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The Lack of Effects on Human Muscle Strength of Light Spectrum and Low-Frequency Electromagnetic Radiation in Electric Lighting

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Introduction

In 1982, John Ott summarized his views on adverse effects of fluorescent lighting on human health in *Light, Radiation, and You.*[1] One claim in this popular book was that fluorescent lighting could influence human muscle strength, as measured by so-called "kinesiology testing."

"Kinesiology testing," claimed by some to aid in diagnosing a variety of "illnesses,"[2,3] involves a subjective evaluation of muscle tone in a contracting muscle. In the method used by Ott, the subject stands with an arm held forward, parallel with the floor, and with the palm down. The tester then pushes downward at the wrist while the subject resists this motion. Ott has publicly demonstrated that when a subject looks at a cool-white fluorescent light he is noticeably "weaker" than when the light is turned off or when a "full-spectrum" fluorescent tube is viewed. Using the same "kinesiology testing," he also concluded that electric-field shielding (by wire mesh) was needed on fluorescent light fixtures to prevent muscle "weakness."

Given the widespread use of such lighting, a muscle weakening due to fluorescent light could be a cause of concern. We carried out two studies on human subjects to evaluate rigorously and objectively whether such an effect exists. In the first study we used a strain gauge to measure the effects of incandescent and cool-white fluorescent light on muscle strength. In the second study we used "kinesiology muscle testing" in a double-blind test of the effect of grounding or not grounding a wire-mesh electrical shield on a fluorescent light fixture. This second study was the first time "kinesiology testing" had been studied in a strict double-blind experimental protocol.

Our results do not support the contention that muscle strength is affected by these lighting variables, and they clearly show that Ott's "kinesiology testing" is not a valid measure of muscle strength because the result is highly influenced by the beliefs (and possibly the suggestions) of the tester.

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Study I—Effects of incandescent and fluorescent light on shoulder muscle strength

Methods

Ten subjects (3 female, 7 male) who ranged in age from 20 to 36 years were studied. Testing was done in a chamber measuring 6½ x 6½ ft with a 7-ft ceiling, which was painted inside with White Reflectance Coating (Kodak) paint to avoid selective reflectance in the visible spectrum. Subjects sat with the left arm stretched forward and with the wrist pushed upward on an upholstered pad attached to a stationary arm of an Orthotron exercise machine to which a strain gauge was added. The strain gauge measured maximum voluntary muscular effort in forward extension of the arm (anterior deltoid), at 90 degrees. The output of the strain gauge (which was linear over the range studied) was measured by the 10-bit analog-to-digital (A-to-D) converter in a PDP-8 computer, at the rate of 100 conversions per second for three seconds. Five consecutive conversions were averaged, and the largest mean value in the three-second interval was taken as the maximum strength for that trial. Data were analyzed in A-to-D units, without conversion to absolute values since only within-subject differences were analyzed. Differences in maximum muscle strength for the same subject were studied under two lighting conditions: incandescent (six 60-W standard frosted bulbs) and cool-white fluorescent (two 40-W tubes), all operated at full voltage and intensity. The lights were mounted in a fixture above the subject's head, bathing with light both the subject and the wall viewed. The vertical illuminance at eye level was about 538 ± 10 lx (50 ± 1 fc) as measured by a Tektronix J16 illuminance meter placed in the position that the eye had occupied, aimed at the wall. The sequence of lighting was randomized by the PDP-8 computer to counterbalance the effects of fatigue during testing. A single trial consisted of two tests of muscle contraction, one under each light, with the order within each trial determined by a random choice by the computer. The light exposure before muscular contraction was about 30 seconds; thus, there was at least 30 seconds between successive contractions. The relatively rapid alternation between incandescent and fluorescent lights was not judged to be a problem since in public demonstrations Ott would switch even more rapidly from one light source to another and still obtain the effect. Furthermore, the effects, as publicly demonstrated, occurred within a few seconds of changes in the lighting. Each subject participated in three consecutive testing sessions, which were separated by a rest period of about 30 minutes.

Each session had 17 trials. The data for each subject consisted of 102 contractions (51 under each lighting condition), randomly ordered within pairs.

Results

Although the muscular effort was, at times, enough to cause fatigue, this did not affect the results because the lighting exposures were paired throughout the testing session, and the statistical analysis compared differences in the pairs. An example of the effects of fatigue during the testing is shown in **Figure 1**, where a gradual decline of maximal strength during the testing can be seen. **Figure 2** shows average variability within a testing session, no decrease in mean strength over time, and a within-session variance that was larger than usual.

The results showed that the differences in maximal muscle strength between the two lighting conditions was essentially zero, consistent with no effect of lighting condition on muscle strength. Across all subjects, the mean difference in muscle strength was less than 0.2 percent of the average strength measured, despite high variability (mean = 0.17 ± 2.3 A-to-D units), with a very high probability of being due to chance ($p > 0.85$ by t-test). We also examined the data for each individual subject with a paired t-test to determine the probabilities that the differences in muscle strength in that subject were due to chance. The values are shown in **Table 1**; four of the subjects had p values of less than 0.05. However, the Bonferroni correction indicates that for statistical significance to be reached in this many tests, the p value of any one subject must be less than 0.05/10, *i.e.*, 0.005. None of the subjects reached that level. Of the two that were near this value (subjects 3 and 6), the differences were counterbalanced, one showing a positive difference and the other a negative difference. The same was true of the two other subjects that had probabilities of less than 0.05 (subjects 2 and 4).

To further test for the possibility that the differences in muscle strength in these subjects were due to chance variations, the data from these four subjects were further analyzed by applying a t-test to the odd-numbered trials separately from the even-numbered trials. **Table 2** shows that the data within subjects were quite variable, with little agreement as to the size of a lighting difference in muscle strength between the two halves of the data, though the differences were still always in the same direction. This further suggests that the differences recorded in these four subjects were due to chance. Repeated observations on these subjects would be needed to resolve whether the effects were chance variations or reliable effects.

