Pre-Analysis Plan for: Incorporating Choice into Design and Analysis of Survey Experiments

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Contents

Overview

This study incorporates choice into the design and analysis of survey experiments examining how race and gender condition the impact of arguments in support of the Black Lives Matter and Me Too social movement. Using a parallel design, it combines a traditional survey experiment in which subjects are randomly assigned to treatment and control, with a parallel condition in which subjects have the option to select into or out of treatment to identify the average choice-specific treatment effects (ACTE). The design also incorporates a second round of randomization, assigning some subjects who initially opted out of receiving the treatment to receive a similar or alternative treatment, allowing for the identification of additional ACTEs conditional on subjects initial choices.

What are the hypotheses to be tested/quanities of interest to be estimated?

- How do race and gender condition the persuasive effects of arguments about the #BlackLivesMatter #MeToo movements?
- Average choice-specific treatment effects (ACTE) among people likely to seek out or avoid treatment
- Conditional average choice-specific treatment effects (CACTE) among people likely to avoid treatment who are then randomly assigned to treatments or control

Sample Size (# of Units)

- Pilot Studies: Two surveys of 1,000 respondents each recruited by Amazon Mechanical Turk
- National Studies: 1,000 respondents recruited by Qualtrics online panel

Was a power analysis conducted prior to data collection?

- Yes

IRB approval

- IRB Number: #1808002175
- Date of IRB Approval: 8/17/2017

Part I

Pre-Analysis Plan

Motivation and background

Politics, like life, involves choice. Yet the dynamics of choice are often absent from how scholars study citizens' attitudes and behavior. Survey experiments, for example, are prized for their ability to yield unbiased estimates of the average treatment effect of some intervention. Yet the average effect if every subject received the treatment is not always the most politically relevant estimate of interest (Gaines and Kuklinski 2011). In their everyday lives, people have some choice over what they read or who they talk to, and the political consequences of these experiences will likely vary with the frequency of their occurrence. The average effect of an editorial from the *New York Times*, for example, will likely differ from the separate effects among subscribers and non-subscribers. While scholars can and do test for heterogeneous responses by estimating separate effects by subgroup – such practices can have pernicious consequences particularly when scholars test many possible, plausible comparisons but only report the subset of significant results. (Gelman and Loken 2013).

In this project, we build on approaches that seek to increase the external validity of survey experiments by incorporating the opportunity for choice into the design and analysis of experiments. Using a parallel-design similar to those employed by Gaines and Kuklinski (2011) and Knox et al. (2014), participants are randomly assigned to one of two conditions: The first condition mirrors that of a standard survey experiment in which some subjects are randomly assigned to read to some information (treatment) or not (control). In the second condition, subjects are first given a choice: they can opt-in to viewing the treatment that others were randomly assigned to see, or they can opt-out.

As Gaines and Kuklinski (2011) show, the average treatment effect in the standard survey experiment can be expressed as a weighted average of the treatment effects among those who would opt-in and out of exposure. The parallel design provides an estimate of the proportion selecting treatment, which, as we detail below, can be used to estimated what Knox et al. (2014) call – the average choice-specific treatment effects (ACTE) among both those likely and unlikely to receive treatment.

We extend this parallel design by incorporating a second round of randomization in the selection arm of the study. Specifically, among those who initially opted out of receiving the treatment, we randomly assign these participants to either receive no information as in the control condition, or to the receive the same information from either a source similar to the one they were trying to avoid or a source they may possibly view as more sympathetic. This second round of randomization provides another set of choice-specific treatment effect conditional on subjects initially opting out of the initial treatment (CACTE). Substantively, they allow us to assess whether an alternative treatment might be more effective among this hard to reach group. Methodologically, if,as seems likely, subjects' choices to select in to or out of treatment are correlated with the outcomes, then randomizing conditional on subjects choice, provides a natural form of covariate adjustment that should increase the precision of these estimates.

We use this design to examine how race and gender condition the persuasiveness of arguments in support of the #BlackLivesMatter and #MeToo movements, respectively. A large literature in political science and related disciples suggests individuals use information about the source of arguments as a heuristic or cue to help them process information (Mondak 1993a; Mondak 1993b; Lau and Redlawsk 2001), and that information about race (e.g. Mendelberg 2001; White 2007) and gender (McDermott 1998, Karpowitz and Mendelberg (2014)) can be particularly particularly influential cues. Similarly, a related literature in political communication finds people often seek out sources they expect to agree with and avoid those they expect to disagree with often based on cues about the partisan leanings of a source (Stroud 2008; Stroud 2010), and the effects of that information tend to vary based on the likelihood of receiving it (Arceneaux, Johnson, and Murphy 2012).

