Scientific Evidence for Claims about Full-Spectrum Lamps: Past and Future

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Background

Many people are uncomfortable with electric lighting for indoor activities. There is widespread concern that it simply isn't natural, which leads rather easily to the suspicion that it isn't good for people (Veitch, Hine, & Gifford, 1993). One technological response to this has been the development of fluorescent lamps that emit a broader spectrum of electromagnetic radiation in the visible range: the full-spectrum lamp. The sometimes implied and sometimes directly stated promise is that light which more closely mimics sunlight will produce a range of benefits, including better health, greater productivity, and more satisfaction.

Much research on this topic has been inexpert, yet quite strong claims about full-spectrum lighting based on it have been made in some quarters. In 1986, the Food and Drug Administration issued a fraud warning about one manufacturer whose claims exceeded what the FDA felt the research warranted. It is time for a reminder that strong claims require quality research. Before some research guidelines are described, it may be of interest to speculate why research in this area has been suboptimal.

First, some researchers in the area have lacked training in research methods. Second, the actual findings of studies sometimes have been misinterpreted in self-serving ways by secondary authors. Third, in some respects, research with electric lighting is inherently difficult. Nevertheless, a considerable number of studies that putatively demonstrate the superiority of full-spectrum lamps have been published or circulated. Table 1 lists all the benefits of full-spectrum lighting that I could find reported in the literature (the studies I could locate are listed in Appendix A).

Table 1
Some Outcomes Attributed to Full-Spectrum Illumination

Physiological:	Improved Visual Acuity
	Reduced Fatigue
	Fewer Dental Caries
	Accelerated Maturation
	Improved Neural Functioning
Clinical:	Reduced Seasonal Depression
	Reduced Bulimia
	Reduced Hyperactivity and Repetitive Behaviour
Performance:	Greater School Achievement
	Improved Attendance
	Greater Attentiveness
Affect and Cognition:	Improved Mood
	Preferred Over Other Illumination Spectra

Bad science primarily would be of concern only to academics if it were not the case that full-spectrum lamps are much more expensive than conventional fluorescent lamps. The research reports that have circulated, together with media reports based on them, have managed to create an important demand for full-spectrum lamps. Many offices and entire schools have been re-lamped with full-spectrum lamps at considerable cost because someone demanded them. If the lamps are not superior to conventional lamps, this is a waste of funds that might be used for other, more worthy, purposes.

Thus, the issue for scientists simply is whether the lamps really make any difference to human functioning, but the issue for facility managers is whether their superiority, if any, is worth the extra cost (the lamps themselves cost about 4 to 8 times as much as conventional bulbs, and they typically produce around 30% less light). Both scientists and facility managers must base their decisions on research. But the research must be of the highest possible quality.

The Quality of Research on Full-Spectrum Lighting

It is important to remark at the outset of an admittedly critical presentation that no single study is perfect. The best studies in any discipline from astronomy to zoology all have flaws that can only be overcome by conducting further studies which also have flaws, but may at least be different flaws. The best studies have few flaws. Gradually, although no individual study is perfect, a picture of the phenomenon in question emerges that is clear and quite probably correct.

The problem in full-spectrum lighting research is that so many studies have so many flaws that the picture is very cloudy. Many flaws may be found that could quite easily have been corrected, thus improving the credibility of the study. Even the best studies seem to have at least one flaw, like Achilles' heels: every reasonable effort still leaves a soft spot in the research design. However, the impetus for this paper, put bluntly, was the realization that many full-spectrum lighting studies have Achilles' heels all over their bodies!

A Research Checklist

A brief paper such as this cannot hope to serve as a text on research methods. However, it may serve as a reminder of some research design issues that lighting researchers may find helpful. The goal is to restrict the list of design issues to those I have personally seen mishandled in the lighting literature listed in the Appendix, although I do not wish to single out particular studies; the goal is to improve research in the area, not to denigrate individual researchers.

In the following list, I attempt to emphasize issues that seem particularly important to lighting research as opposed to social science research in general. None of these issues can be considered in the depth they deserve; the word "checklist" is appropriate. Researchers who aren't sure what I'm talking about would be well-advised to consult a standard research methods book, such as Sommer and Sommer (1991). A selection of titles is listed in Appendix B.

