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Investigating Scopesthesia: Attentional Transitions, Controls and Error Rates in Repeated Tests

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Abstract—The sense of being stared at, or scopesthesia, was investigated experimentally with participants working in pairs. Two participants were tested repeatedly and the effect of attentional transition was investigated. In some tests, in the pre-trial period the starer stared at the staree, who was blindfolded, and in others the starer did not stare during the pre-trial period. Their overall hit rate in these attentional transition tests was 52.8% (2,800 trials; $p = 0.002$), but there was no significant difference in hit rates between the two kinds of test. Participants were given trial-by-trial feedback, so if there was any learning, there should have been a progressive increase in hit rates. This did not happen. The participants also took part in a control test in which there was no staring at all. In these tests hit rates were at chance levels, indicating that other forms of ESP, such as telepathy and clairvoyance, could not account for the results in scopesthesia tests. There were only 3 recording errors in 2,800 trials (0.1%), and two of these cancelled out, leaving a net error rate of 0.04%.

Keywords: scopesthesia—sense of being stared at—attentional transitions—response bias—error rates—feedback

The sense of being stared at is well known. Most people claim to have turned around to find that someone was looking at them; most people also claim to have caused other people to turn round by looking at them (Sheldrake, 2003). Scopesthesia is a newly coined scientific term for this phenomenon (Carpenter, 2005), which is also referred to in the research literature as ‘‘unseen gaze detection’’ (Wiseman & Smith, 1994), ‘‘staring detection’’ (Braud, 2005), ‘‘non-visual staring detection’’ (Sheldrake, 2005b) or ‘‘remote staring detection’’ (Baker, 2005).

The simplest tests for this phenomenon involve people working in pairs. One person, the staree, sits with her back to the other, and usually wears a blindfold. In a randomized sequence, the starer either stares at the back of the staree’s neck or looks away and thinks of something else. The beginning of each trial is signalled by a click, beep or bell. In over 30,000 trials of this kind the overall hit rate was 55%, very significantly above the mean chance expectation of 50%. The hit rates were also significantly above chance in several studies in which starers and starees were separated by windows or one-way mirrors (for a review, see Sheldrake, 2005a).

It is a general principle of sensory physiology that the senses detect changes or differences. Perhaps the same principles apply to scopesthesia. In all the tests conducted so far, before the trial the starer was not looking at the staree. Then in looking trials, he started looking at her, while in not-looking trials he continued not looking. Thus, at the beginning of looking trials there was a change in his attention, and in not-looking trials there was not. In the tests described in this paper we explored whether starees were more sensitive when the starer changed from not looking to looking, or vice versa. In half the tests, all 20 of the trials in the test began with the starer looking. Then, as a signal was given to the staree, the starer either looked or did not look according to a randomized schedule. Thus, in the looking trials there was no change, and in the not-looking trials there was a transition of attention. In the other tests, as in standard staring experiments, the situation was opposite: in looking trials there was a change of attention from not-looking to looking, while in not-looking trials there was no transition.

Most experiments on scopesthesia have tested participants only once or twice. In the present series of investigations, the same participants were tested repeatedly. This enabled us to find out whether or not there were any changes over time in the staree's hit rates. In these tests, starees received immediate feedback after each trial as to whether their guesses were correct or not. Their hit rates would increase if they were improving with practice, or decline if they were becoming bored with the testing process or losing their ability to make accurate guesses for any other reason.

We also carried out control tests to find out what patterns of guessing occurred in the absence of scopesthesia. Control experiments enable several fundamental questions to be explored. Are above-chance results in scopesthesia tests due to some ability other than scopesthesia? If so, what? First, significant positive results may depend on a detection of subtle sensory cues. Second, the staree might pick up the starer's intentions telepathically, rather than through staring itself. Third, the staree might be picking up the starer's written instructions by clairvoyance. Fourth, in tests in which trial-by-trial feedback is given, starees might be picking up the answers precognitively.

If positive results in scopesthesia tests depend on any of these other kinds of information transfer, then when scopesthesia is eliminated in control experiments, the hit rates should still be above chance.

One way to carry out control experiments would be to tell the starees that these are standard staring tests and that they will be stared at, or not stared at, in a random sequence. In fact, they are not looked at in any of the trials.

