Field Experiments: Design, Analysis and Interpretation
Solutions for Chapter 12 Exercises

Alan S. Gerber and Donald P. Green*

Question 1

Stewart Page performed an audit study to measure the extent to which gay people encounter discrimination in the rental housing market.[1] Answer the following questions, which direct your attention to specific page numbers in the original article.

a) Who are the subjects in this experiment (p. 33)?
Answer:
The subjects are landlords who advertised rental housing in two Canadian cities (Windsor and London, Ontario; N=60 for each city) and Detroit Michigan (N=60). The landlords were selected based on advertisements they placed for rental property in the classified ads section of the most recently available newspaper in each city. It is not clear how the sample was drawn from the available ads, except that some restrictions are described: advertisements were excluded if there was no phone number listed in the ad or if the ad listed preferences or specific conditions for prospective tenants. In addition, once the experiment was underway subjects were discarded if they did not understand the meaning of the call (page 34). It is unclear, but it appears that subjects were dropped if the caller could not reach the landlord, either because the line was repeatedly busy or not answered. Finally, subjects were dropped if the person who was reached was either not in charge of renting the room or was authorized to give a definite answer regarding its availability (see p. 33). After these exclusions, there remained 180 landlords (assuming that no landlord in the sample was associated with more than one rental property).

b) What is the treatment (pp. 33-34)?
Answer:
Subjects were assigned to receive the control call (an inquiry about the current availability of the advertised apartment) or the treatment call (an inquiry about the availability of the apartment prefaced by the statement: "I guess it's only fair to tell you that I'm a gay person" (or "a lesbian"). For each city 30 calls were made by a male caller and 30 by a female caller (p. 33). Thus there was assignment into 4 groups (gender x sexual orientation) with 15 subjects in each group in each city. Both caller gender and the sexual orientation prompt may both be considered as treatments. According to the write up, there was no fixed script but calls were kept "as brief as possible, generally of only seconds in duration, and were limited to direct inquiries." (p. 33).

c) One criticism of audit studies is that in addition to differing with respect to the intended treatment (in this case, sexual orientation of the renter), the treatment and control group also differ in other ways that might be related to the outcome variable. What is the technical name

*Solutions prepared by Peter M. Aronow and revised by Alexander Coppock
[1]Page 1998.

for the assumption that audit studies may violate?
Answer:
Audit studies may violate the exclusion restriction. Let Y be the stated availability of the apartment, D the presumed sexual orientation of the potential renter, and Z the assignment to the treatment or control script. If the script intended to convey a gay sexual orientation (D) strikes the landlord as disagreeably defensive or odd in the context of a call regarding the rental property, for example, then Z may affect Y through pathways other than the putative sexual orientation of the prospective tenant. Similarly, if those making the calls have a viewpoint regarding anti-gay bias, this may affect how the calls are delivered apart from conveying the sexual orientation of the potential renter.

d) Suppose that the experiment used one male caller to make calls that mentioned sexual orientation and another male caller to make calls that did not. How would this procedure affect your interpretation of the apparent degree of discrimination against gay men?
Answer:
There would be a potential violation of the exclusion restriction. If there are two callers, the difference in average outcomes for the straight and gay script groups will estimate the effect of the combination of the qualities of person 1 and the gay orientation script versus the qualities of person 2 and no gay orientation prompt. Unless there is no difference in how renters respond to person 1 versus person 2, this estimand is not the average effect of sexual orientation.

e) Take a careful look at the treatment and control scripts, and consider some ways that the treatment and control conditions might differ in addition to transmitting information about the potential renter's sexual orientation. Are the scripts the same length? Do both scripts seem similar in terms of tone and style? How might the incidental differences between scripts affect the generalizations that can be drawn from this study?
Answer:

Suppose the goal is to use the findings to draw conclusions about the gay person's real world experience when calling a landlord who is made aware of the caller's sexual orientation about the availability of an apartment. To evaluate generalizability, we consider whether the scripts parallel what a gay and straight person might actually say to a potential landlord and whether, in instances in which the scripts deviate from this, any deviations might affect potential outcomes.

The gay and straight scripts are different in several ways in addition to conveying different sexual orientations. The script with the sexual orientation prompt contains more information than the control script and is also longer. It is unlikely that these difference parallel real world differences in how a gay versus straight person will interact with a landlord, and so if these differences matter for landlord response this will compromise the generalizability of the experiment. However, it is unlikely that these differences are important in this particular context. Raising the issue of sexual orientation in a preliminary inquiry may convey a degree of assertiveness, political commitment, or a lifestyle that might have an independent effect on the desirability of the potential tenant apart from the particular question of sexual orientation. If sharing one's sexual orientation in a preliminary phone call is not a common feature in real rental experiences, and this script feature is considered odd or shocking by some landlords, the results may not generalize to the typical real world rental experience.

f) How might you design an experiment to eliminate some or all of these incidental differences between scripts?

Answer:
A key design challenge in this experiment is conveying sexual orientation in a brief interaction in a naturalistic way. A treatment script that used an indirect strategy would not have the same potential to carry the baggage (convey assertiveness, etc.) that is incidental to the intended treatment. For instance, a script that referred to the potential renter's boyfriend or girlfriend ("the location is great because my boyfriend/girlfriend goes to school or works in the neighborhood") might convey sexual orientation in a more subtle fashion. This script strategy also has the benefit of eliminating differences in script length and information content.

Given that any particular script for conveying sexual orientation might be less than ideal, the researcher might diversify and try a variety of indirect methods and see if the effects are different across scripts. If the scripts are equally effective, this suggests that the common element across the scripts (sexual orientation) rather than idiosyncratic features of the scripts is driving any results you observe.

g) Based on the description on pages 33-34, how are subjects assigned to the treatment groups? What is the implication if random assignment was not used?
Answer:
The allocation to groups is described as follows: "Calls to the same city were assigned to the two conditions by way of systematic alternation of telephone numbers." It is not entirely clear what this entails. Suppose it means that the list for a city was first sorted in ascending order by phone number and then the subjects were assigned to each of the 4 conditions in an alternating fashion such that the first number (and fifth and ninth etc.) was assigned to, say, the no gay prompt and male caller group.

If telephone numbers are randomly assigned to landlords and the order of assignment to the 4 treatment and control groups was independent of the telephone numbers, the alternation method is equivalent to random assignment.

However, it is (theoretically) possible that the allocation method is not equivalent to random assignment. First, subject potential outcomes may be correlated with phone numbers. If so, the sampling distribution produced by randomization inference under the sharp null will be incorrect. Second, if there is a relationship between the potential outcomes and telephone numbers, it is possible that conscious or unconscious bias might lead the researcher to assign some numbers to certain groups, which will lead to biased estimates.

