LESSONS FROM SCREWTAPE:

HOW TO ARGUE FOR A FALSE NULL HYPOTHESIS -

A GUIDE FOR STUDENTS, ATTORNEYS, AND OTHER PROFESSIONALS

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Summary -- Students, lawyers, and other professionals are sometimes unfamiliar with scientific methodology or the proper use of statistics. Even so, it is not unlikely that they will confront attempts to use social science for proving the null hypothesis when actual data might suggest otherwise, if properly handled. As a didactic mechanism, advice is provided on how to misuse social science for such purposes, in hopes that future scholars will not be misled by such inappropriate, though not infrequent, scientific practices.
This author has always believed that science should serve the interests of truth, with as little political interference as possible. However, nearly 30 years of research experience and recent legal experience (Schumm, 2005, 2010a; Schumm & Nass, 2006; Schumm, Nazarinia, & Bosch, 2009) has taught me that politics can play a large role in how science is “managed” or “used.” There is a story about an old Native American chief who described white men as using religion and law (he could have added science) as servants, calling upon them whenever they might be useful for proving a desired outcome in the white man’s favor. The chief said that Native Americans, in contrast, viewed religion and law as guidelines to live by, a means to the end of promoting justice for all people.

Pedagogically, sometimes the truth is best explained by illustrating its opposite, even if facetiously (e.g., Lewis, 1958). Although Cohen (1988, p. 16) has clearly explained how the null hypothesis can never be proven scientifically, certainly, there are aspiring graduate students, professionals, and attorneys who would like to learn at the earliest opportunity how to win their arguments or cases when the desired outcome is such “proof” of the null hypothesis. Hence, the following suggestions are presented, in rough priority of least difficulty, for “proving” the null hypothesis.

**Approach A: Name It and Claim It (Unless You Don’t Like It)**

The first approach is to merely claim that there are no known differences, even if no research has ever been done in that area of study. Because if no research has ever been done, then there are no known differences, so the argument is strictly true, and it’s hard to argue with truth, right? To show such a claim to be incorrect, someone would
have to conduct research and present it, which is a long, difficult process in most cases. If the misfortune should occur that some research has been conducted, perhaps much of it can be ignored; after all, it’s not your job to look at research from both sides of the issue or to dig deeply into the research literature, is it? Besides, that would take at least twice as much effort (and time is money, right?). For example, if a study finds no differences on two of three variables, why not just forget the difference on the third variable? A corollary of this principle is to keep your opponents, by any means necessary, from searching for or presenting research results if those results do not favor your case (e.g., Schumm, 2010c). If the court or judge never hears the truth, then they cannot rule according to it. If an opponent does bring research evidence favoring a rejection of the null hypothesis, then try to disqualify their expertise on whatever grounds may appeal to the court, regardless of their actual validity. One useful approach is to ask if the researcher is personally familiar with the area; for example, if he is studying the effects of smoking, ask if he is a smoker himself and if not, if his religious views account for that. If so, try to disqualify him as inherently biased and incapable of rendering a legitimate expert opinion.

Counterpoint A: If there is no research, it is presumptuous to assume that any hypothesis, null or otherwise, is true. Overlooking research that does not fit one’s “position” is not considered ethical from a scientific perspective. If reviews of scientific literature consistently overlook results from one “side” or another (Schumm, 2008, 2010d), one should suspect them to be biased. Likewise, if different methodological criteria are applied to research from one side or the other, again, one should suspect bias. Experts should be evaluated on the accuracy of their research or testimony, not
their values, religious views, or demographic characteristics. If one side is afforded a chance to criticize an expert, that expert should be afforded a chance to rebut the criticisms.