We did not want to base an experiment on deception. Instead, in our control tests, the starer and staree sat with their backs to each other. The starer did not look at the staree at all, and the staree knew that this was the case. The staree was asked to guess what instruction the starer was receiving in each trial. Using standard randomized instruction sheets, the starer looked at the instruction, signalled the beginning of the trial to the staree by means of a standard mechanical click, and the staree then guessed whether the instruction was ‘‘look’’ or ‘‘no’’. The staree received immediate feedback as to whether the guess was correct or not. We analysed the results of tape-recorded experiments to find out how frequently errors occurred in the recording of data.

Method

Participants

There were two participants in these tests, who took turns at being starer and staree: Pam Smart (PS) and her 14-year-old niece, JM, who was paid for her participation. These participants had already taken part in standard staring tests and were familiar with the general procedure.

Tests

The tests took place in JM’s family’s house with both participants in the same room, sitting about 2 metres apart. The staree was blindfolded. Each test consisted of 20 trials and was conducted in accordance with a randomized instruction sheet. There were 20 different randomized sheets altogether, and the sequence of looking and not-looking trials was determined by a random number generator. These 20 sheets were used repeatedly, but in a different order each time. Some sheets had equal numbers of looking and not-looking trials, while others had unequal numbers. Thus, by chance in some sets of data there were unequal numbers of looking and not-looking trials.

The participants carried out a series of between 14 and 16 tests in a session, and in each test there were 20 trials. After one 20-trial test was completed, the participants changed roles. The sessions occurred at roughly weekly or two-weekly intervals. The dates of these sessions are given in Table 1. For the tests on attentional transitions there were 10 sessions, followed by 3 sessions of control tests. All sessions began at 4 pm, apart from the session of February 15, which began at 1 pm.

Just before the beginning of each trial, the starer looked at the instruction sheet and read the instruction ‘‘look’’ or ‘‘no’’, then signalled the beginning of the trial by means of a mechanical clicker, which gave a sound of standard intensity. For the tests on stimulus transitions, each trial was divided into two parts, the first lasting 3 seconds. The beginning of the second part of the trial was signalled by another click. A device used for training dogs emitted these clicks: the first was produced by pushing in a metal flange, and the second by releasing it 3 seconds later.

There were two kinds of test: in ‘‘looking tests’’, during the first part of each trial, the starer looked at the staree, and then, 3 seconds later, as the second click

TABLE 1
Dates of Sessions of Attentional Transition Tests (Sessions 1–9) and Control Tests (Sessions 10–12). All Took Place in 2005

Session	Date	Session	Date
1	January 31	8	April 12
2	February 7	9	April 18
3	February 15	10	April 25
4	February 21	Controls	
5	February 28	11	May 16
6	March 7	12	May 23
7	March 14	13	June 6

was sounded, followed the randomized instruction ‘‘look’’ or ‘‘no’’. These were designated L tests. In the second kind of test, designated N (‘‘not-looking’’), during the first part of each trial the starer did not look at the staree, and then after 3 seconds either looked or continued not looking in accordance with the randomized instruction. Thus, in L tests, at the second click the starer either continued to look or changed to not looking; in the N tests at the second click the starer either continued not looking or changed to looking. The staree knew whether the test was an L or an N test.

PS determined at random, by the toss of a coin, whether the first test in a session was L or NL, and then each staree alternated between L and NL tests throughout the session. The starees knew whether they were taking part in an L or N test.

The staree guessed out loud ‘‘looking’’ or ‘‘not looking’’ within 10 seconds of hearing the second click and received immediate feedback as to whether this guess was correct or not. The starer recorded the result on the instruction sheet and proceeded to the next trial.

In the control tests, the starers and starees sat with their backs to each other. As usual, the staree was blindfolded and just before each trial the starer looked at the instruction sheet to see if the trial was ‘‘look’’ or ‘‘no’’, and signalled the beginning of each trial by means of a mechanical clicker. Unlike the dog clicker used in the stimulus transition tests, this clicking device emitted a standard single click. The starer did not look at the staree at any stage during the test. The staree was asked to guess what instruction the starer had received and made this guess within 10 seconds of the trial beginning.