Finally, we calculated the power of our overall results to determine the likelihood that we had failed to detect a difference between the two lighting conditions that may have been present (Type II error). We calculated that, with 90-percent confidence, *if* there was an average population difference in muscle strength as a result of different lighting conditions, it was *less* than 4 percent of the measured strength. This is of interest since the effects of suggestion upon muscle strength have been reported to be 8 to 10 percent,[4] with the implication that if there were any effects of lighting on shoulder muscle strength, they could easily be obscured by psychological effects (such as suggestion or "mindset"). The use of double-blind experimentation is clearly possible in experiments in which lighting is changed, and the subject is aware of the changes, as in this experiment.

Study II—Effects of grounding of fluorescent-light shielding on kinesiology muscle testing

Since the first study proved negative, a number of objections were raised by Ott in personal communications concerning the adequacy of the study to show the purported effects. The method of measuring "muscle weakness" was certainly different from Ott's. Although the same muscles were used, the measure of the peak voluntary contraction was not the same as the kinesiology testing method. That method involves a more prolonged effort on the part of the subject, and the tester evaluates not only the strength, but the "springiness" or resiliency of the rigid arm. Nor were the experimental conditions similar in our study in that the experiment was conducted in a metal-shielded room, with external electronic equipment recording the results. Ott hypothesized that the difference in method and conditions may have affected our results. For these reasons we designed an experiment in which the independent variable was electrical shielding of a fluorescent light fixture, and the measurement was done using "kinesiology testing" by Ott himself in a noninstrumented apartment, away from electronic equipment, incorporating a fully double-blind test.

Ott has described a number of factors which result in muscle "weakness" during kinesiology testing.[5] One of these factors is the absence of electrical shielding on fluorescent light fixtures. Since the shielding (a metal wire mesh not present in standard fixtures) could be connected or not connected without the knowledge of either the subject or the tester, this variable provided an excellent means to test whether the weakness found by kinesiology testing would occur under double-blind conditions. With Ott's cooperation, we set up an experiment that met all of his requirements, with both unblinded and double-blind testing of the same subjects. Thus, Ott could be sure that the phenomenon to be tested (weakness when the shielding was ungrounded) was being observed both before and after the period of double-blind testing.

Methods

Ott personally screened, pretested, and accepted 14 subjects (8 male, 6 female) for inclusion in the study. He instructed the subjects not to ingest coffee, sugar, or tobacco for four hours before testing, and to wear natural-fiber clothing. The testing was done in the daytime in the living room of an apartment several blocks from the University of California, San Francisco, in a building with wooden floors, and with the windows uncovered. The gas hot-air furnace was not on. Each subject stood, looking directly into a fluorescent light fixture, which was at eye level about 5 ft away. The fixture (**Figure 3**), manufactured by Environmental Systems, Inc. and supplied by Ott, contained four 40-W fluorescent lamps and a 15-W black light, a combination which provided a "close-to-daylight" spectrum. The lamps were surrounded by a two-layer wire mesh screen to provide shielding of the 60-Hz electric field generated by the lamp/ballast system. A long cord containing a switch could connect or disconnect the grounding of the fixture, including the wire mesh shielding. This switch was controlled by a technician on the opposite side of the room, out of sight of the tester and subject. The sequence of connection and disconnection had been determined before the testing, and was unknown to Ott. When the technician announced the position of the switch before a trial, both the subject and Ott were unblinded. When the position of the switch was not announced, both the subject and Ott were blinded to the actual position of the switch (double-blind). Without the knowledge of Ott or the subjects, for two of the subjects the position of the switch that was reported in the unblinded trials was the exact opposite of its real position.

The testing was divided into four blocks of eight trials each, as follows:

Block	Condition
1	unblinded
2	double-blind
3	double-blind
4	unblinded

Any differences between blocks 1 and 4 would demonstrate systematic changes during the blinded testing. All of these plans were made with the full agreement and knowledge of Ott. For 11 subjects, each trial consisted of the subject looking into the light and then squeezing maximally a hand dynamometer (Lafayette Co.) with the preferred hand. This was immediately followed by the "kinesiology test" by Ott, in which Ott made a forced choice as to whether the muscle was "weak" or "normal." An observer (MAG) recorded the dynamometer readings and Ott's announced observations without comment. Ott did not know the results of the dynamometer test when he rated the muscle. There was a two-minute rest period between each block of trials. For three other subjects, a spring scale was used instead of the "kinesiology test." The scale (American Family Scale Co.) was suspended from a short rope that was looped over the wrist of the subject. Another rope hung from the scale, which was pulled downward by Ott until the subject could no longer hold his/her arm parallel to the floor. Ott announced the scale reading, which was recorded. The code for the switch position was not broken until all of the subjects had been tested.

The effectiveness of the radiated electric field shielding of the lamp fixture was tested with an active monopole antenna and an electromagnetic spectrum analyzer. Electric fields generated by the fixture were measured in a laboratory, and the results compared with measurements taken by the same equipment in a single-dwelling house.

Results

Kinesiology testing—Ott reported muscle weakness in all subjects during the unblinded blocks of trials. On 170 of the 176 unblinded trials (in 11 subjects), Ott's reports of muscle strength agreed with his previous unblinded observations—"normal" when the shield was reported as grounded, and "weak" when reported as ungrounded. The high agreement (97-percent concurrence) between the first and last set of unblinded trials indicates that there were no order effects present in the experiment, nor "deterioration" or "fatigue" during the testing. Thus, the results of the first eight unblinded trials were combined with the last eight unblinded trials (blocks 1 and 4 were combined) for data analysis. Similarly, the double-blind trials (blocks 2 and 3) were also combined.