**Approach B: Muddle the Definitions or Focus on Irrelevant Outcomes**

Exactly how do we define a “smoker”? Is it someone who actually smokes tobacco? Is it someone who smokes but does not inhale? Is it someone who is attracted to smoking tobacco but chooses not to do so? Is it someone who believes in smokers’ rights for political reasons but does not smoke? Is it someone who smoked tobacco as an adolescent but later gave it up? What about someone who smokes for a few years, then quits, and then takes up smoking again? What if a person has smoked two packs a day for 40 years but quit in the past month – is that person still a “smoker” or not? Half the battle can be won if the definitions are “right”. For example, who would want to base laws about tobacco use on attraction to smoking? After all, people can’t help their attractions, right? Even so, that does not actually negate the damage done by smoking – so by all means, work to shift definitions away from behavior, especially risky behavior, to psychological considerations! That will greatly improve the chances of supporting the null hypothesis!

With respect to outcomes, there are always many possible outcomes. You don’t want to do research on those most likely to be adversely affected by smoking, such as disease or other health outcomes. For example, if some were concerned about the effects of parental smoking on children, don’t bring up secondhand smoke. Rather, study whether parents who smoke feed their children at least one meal a day or if they seldom abuse them physically or emotionally. In other words, focus on things that may be
important but that are unlikely to be related to whether a parent smokes or not. If you can study 50 such factors and find no support for differences between the children of smokers and nonsmokers, then even if opponents find significant differences on relevant variables, their significant findings will seem minor in comparison to the multitude of your null findings.

Counterpoint B: Clarity of definitions is essential in science. While attractions to smoking likely increase the probability of smoking behavior, any damage done by smoking is most likely done by behavior rather than mere thinking about smoking. Furthermore, there are probably specific aspects of smoking behavior that increase the risk of adverse effects. Identification as a smoker may not appear to be harmful but if it reinforces continuance of risky behavior, then it could facilitate harm to self directly and others indirectly. If causes or effects are not measured clearly, it is much more likely that a false null hypothesis will be accepted. Unless theoretically relevant outcomes are assessed, it is far less likely that null hypotheses would be rejected. You might report dozens of tests of some outcomes showing no differences between smoking and non-smoking parents but those results would not negate the harm done by parental secondhand smoke.

Approach C: Establish Impossible Criteria for Disproving the Null Hypothesis

It is very important to start with an argument that cannot be refuted. Once such a premise is established, victory is almost guaranteed! For example, you might argue that unless more 95% of all smokers (by your muddled definition, no doubt) die of lung cancer by age 30 compared to less than 5% of non-smokers, you will not consider the null hypothesis rejected. Argue that it is not “fair” to restrict smoking tobacco unless
every single smoker (by your definition) dies of lung cancer within a short time of smoking while no non-smoker ever dies of lung cancer.

Counterpoint C: There are probably almost no variables in social science that will ever be strong enough to yield 95/5 splits on outcomes. For example, Luntz (2009) reported that “Two-thirds (66 percent) of nonreligious Americans agree with the statement “If it feels good, do it,” despite its selfish, dangerous undertones. By comparison, fully 71 percent of religious Americans disagree with the concept of instant gratification. What we have here is a chasm between the value systems of these two American camps.” (p. 261). Even though with N = 200, such percentage differences would yield an odds ratio of 4.75 (95% CI, 2.61 to 8.64; r = .37; effect size, Cohen’s d = 0.79), a substantial as well as a statistically significant (p < .001) difference, the result is far from a 95/5 split. However, that in no way should be used to justify instant gratification as a lifestyle.

In reality, risky behaviors can lead to instant adverse effects on rare occasions but more often, the risk accumulates over longer periods of time. For example, texting on a cell phone while driving can lead instantly to a fatal accident but most of the time it happens, there are probably no adverse effects. Despite that “lack of evidence”, many states have legally banned texting while driving – not in the expectation that all texting will cease but that the cumulative risk will be reduced. Furthermore, some would argue – even accurately – that they are such good drivers that they can text and drive without increasing risk to anyone, that they have been doing so for years without an accident. If the existence of a few examples of successful risk takers were sufficient to discredit
legislation against risky behavior, probably no legislation regarding any risk could ever be justified.