Error Detection

All attentional transition tests were tape-recorded so that the pattern of clicks and responses could be evaluated independently at a later date in a ‘‘blind’’ fashion. This evaluation was carried out by Kayleigh Allenby (KA), who did not know either of the participants and lived 200 miles away. KA listened to the tapes of the trials, noting down the trial number and date and then recording what guess the staree made in each trial. While Rupert Sheldrake (RS) had the original score sheets, PS then entered the guesses recorded by KA on duplicates of the original score sheets for each test. RS then compared these score sheets with the originals so that discrepancies could be detected. When such discrepancies were found, RS listened to the tape recording of that test to determine whether there was any error in the evaluation of the tape by the evaluator. If there had not been then the discrepancy was due to a recording error by the starer.

Scoring and Statistics

As usual in staring tests, the number of correct and incorrect guesses in looking and not-looking trials were tabulated separately, along with the total for each test (Sheldrake, 2000). As in previous research, the totals were also evaluated by means of the sign method, with scores of 11 or more out of 20 given a " + " sign, scores of 9 or less a "–" sign and scores of 10 an "=" sign. The advantage of the sign method is that it gives an equal weighting to each test. The chance expectation was that 50% of the guesses would be correct, and also that the number of + signs would be equal to the number of – signs, ignoring the number of = signs. The null hypothesis was tested using the binomial test. For comparisons of data from tests under different conditions the 2 × 2 Chi-squared test was used.

Results

Attentional Transition Tests

The overall hit rate for all the attentional transition tests was 1,477/2,800, or 52.8% ($p = 0.002$). By the sign method the results were 69 + 34 – 23 = ($p = 0.0005$).

For the L tests, in which the staree was looked at during the first 3 seconds of the trial, the hit rate was 52.9% ($p = 0.01$) or 39 + 23 – 13 = ($p = 0.02$) (Table 2).

For the N tests, in which the staree was not looked at during the first 3 seconds of the trial, the hit rate was 52.5% ($p = 0.03$) or 36 + 16 – 13 = ($p = 0.004$). The hit rate for the L tests was significantly greater than for the N tests on the basis of scores ($p = 0.03$) but not on the basis of signs.

The two starees had slightly different hit rates (Table 1): overall, PS scored 53.2% ($p = 0.01$) or 37 + 21 – 12 = ($p = 0.02$) and JM scored 52.3% ($p = 0.04$) or 38 + 18 – 14 = ($p = 0.005$). These differences between the two starees' scores were not statistically significant.

In the L tests, the hit rates in looking trials were slightly higher than in not-looking trials, 53.5% and 52.3%, respectively. In the N tests, the reverse was the case, with 52.4% in looking and 52.7% in not-looking trials. Overall, the score

TABLE 2
Scores in Staring Tests Where Trials Began with 3 Seconds Looking (L) or Not Looking (N) with 2 Starees (PS and JM). The Numbers of Hits and Misses Are Shown for Looking Trials, Not-Looking Trials and Totals

		Looking		Not looking		Totals			
Staree	Test	Hit	Miss	Hit	Miss	Hit	Miss	Hit %	Signs
PS	L	215	156	178	171	393	327	54.6	21 + 11 − 4 =
PS	N	189	158	163	170	352	328	51.8	16 + 10 − 8 =
JM	L	189	195	212	184	401	379	51.4	18 + 12 − 9 =
JM	N	164	163	167	126	331	289	53.4	20 + 6 − 5 =
					Totals				
PS		404	314	341	341	745	655	53.2	37 + 21 − 12 =
JM		353	358	379	310	732	668	52.3	38 + 18 − 14 =
	L	404	351	390	355	794	706	52.9	39 + 23 − 13 =
	N	353	321	330	296	683	617	52.5	36 + 16 − 13 =
Grand totals		757	672	720	651	1,477	1,323	52.8	75 + 39 − 26 =

Note: The signs indicate the number of tests in which the hit rate was 11/20 or more (+), 9/20 or less (–) or 10/20 (=).

was slightly higher in looking trials (53.0%) than in not-looking trials (52.5%), but these differences were not statistically significant.

In most previous staring experiments, the total number of “looking” guesses was greater than the total number of “not-looking” guesses; in other words, there was a response bias in favour of “looking” (Sheldrake, 2005). In these tests, the total number of “looking” guesses was 757 (looking/hits) + 651 (not-looking/misses) = 1,408/2,800, or 50.3%, not significantly different from the chance level of 50%. However, this average figure conceals a striking difference between the two starees. PS guessed “looking” in 53.2% of the trials, while JM did so in only 47.4% of the trials, a significant difference ($p = 0.002$).