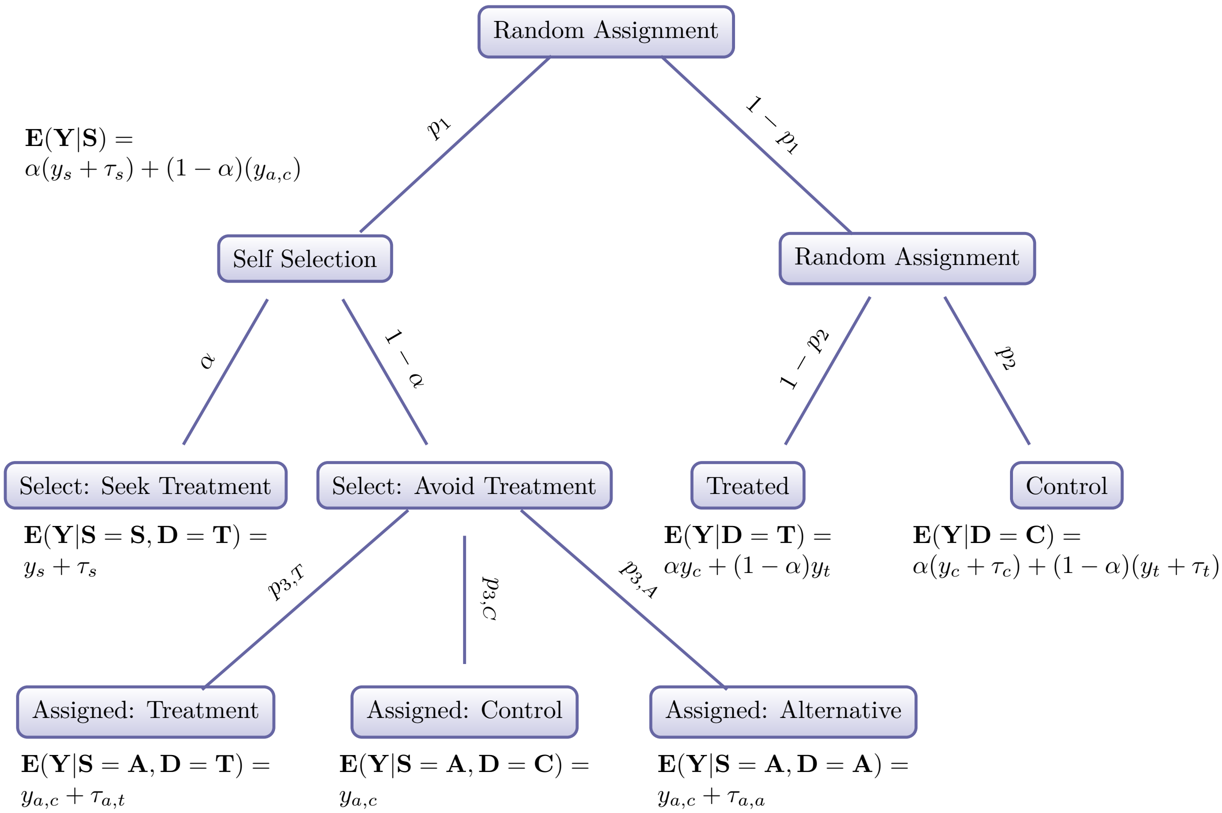
In our present study, then, we expect that the race or gender of someone making an argument in support of #BlackLivesMatter or the #MeToo movement, will condition the persuasive impact of their claims, in ways that may be difficult for traditional survey experiments to identify. For example, we might expect that someone is open to hearing the opinions of a black man on the #BlackLivesMatter movement will update their opinions in a manner consistent with those of the speaker, while someone who would tend to avoid such views, may respond in the opposite direction when exposed to them. Depending on the distribution of such people in our sample, the average treatment effect from a survey experiment may be null, while the effects of among these subgroups may be significant in offsetting directions. By incorporating choice into our experiments we can identify these effects, while also exploring other substantively interesting questions, like whether the same argument from a different source (e.g. a white man) would have a different effect.