Question 2

Over the past several decades, trust in government has declined. Among the possible culprits is the rise of confrontational TV news shows, which are thought by some to produce citizen disgust and disengagement. An influential study by Mutz and Reeves investigated the effects of uncivil political discourse by scripting and producing two versions of a candidate debate.[2] Subjects were randomly assigned to be shown either the uncivil (treatment) or civil (control) debate. After viewing the treatment or control video, subjects were asked about their level of trust in government.

a) Who are the experimental subjects in the first Mutz and Reeves experiment (p.4)?
Answer:
Footnote 7 describes the subjects. The subjects were adults from the community and college

[2] Mutz and Reeves 2005.

students. The adult subjects were recruited through temporary employment agencies, and college students were recruited from political science courses in response to offers of extra credit. 75% of the subjects in the first experiment were college students.

b) Let the variable X_i categorize subjects according to whether they regularly watch political television shows ($X_i = 1$) or not ($X_i = 0$). Let the conditional average treatment effect be denoted $E[(Y_i(1) - Y_i(0))|X_i = 1]$ and $E[(Y_i(1) - Y_i(0))|X_i = 0]$. Does your intuition suggest that these CATEs will be similar or different? Why?
Answer:
Those who regularly watch political shows may differ from those who do not in several ways that are likely to be relevant for potential outcomes Y(1) and Y(0). Regular viewers of political shows are likely to be better informed and more interested in politics and government. As suggested by their viewing habits, regular viewers of political shows are, compared to those who avoid political shows, more likely to be engaged by, rather than shocked or offended by, the argumentative interactions typical of today's political shows. Further, it is plausible that such differences in knowledge and taste would affect the subgroup's ATE. For instance, if the theory linking incivility to trust is that those who are shocked by incivility will think less of politicians who display incivility and consequently have less trust in government, those who regularly watch political shows, who are presumably inured to or attracted by incivility might have a smaller or no negative treatment effect. Indeed, for those who like such things, the vigorous debate and spirited if sometimes nasty exchanges in the treatment condition may be viewed as how a strong democracy deliberates, leading to an increase in trust in government. On the other hand, those who watch political shows might watch the experimental treatments more intensely, leading to a larger negative treatment effect.

c) Write the expression for the average treatment effect as a weighted average of the CATEs of those who do and do not watch political TV shows.
Answer:

$$ATE = E[(Y_i(1) - Y_i(0))|X_i = 1] * E[X_i] + E[(Y_i(1) - Y_i(0))|X_i = 1] * (1 - E[X_i])$$

d) The researchers estimated the average treatment effect and found the uncivil video reduced trust in government. Suppose that only 5% of the general public watches shows that convey this treatment. To what extent does the experiment support a claim that exposure to uncivil political programs caused a decline in trust in government among the general public?
Answer:
Suppose that any effect of exposure to uncivil television shows is confined to those who view such shows. Any effect on trust would be due to a negative CATE for this subgroup (X=1). However, as C shows, a negative ATE does not imply a negative value for the $ATE|X = 1$ because the overall negative effect might be produced by a negative CATE for the X=0 subgroup. Given the small proportion of X=1 subjects, it is plausible that the ATE is essentially the CATE for the X=0 group. Further, there is no reason to suppose that the CATE for those who choose to regularly watch political shows will be the same as the CATE for those who do not, and for reasons provided in B. the CATE for those who watch political shows might be smaller, zero, or even positive. On the other hand, if the treatment of frequent viewers has important spillover effects on non-viewers, the treatment of a small fraction of the population could conceivably have a large aggregate effect on the whole population.

e) Critics of cable TV shows argue that the programs should be encouraged to be more civil. Can the estimated ATE be used to predict the effect of increasing the civility of cable shows on the overall public level of trust in government?
Answer:
There are a number of (familiar and fairly generic) reasons why estimates from the Mutz and Reeves laboratory experiment may not generalize. Among these is the concern that subjects viewing treatments in a lab are aware they are being monitored and therefore the viewing experience is artificial. More specific to the Mutz and Reeves application, although the estimated CATE for the X=1 subgroup might provide some guidance, the ATE is a mixture of CATEs and so might be a misleading guide to the effect of changing the content of cable shows.

f) Suppose that a company which tracks television viewers provides you a list of three million potential subjects, along with data on their TV viewing habits. How would you select the subjects for a follow-up experiment if you were interested in estimating how trust in government would change if political TV programs were to become more civil?
Answer:
Rather that assemble a random sample of the potential subjects, an alternative approach would be to oversample subjects with high, medium and low levels of consumption of political shows. Then perform the Mutz and Reeves experiment and measure the CATE for each of these groups. Focusing on those who watch political shows will provide an estimate of the treatment effect among those in the population actually exposed to the treatment. Selecting subjects with varying levels of prior exposure to political shows will permit the researcher to investigate the possibility that those who appear to be the most interested in political shows (who watch the most) have the smallest negative (or perhaps positive) treatment effects. Result of this sort should be interpreted with greatest caution, however, since those who watch different amounts of political television shows may differ in ways other than their television viewing: pre-treatment viewing habits are not randomly assigned. If the researcher is truly interested in measuring the causal effect of prior television viewing, a superior approach would be to produce variation in viewing through an encouragement to view political shows, and then see if the treatment effect if different across those randomly encouraged to watch political shows and those who are not so encouraged.

g) The researchers also measure whether aggressive shows are more engaging to audiences. They use multiple outcome measures: a survey item response and a physiological measure, galvanic skin response (see pp. 10-11 for a discussion). What is the rationale for using the physiological measure? What potential problem with survey response is it designed to address?
Answer:
Survey response may be inaccurate for a variety of reasons. Respondents may misreport their feelings and opinions to conform to researcher expectations (demand effects) or to provide socially normative responses. Respondents may not accurately perceive their own psychological states such as levels of arousal in response to a stimulus, and therefore they are unable to provide accurate reports. The most obvious potential problem with survey response in the Mutz and Reeves application is that it may be socially desirable to say that you did not find the uncivil show engaging. The rationale for using a physiological measure is to avoid these potential survey reporting pitfalls. The key assumptions are, first, that the physiological measure (galvanic skin response) is a more direct window into the subject's true response. It is an involuntary response to the stimulus and is uncontaminated by concerns about investigator expectations or social norms. Second, skin response is a proxy for level of engagement with what is being viewed.