Control Tests

The results of these control tests are shown in Table 3. The overall hit rate of 49.3% was not significantly different from the chance level of 50%, nor were the scores of the individual participants: JM’s hit rate was 48.5% and PS’s was 50.0%.

Both participants scored below the chance level in looking trials and above the chance level in not-looking trials. This effect was due to a response bias whereby both of them guessed “not looking” more often than “looking”: only 42.2% of JM’s guesses were “looking”, while 46.2% of PS’s guesses were “looking”. Overall, the percentage of “looking” guesses was 44.3%.

Changes with Time

The hit rates in the 10 sessions of attentional transition tests and for the 3 sessions of control tests that followed them are shown in Figure 1.

TABLE 3
Control Tests. The Numbers of Hits and Misses Are Shown for Looking Trials, Not-Looking Trials and Totals for 2 Starees (JM and PS)

	Looking		Not looking		Totals		
Staree	Hits	Misses	Hits	Misses	Hits	Misses	Signs
JM	95	138	128	99	223	237	8 + 13 − 2 =
%	40.8		54.0		48.5		
PS	111	129	129	111	240	240	11 + 12 − 1 =
%	46.3		53.8		50.0		
Total	206	267	257	210	463	477	19 + 25 − 3 =
%	43.5		55.0		49.3		

Note: The signs indicated the number of tests in which the hit rate was 11/20 or more (+), 9/20 or less (–) or 10/20 (=).

There was no systematic trend in the data, neither a regular improvement from session to session nor a regular decline. In the first three attentional transition tests and in the control tests the fluctuations for the two starees moved in similar directions, but in most other sessions they moved in opposite directions.

Within each session, each staree took part in 3 or 4 L and N tests. The average data testwise are shown in Figure 2. Again there was no clear trend. In the L tests the hit rates were higher in tests 3 and 4 than in the first 2 tests, but this was not the case in the N tests.

Error Rates

The number of errors in recording the data was determined from tape recordings of all the attentional transition tests. Out of a total of 2,800 trials, there were 5 discrepancies. Of these, two were owing to errors made by the evaluator in writing down the guesses on the tape recording. Three were errors made by the starer in noting down the staree's guesses, an overall error rate of 0.1%. All three errors were in not-looking trials; two were false positives and one a false negative, giving an overall error of just one false positive, giving a net error rate of 0.04%.

Discussion

This is one of the first studies on scopesthesia in which the same participants have been tested repeatedly. A surprising result was that there was no obvious tendency for hit rates to increase or decline (Figure 1). Since the starees were receiving trial-by-trial feedback, their hit rates might have been expected to improve with practice, but this was not the case. In the first 3 sessions and in the control sessions, the fluctuations were similar with both starees, suggesting that external factors may have influenced both similarly, but there was no similar pattern in the other sessions. Within sessions, there was a tendency for hit rates

