 – Very sleepy, great effort to keep awake, fighting sleep ☐

Appendix K: Non-dominant hand

Left hand

Right hand

Appendix L: Spontaneous sensation test report form

Sensations	How intense was the sensation?	How pleasant was the sensation?	How certain are you that there was a sensation?
	1 2 3 4 5 6 7 8 9 10 Weak Strong	-4 -3 -2 -1 0 1 2 3 4 Unpleasant Neutral Pleasant	0 1 2 3 4 5 Uncertain Certain
	1 2 3 4 5 6 7 8 9 10 Weak Strong	-4 -3 -2 -1 0 1 2 3 4 Unpleasant Neutral Pleasant	0 1 2 3 4 5 Uncertain Certain
	1 2 3 4 5 6 7 8 9 10 Weak Strong	-4 -3 -2 -1 0 1 2 3 4 Unpleasant Neutral Pleasant	0 1 2 3 4 5 Uncertain Certain
	1 2 3 4 5 6 7 8 9 10 Weak Strong	-4 -3 -2 -1 0 1 2 3 4 Unpleasant Neutral Pleasant	0 1 2 3 4 5 Uncertain Certain
	1 2 3 4 5 6 7 8 9 10 Weak Strong	-4 -3 -2 -1 0 1 2 3 4 Unpleasant Neutral Pleasant	0 1 2 3 4 5 Uncertain Certain

Appendix M: Verbal instruction given in the manipulation phase of the experimental condition of Studies 1 and 2

Now you will go through another phase of the same tactile vibration task that you just finished. Try to identify correctly the presence or absence of vibration. You will now win 10 points for some of your correct responses and lose the same amount for some of your incorrect responses. This is to assist you to improve your performance. If you score more than zero, you will get money equivalent to that value (1 point = 1p). This is bonus money that you will get in addition to the credits or money that you get for taking part. Don’t worry if you score less than zero. In that case, you will not win anything extra. So, try your best to win as much as possible!

Appendix N: Secondary analysis for Study 2

N.1 Effects of Light on SSDT Response Outcomes

N.1.1 False Alarm Rate

There was a significant main effect of light, $F(1, 28) = 32.78$, $p < .001$, $\eta^2_p = .54$. A Bonferroni corrected post-hoc test indicates that the false alarm rate was significantly higher when light was present that when it was absent, mean difference = .26; 95% CI = .17, .35; $p < .001$. The remaining main and interaction effects were not significant (see Table N1).

N.1.2 Hit Rate

The main effect of light was significant, $F(1, 28) = 38.89$, $p < .001$, $\eta^2_p = .58$. A Bonferroni corrected post-hoc test indicates that the hit rate in light present trials was significantly higher than that in light absent trials, mean difference = .14; 95% CI = .10, .19; $p < .001$. Other main and interaction effects were not significant (see Table N1).

N.1.3 Bias

The main effect of light, $F(1, 28) = 30.57$, $p < .001$, $\eta^2_p = .52$, was significant. A Bonferroni corrected post-hoc test indicates that participants were less likely to respond yes in light absent than in light present trials, mean difference = .43; 95% CI = .27, .60; $p < .001$. Other main and interaction effects were not significant (see Table N1).

N.1.4 Sensitivity

None of the main or interaction effects were significant. These are presented in Table N1.

N.2 State Anxiety

Square root transformation brought all the state anxiety distributions (baseline and follow-up in the control and experimental conditions) to normal and also satisfied the other assumptions of a mixed ANOVA. In the analysis, phase (baseline vs. follow-up) and condition (control vs. experimental) were the within-subjects variables and session (control condition in the first session vs. experimental condition in the first session) was the between-subjects factor.

The mixed ANOVA analysis indicates that none of the main and interaction effect was significant. These are presented in Table N1.

N.3 Sleepiness

The assumptions of a mixed ANOVA were examined to determine whether this test could be carried out with the sleepiness data. There were some violations of the normality assumption but all the distributions looked reasonably normal. The assumptions of homogeneity of between-group variance and sphericity of within-group variance were satisfied. As ANOVA is quite robust to minor violations of normality (Field, 2009), we decided to proceed with a mixed ANOVA analysis.

The main effects of phase, $F(2, 56) = 24.80$, $p < .001$, $\eta^2_p = .47$, and condition, $F(1, 28) = 6.76$, $p < .05$, $\eta^2_p = .20$, were significant. Bonferroni corrected post hoc tests indicate that participants were sleepier in the control than in the experimental condition, mean difference = .74; 95% CI = .16, 1.33; $p < .05$. The post hoc test further indicates that participants were significantly sleepier before the SSDT manipulation, mean difference = 1.53; 95% CI = .98, 2.09; $p < .001$, and follow-up phases, mean difference = 1.62; 95% CI = .84, 2.41; $p < .001$, than before the baseline phase. Severity of sleepiness did not differ significantly between the SSDT manipulation and follow-up phases, mean difference = -.09; 95% CI = -.71, .53; $p = 1.00$.

The remaining main and interaction effects were not significant (see Table N1).

Table N1

Non-Significant Main and Interaction Effects Found for Light on the SSDT Response Outcomes, and State Anxiety and Sleepiness

Variables	*F*	*df*	*p*	η^2_p
False alarm rate				
Light X Phase	.33	2, 56	72	.01
Light X Condition	.06	1, 28	.81	.002
Light X Session	2.36	1, 28	.14	.08

(continued)

Variables	*F*	*df*	*p*	η^2_p
Light X Phase X Condition	.02	2, 56	.98	.001
Light X Phase X Session	.06	2, 56	.28	.04
Light X Condition X Session	.57	1, 28	.46	.02
Light X Phase X Condition X Session	.41	2, 56	.67	.01
Hit rate				
Light X Phase	3.26	2, 56	.05	.10
Light X Condition	2.32	1, 28	.14	.08
Light X Session	.92	1, 28	.35	.03
Light X Phase X Condition	3.56	2, 56	.04	.11
Light X Phase X Session	1.76	2, 56	.18	.06
Light X Condition X Session	.41	1, 28	.53	.01
Light X Phase X Condition X Session	.95	2, 56	.39	.03
Response bias				
Light X Phase	.54	2, 56	.59	.02
Light X Condition	1.28	1, 28	.27	.04
Light X Session	1.96	1, 28	.17	.07
Light X Phase X Condition	2.21	2, 56	.13	.07
Light X Phase X Session	.45	2, 56	.64	.02
Light X Condition X Session	2.07	1, 28	.16	.07
Light X Phase X Condition X Session	1.34	2, 56	.27	.05
Sensitivity				
Main effect of light	1.47	1, 28	.24	.05
Light X Phase	2.89	2, 56	.06	.09
Light X Condition	.82	1, 28	.37	.03
Light X Session	.37	1, 28	.55	.01
Light X Phase X Condition	1.33	2, 56	.27	.05
Light X Phase X Session	1.13	2, 56	.33	.04
Light X Condition X Session	1.77	1, 28	.06	.06

(continued)

Variables	*F*	*df*	*p*	η^2_p
Light X Phase X Condition X Session	2.15	2, 56	.13	.07
State anxiety				
Main effect of phase	.13	1, 28	.73	.004
Main effect of condition	.13	1, 28	.17	.07
Main effect of session	2.05	1, 28	.16	.07
Phase X Condition	.53	1, 28	.47	.02
Phase X Session	.12	1, 28	.73	.004
Condition X Session	1.04	1, 28	.32	.04
Phase X Condition X Session	.001	1, 28	.98	< .001
Sleepiness				
Main effect of session	.85	1, 28	.37	.03
Phase X Condition	1.81	2, 56	.17	.06
Phase X Session	.68	2, 56	.51	.02
Condition X Session	.29	1, 28	.59	.01
Phase X Condition X Session	.23	2, 56	.80	.01

Correlations of state anxiety and sleepiness with SPS variables and SSDT response outcomes in the control and experimental conditions were determined. As the distributions of a number of variables were non-normal, nonparametric (i.e. Spearman's) correlation was used (see Tables N2 and N3). To identify significant coefficients, alpha levels were adjusted (Bonferroni) to .002 and .005 for the SSDT and SPS variables respectively.

In the experimental condition, six participants in the baseline and four participants in the follow-up phases, and in the control condition, six participants in the baseline and five participants in the follow-up phases did not report any SPS. They were excluded from their respective phases to calculate correlation coefficients for SPS pleasantness and certainty. Thus there were 24 and 26 participants respectively in the baseline and follow-up phases of the experimental condition. Similarly, to obtain the same correlation coefficients, there were 24 and 25

participants respectively in the baseline and follow-up phases of the control condition. The rest of the correlational analyses were carried out with 30 participants.

In the experimental condition, state anxiety measured before the follow-up phase had significant negative correlation with SPS pleasantness in the baseline phase, $r = -.60$, $p < .005$, meaning that the less the baseline SPS were perceived pleasant (i.e. the more they were unpleasant) the higher was the state anxiety before the follow-up phase. The other correlation coefficients between state anxiety and SSDT and SPS variables in the experimental and control conditions were not significant.

Sleepiness did not have significant relationship with SPS and SSDT variables both in the experimental and control conditions.

Table N2

Summary of Medians and Interquartile Ranges (IQR) for Sleepiness and State Anxiety and Their Correlations With SPS Measures and SSDT Response Outcomes in the Experimental Condition

Variables	Sleepiness			State anxiety	
	Before the baseline phase	Before the manipulation phase	Before the follow-up phase	Before the baseline phase	Before the follow-up phase
Baseline SPS					
Total number	.01	-.02	-.09	-.06	-.08
Intensity	.25	.05	.16	-.03	-.04
Pleasantness	-.39	-.23	-.01	-.53	**-.60***
Certainty	.23	.06	.16	-.07	-.08
Extent	.31	.22	.06	-.10	-.27
Follow-up SPS					
Total number	-.10	-.09	-.02	-.26	-.17
Intensity	-.29	-.28	-.07	-.32	-.24
Pleasantness	-.12	-.07	.10	-.14	-.21
Certainty	-.37	-.30	.00	-.27	-.20
Extent	-.03	.06	.09	-.12	-.11

(continued)

Variables	Sleepiness			State anxiety	
	Before the baseline phase	Before the manipulation phase	Before the follow-up phase	Before the baseline phase	Before the follow-up phase
SSDT response outcomes in the baseline phase					
Hit rate in light absent trials	.04	-.12	.11	.21	.14
Hit rate in light present trials	.18	-.02	.21	.21	.14
False alarm rate in light absent trials	-.23	-.47	-.09	-.13	-.08
False alarm rate in light present trials	-.01	-.13	-.11	.14	.04
Sensitivity in light absent trials	.14	.11	.12	.33	.24
Sensitivity in light present trials	.12	.03	.21	.21	.16
Bias in light absent trials	.18	.39	-.06	.04	.02
Bias in light present trials	-.05	.12	-.05	-.18	-.14
SSDT response outcomes in the manipulation phase					
Hit rate in light absent trials	-.01	.15	.19	-.01	.17
Hit rate in light present trials	.09	.21	.35	.19	.35
False alarm rate in light absent trials	.06	.19	.24	-.08	.06
False alarm rate in light present trials	.29	.32	.01	.11	.36
Sensitivity in light absent trials	-.02	.04	.05	.09	.20
Sensitivity in light present trials	-.01	.07	.16	.17	.17
Bias in light absent trials	.01	-.18	-.25	.02	-.19
Bias in light present trials	-.15	-.26	-.32	-.17	-.38
SSDT response outcomes in the follow-up phase					
Hit rate in light absent trials	-.12	.07	.15	-.30	-.09
Hit rate in light present trials	-.04	.13	.15	.07	.15

(continued)

Variables	Sleepiness			State anxiety	
	Before the baseline phase	Before the manipulation phase	Before the follow-up phase	Before the baseline phase	Before the follow-up phase
False alarm rate in light absent trials	.15	.10	.31	-.10	-.03
False alarm rate in light present trials	.24	.40	.38	.14	.17
Sensitivity in light absent trials	-.23	.03	.03	-.30	-.12
Sensitivity in light present trials	-.16	-.03	-.07	-.02	.04
Bias in light absent trials	.08	-.05	-.26	.23	.06
Bias in light present trials	-.08	-.26	-.35	-.09	-.12
Median	4.5	7	6	9	9
IQR	3 to 6	5 to 7	4 to 7	7 to 10	6 to 11

Note. SPS = spontaneous sensation; SSDT = Somatosensory Signal Detection Task.