In the testing of the two subjects in which Ott was deceived about the actual position of the switch, there was 100-percent agreement between the muscle strength and the *reported* position of the switch (0-percent agreement with the *actual* position of the switch). When the testing was double-blind, the agreement between the switch position and the "weakness" observed by Ott across all subjects was only 53 percent. For the 11 subjects, the fraction of trials in the unblinded tests in which the rating of muscle strength agreed with the reported switch position ranged from 0.81 to 1.0 (**Table 3**). When the testing was double-blind, the range was 0.31 to 0.75 (**Table 3**). The mean proportion "correct" was computed using arc-sin transformed data;

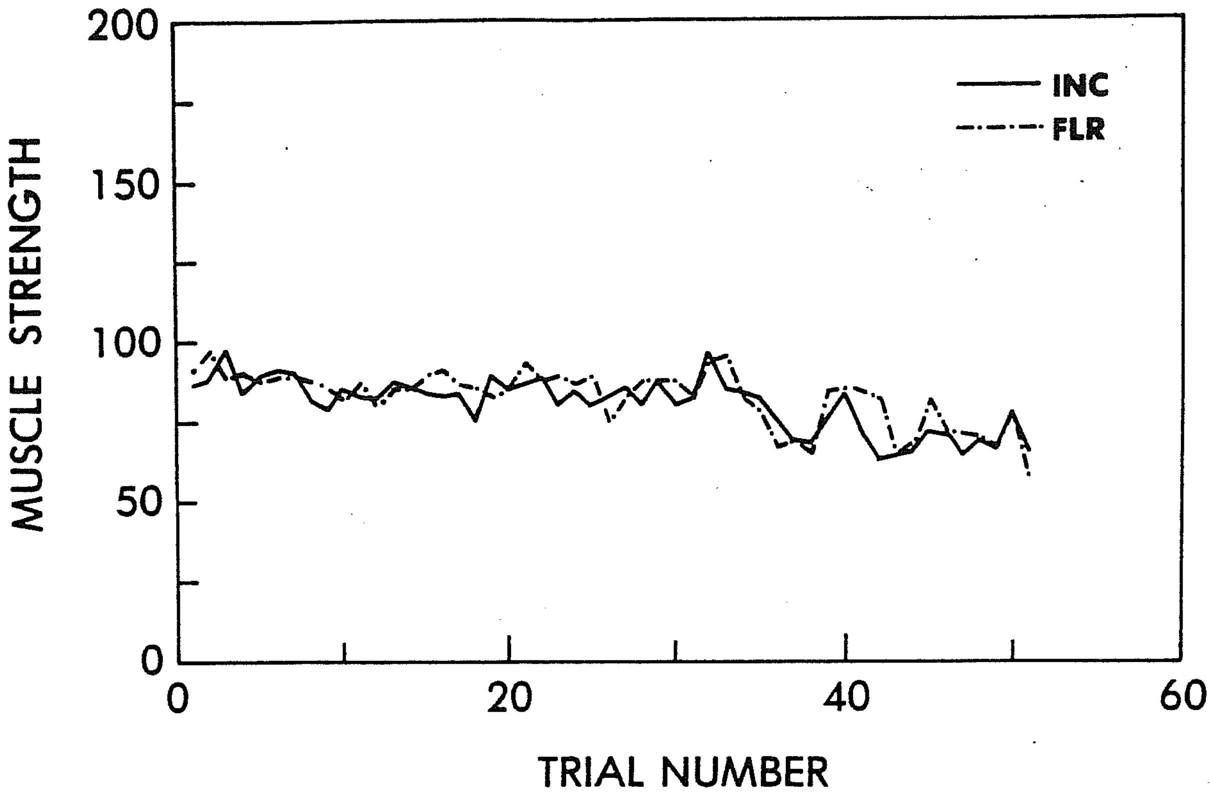

Figure 1—Muscle strength, in one subject, as a function of the number of the trial, showing variability and general decrease in strength as the number of trials increases.