Here, then, is the checklist:

Section I: Research Participants and Measures

1. Sample Size. Everyone knows that if the goal of the research is to generalize the results to a population, then larger sample sizes allow more certain tests of the researcher's hypotheses (of course, if the goal is to understand a particular individual, a sample size of 1 is perfectly acceptable!). Numerous studies of Seasonal Affective Disorder (SAD), for example, have employed sample sizes smaller than 20, even smaller than 10. It might be claimed that this is necessary because there aren't many SAD cases available. However, this contrasts with claims that as many as 14% of the population suffers from SAD-like symptoms ("Winter blues...", 1989). This means that as many as 40 million people in North America are available as potential subjects. This would seem an ample population from which to draw an adequate-sized sample!

The particular number of subjects needed depends on the size of the effect (see below) the researcher hypothesizes for the effect in question (i.e., how strong the influence of full spectrum lighting is on this particular outcome). The use of too few subjects results in a low-powered design (Cohen, 1988), which means the investigator is less likely to find an effect that really does exist, unless that effect is a very powerful one.

Many lighting effects appear to be subtle, that is, to have small effect sizes; these will not be found when the number of subjects is small. Thus, a small sample size actually handicaps researchers who hope to report that full-spectrum lighting has some effect on people. A recent book is entirely devoted to sample size (Kraemer & Theimann, 1987); interested researchers should consult it and the book on statistical power by Cohen (1988).

2. Sample Selection. If a researcher wishes to generalize results to a particular population (or all people), a reasonable effort must be made to acquire a sample that is representative of that population. Psychological researchers in general have been quite lax about this, but they recognize the principle as valid.

Once again, however, some lighting studies are worse than merely lax. For example, SAD subjects have been recruited through newspaper ads that describe the symptoms of SAD, the method of treatment, and even the researchers' success with previous SAD patients. Such an ad seems very likely to draw people who lean toward belief in the SAD syndrome and phototherapy as a treatment, and not to attract anyone who is sceptical. The result of such as study may safely be generalized only to true believers (or those who are willing to become true believers), rather than to the whole population of SAD sufferers.

3. Representativeness of the Sample. Participants in a study should adequately represent the population to which the researcher hopes to generalize the study's conclusions. The typical shortcoming here is that results from studies of university undergraduates are generalized to the general population. In lighting research, some less-obvious problems can develop. For example, results based on individuals who live far enough from the equator that their winters provide very little daylight may not apply to persons who live farther south. In the winter, people who live in Stockholm or Edmonton receive significantly less daylight than those living in Spain or California, and these are not the most dramatic differences one could find.

4. Quality of Research Measures. There's no sense measuring the effects of lighting with the equivalent of a cracked microscope, or with a microscope when a telescope would be the proper instrument. Yet one can see studies in which this is done. For example, changes in reaction time have been reported as changes in performance. Technically this is true, but without further explanation of how "performance" was measured, most educated readers would assume that a battery of work- or school-relevant tests had been administered to subjects, not that mere reaction time was the sole measure of performance. Each measure should be one that purports to measure the same concept that is under examination.

Each measure should have good or excellent reliability and validity. This may have been established by previous researchers. If not, or if researchers construct their own measures, it is incumbent upon those researchers to establish that each measure they use is good (i.e., is reliable and valid). If a measure is some kind of judgment, for example, then multiple raters' judgments should be made and these should agree to a significant degree. If some attitude or belief scale is used, the scale should at least have adequate internal consistency.

Section II: Random Assignment and Comparability of Conditions

5. Random Assignment. One keystone of experimental research is random assignment of participants to conditions. Without it, claims about causal relations between the independent and dependent variables are seriously jeopardized. Random assignment is extremely difficult to achieve in field settings such as offices and classrooms, but without it a number of plausible counter-explanations for the study's findings become viable: the participants or the settings may not be equivalent in some important way, and this difference may be the true cause of any significant finding in the study.