$^{*}p < .005$.

Table N3

Summary of Medians and Interquartile Ranges (IQR) for Sleepiness and State Anxiety and their Correlations with SPS Measures and SSDT Response Outcomes in the Control Condition

Variables	Sleepiness			State anxiety	
	Before the baseline phase	Before the manipulation phase	Before the follow-up phase	Before the baseline phase	Before the follow-up phase
Baseline SPS					
Total number	.13	-.05	-.01	-.05	-.05
Intensity	-.07	-.17	-.20	-.43	-.31
Pleasantness	.28	.06	.17	.04	.00
Certainty	.17	-.04	-.02	-.23	-.21
Extent	.06	-.13	-.14	-.40	-.32
Follow-up SPS					
Total number	-.22	-.22	-.19	-.07	.01

(continued)

Variables	Sleepiness			State anxiety	
	Before the baseline phase	Before the manipulation phase	Before the follow-up phase	Before the baseline phase	Before the follow-up phase
Intensity	-.16	-.15	-.30	-.28	-.29
Pleasantness	.12	.14	.09	-.30	-.35
Certainty	-.17	-.09	-.22	-.21	-.26
Extent	-.31	-.17	-.25	-.25	-.19
SSDT response outcomes in the baseline phase					
Hit rate in light absent trials	-.14	-.06	.18	-.09	-.17
Hit rate in light present trials	.17	.14	.28	.08	.08
False alarm rate in light absent trials	.08	-.26	.06	.02	-.04
False alarm rate in light present trials	.27	.21	.35	.41	.31
Sensitivity in light absent trials	-.13	.15	.15	-.10	-.14
Sensitivity in light present trials	-.05	-.05	.03	-.23	-.21
Bias in light absent trials	.03	.19	-.17	.03	.14
Bias in light present trials	-.25	-.24	-.41	-.29	-.24
SSDT response outcomes in the manipulation phase					
Hit rate in light absent trials	-.19	-.16	.16	-.01	-.08
Hit rate in light present trials	.07	.03	.22	.26	.24
False alarm rate in light absent trials	.18	-.05	.33	.27	.16
False alarm rate in light present trials	.21	-.01	.23	.27	.24
Sensitivity in light absent trials	-.35	-.07	.00	-.19	-.21
Sensitivity in light present trials	-.14	-.02	.06	.02	-.02
Bias in light absent trials	.05	.15	-.24	-.13	-.02
Bias in light present trials	-.11	.05	-.21	-.28	-.25

(continued)

Variables	Sleepiness			State anxiety	
	Before the baseline phase	Before the manipulation phase	Before the follow-up phase	Before the baseline phase	Before the follow-up phase
SSDT response outcomes in the follow-up phase					
Hit rate in light absent trials	-.06	-.08	.17	-.02	-.09
Hit rate in light present trials	.15	.09	.14	.08	.08
False alarm rate in light absent trials	.13	-.03	.42	.06	.00
False alarm rate in light present trials	.29	.08	.31	.25	.25
Sensitivity in light absent trials	-.16	-.11	-.19	.00	-.04
Sensitivity in light present trials	-.20	-.08	-.13	-.16	-.20
Bias in light absent trials	-.04	.02	-.31	.01	.07
Bias in light present trials	-.22	-.05	-.24	-.18	-.19
Median	5.5	7	7	10	9
IQR	3 to 7	6 to 8	6 to 8	7 to 12	6 to 12

Note. SPS = spontaneous sensation; SSDT = Somatosensory Signal Detection Task.
$^{*}p < .005$.

Appendix O: Non-significant main and interaction effects for SPS variables (Study 2)

SPS variables	*F*	*df*	*p*	η^2_p
Intensity				
Phase	.001	1, 15	.97	< .001
Condition	.47	1, 15	.51	.03
Session	.49	1, 15	.50	.03
Phase X Condition	.07	1, 15	.79	.01
Phase X Session	2.58	1, 15	.13	.15
Condition X Session	.37	1, 15	.55	.02
Phase X Condition X Session	2.05	1, 15	.17	.12
Pleasantness				
Phase	.45	1, 15	.51	.03
Condition	1.25	1, 15	.28	.08
Session	.08	1, 15	.78	.01
Phase X Condition	1.85	1, 15	.19	.11
Phase X Session	.28	1, 15	.60	.02
Condition X Session	2.73	1, 15	.12	.15
Phase X Condition X Session	.00	1, 15	.99	< .001
Certainty				
Phase	.36	1, 15	.56	.02
Condition	1.36	1, 15	.26	.08
Session	1.37	1, 15	.26	.08
Phase X Condition	.08	1, 15	.78	.01
Phase X Session	4.13	1, 15	.06	.22
Condition X Session	.16	1, 15	.70	.01
Phase X Condition X Session	.02	1, 15	.88	.002
Extent				
Phase	1.33	1, 15	.27	.08
Condition	2.10	1, 15	.17	.12

(continued)

SPS variables	*F*	*df*	*p*	η^2_p
Session	.11	1, 15	.75	.01
Phase X Condition	.21	1, 15	.67	.01
Condition X Session	2.25	1, 15	.15	.13
Phase X Condition X Session	.83	1, 15	.38	.05

Appendix P: Auditory paradigms

Study	Task design	Participants	Responses	Results
Merckelbach and van de Ven (2001)	• White noise only • No. of trials: 1 • Task duration: 3 minutes	44 undergraduate students	• Pressing a button if the White Christmas song was heard in the white noise. • Multiple responses were allowed (if several fragments of the song were heard). • Rating of confidence (1 to 100) about hearing the song.	• 14 participants (32%) pressed the button at least once. • Participants who pressed the button were higher in hallucination proneness than those who did not.
Hoskin, Hunter, and Woodruff (2014)	• Participants listened to sentences spoken in a neutral male voice. • In a pilot study, 72 sentence frames were divided into high and low semantic expectation sentences. • In half of the trials the last word was masked and in the other half it was replaced by white noise. • Signal to noise ratio was determined in a second pilot study to make the speech identifiable approximately 80% of the time. SNRs varied between -15 and -25. • Volume for auditory stimuli: approximately 70db. All auditory stimuli were normalized to the same root mean squared amplitude. • Total trials: 288 (6 blocks x 48 trials) • Task duration: more than 30 minutes (estimated for 6 blocks)	• Pilot study to determine semantic expectations: 31 individuals • Main study: 70 (university staff and students)	• Yes or no to indicate if there was speech in white noise. • Rating of response certainty ranged from 1 (uncertain) to 4 (certain). • It was not required to identify what the words meant.	• Under stress, high trait anxiety participants produced more false alarms. • Semantic expectation marginally increased false alarms.

Study	Task design	Participants	Responses	Results
Vercammen, de Haan, and Aleman (2008)	• Verbs and nouns (60-65dB) embedded in white noise (72dB) were used as target words. • Noise free probe words were presented 2 seconds after the target words. • Half of the probe words were the same as the target words. • Total trials: 50 • Task duration: around 5 minutes (estimated)	• 15 hallucinating patients • 15 non-hallucinating patients • 17 healthy individuals	• Five point rating scale was used ranging from certainly not to certainly to indicate whether the probe word was the same as the target	• Responses were converted to hits and false alarms • Signal detection sensitivity of patients was significantly lower than that of the control participants. • Hallucinating patients had higher sensitivity than non-hallucinating patients. • Only the hallucinating patients had significant positive response bias (i.e. they were more likely to report the target and probe stimuli as the same).
Bentall and Slade (1985)	• Each of the 15-second trials started with a 1-second tone followed by 1-second silence, 5 seconds of white noise or signal plus white noise, and 8 seconds of silence to record a response. • The word 'who' was used as the signal. • 50 noise and 50 signal plus noise trials were presented randomly so that the same trial did not occur more than three times in a row. • The researchers and an audio technician decided the signal-to-noise ratio. • Task duration: around 25 minutes	• Study 1: 10 high and 10 low psychosis prone individuals • Study 2: 10 schizophrenia patients with ongoing symptoms of hallucination at the time of the study; 10 schizophrenia	• Response on a rating scale ranging from 1 (sure of hearing no voice) to 5 (sure of hearing a voice)	• Study 1: High hallucination prone participants were more likely to report the presence of a signal (i.e. their positive response bias was higher) than low hallucination prone individuals. • Study 2: Hallucinating schizophrenic patients had higher positive response bias than non-

Study	Task design	Participants	Responses	Results
		patients— five of them had no history of hallucinations and five did not report hallucinations for at least 6 months prior to the time of the study.		hallucinating schizophrenic patients.
Barkus, Stirling, Hopkins, McKie, and Lewis (2007)	• Some 5-second white noise trials contained a 1-second androgynous voice in the middle of it. • The white noise trial was followed by a 3-second silent phase to allow participants to respond. • Total no. of trials: 60; 36 of them had the voice, 12 of which were clearly audible and the rest were at the threshold level. Each trial was repeated three times. • A hearing test was administered to a pilot sample to determine the amplitude of the voices. • Task duration: around 24 minutes.	• 63 university students.	• Yes or no responses to indicate whether the voice was present in white noise.	• High hallucination prone participants gave significantly more false alarms than low and average hallucination prone individuals.
Moseley, Fernyhough, and Ellison (2014)	• Each trial was 5 seconds long and was followed by a 3-second silence. • In 80 trials, a 1-second neutral androgynous voice (reading text from an instruction manual) embedded in the middle of 5-second white noise. • Only white noise was presented in 64 trials. • Four voice amplitudes that 100%, 75%, 50%, and 25% of the participants of a pilot sample consistently detected were used. • The trials were presented in a pseudo random order so that a trial was not repeated more than three times in a row.	• For the threshold task: 8 • For the main test: 30 (18-26 years of age)	Yes or no to indicate if a voice was present in white noise.	Increased activity in the left superior temporal gyrus associated with higher rate of false alarms.