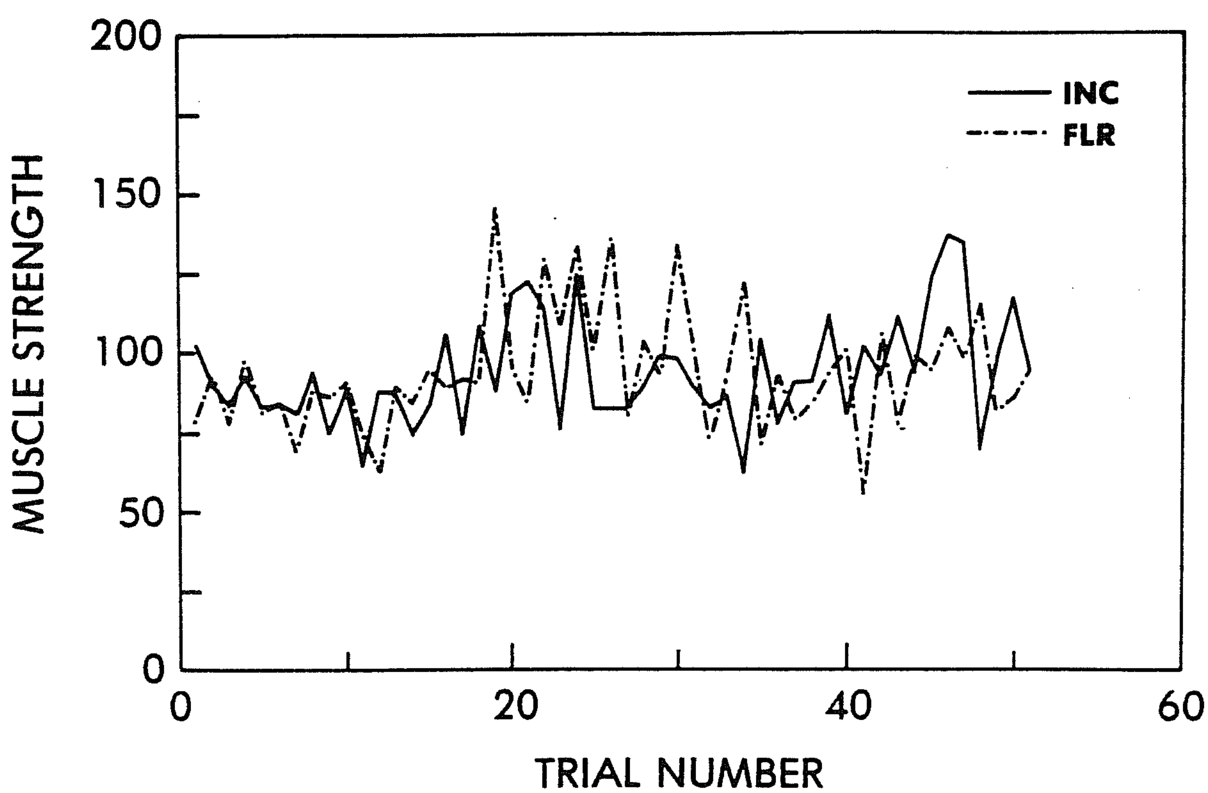

Figure 2—Muscle strength, in another subject, as a function of the number of the trial, showing variability but no systematic change with the number of trials.

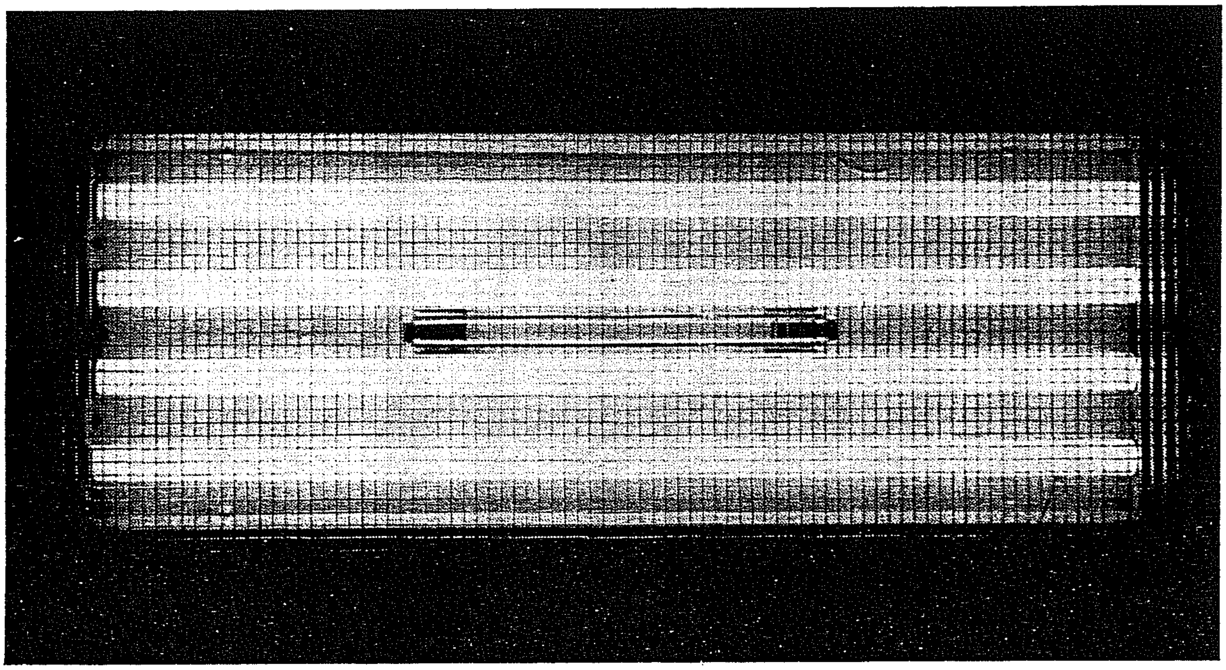

Figure 3—Photograph of the lighting fixture with which the testing of grounded and ungrounded shielding was conducted.

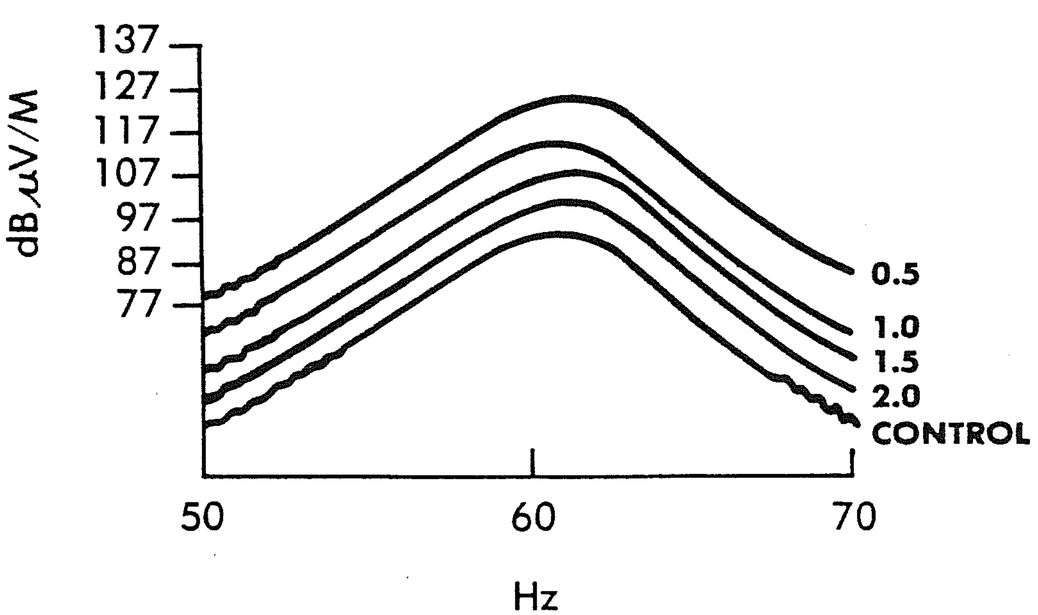

Figure 4—Measured electric fields at frequencies from 50 to 70 Hz, at different distances in meters from the light fixture when ungrounded. The lowest curve labeled "control" is the room background radiation with the lamp turned off. This curve also corresponds to the grounded light fixture.