Research Design

Our goal in this study is to explore race and gender condition perceptions of the #BlackLivesMatter #MeToo movement. We explore these questions through an experimental design in which some subjects are given an opportunity to choose whether read the opinions of black man/white woman, while others are randomly assigned to read opinions that they may or may not have otherwise chosen to avoid. The general details of this design are outline in Figure 1.

After completing a brief demographic battery, subjects are told they are being asked to participate in a study about their opinions on current events. Subjects are then randomly assigned to one of two experimental

Figure 1: Study Design



settings. Approximately 40 percent ($N \approx 400$) of subjects are assigned to a simple experimental design in which they are randomly assigned to treatment or control with equal probability. Treated subjects are someone else opinions on these topics before answering a set of survey questions. Subjects in control proceed directly to the questions.

The remaining 60 percent ($N \approx 600$) of subjects are assigned to a "selection" experiment. The are told they will be asked some questions about current events, and asked if they would first like to read the opinions of another person who's picture is displayed on the page, first. Subjects who opt-out of the treatment, are then randomly assigned to one of three conditions. Approximately half proceed directly to the questions, as in the control. The remaining half are randomly assigned to read a statement attributed to a person that either shares the race and gender of the person they opted out of seeing or is of a different race (#BlackLivesMatter study) or a different gender (#MeToo study) Subjects are then ask a set of questions about these movements and related issues.

By including this option for self selection, we can estimate the proportion α of the sample willing to here the opinions of someone of a particular race/gender on theses issues. Then we can recover the effect of treatment among those open to such arguments, τ_s, by taking the difference between the average outcome in the selection condition (that is, the weighted average of those who selected both into and out of treatment, $E(Y|S)$), and the average of those in the control, in this study, those randomly assigned to read treatment ($E(Y|D = T)$), and weighting this estimate by the proportion of people likely to select that treatment:

$$\tau_s = \frac{E[Y|D = \mathrm{S}] - E[Y|D = \mathrm{T}]}{\alpha}$$

Similarly, we can recover the effect of treatment on those likely to avoid it, by taking the difference between the average outcome among those assigned to treatment (here: $E(Y|D = \mathrm{T})$), and the average outcome among those allowed to select into or out of treatment ($E(Y|S)$), and weighting this difference by the proportion of those likely to avoid that treatment ($(1 - \alpha)$).

$$\tau_a = \frac{E[Y|T = \mathrm{Black}] - E[Y|\mathrm{Selection}]}{(1 - \alpha)}$$

Standard errors for a ratio of estimates can be constructed via the delta-method (Cameron and Trivedi 2005)

Furthermore, we can estimate an additional set of choice specific treatment effects among those who initially opted out of treatment. The effect of receiving essentially the same treatment among those who opted to avoid it, is

$$\tau_a, t = E[Y|S = \mathrm{A}, D = T] - E[Y|S = \mathrm{A}, D = C]$$

While the effect of receiving the treatment associated with person of the opposite race or gender is:

$$\tau_a, a = E[Y|S = \mathrm{A}, D = A] - E[Y|S = \mathrm{A}, D = C]$$

This first estimate, the treatment effect on those who initially who would have opted to avoid the treatment, provides another estimate of the potential for treatment to produce a backlash among some respondents, while the second estimate offers the opportunity to assess whether an argument might be more effective when presented in a different manner (here when attributed to someone of a different race or gender).

Treatments

The treatments in this study contain two parts: an informational text offering general support for a specific social movement, and photo of attributing the opinions in the text to a speaker of a specific race and gender. Images are taken from the Chicago Face Database (Ma, Correll, and Wittenbrink 2015)

Race and #BlackLivesMatter

Text:

> I think the Black Lives Matter movement is really important. Without Black Lives Matter, most of us wouldn't know who Michael Brown is or Freddie Gray is, and we wouldn't be having the conversations about race and criminal justice that we are.
>
> And sure, All Lives, Blue Lives, all that stuff matters. But I think saying that sort of misses the point. It's like a fireman telling you all houses matter, while yours is burning down. It's about recognizing that's some thing is wrong in a country where black people make up 13 percent of population but account for about third of the total prison population and are more than 2.5 times more likely to be shot and killed by the police. It's about trying to understand what's it like to be followed every time you go into a store or have the police called on you when you're having a BBQ.
>
> People try to say that race isn't an issue anymore or that it's better than it used to be, but I saw these studies where they'd apply to a bunch of jobs or to rent an apartment and the only thing that would differ on the application was whether the name sounded white or black, like Greg or Jamal. The applications with the white names were like 50 percent more likely to get call back.
>
> So yeah, I think having a movement like Black Lives Matter is really important right now.