Study	Task design	Participants	Responses	Results
	• The trials were divided into two blocks. • Task duration: around 25 minutes (including a 5-minute break between the blocks)			
Vercammen and Aleman (2010)	• 5-7 word sentences were used as stimuli. • The last words in 50 highly predictable sentences were those that 70% participants of a pilot sample used to complete the sentences. They were replaced by the words that none of the participants used to obtain 50 unpredictable sentences. Last words in all the sentences were masked by white noise. 50 more sentences were constructed using the same stimuli but replacing the last words with white noise. • The sound-to-noise level at which the target words were correctly identified in 70% of the trials in a pilot experiment was used in the final testing. • A standard audiometric test was used to ensure that each participants' auditory perception was adequate for the study. • Task length: not mentioned	• Pilot study: 28 (to determine the predictability of the last words). • Main study: 42 undergraduate students. • No description was provided about the participants who were tested to determine the sound-to-noise level.	Participants pressed a button if they heard a word and said the word loud if they were sure about its identity, otherwise they identified the response as unsure.	High hallucination prone participants were more likely to hear words that fit a sentence when they were not presented.
Randell et al. (2011)	• 6 white noise only trials. • 2 trials containing concrete words embedded in white noise. • 2 trials containing abstract words embedded in white noise. • There were 30-second breaks in between the trials. • Counterbalanced presentation of abstract and concrete word pairs in the middle of four successive white noise trials. • Task duration: 14.5 minutes	• 41 undergraduates	After each trial, participants tick a response sheet if they heard a voice and recorded what they heard if they were sure about it.	• High unusual experience scorers heard more words that were not present than low unusual experience scorers. • High unusual experience participants reported more abstract than concrete type hallucinations.

Study	Task design	Participants	Responses	Results
Galdos et al. (2011)	• 25 white noise only trials • 25 clearly audible neutral speech embedded in white noise trials • 25 barely audible neutral speech embedded in white noise trials • Task duration: 15 minutes	• 30 patients • 28 siblings of patients • 307 controls	After each trial, participants pressed one of five response buttons to indicate the affective salience of a voice heard: 1 for positive voice, 2 for negative voice, 3 for neutral voice, 4 for no speech, and 5 for uncertain.	• Patients reported more speech illusions than controls. • Speech illusion in the controls and siblings was strongly associated with positive schizotypy but not with negative schizotypy. • The rate of speech illusion increased as the familial risk for psychotic disorder increased.
Hoffman et al. (1995)	• Participants were presented with 10 continuous spoken narratives (five in male and five in female voice) composed of 90-135 words and selected from fiction and popular magazines. • Phonetic noise was used to mask the spoken narratives. To create the meaningless noise, speech sounds of 6 males and 6 females reading different neutral scientific texts were superimposed. • The spoken narratives were presented in low noise, moderate babble, and high babble conditions. • Signal-to-noise ratio was determined in a pilot study. • Four narratives (two in male and two in female voice) were presented in low noise condition in which the signal exceeded noise by 4dB. • Three narratives (one in male and two in female voice) in moderate babble condition had the mean	• 12 healthy individuals • 17 patients with schizophrenia who reported hearing voices during the week prior to testing. • 14 patients with schizophrenia who did not hear voices during the week prior to testing.	• Participants reported verbally what they heard while listening to the narratives. • In the noise only trial, participants repeated words if they heard any.	• Speech perception impairments as found in hallucinatory schizophrenic patients indicated reduced anatomical connectivity but enhanced neuronal activation. • The impairment was not found in schizophrenic patients without hallucinatory symptoms.

Study	Task design	Participants	Responses	Results
	signal-to-babble difference of -4.9dB. • Three narratives (two in male and one in female voice) in high babble condition had the mean signal-to-babble difference of -5.9dB. • Participants heard only noise for 1 minute after the narratives had been presented. • Length of the task was not mentioned and could not be determined from the description.			
Feelgood and Rantzen (1994)	• 1-second sections were randomly chosen from a 5-minute male voice played backward and then they were spliced together. • This was a one trial task.	12 high and 10 low psychosis prone individuals (selected from 136 first year psychology students).	Participants recorded words and phrases on a response sheet that they heard in the voice.	The high hallucination prone participants reported meaningful auditory experiences more than the lows.
Ilankovic et al. (2011)	• Auditory stimuli: 192 personal adjectives (96 in participants own voice and 96 in unfamiliar male or female voice as recorded before carrying out the main task) • Volume of all the voices was normalized. • Half of own and alien speech was distorted by shifting the pitch by -4 semitones. • In valid trials, a picture cue of participant's own face was followed by his or her own voice. Similarly, an alien picture was followed by alien speech. • In invalid trials, an alien face was followed by a participant's own voice and his or her picture was followed by an alien speech. • In unpredictive cue condition, cue validity was 50%. • In predictive cue condition, cue validity was 80%.	• 23 paranoid schizophrenic patients. • 23 healthy volunteers.	After each trial, participants pressed one of three response buttons: 1 to indicate own voice, 2 to indicate alien voice, and 3 to indicate uncertainty.	• Distorted self-spoken words produced response errors (i.e. misidentifying own speech as produced by others) more in patients than in controls. • Patients made more errors across all the invalid cue conditions.

Study	Task design	Participants	Responses	Results
	• There were two runs (one for the predictive and the other for the unpredictive cue condition) of 192 trials. • Task length: approximately 30 minutes.			

Appendix Q: Advert I for Study 3

MANCHESTER 1824
The University of Manchester

RESEARCH PARTICIPANTS NEEDED

School of Psychological Sciences

Title of the Research: Voice Detection Study

About the Experiment

We are looking for participants for a 30-minute experiment. As part of the experiment, you will be asked to fill in some questionnaires and complete a simple perceptual task relating to hearing of voices.

To take part in this study you must satisfy the following criteria

- Be aged between 18 to 40
- Understand instructions in English well
- Not having any medical conditions that might affect the sense of hearing
- Did not take part in the previous learning and tactile perception study

Compensation

Undergraduate students in psychology are eligible to receive 2 research participation credits at the end of the testing session. For others, it is a voluntary participation (i.e. financial or other benefits are not available).

Name of the research ethics committee:

University of Manchester Ethics Committee 4

Ethics Approval Number: 14329

If you are interested to take part in the experiment, please email mdakibul.huque@manchester.ac.uk

MANCHESTER 1824
The University of Manchester

Appendix R: Participant information sheet I for Study 3

Title of the Research: Individual Differences in Voice Detection Study

Introduction

You are invited to take part in a research study about detection of voices. Please read this information sheet carefully so that you are able to make an informed decision about whether or not to participate in this study. If anything is unclear or you have any questions about the research, you are very welcome to contact me (see the contact details at the end of this form) and I will do my best to provide the information you need.

What is the aim of the research?

The main objective of the present study is to investigate individual differences in the detection of voices and how these relate to people's everyday sensory experiences

Why have I been chosen?

Anyone from the University of Manchester aged between 18-40 can participate in this study. Our aim is to collect data from at least 85 individuals. **Please note that you are not eligible for this study if you (i) took part in the previous experiments on learning and tactile perception and (ii) have any medical conditions that might affect the sense of hearing.**

What would I be asked to do if I took part?

If you decide to take part in this study, you will be asked to attend a 30-minute testing session that comprises a simple voice detection task, which involves detecting voices embedded intermittently within headphone-presented white noise. You will also be asked to complete four questionnaires. Two of them are about physical symptoms (e.g. headache) that you might experience right now, at this moment or might have experienced in the past month and the other two are about your thoughts, feelings, and behaviours.

During the experiment, you will also be asked to sit in the same place for much of the testing session, although you will have the opportunity to take breaks if you wish. At the end of the study, we shall ask you few questions to know your thoughts and beliefs about the tasks you have performed. We may note down your answers for our record, which we might use later only to interpret and understand study findings but not to publish as a verbatim report.

Will my data be confidential?

Yes, we will manage all of your data in a secure way to ensure that your confidentiality is protected. We will do this by ensuring that:

- Any printed copies of your personal information (such as your name, contact address, and date of birth) will be kept in a locked file cabinet on University premises.
- None of your experimental data will be stored in the same place as any personal information. We do need to know who provided what data (to ensure that data from the tasks and questionnaires are identified as coming from the same person), but we will do this by using a unique identification code rather than personal information. Any documents linking personal information to identification codes will be kept separate from the data itself and stored in a locked file cabinet on University premises.
- Any electronic files containing personal information will be encrypted.
- Only the research team will have access to data.

What happens if I do not want to take part or if I change my mind?

It is entirely up to you whether you take part or not. If you do decide to take part, you are free to withdraw at any point without having to give a reason.

What are the benefits and risks to taking part in the study?

If you are a student of psychology, you are eligible to receive 2 research participation credits which will be available at the end of the testing session. For others, it is completely voluntary and there are no monetary or other benefits available as reimbursement.

It is very unlikely that the present research will cause any physical or psychological harm to you. The experimental procedures are simple and benign in nature and have been used in a number of studies with ethical approval from the university.

The questionnaires that will be used in the present experiment are highly regarded by both clinicians and researchers around the world. There is no known potential risk in using them, although there is a very small chance that some participants will be mildly upset answering questions about their physical health (e.g. During the past 4 weeks, how much have you been bothered by stomach pain, back pain, dizziness, trouble sleeping, etc. or whether you are experiencing symptoms such as headache, watering eyes, racing heart, etc. right now, at this moment), thoughts and perceptual experiences (e.g. Please answer each item true or false: sometimes people whom I know well begin to look like strangers, I sometimes have had the feeling that my body is abnormal, no matter how hard I try to concentrate, unrelated thoughts always creep into my mind, sometimes my thoughts seem as real as actual events in my life and so on).

Where will the research be conducted?

The research will be carried out in labs either in the Coupland or Zochonis Building of the University of Manchester.

Will the outcomes of the research be published?

The outcomes of the research will be submitted for publication in peer-reviewed journals. However, the papers will not contain your name or address. If you want, we will send you a summary of the findings after the final data analysis.

What if something goes wrong?

We do not expect anything to go wrong. In the very unlikely event that something untoward does happen, you can contact me in the first instance. You can also contact my supervisor (richard.j.brown@manchester.ac.uk). If there are any issues regarding this research that you would prefer not to discuss with members of the research team, please contact the Research Governance and Integrity Team by either writing to 'The Research Governance and Integrity Manager, Research Office, Christie Building, The University of Manchester, Oxford Road, Manchester M13 9PL', by emailing: Research.Complaints@manchester.ac.uk, or by telephoning 0161 275 7583 or 275 8093.

Contact details of the researcher

Akib Ul Huque, PhD Student
School of Psychological Sciences, University of Manchester
Room S42, 2nd Floor Zochonis Building, Brunswick Street, Manchester M13 9PL
Email: mdakibul.huque@manchester.ac.uk

This project has been approved by the
University of Manchester Research Ethics Committee 4 (Ref. 14329)

MANCHESTER 1824
The University of Manchester

Appendix S: Consent form for Study 3

Project Title: Voice Detection Study

CONSENT FORM

If you are happy to participate please complete and sign the consent form below.

Please initial box

1. I confirm that I have read the attached information sheet on the above project and have had the opportunity to consider the information and ask questions and had these answered satisfactorily.	
2. I understand that my participation in the study is voluntary and that I am free to withdraw at any time without giving a reason and without detriment to any treatment/service.	
3. I understand that the researcher will note down interview answers to interpret study findings but not to publish as a verbatim report.	
4. I agree that any data collected may be passed as anonymous data to other researchers.	
5. I give consent to store my data for use in future studies.	

I agree to take part in the above project.