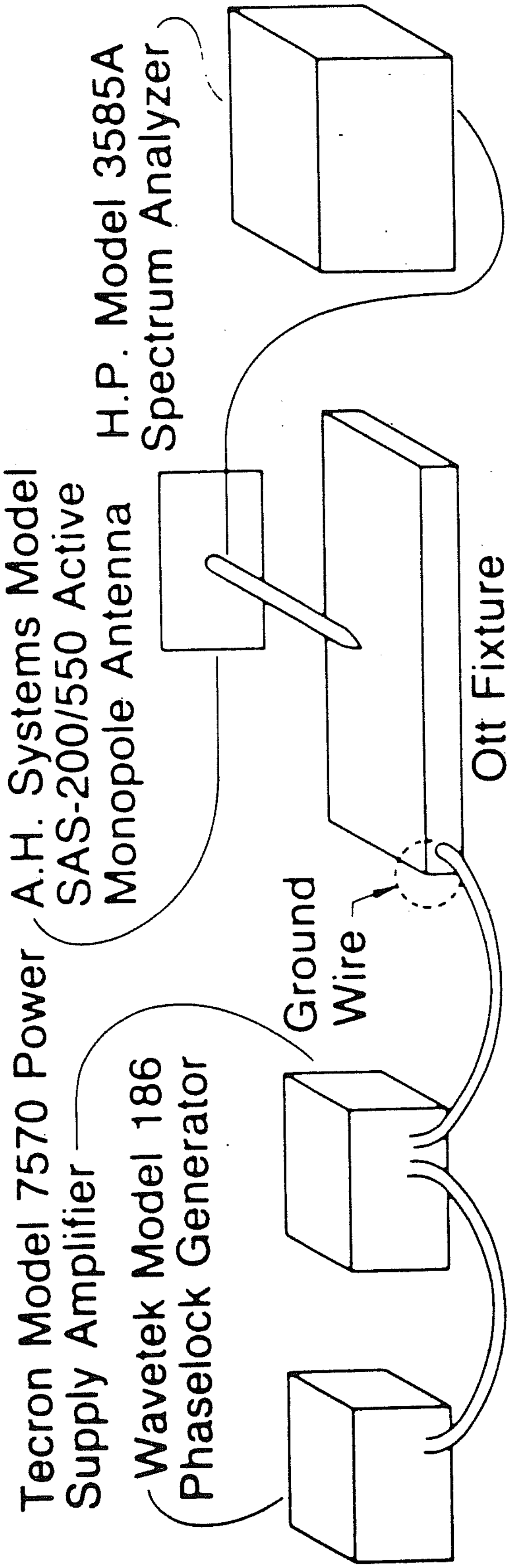

Figure 5—Experimental arrangement under which the electromagnetic emission spectrum was measured to determine the attenuation provided by the shielding. The Wavetek generator drove the energizing power supply at 79 Hz. In this manner, the effect of the shielding could be measured at a frequency where the background was much smaller than at 60 Hz. For the wavelengths under consideration, the effect of the shielding will be essentially the same at either 60 Hz or 79 Hz. The incoming ground wire from the power supply was separately switched in a manner similar to the original Ott fixture.

the mean proportion correct in the double-blind testing was 0.53, compared with 0.97 in the unblinded testing. The difference between these two proportions was highly significant (p < 0.00001 by paired t-test). Clearly, changing the experimental conditions from unblinded to blinded without otherwise changing any other variables significantly altered the results.

Despite this conclusion concerning the kinesiology testing method, we wanted to be sure that we had not overlooked a small effect, *e.g.*, since the blinded result was 53 percent and not exactly 50 percent (pure chance), perhaps the 53-percent agreement when blinded might indicate a small muscle weakness that was lost in unblinded testing because of the inaccuracy of the kinesiology method. However, statistical testing (described in detail in Appendix I) showed that given the small number of subjects that we studied, a 53-percent result would be expected by chance variations alone. Our data support the null hypothesis in its strongest statement—that all variation was due to binomial sampling error, and the true effect was 0.5 (chance) *for all subjects*—rather than the weaker null hypothesis that there was a variation of muscle strength across subjects, with a mean of 0.5 (see Appendix I). A true effect of 0.5 across subjects is consistent with random guessing by the kinesiology tester.

We also determined the power of our test, to evaluate how small a difference in muscle strength we would have detected, had it existed. The power computation for this type of experiment is complicated, and will be presented in detail elsewhere (Raz and Fein, submitted for publication). Briefly, the statistical test used is a t-test to determine if the mean of the arc sin-transformed proportions differs from 0.5 (the chance level). Power tables for the t-test are readily available, but they are tabulated as a function of the variance of the underlying measure, in this case the p_i. Since the alternative hypotheses are stated in terms of the true proportions for individuals π_i, we used the relationships presented in the Appendix to derive the variance of p_i from the variance of π_i. The power was then obtained from the tables in Cohen.[5] As a check that the arc sin-transformed data did not deviate from the t-test assumptions in a way that would distort the power computations, power was also computed using a random simulation procedure (described in Raz and Fein, submitted for publication). The simulation results were very similar to the tabulated powers, indicating that the t-test is valid in these conditions.