Image:

Figure 2: Black Male

The image on the left corresponds to the primary race cue. The image in the center corresponds to the gender cue among subjects who opted to initially avoid the initial race treatment, but were assigned to receive the alternative black racial cue. The image on the right corresponds to the alternative white racial cue.

(a) default

Gender and #MeToo

Text:

I think the Me Too movement is really important. Hearing these stories through social media has shown just how common the problems of sexual assault and harassment are in society. It doesn't just happen in Hollywood, or the media, or politics. This stuff happens everywhere and it needs to stop.

I've heard people say that the MeToo movement has a mob mentality, that they accuse people without clear evidence. But we've heard that kind of talk before. For years, when women came forward about their experiences, they were not taken seriously. I read a study that 2 out of 3 sexual assaults go unreported. MeToo shows that women are finally being taken seriously. So right now, given the way things are, it seems like the benefits of helping people tell their stories, of believing women, and not assuming they are lying or exaggerating, outweigh the costs.

Some people say gender issues aren't that important, but the MeToo movement showed that wasn't so. With this big push from the MeToo movement, women are finding new support to confront their harassers and those who enabled them. We've got a long way to go, but Me Too seems like a step in the right direction.

Image:

Figure 3: White Female

The image on the left corresponds to the primary gender cue. The image in the center corresponds to the gender cue among subjects who opted to initially avoid the initial gender treatment, but were assigned to receive the alternative female gender cue.The image on the right corresponds to the alternative male gender cue.

Data and Measurement

The studies will be piloted on MTurk and then fielded on Qualtrics' online panel to obtain a nationally representative sample on age, race, gender, and income. The sample size for each study will be 1,000 respondents. Subjects will be paid \$0.65 for a survey that will take no longer than five minutes corresponding to an hourly rate of \$7.80 an hour). Roughly 400 subjects will be assigned to the standard experiment split evenly between treatment and control. Of the 600 assigned to the selection arm of the experiment, we expect roughly 400 to select into the treatment and 200 to select out. Among those who select out of treatment, approximately one quarter (~50) will be assigned to receive the treatment with a substantively similar racial/gender prime, one quarter (~50) will receive the treatment with the opposite racial/gender prime, and the remaining subjects (~100) will receive no treatment.

Primary Outcomes

The primary outcomes of interest for each study are attitudes about issues of race/gender measured as follows:

Primary Outcomes: Black Lives Matter Study

The six-item battery for the Black Lives Matter study asks subjects the extent to which they agree or disagree with the following statements [0=completely disagree, 100 = completely agree]:

- Criminal justice reform must address racial disparities
- Too much emphasis is placed on race right now (reverse coded)
- Racial discrimination isn't really an issue in the U.S. any more. (reverse coded)
- I support Black Lives Matter
- Black Lives Matter has helped improve race relations in the US
- The police treat all people equally regardless of their race (reverse coded)

The statement order is randomized. The measures will be scaled together using principal components analysis with higher values indicating greater support for the principles of the movement.

Primary Outcomes: Me Too Movement Study

The six-item battery for the Black Lives Matter study asks subjects the extent to which they agree or disagree with the following statements [0=completely disagree, 100 = completely agree]:

- The Me Too movement can sometimes go too far (reverse coded)
- Sexual harassment and assault are still far too common in the U.S.
- In general, women have the same rights and opportunities as men (reverse coded)
- Concerns about gender inequality are overblown (reverse coded)
- I support the Me Too movement
- The Me Too movement helps raise awareness about sexual assault, harassment and discrimination

The statement order is randomized. The measures will be scaled together using principal components analysis, with higher values indicating greater support for the principles of the movement.

Covariates/Heterogenous Response

While our focus is on subjects revealed choices, we will also explore subgroup effects by, race, gender, partisanship, and familiarity with these movements

Secondary Outcomes

We also consider two secondary outcomes:

1. Recall of facts stated in arguments
2. Open response to arguments

The factual recall measure in the Black Lives Matter study is a binary outcome, coded as 1 if subjects correctly recall the percentage of the U.S. population that is black (+/- 3 percent).

The factual recall measure in the Me Too study is a binary outcome, coded as 1 if subjects state they believe 2 out 3 sexual assaults go unreported and 0 if they choose one of three other response options.

Subjects' responses to the open response portion of the outcome will be used to construct two outcomes:

1. A binary indicator of any response
2. A numeric indicator of the total length in characters of that response.

Depending on frequency and length of subjects open responses, we may also report some descriptive analyses of these texts by treatment condition.