Question 3

In an experiment designed to evaluate the effects of political institutions, Olken randomly assigned 49 villages in Indonesia to alternative political processes for selecting development projects.[3] Some villages were assigned to the status quo selection procedure (village meetings with low attendance), while others were assigned to use an innovative method of direct elections (a village-wide plebiscite). Consistent with expectations, participation in the plebiscite was 20 times greater than attendance at the village meetings. Olken examines the new procedure's effect on which projects are selected and how the villagers feel about the selection process. He finds that there are minimal changes in which projects are selected. However, a survey after the project selection found that the villagers who were assigned to the plebiscite reported much greater satisfaction with the project selection process, and were significantly more likely to view the selection as fair, and the project as useful and in accordance with their own and the people's wishes.

a) One part of this experiment focuses on whether the treatment influences which projects villages select. These results are reported in Figure 1, and the study is described on pp. 244-247. Describe the experimental subjects. What units are assigned to treatment versus control? What is the treatment?
Answer:

The subjects are 49 villages in Indonesia. Villages in three regions were randomly assigned to treatment or control: North Sumatra (5 plebiscite, 14 meeting), East Java (3 plebiscite, 7 meeting) and Southeast Sulawesi (9 plebiscite, 11 meeting). These villages are all eligible to propose development projects for possible funding through a government program. The treatment involves altering the process whereby villages select which projects they will propose for funding. The standard method (control group) involves assembling two lists of possible projects (a general project selected from the ideas produced by meetings attended by men or by both genders and a women's project selected from ideas produced by meetings of women). The final step in the project proposal process is to take the list of project ideas to sparsely attended general meetings (one to which the whole village is invited, one just for women) to select which two project ideas will be proposed. In the alternative decision process, which is the treatment, the final step in this process (the village-wide meeting) is replaced with a village-wide election (one election for the general project and one for the women's project) to determine which of the project ideas will be proposed. Further details of the election procedure are found on page 247.

b) Suppose that in Indonesia, the plebiscite method is rare, but the village meeting is very common. How would this affect your interpretation of the findings?
Answer:
If the treatment is novel, the treatment is the effect of a combination of two things, the introduction of a novel form of decision making and the introduction of the particular political structure. If the estimand of interest is, say, the consequences of varying the degree of participation holding the degree of novelty of the political process constant (which is arguably what the contrast between the plebiscite and status quo decision process is attempting to capture), the difference in average outcomes across groups will not estimate this. In addition, the novelty of the method may wear off, which suggests the effects will not generalize to long term effects.

c) The level of satisfaction is measured by survey responses. From the description on p. 250, can you tell who conducted the surveys and whether the interviewers were blinded as to the

[3] Olken 2010.

respondents' assignment to treatment or control? Why might survey measures of satisfaction be susceptible to bias?

Answer:

It is not entirely clear from the information contained in the data section of the article how the survey was designed and implemented. In particular, it is not clear who interviewers were, whether they were blinded as to subject treatment or control group status, and how subjects were assigned to interviewers. The use of survey response raises two important issues regarding the accuracy of variables measured by surveys. First, there is a danger that interviewers' biases may affect the measurement. When the interviewer is not blinded as to the respondent's assignment there is a danger the results may be shaped by intentional or unintentional favoritism. This can occur in several ways. There is often some discretion in how answers are coded. For example, respondents often do not use the categories supplied and the interviewer then asks follow-up questions to determine how to classify responses within the survey categories. Efforts to obtain responses may also vary with interviewer expectations about how the respondent is likely to answer the questions. Second, respondents may shape their responses to please the interviewer or to conform to a social expectation regarding proper response. Respondents may infer what answers would please the interviewer. In this context, if the respondents assume that the interviewer is connected to the development program, there might be a tendency to report a favorable response to the novel process introduced by the interviewer's presumed organization. Setting the interviewer aside, respondents may simply believe that any novel program represents a "gift" and to respond negatively would show ingratitude. This problem is heightened if the interviewer is assumed to provide the gift. Relatedly, the response might have a strategic component: the respondent might believe that a more favorable evaluation of the intervention will lead to additional benefits. Some of these difficulties can be remedied through survey design and implementation. The subjects should be randomly assigned to interviewers to prevent interviewers from sorting themselves to certain subjects. Interviewers should be blinded as to subject group to prevent biased coding or surveying. Ideally, respondents should be unaware the survey has any link to the program that is being evaluated.

d) There is no indication that the treatment and control villages had contact with each other. Imagine, however, that people regularly communicated across village lines. What assumption might be violated by this interaction? Discuss how cross-village communication might affect treatment effect estimates. What design or measurement strategy might address possible concerns?

Answer:

The interaction across villages violates the non-interference assumption. It is conjecture as to how the inference might affect the results. Assume that the researcher wishes to estimate the effects under the assumption of global non-interference. If projects may be viewed as substitutes (if one village does a water project, the neighboring village will not), communication may exaggerate the estimated effects of the intervention on project choice, since this is based on a comparison of the treatment and control group choices. On the other hand, if villages tend to copy one another's project choices, communication will attenuate the treatment effect. Communication across villages may affect the subjective assessment of the treatment intervention as well. For instance, learning of the introduction of a novel scheme of decision making in a neighboring village may lead to reduce satisfaction with the status quo institutions.

e) Olken concludes that, consistent with the views of many democratic theorists, participation in political decision making can substantially increase satisfaction with the political process

and political legitimacy. Does the experiment provide convincing evidence for this general proposition? What are some of the limitations noted by Olken (see pp. 265-266)? What additional limitations does the experiment have? How might you address these concerns in a future experiment?
Answer:
Olken discusses several limitations. Fist, the subjects are 49 villages in 3 Indonesian provinces and results may not generalize outside the subject pool. Second the study observed outcomes over a relatively short period of time. Satisfaction levels may change decay over time if actual project choices remain unchanged. There might be strategic adaptation to the new environment which might affect the results. Third, the study was small and so might have been insufficiently powered to detect some treatment effects. These concerns can be addressed by performing a larger study over a longer period of time with a broader subject population. Running the study for a longer period of time would also address the concern that the novel of the intervention is an important factor in the subject response to the introduction of the plebiscite.

f) It is often claimed that short-term effects may diminish over time, but the short-run outcome measurements nevertheless reliably indicate the direction, if not the magnitude, of the long-term effects. However, if an institutional change is thought to be a durable feature of the political world, leaders and voters may change their behavior and the way they compete for power. Speculate on why the long-term effects of the plebiscite on satisfaction with the decision process might be negative despite the initial positive response.
Answer:
A more participatory process may lead over time to more political factions and more conflict and social tension, which may cause dissatisfaction. The short term positive response could be due to anticipated benefits of the new process and if the performance of the new system does not meet these expectations, this may lead to greater frustration and disappointment.