**Approach C: Forget to Report Effect Sizes**

One’s results, especially with small samples, may not be statistically significant even though the effect sizes are medium to large substantively. If you think this might be a problem, it is very useful to “forget” to report means and standard deviations, without which effect sizes cannot be calculated. Oh the horror of it – if you do report such things, someone (e.g., Schumm, 2010b) might actually discover your effect sizes to your great embarrassment. It also helps to “forget” to report sample sizes or t-test values, if you can get away with that.

**Counterpoint C: Enough information (e.g., means and standard deviations) should always be provided to permit the calculation of effect sizes. Better yet, in addition to discussions of statistical significance, effect sizes should be reported, along with discussions of their importance to the proper interpretation of the results.**

**Approach D: Use Small Samples**

If research must be done or some of it is discovered in contemporary archives, then one must steer away from large samples and focus on very small samples or, at least, report only the results from very small samples, so that only very, very large effects would ever be detected. Don’t be afraid to do lots of such small studies because none of them are very likely to produce much in the way of rejecting the null hypothesis (Katz, 2006, p. 138) – and being able to say that lots of (small) studies have supported a “no differences” hypothesis sounds pretty impressive to the uninformed. If one does occasionally produce a rejection of the null hypothesis, attribute that problem to random
chance \((p < .05)\). Citing the benefits of qualitative research is an especially high sounding way to justify the continued use of very small samples. After all, large samples are so impersonal, right?

Sometimes, you might find yourself with a problem, an unenviably large sample. No need to worry! One approach is to consider if oversampling was used to collect larger amounts of data from typically undersampled populations. If so, instead of correcting the problem by sizing the regular sample up, try sizing the rare group downwards. You might start with a group of 200 rarely studied subjects and find your downsized sample with only 30 such subjects. That would greatly reduce your chances of rejecting the null hypothesis.

Perhaps you still have a problem – even after downsizing, you still have 150 subjects in your group, which would leave you with a much better chance of rejecting the null hypothesis. No worry! Break up the group into subgroups, preferably four or five. That way you can still end up with comparison groups that are very small and unlikely to afford enough statistical power to permit rejection of the null hypothesis.

Another approach is to use variables that have large amounts of missing data. You might start with a group of 100 subjects but if half of those subjects skipped a question on your survey, then your de facto group size is only 50, much smaller and much less likely to yield significant findings, not to mention that you can justify disregarding any significant findings by citing the bias introduced by having had so much missing data.

**Counterpoint D:** Many scholars would suggest that a minimum size for a social psychological study with two variables would be 30 participants with an
additional 10 participants added for each additional variable used. Studies with fewer than 100 participants that do not discuss the statistical power of their analyses or the effect sizes of their results should be viewed with caution. Small sample studies can be very useful for developing new ideas and future directions for research but are not very useful for detecting the presence of small effects that would be statistically significant in larger samples. When evaluating the role of subgroups, one should probably look at results for the subgroups separately and for them combined. If missing data represent less than ten percent of the sample, the “damage” is probably minimal. However, as missing data increase, the risk of selection bias increases and should not be discounted. There are a variety of approaches for dealing with larger amounts of missing data, generally beyond the scope of this report. Some results can be significant by chance alone, so one must be careful to not over-interpret scattered significant findings observed when conducting multiple statistical tests.