Hypotheses to be Tested / Expectations

Broadly, we expect the arguments provided in each study to increase general support for the principles of the Black Lives Matter and Me Too social movements, but that the magnitude and direction of these effects will vary based on the likelihood that subjects would choose to encounter these treatments. We frame our expectations in terms of three scenarios:

Scenario 1: Homogenous effects In this baseline scenario the effects of treatment are positive and consistent across treatment conditions regardless of the race or gender attributed to the speaker. In this case, ATEs from the experimental arm of the studies should be similar to the ACTEs and conditional ACTEs estimated from the experiments incorporating self selection.

Scenario 2: Heterogenous and offsetting effects across respondents In this scenario, the effects of treatment are conditional on the likelihood that a subject would be willing to hear the views of black man/white woman on the Black Lives/Me Too movement. We expect that subjects open to these views will respond consistent with the treatment and so the ACTE among selectors should be positive, while subjects who would opt to avoid these views will either be unmoved or upset, leading to either null or negative ACTEs. Depending on the distribution of these two "types," the overall ATE may be positive (more selectors than avoiders), negative (more avoiders than selectors), or non-significant (roughly similar numbers of selectors and avoiders responding to treatment in opposite and off-setting manners). In this scenario the ACTE estimated among the avoiders, should be similar in sign and magnitude to the conditional ACTE among avoiders who received the same racial/gender treatment after initially opting to avoid it. Among those avoiders who received the alternative treatment, it is possible that hearing the same argument from a different source will have an effect a more positive (or at least less negative) effect

Scenario 3: Heterogenous effects among a subset of respondents In this scenario, the effects of treatment are again conditional on the likelihood that a subject would be willing to hear the views of black man/white woman on the Black Lives/Me Too movement. In this scenario, however, only some respondents are influenced by treatment. For example, it may be that those who would seek out the views of someone whose race or gender makes it likely to think that they would support the goals of the Black Lives Matter/Me Too movements, may already agree with many of the principles of those movements in which case the effect of hearing such information may me muted and the ACTE among selectors will be null. Similarly, it is possible, although perhaps unlikely, that respondents who would initially choose not to encounter such information or the ones most open to persuasive appeals. Rather than responding in an opposite direction from selectors, then we might find that the ACTE among avoiders to be larger than the ACTE among selectors. Again, the overall ATE will depend on the distribution of these types, but should tend to be smaller in size than the choice-specific estimates.

Overall, we believe something approaching scenario 2 to be most likely, however, our primary interest is identifying heterogeneity that may be obscured by standard designs estimating only a simple ATE.

- **Additional expectations/Secondary outcomes** For secondary outcomes, we again expect response to vary conditional on subjects choices and likelihood of encountering information. While we expect significant differences across conditions and estimates, we are less certain about the sign of these effects. For example, it seems plausible that people open to hearing arguments in support of Black Lives Matter or the Me Too movement may be more familiar with the factual information presented in the treatments and so rates of recall should be higher. Alternatively, such subjects may process this information less deeply and skim over these facts, leading to lower rates of recall. For the open response items, we generally expect that subjects who would prefer to avoid these topics may have less to say about them, although it is possible that those subjects forced to such arguments after initially stating their preference to avoid them may be angered by having to encounter them and motivated to defend their positions.

Planned Analyses and Estimation

We will report the following estimates in our analysis:

- For subjects in the selection condition of experiment, we will report the difference of means for a set of demographic and attitudinal covariates between those initially opting in and out of receiving treatment, as well as omnibus tests of covariate balance suggested by Hansen and Bowers (2008).
- The ATE estimated with a difference of means between treatment and control groups in the experimental arms of the studies
- The ACTEs estimated using the procedure outlined by Gaines and Kuklinski (2011) with standard errors obtained through the delta method. Because a portion of the subjects choosing to avoid treatment in the selection condition of the experiment are then randomly assigned to one of two treatment conditions, we use only the responses of avoiders assigned to control (weighted to reflect the total number of subjects opting to avoid treatment) when calculating the average response of subjects in the treatment condition $E(Y|S)$
- The conditional ACTES among avoiders are estimated using a difference of means between treatment and control groups among those who initially chose not to receive the treatment.