Name of participant	Date	Signature
Name of person taking consent	Date	Signature

Appendix T: Launay-Slade hallucination scale

Items	Certainly applies	Possibly applies	Unsure	Possibly Does Not Apply	Certainly Does Not Apply
1. No matter how hard I try to concentrate, unrelated thoughts always creep into my mind.					
2. In my daydreams I can hear the sound of a tune almost as clearly as if I were actually listening to it.					
3. Sometimes my thoughts seem as real as actual events in my life.					
4. Sometimes a passing thought will seem so real that it frightens me.					
5. The sounds I hear in my daydreams are generally clear and distinct.					
6. The people in my daydreams seem so true to life that I sometimes think they are.					
7. I often hear a voice speaking my thoughts aloud.					
8. In the past, I have had the experience of hearing a person's voice and then found that no one was there.					
9. On occasions, I have seen a person's face in front of me when no one was in fact there.					
10. I have heard the voice of the devil.					
11. In the past, I have heard the voice of God speaking to me.					
12. I have been troubled by hearing voices in my head.					

Appendix U: Perceptual aberration scale

Please answer each item true or false. Please do not skip any items. It is important that you answer every item, even if you are not quite certain which is the best answer. An occasional item may refer to experiences that you have had only when taking drugs. Unless you have had the experience at other times (when not under the influence of drugs), mark it as if you have not had that experience.

Some items may sound like others, but all of them are slightly different. Answer each item individually, and don't worry about how you answered a somewhat similar previous item.

Circle either "True" or "False":

True False 1. I sometimes have had the feeling that some parts of my body are not attached to the same person.

True False 2. Occasionally I have felt as though my body did not exist.

True False 3. Sometimes people whom I know well begin to look like strangers.

True False 4. My hearing is sometimes so sensitive that ordinary sounds become uncomfortable.

True False 5. Often I have a day when indoor lights seem so bright that they bother my eyes.

True False 6. My hands or feet have never seemed far away.

True False 7. I have sometimes felt confused as to whether my body was really my own.

True False 8. Sometimes I have felt that I could not distinguish my body from other objects around me.

True False 9. I have felt that my body and another person's body were one and the same.

True False 10. I have felt that something outside my body was a part of my body.

True False 11. I sometimes have had the feeling that my body is abnormal.

True False 12. Now and then, when I look in the mirror, my face seems quite different than usual.

True False 13. I have never had the passing feeling that my arms or legs have become longer than usual.

True False 14. I have sometimes felt that some part of my body no longer belongs to me.

True False 15. Sometimes when I look at things like tables and chairs, they seem strange.

True False 16. I have felt as though my head or limbs were somehow not my own.

True False 17. Sometimes part of my body has seemed smaller than it usually is.

True False 18. I have sometimes had the feeling that my body is decaying inside.

True False 19. Occasionally it has seemed as if my body had taken on the appearance of another person's body.

True False 20. Ordinary colors sometimes seem much too bright to me.

True False 21. Sometimes I have had a passing thought that some part of my body was rotting away.

True False 22. I have sometimes had the feeling that one of my arms or legs is disconnected from the rest of my body.

True False 23. It has seemed at times as if my body was melting into my surroundings.

True False 24. I have never felt that my arms or legs have momentarily grown in size.

True False 25. The boundaries of my body always seem clear.

True False 26. Sometimes I have had feelings that I am united with an object near me.

True False 27. Sometimes I have had the feeling that a part of my body is larger than it usually is.

True False 28. I can remember when it seemed as though one of my limbs took on an unusual shape.

True False 29. I have had the momentary feeling that my body has become misshapen.

True False 30. I have had the momentary feeling that the things I touch remain attached to my body.

True False 31. Sometimes I feel like everything around me is tilting.

True False 32. I sometimes have to touch myself to make sure I'm still there.

True False 33. Parts of my body occasionally seem dead or unreal.

True False 34. At times I have wondered if my body was really my own.

True False 35. For several days at a time I have had such a heightened awareness of sights and sounds that I cannot shut them out.

Appendix V: Positive and negative affect scale (PANAS)

This scale consists of a number of words that describe different feelings and emotions. Read each item and then mark the appropriate answer in the space next to that word. Indicate to what extent **you generally feel this way, that is, how you feel on the average.**

1	2	3	4	5
very slightly or not at all	a little	moderately	quite a bit	extremely

_____ interested	_____ irritable
_____ distressed	_____ alert
_____ excited	_____ ashamed
_____ upset	_____ inspired
_____ strong	_____ nervous
_____ guilty	_____ determined
_____ scared	_____ attentive
_____ hostile	_____ jittery
_____ enthusiastic	_____ active
_____ proud	_____ afraid

Appendix W: Pennebaker symptom checklist (A state measure)

Right now, at this moment, I am experiencing:

e.g. ___ ___ ___ ✓ ___ ___ ___

No cold hands — Cold hands

0	1	2	3	4	5	6
No headache						Headache
0	1	2	3	4	5	6
No watering eyes						Watering eyes
0	1	2	3	4	5	6
No racing heart						Racing heart
0	1	2	3	4	5	6
No congested nose						Congested nose
0	1	2	3	4	5	6
No tense muscles						Tense muscles
0	1	2	3	4	5	6
No upset stomach						Upset stomach
0	1	2	3	4	5	6
No flushed face						Flushed face
0	1	2	3	4	5	6
No sweaty hands						Sweaty hands
0	1	2	3	4	5	6
No shortness of breath						Shortness of breath
0	1	2	3	4	5	6
No cold hands						Cold hands
0	1	2	3	4	5	6
No dizziness						Dizziness
0	1	2	3	4	5	6
No ringing in ears						Ringing in ears
0	1	2	3	4	5	6
No pain						Pain
0	1	2	3	4	5	6
No fatigue						Fatigue

Appendix X: Secondary analysis of Study 4

X.1 Effects of Light on SSDT Response Outcomes

X.1.1 False Alarm Rate

The main effect of light was significant, $F(1, 73) = 5.91$, $p < .05$, $\eta_p^2 = .08$, so was the interaction effect between phase, condition, and light, $F(2, 146) = 3.13$, $p < .05$, $\eta_p^2 = .04$. Bonferroni corrected post hoc tests indicate that light produced significantly more false alarms in the manipulation phase of the control condition (mean difference between the false alarm rates in light present and light absent conditions = .22; 95% CI = .12, .33; $p < .001$). Statistics on non-significant interactions are presented in Table X1.

X.1.2 Hit Rate

The main effect of light was significant, $F(1, 73) = 55.13$, $p < .001$, $\eta_p^2 = .43$. Also the interaction between phase and light was significant, $F(2, 146) = 9.53$, $p < .001$, $\eta_p^2 = .12$. Bonferroni corrected post hoc tests indicate that the light did not have any effect on the baseline hit rate (mean difference between the hit rates in light present and light absent condition = .01; 95% CI = -.003, .03; $p = .109$). However, the hit rate was significantly higher in the light present condition both in the manipulation and follow-up phases (mean difference = .05; 95% CI = .03, .06; $p < .001$, and mean difference = .04; 95% CI = .03, .06; $p < .001$, respectively). Statistics on non-significant interactions are presented in Table X1.

X.1.3 Sensitivity

The main effect of light was significant, $F(1, 73) = 38.90$, $p < .001$, $\eta_p^2 = .35$. Bonferroni corrected post hoc tests show that sensitivity in light present condition was significantly higher than that in the light absent condition (mean difference = .01; 95% CI = .01, .02; $p < .001$). Statistics on non-significant interactions are presented in Table X1.

X.1.4 Response Bias

The main effect of light was significant, $F(1, 73) = 38.90$, $p < .001$, $\eta_p^2 = .35$. The interaction between phase and light was also significant, $F(1, 73) = 4.89$, $p < .01$, $\eta_p^2 = .06$. Bonferroni corrected post hoc tests indicate that in these phases

participants were more likely to say yes (i.e. their response criterion became more liberal) in light present trials [mean difference in bias between light absent and light present trials = .04 (95% CI = .01, .06), .08 (95% CI = .05, .10), and .07 (95% CI = .05, .09) respectively for the baseline, manipulation and follow-up phases (all *p*s < .01)]. Statistics on non-significant interactions are presented in Table X1.

X.2 State Anxiety

Data on state anxiety were examined and found that they satisfied the assumptions of Mixed ANOVA. The statistical analysis (with state anxiety as the within-group independent variable and condition as the between-group independent variable) demonstrates that none of the effects were significant (see Table X1).

X.3 Sleepiness

Data on sleepiness violated the assumption of sphericity for Mixed ANOVA (ε = .87, $p < .01$). As the sphericity estimate was greater than .75, Huynh-Feldt correction was used for the analysis (Girden as cited in Field, 2009). Mixed ANOVA (with sleepiness as the within-group independent variable and condition as the between-group independent variable) indicates that the main effect of time (i.e. sleepiness before the baseline vs. before the manipulation vs. before the follow-up phases) was significant, $F(1.83, 133.85) = 42.08$, $p < .001$, $\eta_p^2 = .37$. Bonferroni corrected post hoc tests indicate that sleepiness before the manipulation and follow-up was significantly higher than that in the baseline (mean difference in sleepiness between time 2 and time 1 = 1.87; 95% CI = 1.36, 2.37; $p < .001$, and between time 3 and time 1 = 1.44; 95% CI = .83, 2.04; $p < .001$). Sleepiness before the manipulation phase did not differ significantly from that before the follow-up phase (mean difference = .43; 95% CI = -.02, .87; $p = .06$). Statistics on non-significant interactions are presented in Table X1.

X.4 Correlational Analysis

Correlation of state anxiety and sleepiness with the SSDT response outcomes, SPS variable, and VHT responses was determined. Spearman's correlation (see Tables X2 and X3) was calculated as a number of variables were non-normal. Alpha values were Bonferroni corrected to avoid Type I error. Thus, the α was .002 for the SSDT

Table X1

Non-Significant Main and Interaction Effects of Light on the SSDT Response Outcomes, and State Anxiety and Sleepiness

Main and interaction effects	*F*	*df*	*p*	η^2_p
False alarm rate				
Light X Phase	1.67	2, 146	.19	.02
Light X Condition	1.73	1, 73	.19	.02
Hit rate				
Light X Condition	.81	1, 73	.37	.01
Light X Phase X Condition	2.23	2, 146	.11	.03
Response bias				
Light X Condition	.73	1, 73	.40	.01
Light X Phase X Condition	1.24	2, 146	.29	.02
Sensitivity				
Light X Phase	1.45	2, 146	.24	.02
Light X Condition	.22	1, 73	.64	.003
Light X Phase X Condition	2.10	2, 146	.13	.03
State anxiety				
Main effect of time	.22	1, 73	.64	.003
Main effect of condition	.47	1, 73	.50	.01
Time X Condition	.46	1, 73	.50	.01
Sleepiness				
Main effect of condition	.40	1, 73	.53	.01
Time X Condition	2.75	2, 146	.07	.04

variables and .01 for both the SPS measures and VHT responses. Two participants in the baseline and one participant in the follow-up phase of the control condition did not report any SPS. Five participants in both the control and experimental conditions did not report any SPS in the experimental condition. These participants were excluded while calculating correlation coefficients for SPS pleasantness and certainty. Thus in

these analyses, there were 39 and 40 participants in the baseline and follow-up phases of the control condition, and 29 participants in both the phases of the experimental condition. The remaining correlational analyses were carried out with 41 and 34 participants in the control and experimental conditions respectively.