Question 4

In section 12.5, we considered a hypothetical experiment in which leaflets were distributed to publicize an audit that declared local government to be honest or corrupt. Suppose another experiment of this kind were conducted in 40 municipalities, half of which are honest and half corrupt. Half of the honest municipalities are randomly assigned to receive leaflets publicizing the auditor's finding of honesty, and half of the corrupt municipalities are randomly assigned to receive leaflets publicizing the auditor's report of corruption. Outcomes are the incumbent mayor's vote share in an upcoming election. The data from the experiment are used to estimate the following regression:

$$Voteshare_i = \beta_0 + \beta_1 Leaflet_i + \beta_2 Honest_i + \beta_3 Leaflet_i * Honest_i + u_i,$$

where $Voteshare_i$ is the incumbent's vote share (from 0 to 100 percent), $Leaflet_i$ is scored 1 if the municipality is randomly treated with a leaflet (0 otherwise), $Honest_i$ is scored 1 if the municipality receives an audit rating declaring it to be honest (0 if it was declared corrupt), and u_i is the disturbance term. Suppose the regression estimates (and estimated standard errors in parentheses) are as follows: $\hat{\beta}_0 = 30(4)$, $\hat{\beta}_1 = -15(5)$, $\hat{\beta}_2 = 25(5)$, $\hat{\beta}_3 = 35(7)$. Interpret the results, taking care not to assume that the average treatment effect of leaflets announcing the honest rating in honest municipalities is the same as the average treatment effect of leaflets announcing the honest rating in corrupt municipalities. (Hint: Use the regression coefficients to figure out what the regression results would be if honest and corrupt municipalities were analyzed separately. See section 9.4).

Answer:
From these four coefficients, we need to recover four group means: Treated versus untreated in honest municipalities and treated versus untreated in corrupt municipalities.
The intercept, β_0, is the average outcome in untreated corrupt municipalities: 30. $\beta_0 + \beta_1$ is the average outcome in the treated corrupt municipalities: 30 + -15 = 15. $\beta_0 + \beta_2$ is the average outcome in the untreated honest municipalities: 30 + 25 = 55. $\beta_0 + \beta_1 + \beta_2 + \beta_3$ is the average outcome in the treated honest municipalities: 30 + -15 + 25 + 35 = 75.
The average effect of treatment in the corrupt municipalities is a decrease of 15 points in the incumbent mayor's vote share. The average effect of treatment in the honest municipalities is an increase of 20 points in the incumbent mayor's vote share. These treatment effects both appear to be statistically significant, as does the difference between them.

Question 5

The Simester et al. study showed how incomplete outcome measurement can lead to erroneous conclusions. On that note, suppose researchers are concerned with the health consequences of what people eat and how much they weigh. Consider an experiment designed to measure the effect of a proposal to help people diet. Subjects are invited to a dinner and are randomly given regular-sized or slightly larger than regular-sized plates. Hidden cameras record how much people eat, and the researchers find that those given larger plates eat substantially more food than those assigned small plates. A statistical test shows that the apparent treatment effect is far greater than one would expect by chance. The authors conclude that a minor adjustment, reducing plate size, will help people lose weight.

a) How convincing is the evidence regarding the effect of plate size on what people eat and how much they weigh?
Answer:
The outcome measure is how much people eat at a single dinner. This may not be a good proxy for weight loss for a variety of reasons. Subject behavior may change along other dimensions (exercise behavior, snacking). The effects of plate size may wear off over time. Behavior at a dinner to which you are invited may differ from typical eating behavior.

b) What design and measurement improvements do you suggest?
Answer:
Several changes might improve the design. First, because other weight-related behavior may be altered in addition to the food consumption at the single dinner, the researchers would either need to obtain an accurate diary of food consumption and other activities, or else measure the variable of interest (that is, weight, after enough time has passed for digestion of the meal) directly. There are obvious limitations to these additional measures, as the diary may be inaccurate and weight is variable (lots of noise in Y) and the noise will dominate unlikely the treatment effect as weight is not likely to be affected appreciably by variation in consumption during a single meal. In any event, the study is likely far too short term to provide convincing evidence regarding weight loss. Finally, using a more naturalistic setting might also improve the study. Possibilities might be to use different size dishes to see how it affects portions in a cafeterias that people habitually eat in (this would assign groups of cafeteria regulars to treatment and control). Another possibility, this one at the household level, would be to give people a new set of (larger or slightly smaller) dishes to use in their home.

Question 6

As noted in section 12.1, experiments are sometimes motivated by a desire to test two rival explanations for an empirical regularity. Each of the three examples below features a clash between competing explanations. For each topic, propose an experiment that would, in principle, shed light on the causal influence of each explanation. Assume that you have a very large budget and a good working relationship with governments and other organizations that might implement your experiments.

a) Does imprisonment reduce crime because convicts have fewer opportunities to break the law, or does imprisonment deter crime by teaching prisoners about the penalties they face if they re-offend?
Answer:

The first explanation is at the aggregate level: the crime rates of whole towns may decrease if a larger proportion of criminals is incarcerated. The second explanation takes place at the level of the individual criminal: their experience in prison induces them not to re-offend.

A design that addresses both possible explanations will need to randomize increased incarceration at both levels. Suppose there are 500 municipalities and a list of (not currently incarcerated) criminals is available for all 500. First, we select a random 250 municipalities to receive treatment. In the treatment group municipalities, we randomly select half of the criminals to be locked up for 1 year.

We can then compare the crime rates (one year later) of untreated and treated municipalities. If there is a large decrease in the crime rates in treated municipalities, this would be good evidence for the first explanation (though it is possible that such harsh discipline deters *others* from engaging in crime, which is a third possible mechanism).

We can further compare the criminal activity of treated and untreated criminals in the year after the crackdown: If treated criminals are less likely to reoffend, then we have evidence for the second explanation.

Both, neither, or just one of the explanations might be true.

b) Do employers in the United States discriminate against black job applicants because they believe them to be less economically productive than whites, or do employers discriminate against black job applicants because they harbor negative attitudes toward black people in general?
Answer:

The approach that some audit studies take is to test the second mechanism – whether employers harbor negative attitudes towards black people in general – by providing employers with resumes that are identical except for the names, which are racially distinct. This approach tries to hold constant employers' beliefs about the economic productivity of the applicants while varying their race. The trouble is that employers may still believe that some unobservable quality of the black applicants (i.e., a quality not listed on the resume) will make them less productive.

One approach would be to gauge how much better the black applicant's resume must be in order to eliminate the racial gap. Another would be to attempt some sort of prejudice-reducing intervention aimed at employers, perhaps outside the employment setting.

c) Does face-to-face communication with voters before Election Day raise voter turnout because it reminds people about an upcoming election that they might otherwise forget, or because it conveys the importance of the choices that will be presented to voters?
Answer:

To tease apart these causal mechanisms, we can vary features of the treatment. Suppose that we vary both the timing and the content of treatment. There are two timings: one month before election day and one week before election day. There are 3 scripts: placebo, informational, and civic duty. In the informational script, the canvassers only reminds the voter that the election is coming up. In the civic duty script, the canvasser emphasizes the importance of voting.