**Approach E: Minimize Unwelcome Findings**

If eventually larger studies are done, especially with random, representative samples, then it is more likely that some effects will be discovered. At this point, the easiest approach is to note their significance but avoid mention of effect sizes, since some of them might be large and very damaging to the idea of a true null or “no differences” hypothesis. It sounds much better to mention that five barely significant results were found than to mention (horrors!) that the effect sizes were substantial. If the effects do appear to be large, then the easiest approach would be to deny their clinical significance, which being very subjective, is difficult to discredit. However, those who are not
clinicians may not care if the results were “clinically” significant or not. Another argument for discrediting the research is to observe that the study has not been replicated, at least by scholars you consider to be credible. If the results have been replicated, at this point, the best approach is to insist that the results are not meaningful (by your idiosyncratic definition of meaningfulness, of course), even if they are statistically significant, substantial, possibly even clinically significant, and replicated. If you were “big tobacco” you might argue that even if smoking did appear related to poor health outcomes, everyone has to die of something, so the results don’t really mean much. You could also argue that people, being free, can choose not to smoke at any time and thereby minimize any negative health outcomes. Hence the real issue should be the freedom to smoke (or not) rather than possible health outcomes from smoking. At this point, one must be careful to obscure any effects of secondhand smoke or effects on others or society in general from smoking because those issues might be more likely to be viewed as meaningful by neutral parties.

Counterpoint E: If effect sizes are never mentioned in a research report, one should be wary; some journals require the reporting of effect sizes. When results are statistically significant and medium to large in magnitude of effect size, dismissals in connection with “lack of clinical significance” or “meaninglessness” are an indication of grasping for straws. While it is useful to replicate important studies, exact replication is rare. Often one must consider similar studies even if they are not exact replications.

Approach F: Using Group Designs “Creatively”

The ideal in science is to compare two pure groups of approximately equal size in which all of the members in each group accurately represent different qualities.
Unfortunately, such purity allows for a greater chance of rejecting the null hypothesis and must be avoided at all costs. One way to be creative here is to use data sets in which it is not possible to have pure groups because too few questions were asked to permit accurate identification of members of each group. A scientist of lesser cleverness might refuse to use such data, but what a waste! If the two pure groups really were different, then you can increase the chances of not observing that if you can include members of each group in the other group! Perhaps members of group A are in group B at a rate of 20% and members of group B are in group A at a rate of 50%. Now whatever differences may exist between the two groups A and B will be largely obscured, reducing the chances of detecting the true differences between the two groups. For example, perhaps some people smoke tobacco every day while others only once a week; try combining the “once a week” smokers with the “never” smokers before comparing the “non-smokers” to the “smokers”. It may be very helpful to use this approach in combination with undersizing, missing data, or using several subgroups. Another helpful approach would be to drop from your study anyone who died or became seriously disabled from respiratory illnesses; after all, they are in no position to be re-interviewed, right?

Counterpoint F: If concepts are not defined clearly or not measured precisely, one could prove almost anything, in addition to preventing effective replication of one’s research. You should not vaguely define two groups and then assume your results automatically generalize to two far more specifically defined groups. For example, persons who chew tobacco and persons who smoke tobacco may be very different, even though both could be defined as “tobacco users”. Women who chew tobacco may be different from men who chew tobacco, even though both groups are “chewers”. Thus,
generalizing from chewers to smokers or from men to women would not always be appropriate in spite of superficial similarities.

**Approach G: Statistical Controls**

The refuge of last resort for the advocate of the null hypothesis must be statistical control. There are at least four excellent ways to justify the null hypothesis with statistical controls.

“Controlling” for Natural/Logical Outcomes. Suppose you were a researcher with an agenda for “big tobacco.” Suppose you were studying cancer patients in two groups of subjects (Groups A and B, 30 smokers and 30 nonsmokers) and you observed that when they learned they had cancer, there was a tendency for the cancer patients (67%, for both smokers and nonsmokers) to experience depression but a higher tendency for noncancerous smokers to be depressed (33.3%) than for noncancerous nonsmokers (11.1%). Let’s assume that smokers are more likely to have some form of cancer (40% versus 10%), which will yield an odds ratio of 6.00 ($p < .05$). That statistically significant and substantial odds ratio would not look good in court, so something must be done about it.