A significant negative correlation was found between SPS pleasantness in the follow-up phase of the control condition and state anxiety measured before the same phase, r = .39, p =.01, meaning that the higher was the state anxiety before the follow-up phase in the control condition the less pleasant were the SPSs felt in that phase. In the control condition, there were significant positive correlations between sleepiness before the follow-up phase and total voices, r = .40, p =.01, and false alarms, r = .43, p <.01, in the baseline. This means that in the control condition, the more voices or voice false alarms participants reported in the baseline phase the sleepier they were found before the follow-up phase. There was no significant interaction between the variables in the experimental phase.

Table X2

Summary of Medians and Interquartile Ranges (IQR) for Sleepiness and State Anxiety and their Correlations with SPS Measures and SSDT Response Outcomes in the Control Condition

Variables	Sleepiness			State anxiety	
	Before the baseline phase	Before the manipulation phase	Before the follow-up phase	Before the baseline phase	Before the follow-up phase
Baseline SPS					
Total number	.04	-.07	.07	-.17	-.23
Intensity	-.12	-.05	-.03	.24	.09
Pleasantness	.24	.27	.28	-.18	-.37
Certainty	-.11	-.19	-.08	-.17	-.31
Follow-up SPS					
Total number	-.01	.17	.16	-.07	-.02
Intensity	-.07	.20	.02	.25	.21
Pleasantness	.13	.02	.03	-.16	**-.39***

(continued)

Variables	Sleepiness			State anxiety	
	Before the baseline phase	Before the manipulation phase	Before the follow-up phase	Before the baseline phase	Before the follow-up phase
Certainty	-.11	.20	.16	-.13	-.14
Baseline VHT					
Total voices	.25	.07	**.40***	-.18	-.33
Hits	-.07	-.37	-.06	-.13	-.07
False alarms	.37	.35	**.43****	-.12	-.27
Follow-up VHT					
Total voices	.25	-0.05	.09	.00	-.20
Hits	.02	-0.10	.08	-.13	-.18
False alarms	.27	0.09	.20	.13	-.09
SSDT response outcomes in the baseline phase					
Hit rate in light absent trials	-.03	-.13	.05	-.25	.01
Hit rate in light present trials	-.06	-.14	.00	-.12	.09
False alarm rate in light absent trials	.01	-.18	-.22	-.01	.09
False alarm rate in light present trials	.24	.07	.07	.02	.01
Sensitivity in light absent trials	-.04	-.02	.22	-.27	-.03
Sensitivity in light present trials	-.15	-.15	-.05	-.19	.09
Bias in light absent trials	.05	.21	.10	.20	-.03
Bias in light present trials	-.10	.05	-.04	.07	-.07
SSDT response outcomes in the manipulation phase					
Hit rate in light absent trials	-.05	-.21	-.15	-.10	.09
Hit rate in light present trials	.02	-.10	-.04	-.17	-.01
False alarm rate in light absent trials	.08	.05	-.04	-.14	-.40
False alarm rate in light present trials	.07	.07	-.01	-.21	-.36

(continued)

Variables	Sleepiness			State anxiety	
	Before the baseline phase	Before the manipulation phase	Before the follow-up phase	Before the baseline phase	Before the follow-up phase
Sensitivity in light absent trials	-.03	-.21	-.09	-.08	.25
Sensitivity in light present trials	.01	-.10	-.02	-.04	.21
Bias in light absent trials	.11	.21	.21	.14	.13
Bias in light present trials	-.06	.12	.17	.24	.20
SSDT response outcomes in the follow-up phase					
Hit rate in light absent trials	.00	-.17	-.12	-.15	-.02
Hit rate in light present trials	.09	-.11	-.09	-.26	-.10
False alarm rate in light absent trials	.00	.00	-.06	.00	-.09
False alarm rate in light present trials	.06	.14	-.04	-.08	-.27
Sensitivity in light absent trials	.09	-.08	.00	-.32	-.07
Sensitivity in light present trials	.09	-.16	.00	-.23	.07
Bias in light absent trials	.06	.12	.14	.10	.02
Bias in light present trials	-.04	.05	.14	.19	.17
Median	5	7	7	9	9
IQR	3 to 6	5.5 to 8	5 to 8	7.5 to 11	8 to 11

Note. SPS = spontaneous sensation; VHT = Voice Hearing-Task; SSDT = Somatosensory Signal Detection Task.

$^{*}p = .01$. $^{**}p < .01$

Table X3

Summary of Medians and Interquartile Ranges (IQR) for Sleepiness and State Anxiety and their Correlations with SPS Measures and SSDT Response Outcomes in the Experimental Condition

Variables	Sleepiness			State anxiety	
	Before the baseline phase	Before the manipulation phase	Before the follow-up phase	Before the baseline phase	Before the follow-up phase
Baseline SPS					
Total number	.10	-.05	-.30	-.09	-.02
Intensity	-.10	-.17	-.25	.07	-.02
Pleasantness	.31	.21	.04	-.17	.17
Certainty	-.15	-.22	-.41	.18	.03
Follow-up SPS					
Total number	.35	.01	-.21	.10	.31
Intensity	.24	-.15	-.16	.28	.09
Pleasantness	-.29	.12	-.13	.15	.21
Certainty	.19	-.33	-.30	.28	-.02
Baseline VHT					
Total voices	-.13	.02	.09	-.07	.07
Hits	-.03	-.20	-.12	.12	.17
False alarms	-.06	.34	.27	-.17	-.03
Follow-up VHT					
Total voices	-.20	-.08	.04	.03	.06
Hits	.06	-.34	-.16	.07	.04
False alarms	-.19	.07	.05	-.10	.02
SSDT response outcomes in the baseline phase					
Hit rate in light absent trials	-.23	-.34	-.22	-.34	-.42
Hit rate in light present trials	-.22	-.30	-.20	-.30	-.34
False alarm rate in light absent trials	-.08	.13	.21	-.15	-.14

(continued)

Variables	Sleepiness			State anxiety	
	Before the baseline phase	Before the manipulation phase	Before the follow-up phase	Before the baseline phase	Before the follow-up phase
False alarm rate in light present trials	-.08	.34	.36	.05	.05
Sensitivity in light absent trials	-.15	-.39	-.32	-.25	-.34
Sensitivity in light present trials	-.12	-.33	-.26	-.32	-.34
Bias in light absent trials	.28	.22	.11	.34	.40
Bias in light present trials	.28	.19	.08	.29	.32
SSDT response outcomes in the manipulation phase					
Hit rate in light absent trials	.10	.06	-.16	-.37	-.42
Hit rate in light present trials	.05	.05	-.11	-.24	-.23
False alarm rate in light absent trials	.08	.26	.12	.05	-.09
False alarm rate in light present trials	.13	.24	.11	.07	.00
Sensitivity in light absent trials	-.05	-.15	-.20	-.24	-.12
Sensitivity in light present trials	-.08	-.13	-.11	-.23	-.14
Bias in light absent trials	-.11	-.32	-.05	.22	.40
Bias in light present trials	-.09	-.35	-.05	.07	.13
SSDT response outcomes in the follow-up phase					
Hit rate in light absent trials	-.46	-.16	-.07	-.09	-.25
Hit rate in light present trials	-.23	.00	.03	.02	-.18
False alarm rate in light absent trials	.01	.18	.05	-.01	-.09
False alarm rate in light present trials	.02	.19	.14	.15	.13
Sensitivity in light absent trials	-.22	-.16	-.03	.05	.01
Sensitivity in light present trials	-.08	-.01	-.01	-.10	-.12

(continued)

Variables	Sleepiness			State anxiety	
	Before the baseline phase	Before the manipulation phase	Before the follow-up phase	Before the baseline phase	Before the follow-up phase
Bias in light absent trials	.32	-.01	.05	.11	.24
Bias in light present trials	.09	-.23	-.15	-.07	.04
Median	5	7	6.5	9	10
IQR	2.75 to 6	5 to 8	3 to 7	8 to 11	8.75 to 12

Note. SPS = spontaneous sensation; VHT = Voice Hearing-Task; SSDT = Somatosensory Signal Detection Task.

Appendix Y: The mini-psychosis proneness scale

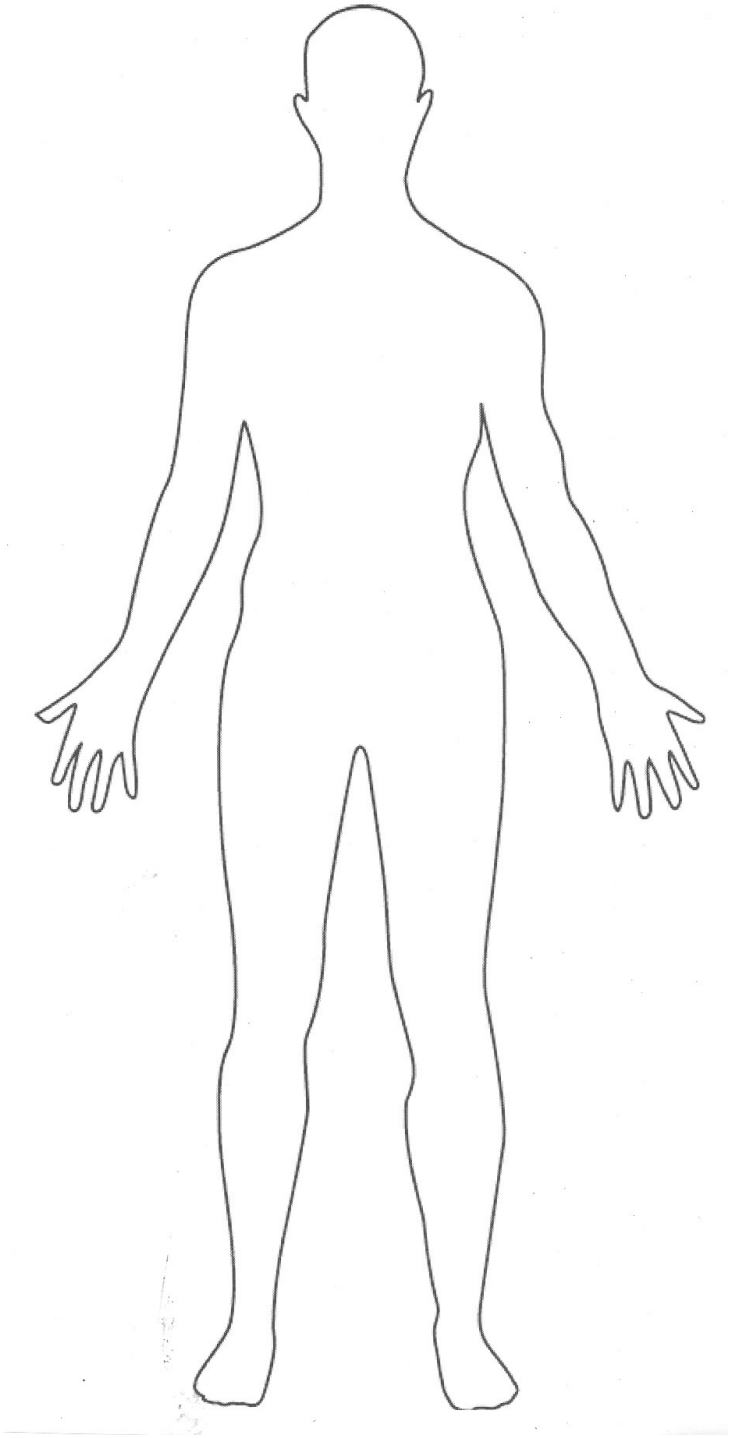
Following are some statements about beliefs, feelings, and behaviours. Please encircle T if the statement is true and F if it is false for you. Please do not skip any items. It is important that you answer every item, even if you are not quite certain which is the best answer.