The difference between the informational and civic duty scripts will shed light on whether the "importance of voters' choices" is a relevant causal mechanism. The difference between the placebo and the informational script will tell us how much causal work the reminder is doing. The timing will also help in this regard: the treatment effects due to "importance" should endure longer than the treatment effects due to "reminder". If the effects of the one-month importance script are similar to the one-week importance script, but the one-month reminder is much weaker than the one-week reminder, then we can conclude that the civic duty script works through the "importance" channel.

Question 7

In the Slemrod et al. experiment, measuring the outcome variables involved some effort and cost to match names and state tax return records. Outcome measurements were obtained for only a randomly selected portion of the households available to serve as control group observations.

a) Suppose that additional resources were made available to the researchers, and they gathered outcomes for randomly selected taxpayers who were not selected for treatment. (Assume that this was the only thing they could spend the money on.) How would including these additional observations in the control group affect the properties of the weighted difference-in-means estimator? Is it still unbiased? How does its standard error change?
Answer:
There are 6 types of households. For any of the 6 types, let N_t be the number of treatment households, and let N_c be the number of untreated households originally selected for the control group and let N_c^* be the number of additional households selected. The set of households originally selected for measurement from the full set of untreated households was a random sample, which implies that the $E[Y(0)]$ for the originally selected group of Nc households is the same as the $E[Y(0)]$ among the households left behind. The proposal is to take a random sample of these remaining households. Since the expected value of a random sample is the average of the group from which the random sample is drawn, the expected value of the additional control households is also equal to $E[Y(0)]$. Therefore the new difference of means estimator is an unbiased estimate of the CATE for each type of household.

Gathering additional households from the untreated for measurement and inclusion in the set of households used for the estimation of the treatment effect will increase the precision of the control group tax change estimates, and the average of the combined sample is an unbiased estimator for the change in tax payments for the subjects when they are untreated. Adding the new observations into the existing control group observations does not introduce bias. Using

formula 3.6, the estimated standard error for the estimates changes from $\sigma * (1/n_t + 1/n_c)$ to $\sigma * (1/n_t + 1/n_c^*)$.

b) Records are sometimes lost over time. Suppose that before the second round of outcome measurement were launched, some taxpayer records went missing. What additional assumption is necessary for the combined old and new control group outcome measurements to be an unbiased estimate of the same estimand as the old outcome measurements?
Answer:
The combined control group after the second round of sampling is a weighted average of a random sample of untreated households from the first round (which is an unbiased estimated of $E[Yi(0)]$, the average outcome when households are untreated) and the average of the households measured in the second round. The expected value of the households that can be measured in the second round is $E[Y_i(0)|R_i(0) = 1]$, where $R_i(0)$ denotes whether an household is missing or not when untreated, and $R_i(0) = 1$ if the household is not missing. Unbiasedness requires that the expected value of the second round random sample be $E[Y_i(0)]$, therefore the requirement for unbiasedness is $E[Y_i(0)|R_i(0) = 1] = E[Y_i(0)]$. This assumption is satisfied if the households are missing at random.

Question 8

According to social psychologists, performance on standardized tests may be affected by seemingly minor contextual features, such as the instructions read to those about to take a test and the similarity between the test-taker and other students taking the test at the same time. This literature implies that subtle asymmetries across treatment and control in how outcomes are measured may have a material effect on test scores. Suppose you were designing an experiment similar to the voucher experiment described in section 12.6. Instead of bringing students to a common testing center for testing, you have decided to use the standardized tests that students ordinarily take in their own schools.

a) What are some important potential sources of asymmetry in outcome measurement? Consider among other things how the test is administered, who proctors the test, who grades the test, the mixture of students in the room for a testing session, and whether the administration and grading is blinded to the subject's group status.
Answer:
If students take the tests in their respective schools, the list of issues raised in subsection (a) may lead to differences in test scores unrelated to academic achievement. First, conducting separate testing sessions opens the door to a number of measurement asymmetries. If different standardized tests are used for treatment and control groups, this clearly leads to measurement differences due to differences in the test. The testing sessions themselves may produce differences in student motivation, attention, and stress levels. These differences may be produced in a variety of ways. There may be differences in how the test is described to the students (whether it measures the results of effort or intelligence), including discussion of expected test performance. There may be differences in testing conditions, such as room temperature, noise levels, and crowding. Proctors in some testing centers may offer hints while others do not. Second, a testing regime may systematically favor one experimental group over the other. If those who administer the test are unblinded, they may treat students differently or explicitly favor one group over the other. If the tests are graded by unblinded graders, this could lead to fudging the results unconsciously or to cheating.

b) How would you design your study to reduce bias due to asymmetric outcome measurement of the treatment and control subjects?
Answer:
Conduct testing using the same tests, mixing control and treatment students together, assigned to randomly selected seating. This will ensure common test and no bias in the physical testing environments. If there is concern that an imbalance in T and C group students will create a less friendly environment for one group versus the other, effort to reduce the effect of the environment might be employed (no talking, space between desks). To avoid priming stereotypes, the instructions for the test should be limited to logistics and not include "welcoming remarks" that refer to private school kids or voucher winners, etc. Those who administrator the test (from first contact inviting the families to the session and on from there) should be blinded to the group assignment of the family. The test graders should be blinded.

c) Suppose you want to investigate the impact of the measurement asymmetries you discuss in part (a). Describe an experimental design to estimate the effect of the measurement asymmetries.
Answer:
The effect of the mix of students T and C group students taking the test during a session can be randomly varied (set up X classrooms and randomly vary the mix across each) to see if it affects test scores. The effect of crowding and other physical room conditions can be randomly varied as well. The effect of different descriptions of the test provided prior to the testing can be studied by randomly varying the descriptions.

The effect of blinding the proctors or graders can be studied by randomly providing some of the subjects (proctors and graders) the group assignments while leaving others unaware. In addition to test differences, classrooms could be videotaped to see if proctors are differentially helpful to one group versus the other. The key design challenge would be to conduct this exercise in an unobtrusive way and so as not tip off the subjects, so that the results provide insight into how unblinded agents behave in a natural setting. An interesting design, if it were possible, would be to provide the subjects (graders) with the group assignment of some but not all of the students under their supervision. In the case of proctoring, this would provide variation in blinding within a classroom, permitting the effect of blinding to be distinguished from other factors that might lead a teacher to favor one group over the other.