I would suggest, in this case, that if you want to discredit the relationship between smoking and cancer, try controlling for depression. There is a danger here that your opponents might observe that depression was a natural outcome of illness and therefore a poor control, but one must hope they are no so astute in terms of theory development. Adding depression to the equation, however, would save the day, yielding a non-statistically significant odds ratio (4.01) for smoking and cancer. Even though cancer would be found to be significantly related to depression, that result can be
disregarded, as meaningless (who cares that cancer leads to depression?). Furthermore, now you could go to court and argue that cancer was not a smoker’s disease because (1) some nonsmokers developed cancer, (2) not all smokers developed cancer, and (3) there was no statistically significant relationship between smoking and cancer once you controlled for the tendency for ill persons (and even healthy smokers) to be more depressed. If the consequence of smoking is cancer and cancer has its own consequences, then controlling for the latter can make it appear that smoking is not harmful at all. Just make sure the other side doesn’t have a methodologist who can sort out such issues!

Counterpoint G1: The model is that smoking causes cancer which causes depression. Controlling for depression violates the causal order in terms of the smoking versus cancer association. It could be argued that depression causes cancer but such an effect would be indirect (e.g., weakening the immune system) as compared to the more logical direct effect of having a potentially terminal illness impact one’s mental health.

“Controlling” for Natural/Logical Antecedents. Another approach I would suggest is to find a variable that is highly correlated with smoking itself, something that is very closely tied to smoking. Let’s leave the data set (N = 60) alone except to add a variable labeled dating. Let’s suppose that 81.5% of nonsmokers without cancer can get dates easily as well as 66.7% of nonsmokers with cancer, whereas only 16.7% of smokers can (perhaps bad breath and stained teeth are at work), for both those with or without cancer. Now, when one controls for dates (which do not significantly predict cancer), the relationship between smoking and cancer is no longer significant. Big tobacco could claim that there was some sort of smoker-phobia out there, restricting the social life of
smokers. In other words, smokers were not to blame for a risky health practice (smoking), but it was the prejudices of people who would not date persons with bad breath and stained teeth. Smoking wasn’t causing cancer, more likely, they would argue, it was social or sexual rejection and unfair dating discrimination. Perhaps society had imposed an internalized smoker-phobia on smokers so they unconsciously hated themselves for smoking and their self-hatred either led to or exacerbated their rejection by others. In other words, blame anyone or anything except the person responsible for their risky choices about using tobacco. As a last resort, remember that people probably have genetic predispositions for nicotine cravings or attraction to wafts of beautiful billowing smoke, so even if they make the choice to smoke, it still really isn’t their personal responsibility.

*Counterpoint G2: The issue is twofold – (1) can the discrimination variable account for any effect and (2) can it completely explain away the relationship between smoking and cancer. The first proposition might be true while the second might not be true. However, unless both are true, one cannot explain the smoking/cancer association as entirely due to the third variable of discrimination. Furthermore, merely speculating that the third variable might have explained away the association if it had been measured and analyzed may be sound philosophy but it’s not sound science.*

*Controlling for the Kitchen Sink. Every time you add a variable, even a totally random variable, to a statistical model or equation, you introduce additional random variance that acts like a smokescreen to obscure any true differences among your variables. It is a well kept secret that just controlling for anything, including the kitchen sink of variables, isn’t a truly scientific enterprise. So, don’t worry, most scholars won’t*
catch you at it. Just throw in whatever variables you want, regardless of their theoretical merit or rationale. The more the better, as it will reduce the chance of finding differences between the groups or variables in question. You might try controlling for each of the 50 states if you have a national sample and state information (e.g., Rosenfeld, 2010); it will sound so logical and yet water down almost anything you evaluate! This is especially wonderful because geography is becoming closely associated with moral and political values in the United States (Bishop, 2008; Cahn & Carbone, 2010; Silk & Walsh, 2008), so that controlling for states can reduce the apparent effects of such values on virtually any outcome of interest. An added benefit is that occasionally, one of your kitchen sink variables will be found statistically significant by chance alone and afford you an excellent opportunity to pontificate on its possible relevance or meaning, to the exclusion of discussion about your other results.