1. Sometimes part of my body seems smaller than it really is.	T	F
2. I have wondered whether the spirits of the dead can influence the living.	T	F
3. In unfamiliar surroundings, I am sometimes so assertive and sociable, that I surprise myself.	T	F
4. It would embarrass me a lot to have to spend a night in jail.	T	F
5. Although there are things that I enjoy doing myself, I usually seem to have more fun when I do things with other people.	T	F
6. Trying new foods is something I have always enjoyed.	T	F
7. Sometimes I feel like everything around me is tilting.	T	F
8. I have felt that I might cause something to happen just by thinking too much about it.	T	F
9. I often have moods where I feel so energetic and optimistic that I feel I could outperform almost anyone or anything.	T	F
10. I usually find myself doing things "on impulse".	T	F
11. I prefer hobbies and leisure activities that do not involve other people.	T	F
12. I seldom care to sing in the shower.	T	F

Appendix Z: Body figure

Encircel the body areas where you felt sesations. Also, name the sesations that you identified.

Right



Left

Appendix AA: Secondary analysis of Study 5

AA.1 SSDT Light Condition

AA.1.1 False Alarm Rate

The main effect of light was significant, $F(1, 74) = 69.47$, $p < .001$, $\eta_p^2 = .48$. False alarm rate in the light present condition was significantly higher than in the light absent condition, mean difference = .25, 95% CI [.19, .30], $p < .001$. Non-significant results are presented in Table AA 1.

AA.1.2 Hit Rate

There were a significant main effect of light, $F(1, 74) = 97.92$, $p < .001$, $\eta_p^2 = .57$ and an interaction between light and condition, $F(1, 74) = 9.36$, $p < .01$, $\eta_p^2 = .11$. Bonferroni corrected post hoc tests indicate that the hit rates of both control and experimental participants in the light present condition was significantly higher than that in the light absent condition, mean differences between the hit rates in light present and light absent trials were .17, 95% CI [.13, .20], $p < .001$, and .09, 95% CI [.05, .12], $p < .001$, in control and experimental conditions respectively. In light absent trials, hit rates did not differ between the control and experimental conditions, mean difference = .07, 95% CI [-.04, .18], $p = .20$. In light present trials, however, hit rates were significantly higher in the control than in the experimental condition, mean difference = .15, 95% CI [.05, .25], $p < .005$. Non-significant results are presented in Table AA 1.

AA.1.3 Response Bias

The main effect of light was significant, $F(1, 74) = 82.72$, $p < .01$, $\eta_p^2 = .53$, as was the interaction between light and condition, $F(1, 74) = 6.84$, $p = .01$, $\eta_p^2 = .09$. Bonferroni corrected post hoc tests indicate that both control and experimental participants were more lenient in reporting the presence of vibration when light was present than when it was absent, mean differences between response bias in light absent and light present trials were .16, 95% CI [.12, .20], $p < .001$, and .09, 95% CI [.05, .13], $p < .001$, in control and experimental conditions respectively. In both light absent and light present trials, participants were more stringent in responding the "yes" response in the experimental than in the control condition, mean differences

Table AA 1

Non-Significant Main and Interaction Effects of Light on the SSDT Response Outcomes, and State Anxiety and Sleepiness

Main and interaction effects	*F*	*df*	*p*	η^2_p
False alarm rate				
Light X Phase	1.36	2, 148	.26	.02
Light X Condition	.67	1, 74	.42	.01
Light X Phase X Condition	1.09	2, 148	.34	.02
Hit rate				
Light X Phase	.23	2, 148	.79	.003
Light X Phase X Condition	3.39	2, 148	.04	.04
Response bias				
Light X Phase	.03	2, 148	.97	< .001
Light X Phase X Condition	3.44	2, 148	.04	.04
Sensitivity				
Light	.29	1, 74	.60	.004
Light X Phase	.11	2, 148	.90	.001
Light X Condition	2.05	1, 74	.16	.03
Light X Phase X Condition	.25	2, 148	.78	.003
State anxiety				
Main effect of time	.85	1, 74	.36	.01
Main effect of condition	.37	1, 74	.54	.01
Time X Condition	1.40	1, 74	.24	.02
Sleepiness				
Main effect of condition	2.20	1, 74	.14	.03
Time X Condition	2.73	2, 148	.08	.04

between response bias in the experimental and control conditions were .11, 95% CI [.04, .18], $p < .005$, and .18, 95% CI [.12, .24], $p < .001$, in the light absent and light present conditions respectively. Non-significant results are presented in Table AA 1.

AA.1.4 Sensitivity

None of the main and interaction effects was significant. They are presented in Table AA 1.

AA.2 State Anxiety

Data on state anxiety satisfied the assumptions for Mixed ANOVA. The analysis (with state anxiety as the within-group independent variable and condition as the between-group independent variable) found the effects non-significant, which are presented in Table AA 1.

AA.3 Sleepiness

Data on sleepiness violated the assumption of sphericity for Mixed ANOVA (ε = .75, $p < .01$). As the sphericity estimate was equal to .75, Huynh-Feldt correction was used for the analysis (Girden as cited in Field, 2009). It was found (with sleepiness as the within-group independent variable and condition as the between-group independent variable) that the main effect of time (i.e. sleepiness at time 1 or before the baseline phase, time 2 or before the manipulation phase, and time 3 or before the follow-up phase) was significant, $F(1.65, 122.38) = 23.69$, $p < .001$, $\eta_p^2 = .24$. Bonferroni corrected post hoc tests indicate that sleepiness before the SSDT manipulation phase was stronger than sleepiness before the baseline, mean difference = 1.53, 95% CI [1.11, 1.95], $p < .001$, and follow-up phases, mean difference = .63, 95% CI [.09, 1.17], $p < .05$. Sleepiness before the follow-up phase was greater than that before the baseline phase, mean difference = .91, 95% CI [.25, 1.57], $p < .01$. The other effects were not significant, which are presented in Table AA 1.

AA.4 Correlational Analysis

Because of non-normality of some of the variables, Spearman's correlation was used to determine the relationships (see Tables AA2 and AA3) of state anxiety and sleepiness with SSDT response outcomes, VHT responses, and SPS measures. To avoid family wise error rates (i.e. Type I errors), alpha values were Bonferroni corrected. Thus the α was .002 for the SSDT variables and .01 for both the SPS measures and VHT responses. Two control and three experimental participants in the

baseline phase and four control and two experimental participants in the follow-up phase did not report any SPS. Therefore, they were excluded when correlational analyses were carried out for SPS pleasantness and certainty in their respective phases. For the rest of the analyses, there were 37 and 39 participants in the control and experimental conditions respectively.

In the experimental condition, significant positive correlation coefficients were found between state anxiety before the follow-up phase and SPS intensity both in the baseline and follow-up phases (see Table AA3). In other words, the higher was the state anxiety before the follow-up phase the more intense SPSs were felt both in the baseline and follow-up phases of the experimental condition. The other correlation coefficients were not significant (see Tables AA2 and AA3).

Table AA2

Summary of Medians and Interquartile Ranges (IQR) for Sleepiness and State Anxiety and their Correlations with SPS Measures and SSDT Response Outcomes in the Control Condition

Variables	Sleepiness			State anxiety	
	Before the baseline phase	Before the manipulation phase	Before the follow-up phase	Before the baseline phase	Before the follow-up phase
Baseline SPS					
Total number	-.11	-.12	.03	-.08	.01
Intensity	-.14	-.13	.10	.19	.14
Pleasantness	-.06	-.15	-.10	.02	.05
Certainty	-.16	-.22	.13	-.09	-.21
Follow-up SPS					
Total number	-.11	-.20	.05	-.06	-.06
Intensity	-.21	-.23	.07	.11	.15
Pleasantness	-.04	-.06	-.03	-.16	-.34
Certainty	-.08	-.09	-.08	-.11	.04
Baseline VHT					
Total voices	.17	.03	-.08	.17	.30

(continued)

Variables	Sleepiness			State anxiety	
	Before the baseline phase	Before the manipulation phase	Before the follow-up phase	Before the baseline phase	Before the follow-up phase
Hits	-.12	-.28	-.25	.04	.20
False alarms	.36	.30	.07	.17	.15
Follow-up VHT					
Total voices	.00	.01	-.23	.09	.21
Hits	-.08	-.04	-.19	.06	.16
False alarms	.19	.11	-.17	.04	.07
SSDT response outcomes in the baseline phase					
Hit rate in light absent trials	.00	-.06	.02	-.30	-.32
Hit rate in light present trials	-.23	-.16	-.05	-.36	-.32
False alarm rate in light absent trials	.09	-.11	-.22	-.04	-.08
False alarm rate in light present trials	-.01	-.05	-.36	-.01	.02
Sensitivity in light absent trials	-.06	.01	.14	-.31	-.28
Sensitivity in light present trials	-.10	-.08	.13	-.36	-.36
Bias in light absent trials	-.02	.12	.08	.24	.29
Bias in light present trials	.18	.13	.15	.31	.26
SSDT response outcomes in the manipulation phase					
Hit rate in light absent trials	.03	-.16	-.12	-.21	-.29
Hit rate in light present trials	-.18	-.29	-.24	-.30	-.31
False alarm rate in light absent trials	.21	.04	-.03	.03	-.18
False alarm rate in light present trials	.00	.00	-.18	.15	.14
Sensitivity in light absent trials	-.08	-.16	-.13	-.27	-.25
Sensitivity in light present trials	-.09	-.22	-.07	-.32	-.33

(continued)

Variables	Sleepiness			State anxiety	
	Before the baseline phase	Before the manipulation phase	Before the follow-up phase	Before the baseline phase	Before the follow-up phase
Bias in light absent trials	-.11	.11	.14	.14	.29
Bias in light present trials	.18	.29	.35	.16	.18
SSDT response outcomes in the follow-up phase					
Hit rate in light absent trials	.02	-.12	-.14	-.37	-.34
Hit rate in light present trials	.00	-.13	.04	-.17	-.20
False alarm rate in light absent trials	.10	-.03	-.01	-.08	-.13
False alarm rate in light present trials	.03	.01	-.04	.28	.14
Sensitivity in light absent trials	-.13	-.23	-.21	-.33	-.27
Sensitivity in light present trials	-.02	-.09	.03	-.37	-.29
Bias in light absent trials	-.07	.08	.09	.27	.31
Bias in light present trials	-.04	.06	.02	-.02	.10
Median	5	7	7	9	10
IQR	3 to 6.5	6 to 8	5 to 7.5	8 to 11	8 to 12

Note. SPS = spontaneous sensation; VHT = Voice Hearing-Task; SSDT = Somatosensory Signal Detection Task.