Question 9

As pointed out in section 12.4, sending resumes via email seems to have several advantages over typical face-to-face audit studies of racial discrimination. However, an email treatment is a more subtle method of communicating race than a face-to-face meeting. What if some employers do not notice the name on the job application or incorrectly guess the race of the applicant? For simplicity, assume that each human resource officer either concludes that the applicant is black or white. Suppose that when sent any white resume, a human resources officer has an 80% chance of surmising that it is from a white applicant. When sent any black resume, a human resources officer has a 90% chance of surmising that it is from a black applicant. Suppose that making a mistaken classification of a white resume is independent of making a misclassification of a black resume. Recall from Table 12.6 that 9.65% of the white resumes received callbacks, as opposed to 6.45% of the black resumes.

a) For definitional purposes, consider assignment to the white resume to be assignment to treatment, and consider assignment to the black resume to be assignment to control. To show how

misclassification is analogous to noncompliance, use the classification system in Chapter 6 to describe the four types of subjects: what proportion of subjects are Compliers, Never-Takers, Always-Takers, and Defiers?
Answer:
Compliers are the HR officers who think the applicant is white (D=1) when the "white" resume is sent (Z=1), and black (D=0) when the "black" resume is sent (Z=0) are 72% of the subject pool. Always Takers (HR thinks the candidate is white regardless of whether Z=1 or 0) are 8% Never takers: 18% Defiers: 2%

b) What is the ITT_D in this case?
Answer:

$$ITT_D = \pi_{compliers} - \pi_{defiers} = 0.72 - 0.02 = 0.70$$

c) What assumption(s) are needed to interpret the ratio of ITT/ITT_D as the Complier average causal effect? Suppose that when analyzing the data in Table 12.6, you assumed that these assumptions were satisfied; what would be your estimate of the CACE?
Answer:
The analyst could assume the absence of Defiers or, alternatively, that the treatment effect is the same for Defiers and Compliers. Under either assumption, the estimated CACE is: (9.65-6.45)/.7 = 4.57.

d) Does the rate of noncompliance have any bearing on the statistical significance of the relationship between race and interviews that the authors report in Table 12.6?
Answer:
No. The calculations in Table 12.6 are intent to treat effects, and the estimation of the ITT and calculation of its statistical significance does not involve the non-compliance rates. As suggested by part (c), the interpretation of the ITT may be affected by the compliance rate, however, since one reason for a small ITT is high rates of non-compliance. To convert an ITT into the CACE, requires either monotonicity or the assumption of homogenous treatment effects for defiers and and compliers. If either assumption holds, the rescaled ITT (ITT/c, where c is the estimated proportion of compliers minus the proportion of defiers, or the difference in the proportion treated in the treatment group minus the proportion treated in the control group)) is an estimate of the CACE. The standard error of the CACE is approximately equal to the ITT standard error divided by c, and the significance level of the CACE is approximately the same as that of the ITT.

e) What steps do Bertrand and Mullainathan take to reduce the rate of misclassification? Do they measure the rate of misclassification? What methods might you use to measure misclassification rates? What are some strengths and weaknesses of your proposal?
Answer:
Bertrand and Mullanathan compiled a list of the most racially distinctive names based on official records. To see if the names conveyed the information they intended, they performed a pilot study and found that individuals guessed the intended race with very high probability. They were however unable to directly measure how much misclassification by employers occurred in their experiment. Additional steps might be taken to investigate how much misclassification might have occurred. From the report provided in the paper, it appears that the pilot work to confirm the racial interpretation of the names did not involve HR workers and did not look at

the black and white names attached to the resumes. It is also unclear how resumes are evaluated by firms. For example, if resumes are sorted by putative race of applicant, this might be studied directly. Perhaps the best method to test the level of misclassification would be to work with a set of employers and have the HR office code each resume they process according to beliefs about the race of the applicant. Putting aside the feasibility of this proposal, introducing this coding might heighten attention to the racial "clues" in the resume. Having the HR worker fill out the form after processing the resume would avoid this issue, but only the first resume would avoid the potential distortion associated with the coding.

A simple modification of the pilot testing in Bertrand and Mullanathan would be to tests the names on HR workers and test names attached to resumes (with HR workers).

Question 10

One limitation of the restorative justice experiment described in section 12.6 is that one cannot identify the distinct effects of an apology or a no-show; instead, one can only estimate the effects of a treatment that is a combination of the two. Suppose that in order to identify the ATE of an apology as well as the ATE of a no-show, you assigned subjects randomly to one of three experimental groups: a control group, a standard encouragement group, and a strong encouragement group. The identification proof posits three different types of subjects: Compliers (those who show up when encouraged in any way), Reluctant-Compliers (those who show up only when strongly encouraged), and Never-Takers.

a) Write the expected outcome in the control group as a weighted average of the expected outcomes among Compliers, Reluctant-Compliers, and Never-Takers.
Answer:
Let the subjects offenders be of three types, T_i, where $T_i = 1$ for the Compliers, $T_i = 2$ for the Reluctant compliers, and $T_i = 3$ for the Never takers). Let there be three potential outcomes for each subject, Y(0), Y(-1), Y(1), for control group assignment, no show, and apology respectively.
Expected Outcome for the Control group:

$$E[Y_i(0)|T_i = 1] * P(T_i = 1) + E[Y_i(0)|T_i = 2] * P(T_i = 2) + E[Y_i(0)|T_i = 3] * P(T_i = 3),$$

where $P(X)$ is the proportion of the subjects for whom $T_i = x$

b) Write the expected outcome in the standard encouragement group as a weighted average of the expected outcomes among Compliers, Reluctant-Compliers, and Never-Takers. Your model should acknowledge that Compliers will offer an apology, but Reluctant-Compliers and Never-Takers will be no-shows.
Answer:
Expected Outcome for the Standard Encouragement group:

$$E[Y_i(1)|T_i = 1] * P(T_i = 1) + E(Y_i(-1)|T_i = 2] * P(T_i = 2) + E[Y_i(-1)|T_i = 3] * P(T_i = 3)$$

c) Write the expected outcome in the strong encouragement group as a weighted average of the expected outcomes among Compliers, Reluctant-Compliers, and Never-Takers. Your model should acknowledge that Compliers and Reluctant-Compliers will offer an apology, but Never-Takers will be no-shows.
Answer:
Expected Outcome, Strong Encouragement:

$$E[Y_i(1)|T_i = 1] * P(T_i = 1) + E[Y_i(1)|T_i = 2] * P(T_i = 2) + E[Y_i(-1)|T_i = 3] * P(T_i = 3)$$

d) Explain why the experimental design allows us to estimate the shares of the three types of subjects.
Answer:
From the strong encouragement group, we can estimate the percentage of $T_i = 3$ types, $P(T_i = 3)$ using the proportion that does not offer an apology. From the standard encouragement group, we can estimate the sum of $P(T_i = 2) + P(T_i = 3)$ using the proportion that does not offer an apology. From these two estimates, we can estimate $P(T_i = 2)$ and $P(T_i = 3)$. Since $P(T_i = 1) = 1 \breve{} P(T_i = 2) \breve{} P(T_i = 3)$, we can estimate the share of the subject pool for each type.