6. Control Conditions. Without control conditions, the experimenter cannot be sure whether the independent measure is having its presumed effect, or whether an observed effect might not be due to some external factor that affected all the experimental conditions. If no effect is observed in the control condition, we can more safely assume that an observed effect in an experimental condition is due to the independent variable. In lighting studies, controls should include shading participants from daylight or presenting participants with light that appears equivalent to them in most ways except the characteristic of interest, e.g., specific spectral qualities.

7. Illuminance Levels. An important kind of control, because illumination level is known to have behavioral effects on humans, is to ensure that participants in all conditions are exposed to equivalent amounts of light. Full-spectrum lamps are less efficient than common fluorescent lamps, so researchers normally need more of them to produce equivalent illumination levels for subjects.

8. Physical and Social Context. Another form of control, assuming the main purpose of the study is to compare the spectral qualities of lighting, is to ensure that social and other physical conditions are equivalent. Typical problems include teachers or supervisors of subjects in different conditions who have different styles or abilities, rooms that are architecturally different, or both. Ideally, peers, supervisors, and the physical surroundings should be identical for all participants.

9. Temporal Equivalence and Exposure Length. All subjects should be exposed to the lighting conditions for the same length of time. Researchers who use short exposure lengths must be very cautious about generalizing their results to contexts in which people are exposed for longer periods to the kind of light in question. Similarly, long-exposure outcomes may not appear if persons are exposed to a certain kind of light for brief periods.

Section III: Sources of Bias

10. Double Blinding. Much evidence shows that experimenters, even those with integrity, can unconsciously bias results if they know the hypothesis and which condition the subjects are in. Participants must also be blind to guard against them trying to "help" the study find the "right" results; most people are very concerned to "do the right thing" and this tendency is magnified in the presence of a respected researcher, doctor, or other authority figure. A problem in much lighting research is that lamps have recognizable hues or other characteristics, so that experienced subjects know which form of lighting they face.

11. Funding Influence. When a study is funded by a manufacturer whose vested, or even sole, commercial interest is in a particular form of light or light-based product, there can be tremendous pressure on researchers to produce favourable results. Most funders and researchers would not overtly falsify the results, but the same unconscious influences mentioned earlier may play an important role. Research sponsored by independent research agencies is preferable because researchers feel less pressure to produce results in the "correct" direction.

Section IV: Technical Lighting Considerations

12. Veiling Reflection. Veiling reflection results when light bounces off a work surface into a person’s eyes. If lighting systems that are being compared for their spectral qualities differ in veiling reflection, once again any observed differences in outcome may result from the difference in veiling reflection rather than their spectral qualities.

13. Flicker. Older fixtures cause lamps to flicker more, which could cause unanticipated results.

14. Lens and Diffraction. A variety of lenses and louvres for fluorescent luminaires are available, each with unique spectral properties for reflectance or transmittance. The same types should be used in each experimental condition in a single study.

15. Distribution of Illumination. Within a room, illumination varies from spot to spot even when luminaires are more less evenly distributed across the ceiling. Individual subjects will experience different levels of illumination in most ordinary rooms. At the minimum, illumination level should be assessed at each subjects’ workspace.

16. Daylight Intrusion. As mentioned earlier, the intrusion of daylight into an experimental space is a serious flaw, if the purpose of the study is to compare the effects of different forms of artificial light. The contamination occurs both in terms of increased illumination and in terms of influencing the spectral composition of light reaching the subject. In addition, daylight intrusion will cause greater variability in illuminance levels, over each day and across seasons.

Section V: Results and Data Analysis

17. Report the Results. I have been surprised at how many reports of lighting studies fail to report basic, essential descriptive statistics such as means and standard deviations. Often, inferential test details such as the name of the test, the number of degrees of freedom used, and the computed

value of the test are omitted. These are important for at least three reasons: to provide an image of the results that readers can form for themselves, so the calculations can checked at least in the sense that they seem reasonable, and so that future meta-analysts can compute effect sizes for the results when they attempt to combine the results of many studies.

18. Dropped Subjects. Some lighting studies blithely report that subjects were dropped from analyses without providing any very compelling reason for doing so. Particularly when the study only examined 7 subjects, dropping one or two subjects represents a very large change. Subjects should not be dropped except for very good reasons that are explained to the reader; it may be a good idea to report the results after analyzing them both with and without the data from dropped subjects.