Table AA3

Summary of Medians and Interquartile Ranges (IQR) for Sleepiness and State Anxiety and their Correlations with SPS Measures and SSDT Response Outcomes in the Experimental Condition

Variables	Sleepiness			State anxiety	
	Before the baseline phase	Before the manipulation phase	Before the follow-up phase	Before the baseline phase	Before the follow-up phase
Baseline SPS					
Total number	.00	.16	.15	.02	.27

(continued)

Variables	Sleepiness			State anxiety	
	Before the baseline phase	Before the manipulation phase	Before the follow-up phase	Before the baseline phase	Before the follow-up phase
Intensity	.12	.16	.07	-.02	**.40***
Pleasantness	-.14	-.15	-.21	-.18	-.18
Certainty	-.07	.09	.04	.03	.03
Follow-up SPS					
Total number	-.06	-.09	-.16	.04	.11
Intensity	.09	.12	.05	.20	**.40***
Pleasantness	.19	.18	-.02	-.34	.01
Certainty	-.06	.01	-.04	.16	.38
Baseline VHT					
Total voices	-.22	-.06	-.07	.14	.08
Hits	-.14	.06	-.05	.19	.38
False alarms	-.29	-.27	-.19	-.08	-.29
Follow-up VHT					
Total voices	.18	.22	.18	.17	.24
Hits	.16	.24	.24	.28	.33
False alarms	-.04	-.03	-.09	-.15	-.07
SSDT response outcomes in the baseline phase					
Hit rate in light absent trials	-.06	.03	-.16	-.05	.04
Hit rate in light present trials	-.19	-.21	-.28	-.03	-.01
False alarm rate in light absent trials	.02	.22	.12	.29	.16
False alarm rate in light present trials	-.27	-.13	-.11	.38	.01
Sensitivity in light absent trials	-.03	-.06	-.15	-.22	-.05
Sensitivity in light present trials	.05	.00	-.12	-.20	.00
Bias in light absent trials	.02	-.17	.04	-.15	-.14
Bias in light present trials	.34	.25	.23	-.17	.10

(continued)

Variables	Sleepiness			State anxiety	
	Before the baseline phase	Before the manipulation phase	Before the follow-up phase	Before the baseline phase	Before the follow-up phase
SSDT response outcomes in the manipulation phase					
Hit rate in light absent trials	.19	.09	-.22	-.16	-.09
Hit rate in light present trials	.05	.06	-.19	-.09	.02
False alarm rate in light absent trials	.19	.27	-.01	.09	.01
False alarm rate in light present trials	-.05	.12	.08	.08	.19
Sensitivity in light absent trials	.11	.01	-.17	-.19	-.08
Sensitivity in light present trials	.04	-.04	-.15	-.13	-.07
Bias in light absent trials	-.19	-.17	.19	.13	.05
Bias in light present trials	-.04	-.12	.15	.02	-.11
SSDT response outcomes in the follow-up phase					
Hit rate in light absent trials	.03	.16	-.15	-.20	.02
Hit rate in light present trials	.04	.14	-.10	-.21	.01
False alarm rate in light absent trials	.04	.18	-.18	.05	-.30
False alarm rate in light present trials	-.16	.04	-.14	.02	-.04
Sensitivity in light absent trials	-.01	.07	-.04	-.17	.17
Sensitivity in light present trials	.11	.09	.02	-.17	.00
Bias in light absent trials	-.09	-.25	.17	.14	.11
Bias in light present trials	.07	-.10	.20	.15	-.06
Median	8	8	3	6	5
IQR	8 to 11	8 to 10	3 to 6	4 to 8	3 to 7

Note. SPS = spontaneous sensation; VHT = Voice Hearing-Task; SSDT = Somatosensory Signal Detection Task.

*p = .01

Appendix AB: Summary of the studies on sequential effects (Study 6, Chapter 7)

Study	Stimulus/Procedure	Task/Psychophysical method used	Participants	Results
Fernberger (1920)	• Weight judgment task. • Two series of five pairs of weight (small brass cylinders) were used.	Method of constant stimuli	Four	• Comparison stimulus was perceived lighter when it followed heaviest pair of stimuli. • Comparison stimulus was perceived heavier when it followed lightest pair of stimuli.
Holland and Lockhead (1968)	• 10 loudness (sinusoid) stimuli. • Participants judged loudness of each stimulus on a scale from 1 (quietest) to 10 (loudest). • Each participant made 2700 responses (150 judgments x 18 sessions)	Absolute judgment	Three	• Judgement in the current trial was similar to the stimulus in the prior trial (known as assimilation effect). • Judgement in the current trial was different from the stimuli in the earlier trials (trials two to five; known as contrast effect).
Jones, Curran, Mozer, and Wilder (2013)	**Experiment 1** • Participants responded to the location of 5mm white dot presented above and below of a white fixation line. • A box with two buttons was used to give response. • Two sessions; each comprising 3744 trials divided into 33 blocks. • Reaction time and psychophysical data were collected **Experiment 2** • Stimulus was a random .6mm white dot kinematogram. • Participants discriminated between leftward and rightward motion. • 192 trials in each of the four test blocks. • Reaction time data was collected.	Visual discrimination task	• Experiment 1: 28 • Experiment 2: 181	• Sequential effects are complex and non-additive in two alternative forced choice tasks. • In these tasks sequential effects can be explained by simultaneous incremental learning of response base rate (frequency/proportion of occurrence for each stimulus or response) and stimulus repetition rate (frequency/proportion of repetition of or alteration from the previous trial).

Study	Stimulus/Procedure	Task/Psychophysical method used	Participants	Results
Stewart and Brown (2004)	• Ten 500-ms sine-wave tones of differing frequency. • Participants classified the stimuli as high and low tones. • Feedback was given after each response. • Total trials: 600 (six blocks x 100 trials).	Auditory discrimination task	• 16	• Stimuli up to 2 trials back had interactive influence on the current response. • Long-term representations of stimulus magnitude were irrelevant to the current response.
Jesteadt, Luce, and Green (1977)	• Three experiments • Stimuli were 1000 Hz tones, each 500-ms long. • Experiment 1 and 3 used 27 tones ranged from 36-88 dB. • Experiment 2 used 16 tones ranged from 36-50 dB and 74-88 dB. • Participants completed between 10 and 15 runs with 60 trials in each run. • Each tone was presented 60 times in each experiment. • Participants estimated the magnitudes of two successive tones and then used the first tone as the numerator and the second tone as the denominator. This provided an estimate of the loudness ratio for two tones.	Magnitude estimation of loudness	• The same four participants in all the experiments.	• Response and stimulus in the immediately preceding trial affected response in the current trial. • Earlier trials did not have any sequential effect on the current trial. • Magnitude of correlation between successive responses depended on the difference between the intensity (dB) of corresponding stimuli.
Ward and Lockhead (1971)	**Study 1:** • Ten 1000-Hz sinusoids ranged between 55 to 64 dB were presented randomly. • Participants judged the loudness of each tone using a number from 1 to 10. • Half of the participants received feedback. • 2000 trials (500 responses x 4 consecutive days) **Study 2:**	Absolute judgment	• Six participants in both Studies 1 and 2 • Three participants in Study 3.	• Current response assimilated to the value of immediately preceding stimulus or response and contrasted the values of the stimuli and responses further back in the sequence (trials N-2 through N-6). • In no feedback condition, assimilation extended up to N-1 stimuli and N-5 responses.

Study	Stimulus/Procedure	Task/Psychophysical method used	Participants	Results
	• Participants judged the length (using a rating scale from 1 for the shortest to 10 for the longest line) of 10 horizontal lines varied in length between 2 and 2.9 cm. • In easy condition, half of the participants gave 1200 responses (150 x 8 days) and the rest gave 600 responses (150 x 4 days). • In difficult condition, participants gave 1200 responses (150 x 8 days) • Feedback was given after each response in both the conditions. **Study 3:** • There was no stimulus. • Participants judged which of the numerals 1-10 would occur next. • Feedback was provided • 500 responses per day for two days.			• In both feedback (specifically when performance deteriorated) and no stimuli condition, assimilation to the previous stimulus was larger than to the previous response. • Usual assimilation and contrast effects were found with the easy task with feedback. • In feedback absent condition, assimilation was large to the N-1 response but less to the N-1 stimulus. The reverse was found when task was difficult but feedback was provided.
Mori (1998)	**Study 1:** • Four numbers (4, 6, 10, 16) of pure tones (100-8000Hz; each was 500ms long in duration) were presented under masking (smaller stimulus information) and no masking conditions. The eight conditions were tested in separate sessions. • Participants' task was to identify and report the frequencies of the tones. • There were 60 responses to each tone. **Study 2:** • Light circles (with 10 or 16 luminance intensities, each presented for 500ms) were used as stimuli.	Absolute identification	• Five participants in both Studies 1 and 2. • Six participants in Study 3.	• Sequential dependencies on N-1 responses increased as stimulus information decreased (Studies 1 and 3). In study 1, stimulus information was measured by stimulus transformation. • Sequential dependencies on N-1 stimulus increased as stimuli were presented with low stimulus information (Study 3). • Sequential dependencies on both N-1 stimuli and responses increased as the number of stimuli increased (Study 1 and 2).

Study	Stimulus/Procedure	Task/Psychophysical method used	Participants	Results
	• Participants' task was to identify the presented predefined luminance. • There were 60 responses per stimulus. **Study 3:** • Horizontally presented 16 pointer positions were the stimuli. They were presented within a small range (low in stimulus information) or a large range (high in stimulus information). • Feedback was provided. • Each participant had a total of 1920 trials (16 pointers x 20 trials x 3 sessions x 2 conditions: small and large range).			
Lockhead and King (1983)	• Stimuli: randomly presented 30 sinusoids, each ½ seconds in duration (1000 Hz, intensity ranged between 51 and 80 dB). • Participants' task was to type a number representing the ratio of loudness of the current stimulus to that of the N-1 stimulus. • Feedback was given immediately after each response. • Total 2000 trials (5 sessions x 4 blocks x 100 trials).	Successive-ratios-judgment task	Two	• Determining ratio of loudness of two successive sound stimuli was a two-step process: First, the stimuli were encoded or identified, and Second, the encoded information in the memory was used to calculate the ratio. • The authors suggested that memory plays important role in sequential effects in scaling tasks, such as magnitude estimation, category judgment, and absolute identification tasks.
Podlesek (2010)		This is a review article and suggested some controls for sequential		• Sequential effects are robust. • Sequential effects can be controlled with the following strategies: i) loading attentional resources ii) using some new and

Study	Stimulus/Procedure	Task/Psychophysical method used	Participants	Results
		effects.		irrelevant stimuli in the series iii) erasing the memory trace of previous stimuli and responses
Stewart, Brown, and Chater (2005)	**Study 1** • Stimuli: two sets of 10 tones, each was 500ms in duration. • Two conditions: wide-spacing and narrow-spacing • In wide-spacing condition the lowest tone had a frequency of 600 Hz. Each subsequent tone increased in frequency by 12%. • In narrow-spacing condition the lowest tone had a frequency of 768.70 Hz. Each subsequent tone increased in frequency by 6%. • Three sets of 6, 8, and 10 stimuli were used. • Trials: seven blocks of 20 tones • Half of the participants were asked to use low numbers to indicate low frequency tones and high number to identify high frequency tones. The other half was given the opposite instruction. • Feedback was given immediately after a response. **Study 2** • 10 stimuli with intensity ranged between 200 Hz to 1490 Hz were used. • There were 20 blocks of 40 stimuli.	Absolute identification	• Study 1: 120 • Study 2: 19	**Study 1** • The current response was assimilated toward the N-1 stimulus. • Earlier stimuli had contrast effect on the current response. • The greatest contrast was found for stimuli three or four trials back. **Study 2** • Error due to misleading feedback in the preceding trial was transmitted to the current response. If false feedback was that the intensity of the N-1 stimulus was large, participants reported large intensity for the current stimulus.