e) Notice that in the three equations (a), (b), and (c) there are four parameters: the ATE of a no-show among Never-Takers, the ATE of a no-show among Reluctant-Compliers, the ATE of an apology among Compliers, and the ATE of an apology among Reluctant-Compliers. No matter how you manipulate the three equations, you cannot solve for each of the four parameters. In other words, with more unknown parameters than equations, you cannot identify either of the apology effects or either of the no-show effects. Suppose you assume instead that the ATE of a no-show is the same regardless of whether a Reluctant-Complier or Never-Taker is at fault and that the ATE of an apology is the same regardless of whether it comes from a Complier or Reluctant-Complier. Now you have reduced the number of unknowns to just two parameters. Revise your equations (a), (b), and (c) to reflect this assumption, and show that it allows you to identify the apology effect and the no-show effect.
Answer:

[This answer is simply a matter of following the steps listed in the problem itself]

Question 11

One reason for concern about attrition in the school voucher experiment described in section 12.7 was that, after the first year, the attrition rate was greater in the control group than the treatment group. Intuitively, the problem with comparing the treatment and control group outcomes is that the post-attrition control group is no longer the counter-factual for the post-attrition treatment group in its untreated state. The trimming bounds described in Chapter 7 attempt to extract from the post-attrition treatment group (which has a larger percentage of the randomly assigned group reporting) a subset of subjects who can be compared to the control group and used to bound the treatment effect. The dataset for this exercise at http://isps.research.yale.edu/FEDAI contains subjects of any race in the Howell and Peterson study who took a baseline math test. The outcome measure (Y_i) is the change in math scores that occurred between the baseline test and the test that was taken after the first year of the study.

a) What percentage of the control group is missing outcome data? What percentage of the treatment group is missing outcome data?
Answer:

```
In [1]: import delim ./data/chapter12/Howell_Peterson_BIP_2002, clear

In [2]: tabulate missing_y1math treat, column nof
```

```
missing_y1 |         treat
      math |         0          1 |     Total
-----------+----------------------+----------
         0 |     75.90      81.10 |     78.61
         1 |     24.10      18.90 |     21.39
-----------+----------------------+----------
     Total |    100.00     100.00 |    100.00
```

24% of the control group has missing outcome data, compared with 19% of the treatment group.

b) Among students with non-missing outcome data, what are the average outcomes for the control group and treatment group?
Answer:

```
In [3]: tabstat y0_1math_change, by(treat) stat(mean) not


Summary for variables: y0_1math_change
     by categories of: treat

   treat |      mean
---------+----------
       0 |  6.486647
       1 |  7.104994
--------------------
```

6.487 for the control group, 7.105 for the treatment group.

c) What is the distribution of outcomes for the treatment group? What is the range of outcomes? What outcomes correspond to the 5%, 10%, 15%, 25%, 50%, 75%, 85%, 90%, and 95% percentiles?
Answer:

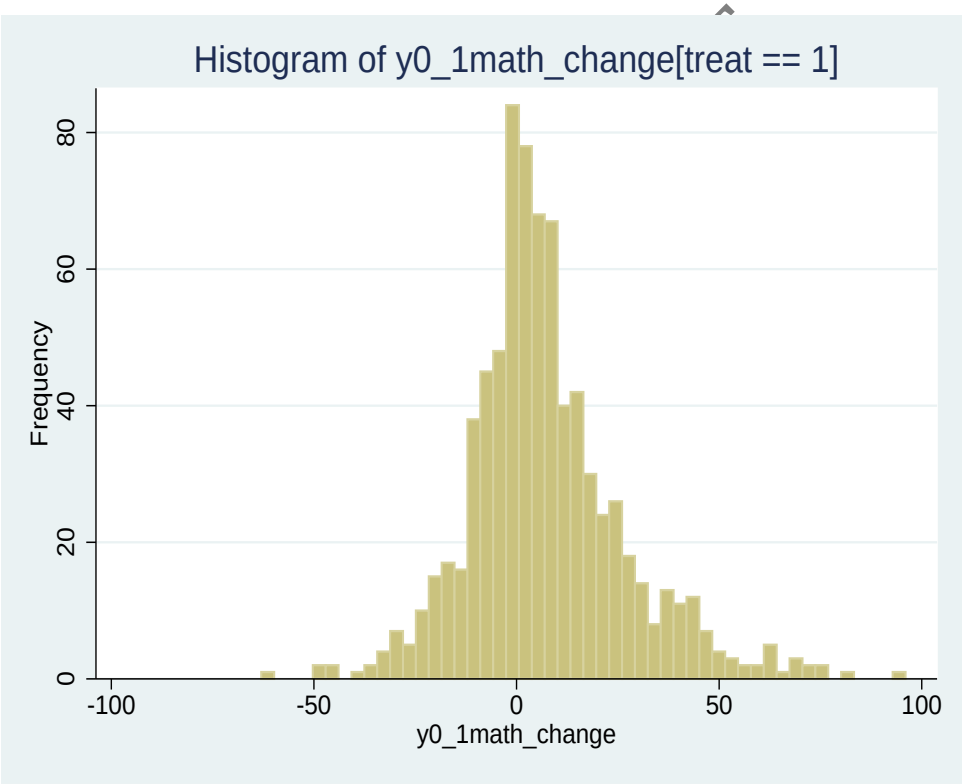
```
In [4]: histogram y0_1math_change if treat==1, bin(50) freq ///
        title("Histogram of y0_1math_change[treat == 1]")

In [5]: tabstat y0_1math_change if treat==1, stat(min max)


    variable |       min       max
-------------+--------------------
y0_1math_c~e |       -63        96
----------------------------------
```

```
In [6]: centile y0_1math_change if treat==1, centile(5 10 15 25 50 75 85 90 95)


                                                       -- Binom. Interp. --
    Variable |       Obs  Percentile    Centile        [95% Conf. Interval]
-------------+-------------------------------------------------------------
y0_1math_c~e |       781           5         -20             -23         -18
             |                    10         -13             -16         -11
             |                    15          -9             -11          -7
             |                    25          -4              -5          -2
             |                    50           4               3           5
             |                    75          16              14          18
             |                    85          25              22          27
             |                    90          32              28          36
             |                    95          43              40          48
```



d) To trim the top portion of the treatment group distribution, what value of Y_i is the 93.6 percentile of the treatment group? (The value 93.6 is the control group reporting rate divided by the treatment group reporting rate.)
Answer:

```
In [7]: centile y0_1math_change if treat==1, centile(93.6)
```

```
                                                        -- Binom. Interp. --
    Variable |       Obs  Percentile    Centile        [95% Conf. Interval]
-------------+-------------------------------------------------------------
y0_1math_c~e |       781       93.6          40              36          44
```