The power of our experiment with respect to various sizes of effect is presented in **Table 4**. For these computations we used $k = 16$, $n = 11$, and assumed that $\text{S.D.}(\pi) = 0.1$. **Table 4** shows that had the mean actual effect in the population been as little as 0.65, we would almost surely ($p > 0.87$) have had a significant result in our experiments. The pure chance level is 0.5; an effect at 0.65 requires a correct identification greater than by chance only 15 times out of 100. Thus, we conclude that even a relatively small effect of grounding does not exist. Thus, the high "detection rate" by kinesiology is entirely artifactual. This conclusion is further supported by the 0-percent agreement between muscle "strength" and grounding in the two cases where during the unblinded trials the announced switch position was opposite from its actual position.

Other strength measures—With other strength measures, we also were unable to find any differences between the shielded and unshielded conditions. The hand dynamometer readings during the blinded trials under the shielded and unshielded conditions gave means of 33.4 and 33.3 kg, respectively. By paired t-test the probability that the small difference was due to chance was greater than 0.5.

For the three subjects in whom the spring scale was used rather than the kinesiology muscle test, the mean scale values for the shielded vs the unshielded conditions during the blinded trials, were 25.6 and 25.8 lb, respectively. The results, both within and across subjects, showed almost total overlap in scores between the two shielding conditions. According to the paired t-test the results were not significant ($p > 0.5$), but to reach statistical significance with such a small number of subjects there would have to have been very large differences which were consistent across all three subjects.

Electric field shielding—In order to test whether the shielding had any effect on the conditions of the physical environment, the electromagnetic fields generated by the Ott lamp and fixture system were studied. The fields generated by the system are comprised of both oscillating electric and magnetic fields with values that predominate at the line frequency of 60 Hz.

To measure the RMS electric field at various distances from the Ott fixture, we used a one-meter-length, active monopole antenna (A.H. Systems model SAS-200/550) and an electromagnetic frequency spectrum analyzer.

Because the manufacturer's antenna calibration did not extend below 10 kHz, it was necessary to establish a calibration procedure to provide an approximate calibration of the measurement system at 60 Hz. To this end, the antenna was placed within 3 mm of, and oriented along the axis of, a fully exposed, ungrounded F-40, T-12, fluorescent lamp, 4 ft in length, with the ballast and incoming wiring being shielded. The voltage drop over the 1-m length of the positive column of the lamp was approximately 100 V rms. Thus the electric field measured by the antenna in this geometry has an upper limit of 100 V/m rms. The value of the antenna gain recorded by the spectrum analyzer was then taken as corresponding to an electric field of 100 V/m. The antenna was then placed at various distances from the ungrounded Ott fixtures and the gain recorded by the spectrum analyzer converted into an electric field value by assuming linearity. Thus the values of the electric field reported here are likely to be higher than the true values although they are in good agreement with the values that were obtained by extrapolating the manufacturer's calibration to 60 Hz.

From measurements on Ott's lighting fixture, we show in **Figure 4** the graph of the generated electric field across the antenna at distances of 0.5 to 2 m when the antenna was at the fixture midpoint and perpendicular to the lamp axes within the fixture. The peak value at 60 Hz and at 1 m corresponds to an electrical field of 0.6 V/m. The lowest curve in **Figure 4** shows the background electric field in the room when the fixture was unplugged and all the lighting turned off, and is some 20 dB below the value seen with the ungrounded fixture at 1 m. Since grounding the fixture reduced the measured electric field to background levels, the net effectiveness of the shield could not be determined in this manner. For this reason the experiment in **Figure 5** was performed, in which four aluminum plates (¼ inch x 1 inch x 46½ inches) replaced the fluorescent tubes in the fixture and were energized as shown in **Figure 5** to produce a much higher electric field. The shielding reduced the emission at a test frequency of 79 Hz by a factor of 68 dB (see **Figure 5**). Since this same 68-dB attenuation ratio can be expected to hold at lower emission levels, and at 60 Hz, the electrical shielding of the fixture used in these experiments, when grounded, must have brought the emitted radiation at 60 Hz far below that of background levels.

The same instruments were used to compare the electric field strength of the ungrounded fixture in the laboratory (including background fields—**Table 5**) with the strength of ambient fields in an ordinary house (**Table 6**) where the internal electrical wiring was copper covered by a plastic coating (*e.g.*, Romex) rather than the metal conduit of the laboratory setting. The values of the 60-Hz electric field in the house were in the vicinity of 5 V/m (**Table 6**), even higher than those of the ungrounded Ott fixture at a distance of 0.5 m in the laboratory (**Table 5**) with this ratio being independent of any antenna calibration factors.

We do not know the electric field levels in the apartment used for the kinesiology testing. However, it is likely that the values there were similar to or greater than those observed in the house, since the electricity in the apartment was carried by widely separated insulated wires held by ceramic "pegs" and there were power lines outside the windows of the living room where the testing was conducted. Thus, since the relative level of 60-Hz electric field in an ordinary house is even greater than that from the fluorescent fixture claimed by Ott to cause weakness in the experimental subjects (in the unblinded cases), then we would expect people to be aware of some weakness upon entering such houses, should the phenomenon actually exist.

The magnitude of the 60-Hz magnetic field close to a F-40, T-12, fluorescent lamp with a typical lamp current of 400 ma would have an upper limit of magnetic field of order 0.5 amps/meter or approximately less than 10^{-2} gauss. The field would be expected to decrease with increasing distance away from the lamp. If the dependence on distance was similar to the electric field fall-off the magnitude of the magnetic field at a distance of one meter would be less than 10^{-5} gauss. However, studies by Tucker and Schmitt[7] have shown that 60-Hz magnetic fields of 7.5 gauss are undetectable by a large sample of human subjects. Thus it is unlikely that there is any provocative magnetic stimulus associated with the lamp.