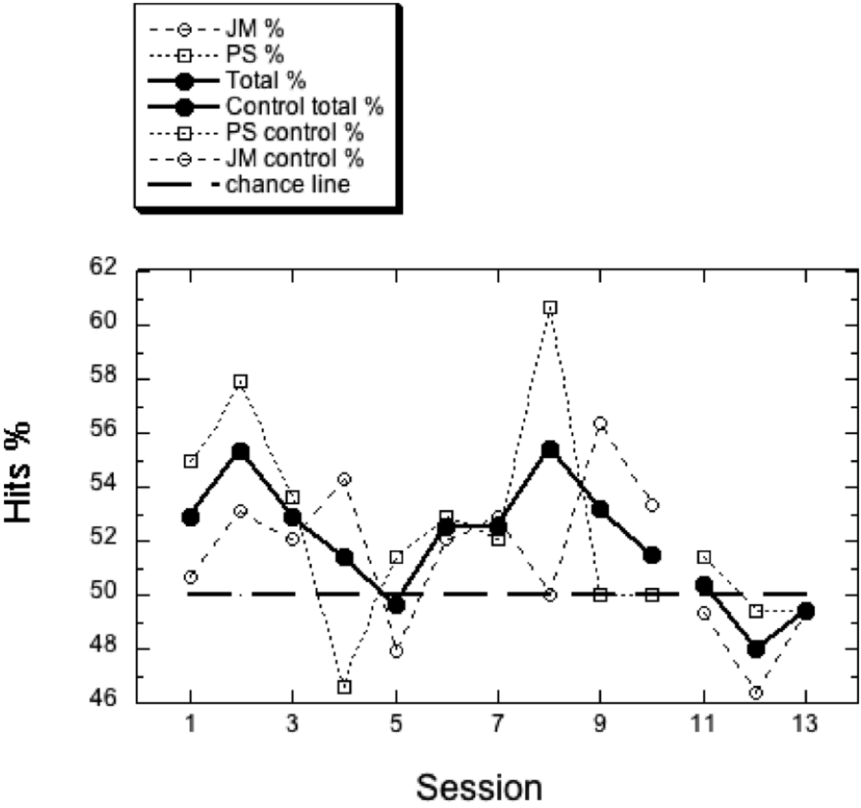

Fig. 1. Changes in hit rate with time in sequential sessions. Sessions 1–10 were for attentional transition tests and Sessions 11–13 for control tests. The dates of these sessions are given in Table 1.

to increase in L tests but not in N tests (Figure 2). The lack of systematic trends suggests either that there was no tendency to improve with practice or that any such tendency was offset by a countervailing tendency, such as boredom.

A second surprise was that the number of errors in recording the data was so low. The net error rate of 0.04% was negligible.

The fact that hit rates were at chance levels in the control tests shows that the above-chance hit rates in the scopesthesia tests cannot be ascribed to telepathy, clairvoyance or precognition, or to any subtle sensory cues that were common to both kinds of test.

There was surprisingly little effect of attentional transition, with very similar overall results from L and N tests. The attentional transitions within these two kinds of tests did not result in higher hit rates than the trials in which there was no transition; indeed, there was a tendency for the reverse to be the case. In the L tests, all of which started with the starer looking at the staree, there was no transition in the looking trials, because the starer simply continued to look. The

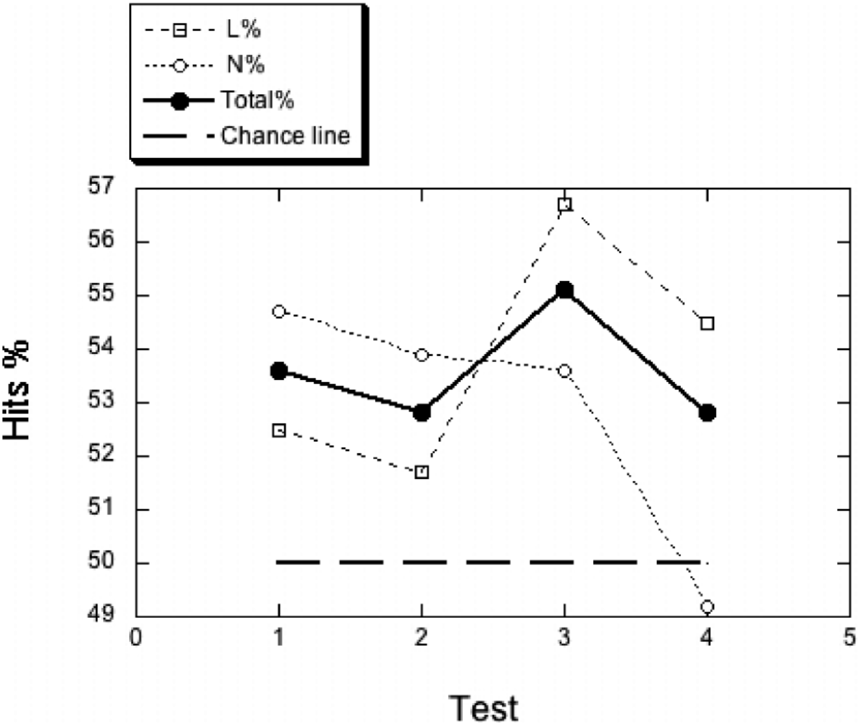

Fig. 2. Changes in hit rate with time in sequential tests within sessions. In L tests, the starees were looked at in each 3-second pre-trial period; in N tests they were not looked at.

transition occurred in the not-looking trials when the starer stopped looking. Yet the looking trials gave a slightly higher hit rate than the not-looking trials. Conversely, in the N trials, all of which started with the starer not looking at the staree, not-looking trials involved no transition and gave a slightly higher hit rate than looking trials.