Counterpoint G3: A non-statistical way of thinking about this is to consider if you were making a soup. If you only have three ingredients, you could probably taste each one. If you add ten more, you would probably not be able to taste all of them, even if they all were still there. Likewise, even if your added ingredient was only water, if you added enough water, you might eventually not be able to taste any of the original ingredients. Statistically you are adding random variance and eventually that random variance, by chance, matches the true variance of the original variable(s) and may appear to account for or cancel it. Adding a plethora of “control” variables may seem sophisticated methodologically, but unless the controls make theoretical sense, they can have the effect of making acceptance of the null hypothesis more likely simply due to random factors.
“Controlling” for Multiple Tests. Another method is to use the Bonferroni procedure and divide the standard 0.05 level of significance by the number of tests conducted. Suppose you have conducted ten statistical tests and seven of them are significant, \( p < .05 \), but none at significant at \( p < .01 \). You could divide 0.05/10 = 0.005 and set 0.005 as the new required level of statistical significance, so that none of the tests were significant by the new criterion. This works especially well if you don’t mention what you were doing in the report because readers will look at the \( t \)-values and expect them to be significant (\( p < .05 \)) and yet you are reporting none so, confusing the daylights out of them.

Counterpoint G4: Using Bonferroni procedures is appropriate when you are trying to avoid granting credence to significant results that are likely due to chance alone. If one uses a criterion of \( p < .05 \), one will find five apparently significant results, on average, if one conducts 100 statistical tests. If you conduct twenty statistical tests and you are worried that the one significant result you found was a chance result, then using the Bonferroni procedure would be appropriate. However, if you conduct 10 statistical tests and find three significant results, use of the Bonferroni procedure may be too conservative.

Integrating Multiple Methods. For best results, of course, using all four types of statistical controls at the same time is highly recommended. It is very unlikely that anyone will catch all of these abuses of statistics. And, don’t forget – it doesn’t matter which variables provide you with the desired results, just keep trying different ones until something works like you want it to work! You have at least a 5% chance of finding something that will work for you! If you try 60 control variables, at least three should
get the job (of statistical control) done the way you want. Keep in mind that you are under no obligation to tell anyone or the court how many times it took you to find something that worked out! You only publish the *one* result you liked, after all, even if you had to try two hundred of them to get that one that fit your preconceived ideas!

*Counterpoint G5:* One should statistically control for preexisting differences between the groups in question or for differences that would be expected theoretically. For example, if you found that non-smokers had higher education than smokers, you should control for education when comparing those two groups lest you confound outcomes of smoking with outcomes of higher (or lower) education. On the other hand, if you theoretically expected smokers to be more neurotic than non-smokers and you knew that neuroticism predicted the outcome variable you were using, you would want, for theoretical reasons, to account for neuroticism. In other words, the statistical decisions should be made for sound scientific or theoretical reasons, not just to find the solution you are looking for.

**Approach H: Statistical Test Avoidance**

The best way to support the null hypothesis is to use *t*-tests. Other statistical tests, such as nonparametric tests or multivariate analyses of variance with repeated measures, though more powerful statistically, may feature a greater risk of rejecting the null hypothesis. Moreover, equivalence testing must be avoided at all costs as a method for evaluating your null hypothesis since it offers perhaps the best chance for rejection of the null hypothesis.

*Counterpoint H:* Equivalence tests are now considered by some statisticians as the new gold standard for evaluating null hypotheses (Cleophas, Zwinderman, & Cleophas,
Aside from equivalence testing, there are many other statistical approaches than may be better than using simple t-tests.