The 93.6 percentile value of Y is 40.

e) What is the average value of the treatment group observations that are less than the 93.6 percentile value? Call this average treatment effect L_B. Confirm that the percentage of the original treatment group that remains is equal to the percentage of the control group with outcome data.
Answer:

```
In [8]: qui mean y0_1math_change if treat==1 & missing_y1math==0 & y0_1math_change < 40
        scalar l_b = _b[y0_1math_change]
        qui count if treat==1 & missing_y1math==0 & y0_1math_change < 40
        scalar l_b_count = r(N)
        qui count if treat==1

In [9]: disp %8.6f l_b

3.701513


In [10]: disp %8.7f 1-l_b_count/r(N)

0.2450675
```

The average value of the observations less than or equal to 40 is 3.70. There are 727 such values, and 1- (727/963) = 24.5%. The rate of missing for the control group is 24.1%.

f) Subtract the control group average from L_B.
Answer:

```
In [11]: qui mean y0_1math_change if treat==0
         disp %18.6f l_b - _b[y0_1math_change]

         -2.785134
```

g) To trim the bottom portion of the treatment group distribution, what treatment group outcome corresponds to the 6.4 percentile? (The value 6.4 is calculated by subtracting 93.6 from 100.)
Answer:

```
In [12]: centile y0_1math_change if treat==1, centile(6.4)


                                                        -- Binom. Interp. --
    Variable |       Obs  Percentile    Centile        [95% Conf. Interval]
-------------+-------------------------------------------------------------
y0_1math_c~e |       781        6.4         -18             -21         -16
```

The 6.4 percentile value is -18.

h) What is the average value of the treatment group observations that are greater than the 6.4 percentile? Call this average treatment U_B. Confirm that the percentage of the original treatment group that remains after trimming is equal to the percentage of the control group with outcome data.
Answer:

```
In [13]: qui mean y0_1math_change if treat==1 & missing_y1math==0 & y0_1math_change > -18
         scalar u_b = _b[y0_1math_change]
         disp %8.6f u_b

9.707586


In [14]: qui count if treat==1 & missing_y1math==0 & y0_1math_change > -18
         scalar u_b_count = r(N)
         qui count if treat==1
         disp %8.7f 1-u_b_count/r(N)

0.2471443
```

The average of the values that remain after trimming off the lower 6.4% is 9.71. The percentage of those reporting with outcomes greater than -18 is 725/963=75.3% for a missing rate of 24.7%. This is approximately equal to the missing rate for the control group of 24.1%

i) Subtract the control group average from U_B.
Answer:

```
In [15]: qui mean y0_1math_change if treat==0
         disp %8.6f u_b - _b[y0_1math_change]

3.220939
```

j) The lower and upper bounds that you calculated in parts (f) and (i) are designed to bound an ATE for a particular subgroup. Describe this subgroup.
Answer:
(3.22, -2.79) are the estimated bounds for the treatment effect for the always reporters.

Question 12

In private school voucher studies, treatment group observations are much more expensive than control observations. Assume the experiment is free from attrition and non-compliance. Suppose that the researchers have a fixed budget of $2M, each treatment group observation costs $2,000, and each control observation costs $200. The table below shows four possible ways to use the budget to form treatment and control groups. Let the standard deviation of outcomes in the treatment

Table 1: Question 12 Table

	Option 1	Option 2	Option 3	Option 4
Treatment	950	750	600	900
Control	500	2500	4000	1000

group and the control group be the same, and equal to s.
Estimate the standard error for the difference-in-means estimator using the formula in equation (3.6), letting the number of observations assigned to treatment be n_t and the number of observations assigned to control be n_c. The standard error may be written:

$$s\sqrt{\frac{1}{n_c}+\frac{1}{n_t}}$$

a) In the table, which allocation of subjects to treatment and control produces the most precise estimate?
Answer:
The standard errors are S times: Option 1, .05525, Option 2: .041633, Option 3: .04378, Option 4: .045947. Therefore, Option 2 produces the most precise estimate.

There is a general method for minimizing the standard error subject to a budget constraint. Suppose the cost per observation in the control and treatment groups are p_c and p_t, respectively, and both groups have the same standard deviation. To minimize the standard error of the difference-in-means, assign subjects to groups in proportion to the square root of the cost ratio. The following questions illustrate the derivation behind this idea.

b) If n_t is the number of subjects you assign to the treatment group, how much money is spent on the treatment group?
Answer:
$p_t * n_t$

c) If n_c is the number of subjects you assign to the control group, how much money is spent on the control group?
Answer:
$p_c * n_c$

d) Express the budget B as the total spent on the treatment group and control group.

$$B = p_t * n_t + p_c * n_c$$

Set up a constrained maximization problem by defining the Lagrangian equation (Dixit 1990)

$$L(q, n_c, n_t) = s\sqrt{\frac{1}{n_c}+\frac{1}{n_t}} - q(B - n_t p_t - n_c p_c)$$

Take the partial derivative of L with respect to n_c, n_t, and q, and set each of the partial derivatives equal to zero. (If your calculus is rusty, use an online calculator to take derivatives.) The values of n_c and n_t that satisfy these conditions minimize the standard error subject to the budget constraint.

partial wrt n_c: (1) $-1/n_c^2 + q * p_c = 0$, which can be written $1/n_c^2 = q * p_c$ Partial wrt n_t: (2) $-1/n_t^2 + q * p_t = 0$, which can be written $1/n_t^2 = q * p_t$ Partial wrt to q: (3) $B = p_t * n_t + p_c * N_t$

e) Set the partial derivative with respect to n_t equal to the partial derivative with respect to n_c. Manipulate the resulting equation to show that

$$\frac{p_c}{p_t} = (\frac{n_t}{n_c})^2$$

Answer:
Three equations and three unknowns: n_c, n_t, q. The values which solve these three equations satisfy the necessary condition for minimizing the standard error subject to the budget constraint. From (1) and (2):

$$\frac{1}{p_c * n_c^2} = \frac{1}{p_t * n_t^2}$$
$$\frac{p_c}{p_t} = (\frac{n_t}{n_c})^2$$

From this result it follows that the ratio of the size of the treatment group to the size of the control group is equal to the inverse of the square root of the ratio of the costs of each type of observation. Thus, if a treatment group observation costs 10 times as much as a control group observation, the standard error minimizing division of resources places $\sqrt{10} \approx 3.2$ times as many observations in the control group.

f) When the cost of treatment and control group observations is the same, what is the appropriate way to allocate the budget to n_t and n_c?

Answer:
If $p_t = p_c$, then $\frac{p_c}{p_t} = 1$, which implies that the treatment and control groups should be the same size.