Discussion

Comparison of the double-blind and unblinded trials clearly shows that the expectations of the tester, subject, or both completely determine the results of the "kinesiology testing" method. (We did no experiments to distinguish the effect due to the subject and that due to the tester, but suspect that the effect occurs because of the expectations of the tester, since some of the subjects did not know the purpose of the testing nor the outcome Ott expected.) The inadequacy of kinesiology testing is shown by the following results: 1) the apparently totally random muscle ratings in the blinded trials, 2) the large differences in the results in the blinded and unblinded trials (53 percent vs 97 percent), and 3) the result when Ott was deceived by being told the opposite of the actual switch position (100-percent agreement of muscle test with expectation, 0 percent with actual switch position).

Furthermore, we have been able, when trying kinesiology testing on each other, to consciously control the "result"—either "normal" or "weak"—by applying the testing force slowly to obtain a "normal" response and rapidly to obtain a "weak" response. This effect of the tester on the kinesiology test result can be understood in terms of the biomechanics of the situation. Just before the application of the downward force the subject's arm is not moving; thus, the muscular forces in the subject's arm must be in equilibrium. The subject has to produce an additional upward force to oppose the force of the tester when the tester presses down on the wrist. If the tester applies force slowly, in a fraction of a second the subject can develop a force sufficient to oppose that of the tester, and the subject will be said to have a normal response; but this cannot be done when the "testing" force is applied rapidly, and the subject will be said to have a weak response.

It is certainly possible that the erroneous estimation of muscle strength was completely unconscious on Ott's part.

We unequivocally conclude that kinesiology testing is easily influenced by the expectations of the examiner, unless the testing is done double-blind. Thus, conclusions based on kinesiology testing cannot be accepted without some other method of verifying the purported effects. Further, under double-blind conditions, we found no evidence that fluorescent light or shielding had any detectable effect on muscle strength. Our experiments had sufficient power to rule out all but very small effects. Given our results and the gross inadequacy of kinesiology testing, the claimed effects of shielding and of fluorescent lighting on muscle strength are now highly suspect. Study 1 was completed before Study 2, but it confirms, as do the hand dynamometer results of Study 2, that the effects on muscle strength of fluorescent lighting and shielding claimed to be discoverable with kinesiology testing are extremely small or nonexistent. In other studies attempting to detect an association between muscular weakness from visual stimuli, the effects were also small, or not significant.[8-10]

We note also that it is unlikely that the changes in the small amount of radiated electric field from the light fixture would affect human muscle strength, given that an ordinary household may have 30 to 60 times the amount of radiation of the lamp. Thus, merely walking around in a house would change the amount of radiation exposure more than the switch on the shielding of the fluorescent fixture that we tested. If the electromagnetic radiation from the fluorescent fixture affected muscle strength, one would expect that such weakness would be even more noticeable at the higher levels of radiation found in houses.

For all of these reasons we conclude that the claims of muscle weakness have been based upon faulty observations. Unless data on muscle strength are provided by some other means than unblinded kinesiology testing, we conclude that the hypothesis that fluorescent lighting causes weakness is not worthy of further study.

Acknowledgments

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Addendum

After Study 2 was finished, Ott revised his book on his findings using the kinesiology method, in which he reports a variety of "causes" of muscle weakness. In this book, Ott describes Study 2 and states that (during the unblinded testing) his "scoring came very close to 100 percent in determining that such items that were thought to cause muscle weakness, in fact, actually did so." He fails to recognize that the experiment showed that the result was entirely dependent upon the knowledge of the tester/subject, and hence in no way can be attributed to the light shielding. He also concluded that our study showed "that the power of thought has ... a direct major effect on muscle strength ...." If this is so, how can he be certain that it was not thoughts that have determined all of his results from kinesiology testing—not only those of fluorescent lights and shielding? For Ott has also concluded that muscle "weakness" is caused by all of the following: video display terminals, battery-powered calculators, digital watches, high-voltage electric transmission lines, ionizing smoke detectors, microwave ovens, Pyrex, synthetic clothing, synthetic shoe soles, synthetic floor coverings, synthetic violin strings, manufactured cigarettes, white flour, certain processed foods, margarine, microwave-cooked hamburger (held in a bag), a chain of people holding hands with one person holding a single-serving packet of white sugar, fluoridated toothpaste, a glass of fluoridated water held in the hand, metal eyeglass frames, ordinary eyeglasses and sunglasses, orange paint, pink cards, blue cards in conjunction with a note of high C, a warmed hammer head previously chilled only if held in the dominant hand, a magnet with the north pole pointing outward only if held in the dominant hand, a lightning rod pointed outward from the chest, a wire pyramid similarly held, pointed prongs on a ring, a wax pencil pointed at one but not both ends, a lead pencil held in ordinary writing position, and a package of Rely tampons.[5]

Appendix I

There are two sources of variation in an experiment such as this: across-subject variance and within-subject variance. Across-subject variance reflects variation in the actual effect of lighting condition on muscle strength across individuals without any sampling error, *i.e.*, the actual effect is the proportion of correctly detected trials that would occur if an infinite number of trials were used. The within-subject variance is binomial sampling error which arises from using a finite number of trials for each individual. If we call the actual proportion for each individual π_i and the obtained proportion p_i, then given k = the number of trials for each subject and n = the number of subjects, the variance of unobservable π_i can be derived from the variance of the observable p_i by the following equations:

For population parameters:

$$\text{var}(\pi) = \frac{k\ \text{var}(\pi) - u + u^2}{k - 1}$$

where u = $E(\pi)$, *i.e*, the expected proportion, and *var* is the variance of each measure.

Using Sample values we get the estimate:

$$\text{var}(\pi) = \frac{k\ s^2\ (p) - \bar{p} + \bar{p}^2}{k - 1}$$

where s^2 is the sample estimate of the variance and $\bar{p}$ is the mean of π for all values of i.

Applying these equations to our data, we find that the variance of the π_i is zero. Thus, for our experimental results in the blinded condition, our best estimate is that all of the variance is binomial sampling error. Another way of putting the conclusion is to state that our data are consistent with a situation in which the results of all trials were obtained at random from a pure binomial guessing situation.