**Approach I: Avoid Complex Models**

It might occur that your null hypothesis might be rejected despite your best efforts. It might occur that your null hypothesis might not be rejected. Either way, one must avoid the development of theoretical models that could be tested statistically. For example, if the null hypothesis was not rejected, there might be mediating or moderating variables of interest. For example, there might be a moderating variable that would show that for one group the null hypothesis was rejected and for another group it was not rejected. Likewise, there might be a mediating variable that was significantly related to both the independent variable and the outcome variable, thus linking the independent variable indirectly to the outcome variable. Either way, such a model might provide evidence that there was a significant, even if indirect or limited to one group, effect of the independent variable on the outcome. If the null hypothesis was rejected, it’s probably wiser to try to dismiss that evidence or reinterpret it as meaningless than to explain it away with an actual theoretical model that could be tested statistically. For example, if smoking damages the lungs and damaged lungs are associated with later lung cancer, the model would sound so logical that your court case that smoking was harmless might not hold up in the eyes of a jury of ordinary citizens. Therefore, it would be wise to avoid any scientific theory or research that considers damage to the lungs as a valid or useful variable with respect to smoking and possible cancer-related outcomes. In general, it’s
even better to avoid theoretical models that include any mediating or moderating
variables. As a last resort, you might have to try to show that a model you prefer is better
than the model the other side prefers, but that is an arduous task, best avoided.

Counterpoint I: The process of science is not about testing hypotheses once and
forgetting about them. The goal is to build valid theory based on multiple tests of
reasonable hypotheses. For example, it is unlikely that smokers and non-smokers (or any
other set of groupos) would be exactly identical as groups on everything except one
factor, such as smoking. A good theory would explain how they were different prior to
becoming smokers (or not) and exactly how smoking (or its antecedents) both benefitted
and harmed both the smoker/non-smoker and their other family members. A mediating
variable explains how one variable influences another variable (e.g., smoking causes
genetic damage that increases the risk of getting cancer later in life; genetic damage is
the mediating variable). A moderating variable explains how the impact of one variable
on another variable may differ across two or more groups (e.g., smoking causes more
genetic damage for women than for men so that women are more likely to be diagnosed
with cancer later in life; gender is the moderating variable). Sometimes control
variables can have subset effects as moderators; one might try to control for religion by
using affiliation (e.g., Christian vs. non-Christian)(Langbein & Yost, 2009). However, if
some Christians are pro-smoking and others are anti-smoking, affiliation will likely not
be a very helpful control variable. Religious commitment, as measured by weekly
attendance, might be a more useful control variable. Both mediating and moderating
variables would be considered both prior to smoking status and afterwards with respect
to benefits and/or harms. For example, at one time, second-hand smoke was considered
harmless to children; today, after many years and better research, we know better.

However, had we established social policy on the earlier opinion, an opinion aligned with the views of those who stood most to profit from that opinion, how many children would have been (or were) harmed?

CONCLUSION

In some sad situations, it may not be possible to avoid evidence that supports a rejection of the null hypothesis. Here it is best to “reframe” the results in a way that supports your case. If your group appears at a disadvantage from the research, blame discrimination or minority status as the cause, so the “bad” results are only a reflection of “bad” things done to them by others. If that’s not enough, blame “internalization” of societal discrimination, a gambit that is especially helpful if many research subjects don’t believe they have been discriminated against. Argue that they are so discriminated against that they are not even aware of it – and that’s precisely the most effective (and dangerous!) form of discrimination. If you really get in a pinch, argue that the research itself doesn’t matter very much anyway because it’s a civil rights or legal rights issue above all else. After all, research is your servant, not your master! If it supports your case, use it; if not, discredit or reinterpret it to support your case.

These guidelines should be very helpful to all aspiring professionals who view science, along with religion and law, as means to an end, ways to get what you want politically, regardless of the consequences for others or society in general. After all, life is all about winning and taking care of number one, right? From his grave, the old Native American chief will congratulate you for continuing the ancient traditions of a legacy “white man.”
Counterpoint J: Screwtape may be able to be outwitted here if followers of truth expose his schemes as enumerated in this report, exposing the weaknesses of his arguments and evidence each in turn, regardless of whether the issue is smoking tobacco or some other controversial matter.

REFERENCES


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