Study	Stimulus/Procedure	Task/Psychophysical method used	Participants	Results
	• Critical stimuli were randomly presented in preselected trials. • Feedback was misleading on 5% of the trials.			
Treisman and Williams (1984)	**Study 1** • Stimulus: An 830-Hz auditory signal was presented for 250ms in 70-dB white noise • Two conditions: signal only and noise only • Signal only condition: There were one practice and two experimental sessions. Signal intensity with 50% detection rate was determined in the practice session. There were four blocks of 158 trials in the experimental sessions. • Noise only condition: Same as the signal only condition except that, unknown to the participants, pure tone signal was presented in the first five trials of the experimental sessions but the intensity was gradually reduced from the first to the fifth trial and the signal was absent in the remaining trials in a block. There were five blocks of 158 trials in the experimental sessions. **Study 2** • Signal was presented at 50% detectability strength. • There were one practice and three experimental sessions (four blocks of 158 trials in each session) in which signal was randomly presented in 50% of the trials. • In two experimental sessions (1200	Signal detection task	**Study 1** • Signal only condition: seven participants • Noise only condition: four participants **Study 2** There were six participants in this study.	Results of both the studies suggest that linear combination of independent indicator traces (i.e. memory traces) modifies response criterion (i.e. bias) and affects sequential dependencies. Independent traces were determined by the responses made, rather than by the magnitudes of stimuli inputs.

Study	Stimulus/Procedure	Task/Psychophysical method used	Participants	Results
	trials), signal was present in randomly selected 20% trials and in other two sessions (1200 trials), signal was randomly presented in 80% trials.			
Brown, Marley, Donkin, and Heathcote (2008)				This article describes a model that explains how memory and attention processes determine responses and how sequential effects operate in decisional stage.

Appendix AC: Violation of the assumptions of mixed ANOVA in sequential analysis (Study 6, Chapter 7)

The following distributions remained non-normal in the false alarm increasing study:

(i) Effect of N-1 "yes" responses on "yes" responses in the follow-up phase of the experimental condition, $D(50) = .15$, $p < .01$.

(ii) Effect of N-1 "yes" responses on "no" responses in the follow-up phase of the control condition, $D(50) = .13$, $p < .05$.

(iii) Effect of N-1 presence of vibration on current "no" responses in the follow-up phase of the experimental condition, $D(50) = .15$, $p < .01$.

(iv) Effect of N-1 presence of light on current "yes" responses in the follow-up phase of the experimental condition, $D(50) = .13$, $p < .05$.

(v) Effect of N-1 presence of light on current "no" responses in the follow-up phase of the experimental condition, $D(50) = .16$, $p < .01$.

The assumption of homogeneity of variance (as Leven's test examined) was violated in the following cases (i.e. control and experimental groups had significantly different variances) of the false alarm increasing study:

(i) Effect of N-1 "yes" responses on current "no" responses in the follow-up phase, $F(1, 98) = 30.67$, $p < .001$.

(ii) Effect of N-1 presence of vibration on current "no" responses in the follow-up phase, $F(1, 98) = 5.01$, $p < .05$.

(iii) Effect of N-1 presence of light on current "yes" responses in the baseline phase, $F(1, 98) = 12.66$, $p < .01$.

(v) Effect of N-1 presence of light on current "no" responses in the baseline, $F(1, 98) = 5.92$, $p < .05$; and follow-up, $F(1, 98) = 9.27$, $p < .01$, phases.

In the false alarm decreasing study, the following distributions remained non-normal:

(i) Effect of N-1 "yes" responses on current "yes" responses in the baseline phase of the control condition, $D(47) = .16$, $p < .01$.

(ii) Effect of N-1 "yes" responses on current "no" responses in the baseline phase of the control condition, $D(47) = .16$, $p < .01$.

(iii) Effect of N-1 vibration on current "yes" responses in the follow-up phase of the experimental condition, $D(47) = .26$, $p < .01$.

(iv) Effect of N-1 vibration on current "no" responses in the follow-up phase of the experimental condition, $D(47) = .17$, $p < .01$.

(v) Effect of N-1 light on current "yes" responses in the follow-up phase of the control condition, $D(47) = .13$, $p < .05$.

(vi) Effect of N-1 light on current "yes" responses in the follow-up phase of the experimental condition, $D(47) = .17$, $p < .01$.

The assumption of homogeneity of variance was violated by the following distributions in the false alarm decreasing study:

(i) Effect of N-1 "yes" responses on current "no" responses in the follow-up phase, $F(1, 92) = 11.85$, $p < .01$.

(ii) Effect of N-1 presence of vibration on current "yes" responses in the follow-up phase, $F(1, 92) = 9.33$, $p < .01$.

(iii) Effect of N-1 presence of vibration on current "no" responses in the follow-up phase, $F(1, 92) = 26.37$, $p < .001$.

(iv) Effect of N-1 presence of light on current "no" responses in the follow-up phase, $F(1, 92) = 18.60$, $p < .001$.

The following false alarm distributions of the false alarm decreasing study about the effects of N-1 events violated the homogeneity of variance assumption:

(i) Effect of N-1 "yes" responses in the follow-up phase, $F(1, 78) = 19.51$, $p < .001$.

(ii) Effect of N-1 presence of vibration in the follow-up phase, $F(1, 78) = 26.01$, $p < .001$.

(iii) Effect of N-1 presence of light in the follow-up phase, $F(1, 78) = 17.29$, $p < .001$.

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Appendix AD: Advert II for Study 3

RESEARCH PARTICIPANTS NEEDED

Response stability on the voice detection task study

School of Psychological Sciences

About the Experiment

We are looking for participants for a two-session study. In a six-minute session, you will be asked to do a simple task relating to hearing of voices. In a 30-minute session, you will be given the same voice detection task and some questionnaires. There will be a gap of approximately three weeks between the sessions.

To take part in this study you must satisfy the following criteria

- Be aged between 18 to 40
- Understand instructions in English well
- Not having any medical conditions that might affect the sense of hearing
- Did not take part in the previous learning and tactile perception study
- Did not take part in the previous voice detection study carried out by Alice Heaney

Compensation

You will receive £5 for your time and inconvenience related to your participation in this study. This will be awarded at the end of the second testing session.

Ethical approval

University of Manchester Ethics Committee 4; Re: 14329

If you are interested to take part in the experiment, please visit **http://goo.gl/omOKdM**
or email me at **mdakibul.huque@manchester.ac.uk**
or scan the **QR code**:

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Appendix AE: Participant information sheet II for Study 3

Title of the Research: Voice detection task reliability study

Introduction

You are invited to take part in a research study about detection of voices. Please read this information sheet carefully so that you are able to make an informed decision about whether or not to participate in this study. If anything is unclear or you have any questions about the research, you are very welcome to contact me (see the contact details at the end of this form) and I will do my best to provide the information you need.

What is the aim of the research?

The main objective of the present study is to investigate how consistent individuals are in detecting voices over time and how this relates to their somatic and sensory experiences.

Why have I been chosen?

Anyone from the University of Manchester aged between 18-40 can participate in this study. Our aim is to collect data from at least 50 individuals. Please note that you are not eligible for this study if you (i) took part in the previous experiments on learning and tactile perception and individual differences in voice detection and (ii) have any medical conditions that might affect the sense of hearing.

What would I be asked to do if I took part?

If you decide to take part in this study, you will be asked to attend two testing sessions with a gap of approximately three weeks between them. In the first session, you will do a simple voice detection task, which involves detecting voices embedded intermittently within headphone-presented white noise. The same voice detection task will be given in the second session along with five questionnaires; two of the questionnaires are about physical symptoms (e.g. headache) that you might experience right now, at this moment or might have experienced in the past month and the other three are about your thoughts, feelings, and behaviours. On average, it takes six and 30 minutes respectively to complete the first and the second testing session.

During the experiment, you will be asked to sit in the same place for much of the testing session, although you will have the opportunity to take breaks if you wish. At the end of the study, we shall ask you few questions to know your thoughts and beliefs about the tasks you have performed. We may note down your answers for our record, which we might use later only to interpret and understand study findings but not to publish as a verbatim report.

Will my data be confidential?

Yes, we will manage all of your data in a secure way to ensure that your confidentiality is protected. We will do this by ensuring that:

- Any printed copies of your personal information (such as your name, contact address, and date of birth) will be kept in a locked file cabinet on University premises.
- None of your experimental data will be stored in the same place as any personal information. We do need to know who provided what data (to ensure that data from the tasks and questionnaires are identified as coming from the same person), but we will do this by using a unique identification code rather than personal information. Any documents linking personal information to identification codes will be kept separate from the data itself and stored in a locked file cabinet on University premises.
- Any electronic files containing personal information will be encrypted.
- Only the research team will have access to data.

What happens if I do not want to take part or if I change my mind?

It is entirely up to you whether you take part or not. If you do decide to take part, you are free to withdraw at any point without having to give a reason.

What are the benefits and risks to taking part in the study?

You will receive £5 for your time and inconvenience related to your participation in this study. This will be awarded at the end of the second testing session.

It is very unlikely that the present research will cause any physical or psychological harm to you. The experimental procedures are simple and benign in nature and have been used in a number of studies with ethical approval from the university.

The questionnaires that will be used in the present experiment are highly regarded by both clinicians and researchers around the world. There is no known potential risk in using them, although there is a very small chance that some participants will be mildly upset answering questions about their physical health (e.g. During the past 4 weeks, how much have you been bothered by stomach pain, back pain, dizziness, trouble sleeping, etc. or whether you are experiencing symptoms such as headache, watering eyes, racing heart, etc. right now, at this moment), thoughts and perceptual experiences (e.g. Please answer each item true or false: sometimes people whom I know well begin to look like strangers, I sometimes have had the feeling that my body is abnormal, no matter how hard I try to concentrate, unrelated thoughts always creep into my mind, sometimes my thoughts seem as real as actual events in my life and so on), and mood (e.g. To what extent you generally feel this way: interested, distressed, upset, etc.).

Where will the research be conducted?

The research will be carried out in labs either in the Coupland or Zochonis Building of the University of Manchester.

Will the outcomes of the research be published?

The outcomes of the research will be submitted for publication in peer-reviewed journals. However, the papers will not contain your name or address. If you want, we will send you a summary of the findings after the final data analysis.

What if something goes wrong?

We do not expect anything to go wrong. In the very unlikely event that something untoward does happen, you can contact me in the first instance. You can also contact my supervisor (richard.j.brown@manchester.ac.uk). If there are any issues regarding this research that you would prefer not to discuss with members of the research team, please contact the Research Governance and Integrity Team by either writing to 'The Research Governance and Integrity Manager, Research Office, Christie Building, The University of Manchester, Oxford Road, Manchester M13 9PL', by emailing: Research-Governance@manchester.ac.uk, or by telephoning 0161 275 8093 or 275 2674.

Contact details of the researcher

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Email: mdakibul.huque@manchester.ac.uk

This project has been approved by the

University of Manchester Research Ethics Committee