19. Omnibus Tests. If the study includes several outcome measures, particularly if these measures are conceptually similar, the data should be analyzed with an omnibus test. These tests examine the impact of the independent variable(s) on the dependent measures simultaneously, which helps control for experiment-wise error rates. If 20 t-tests are computed, we can expect one to be falsely significant by chance at the .05 level.

20. Effect Size. How strongly did the independent variable affect the subjects' behaviour, cognition, or well-being? The answer does not lie in whether the statistical test is significant or not. Statistical test outcomes depend on sample size; a truly strong effect may not appear significant if the experiment includes only a few subjects. A truly weak effect will be statistically significant if a very large sample size is used in the study. A variety of effect-size measures are available, but all are attempts to measure the strength of the effect. Effect sizes should always be reported.

Section VI: Generalizability and Replicability of Results

21. To Which Behaviours? In one often-cited study, subjects answered a long series of questions about their feelings after studying in two kinds of light. On one of these items, subjects reported feeling more tired when exposed to one kind of light than another. Subsequent reviewers have often claimed that the one kind of light produces more fatigue than the other. However, this was true on only one of many items, and it was based on a self-report measure of fatigue. An omnibus test may have shown that across all the items, there was no fatigue outcome. Even if it did, fatigue should be measured by other, more objective measures than a single self-report question. One would expect that both objective and subjective measures of fatigue should show the same pattern of results. In the example above, only one of three objective measures showed a statistically significant difference between the conditions.

Other studies have reported that reaction time was significantly different under two kinds of lighting, but motivated reviewers begin to describe the "improved psychological functioning" that result from exposure to a particular kind of light. In brief: beware of claims that go beyond the specific behaviour assessed in a study toward making generalized claims of favourable changes to behaviour and human functioning.

22. To Which Populations? Earlier it was noted that results that may apply to light-deprived northerners might not apply to light-drenched southerners. There are other examples of misgeneralization that may be cited. One is the generalization of results found with clinically depressed persons to persons whose mood may swing a little on a day-to-day basis. Results from school populations may not apply to the work setting, and vice versa.

23. Replication. The effects of illumination are notably subtle. If effect sizes were even moderate, much of the research would not be necessary; everyone would know from personal experience that a certain outcome follows from exposure to light. For example, we would not do a study to find out whether sunlight produces skin tanning. When an effect is subtle, replication is essential to determine whether the effect is modest but real or whether the modest effect was an ephemeral product of some other feature of the study, perhaps not even produced by light. Amazingly few studies in behavioral science are replicated; this is one a key cause of misunderstanding, mis-reporting, and inflated claims, which in turn harm the reputation of behavioral science.

24. The File-Drawer Problem. Study A reports that full-spectrum lighting reduces warts! Claims like this lead the reader to believe in the wart-reduction properties of full-spectrum lighting, especially if the study was done according to all the other guidelines described above. Yet full-spectrum light may not reduce warts (in case the reader is unfamiliar with the literature, there are

no such studies; this is a hypothetical example). Why? If 20 wart-reduction studies are done (and I assume light has no real wart-reduction properties), one of them will, by chance, produce results significant at the .05 level. Publishing practices being what they are (studies that report significant results are favoured enormously over studies that find null results), the one study that reports a significant outcome will be published and the 19 that do not will not be published; they go into the researchers' file drawers. A reviewer who searches the published literature will find the one study that reports significant results and will not know about all the others that did not. The reviewer will conclude, based on the published evidence, that light reduces warts.

Conclusions

The generally sceptical tone of this paper may lead the reader to believe that I think light has no effect at all on people. This would be entirely incorrect. Light has many well-documented effects on people. However, many effects of light that have been reported would not stand even a partial scrutiny based on the guidelines in this paper. In particular, the effects of full-spectrum lighting have been "established" using research that is particularly susceptible to many of the problems described above. What that proves is that we are not sure just <u>what</u> effects full-spectrum lighting has; it may have many important effects. But most of the studies in the reference list below could not be cited as support for that conclusion. We need more research that addresses most or all the guidelines listed above before we can draw any credible conclusions about the effects of full-spectrum light on human functioning.

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