This lack of effect of the transitions is not what we expected. It implies either that scopesthesia differs from other senses in not responding to changes or differences or that the tests we carried out were too insensitive to detect them or were inappropriately designed. Probably the best way to detect such transitions would not be to signal when they occur, as we did, but to create a situation in which the transitions occurred unpredictably, and to monitor people's response to them physiologically, for example, by the galvanic skin response.

Overall, the pattern of results differed from the typical pattern in staring experiments, where there are usually above-chance hit rates in looking trials, around 60%, and chance-level hit rates, around 50%, in not-looking trials (Sheldrake, 2005). Here, the score was only slightly higher in looking trials (53.0%) than in not-looking trials (52.5%), and the difference was not statistically significant.

The usual pattern in standard staring experiments could arise because of a response bias in favour of saying "looking" rather than "not looking". In the absence of scopesthesia, a 5% bias would give a 55% hit rate in looking trials and a 45% rate in not-looking trials, with an overall average of 50% (Schmidt, 2001). If staring detection occurred in both kinds of trial at 5% above chance, then the hit rate would be 60% in looking trials and 50% in not-looking trials, as observed.

In the attentional transition experiment described in the present paper, there was no significant overall response bias: 50.3% of the guesses were "looking". Thus, in looking trials, taking into account the response bias, the hit rate was 2.7% above chance, and for not-looking trials it was 2.8% above chance, not significantly different. However, the two starees had significantly different response biases. PS's response bias followed the more common pattern in that it was in favour of looking. With her response bias of 3.2% in favour of "looking", the hit rate in looking trials of 56.3% was 3.1% above this chance level. In the not-looking trials her response bias would give a chance hit rate of 46.8%; the actual hit rate of 50.0% was 3.2% above this. JM's response bias went in the opposite direction: only 47.4% of her guesses were "looking", or, in other words, her response bias was -2.6%. Her hit rate in looking trials was 49.6% and in not-looking trials was 55.0%, which were 2.2% and 2.4%, respectively, above the chance level expected on the basis of her response bias.

These results closely fit a simple model for the probability P of a hit in looking and not-looking trials:

$$P(hit/looking) = 1/2 + b + s$$
$$P(hit/not\text{-}looking) = 1/2 - b + s$$

where b is the response bias, positive when the percentage of looking guesses is greater than 50%, and s is the effect of scopesthesia, with equal contributions in looking and not-looking trials.

In the control tests, both starees showed a response bias in favour of saying "not looking", with "not-looking" guesses making up 55.7% of the total. This bias may well have reflected the fact that they both knew that in these tests they were never being looked at.

The fact that both participants served as starees in over 70 tests each makes them unusually experienced, and the results in these experiments may not be representative of naïve participants with little or no previous experience. This is something that only further research can reveal.

A possible problem with the attentional transition test described here was that the starers gave two signals to the starees in each trial using a mechanical clicking device: the first signal indicated that the pre-trial period had begun. In the L tests this meant that the starer was looking at the staree; in the N tests she was not. The second click indicated the beginning of the randomized trial in which the starer would either be looking or not looking. The starer estimated the 3-second interval between the two clicks. This raises the possibility that she might have given subtle cues by unconsciously varying the length of the interval. However, the starees themselves were not aware of any differences of this kind. But perhaps they picked up subtle cues unconsciously. Unfortunately, we were unable to resolve this question definitively by a precise timing of the click interval trial by trial because the tape recordings were inadvertently discarded.

Starees might also have been influenced by other subtle cues, such as slight sounds from the starer as she turned her head. Another possible flaw was that we reused the same 20 randomized sheets and, hence, it is conceivable that starees might have unconsciously remembered the randomized sequences after they were exposed to them repeatedly. If so, the feedback they received should have enabled them to improve their scores very considerably with practice. But this did not happen. In future experiments, possible auditory cueing should be minimized, either by the use of sound-proof windows separating the participants or by the use of ear plugs or headphones. Also, a fresh randomization procedure should be used for each test.

Because the double-click procedure is potentially capable of introducing artefacts, it should be avoided in any further research on attentional transitions. A better method would be to use an electronic beeper that emits two beeps with a 3-second interval between them. An even simpler procedure would be for the starer to give just one signal at the beginning of the trial, having looked for 3 seconds previously in the L tests, or not looked in the N tests.

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