Table 1—Mean strength (in A-to-D units) for each of the subjects under incandescent and fluorescent lighting, randomly alternated, and the differences and probabilities for the differences observed. Note that the differences are of either algebraic sign.

Subj. #	Mean Strength Incandescent	Mean Strength Fluorescent	Difference	p
1	94	94	−0.4	0.9
2	124	126	−2	0.02
3	114	112	+2	0.007
4	83	80	+3	0.04
5	182	177	+4	0.2
6	81	83	−2	0.006
7	116	118	−2	0.2
8	75	74	+1	0.5
9	88	88	−0.2	0.9
10	60	59	+1	0.4

Table 2— Analysis of odd-numbered and even-numbered trials of the low-probability subjects of Table 1.

	Odd-numbered		Even-numbered	
Subject #	Mean difference	p	Mean difference	p
2	−0.3	0.06	−2	0.1
3	+3	0.004	+1	0.2
4	+5	0.03	+0.8	0.7
6	−3	0.04	−2	0.09

Table 3—Fraction of trials with agreement between tester's evaluations and switch position in each of the 11 subjects.

Subject #	Unblinded*	Blinded**
02	0.81	0.63
03	1.0	0.44
04	1.0	0.50
05	1.0	0.56
06	1.0	0.31
07	1.0	0.56
10	1.0	0.50
11***	1.0	0.44
12	0.94	0.63
13	0.88	0.75
14***	1.0	0.56
Geometric Mean	0.97	0.53

*Fraction of trials agreeing with *reported* switch position.
**Fraction of trials agreeing with *actual* switch position.
***The subject in whose test the switch position in unblinded position was reported exactly opposite of the actual position.

Table 4—Calculated power, as a function of mean true effect size, based upon both tables and simulation.

Mean π_i	Power from tables	Power from simulation
0.50	0.05	0.04
0.55	0.25	
0.60	0.60	0.60
0.65	0.87	
0.70	0.98	0.99
0.75	> 0.99	

Table 5—Magnitude of measured 60-Hz field as a function of distance from the lighting fixture, ungrounded, as measured in a laboratory.

Distance between fixture and antenna (m)	Electric field (V/m)
0.5	1.6
1.0	0.6
1.5	0.3
2.0	0.1

Table 6—Background 60 Hz-field strength in and around a single-story house, measured 0.5 m above floor.

Location	Electric field (V/m)
Kitchen	6.5
Living room	3.6
Under power line	2.9

Discussion

I have reviewed the paper and I find it a solid piece of work establishing lack of effect on human muscle strength of exposure to the sources examined under the experimental conditions used. Moreover, the paper demonstrated the subjective nature and unreliability of the "kinesiology testing."

Minor comments:

1. Illuminance should be stated in lux (lx) and not fc. Use SI throughout the text, *e.g.*, chamber measurement should be stated in meters, not feet.

2. At power frequencies, one should speak of fields and not radiation. I fail to see the reason for measurements in the absence of effects. If the authors insist, because of misunderstanding in Mr. Ott's publication, on addressing the E field, they should describe and do it correctly. No such thing as radiated voltage exists. Measurements are performed in instructive fields, the E and H components should be both measured. Fluorescent tubes are extremely noisy electromagnetic sources, *e.g.*, they generate microwave white noise. The fields measured are rather low (0.6 V/m), the system used not very sensitive. The antenna is not described adequately, serious calibration problems exist for measurements of 60 Hz-fields with a monopole. See R. Tell in Gandolfo, M., Michaelson. S.M., Rindi, A., (eds.) *Biological Effects and Dosimetry of Non-Ionizing Radiation: Static and ELF Electromagnetic Fields,* Plenum Press, New York, NY, 1985. What was (hopefully) measured was the RMS electric field strength.

The part of the paper concerning electromagnetic radiation below the optical range may be deleted or has to be revised for credibility's sake.

The whole text could be shorter, and more directed to the demonstration of the obvious unreliability of kinesiology testing, which was convincingly demonstrated, as was also the lack of effects on muscle strength. **Figures 4** and **5** need better description or should be deleted.

Dr. Czerski

The paper appears to be scientifically sound from both a design and analysis standpoint. The authors have done an excellent job of subjecting the claims of Mr. Ott to a rigorous test and have presented their results in a clear and unambiguous fashion.

The only part of the paper that troubles me is the last few paragraphs where the authors discuss Ott's response to the results. This might be better treated as an addendum rather than part of the main paper. The paper stands nicely by itself and the last section, in my opinion, detracts from its objectivity. It may well be necessary to point out Ott's shortcomings, but it would be better done as a separate statement.

This review is short because there is little to say. The experiment is well conceived, properly conducted, and appropriately analyzed. It is well written, if a little long on details which seem to be included to ward off conceivable criticism from Ott's followers.

Alan L. Lewis, OD, PhD

Author's rebuttal

Following the suggestions made by the reviewers, the units for illumination have been given in both SI units with the older English units in parentheses in the one place where previously footcandles had been written.

The portion of the paper dealing with values and measurements of electromagnetic fields generated by the fixture has been expanded and hopefully made more comprehensive.

We feel this information is more valuable to the reader than eliminating this section for two reasons. First, it is useful to know the order of magnitude of the field strengths even when they do not produce an effect because a response might be measurable at a different value. Second, without some knowledge of the relative value of electric fields one commonly encounters in a typical residence compared to the fields generated by the fixture, the reader is left with less quantitative appreciation for the proposition by Mr. Ott.

Furthermore, the *relative* values comparing the fields produced by the fixture with typical fields in a residence will be independent of concerns about antenna calibration.

The reference provided in Dr. Czerski's response is not yet published so that we are unable to review the relevance of this work. Plenum Press indicates release of this publication in March.

With regard to Dr. Lewis' comments, we thought the criticism was apt, and have transferred the final paragraph to an addendum.