Estimates will be displayed graphically with dot plots and 95 percent confidence intervals, using $\alpha = 0.05$ for tests of statistical significance. We will also report separate subgroup analyses by race, gender, partisanship, and familiarity with these social movements. In the appendix we will report analyses excluding respondents who complete the survey outside of two standard deviations of the average response time as well as excluding subjects who fail to complete a brief attention check at the end of the studies.

For any analyses not covered in this document we will follow the standard operating procedures proposed by Lin and Green (2016).

Power Simulations

Below we present the results of a set of power analyses simulating different possible scenarios with 1,000 respondents overall with 40 percent assigned to the experimental arm and 60 percent assigned to the selection condition with an outcome following a standard normal distribution (N(0,1)). In general power is a function of the size and direction of effects among selectors and avoiders, the distribution of these types in the data, and the correlation between likelihood of selecting into or out of treatment and the outcome.

Equal Numbers of Selectors and Avoiders, Equal and Offsetting Effects

Assuming equal and offsetting effects with roughly equal proportions of subjects selecting into and out of treatment, the ACTE among selectors and the conditional ACTEs among avoiders can be expected to detect

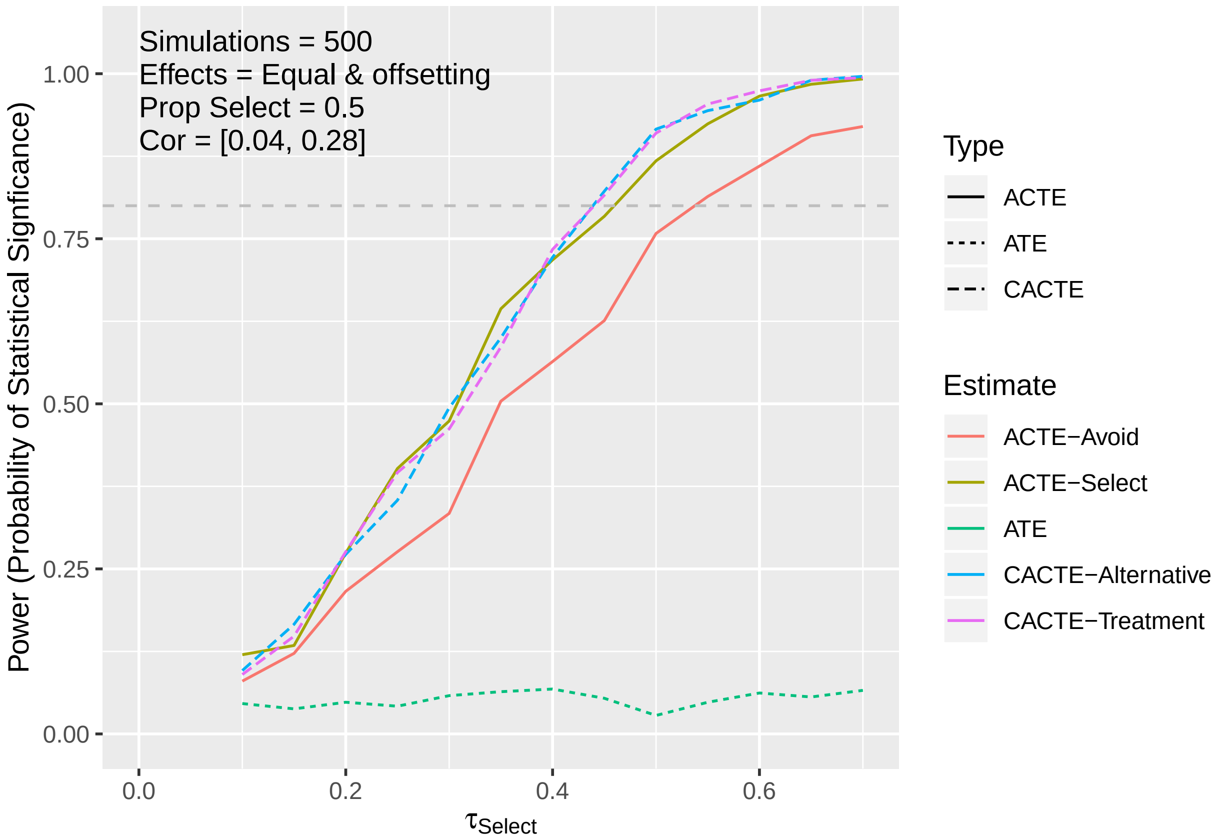


Figure 4

an effect of about 0.45 with about 80 percent power. The ACTE among avoiders can be expected to detect an effect of about 0.55 with about 80 percent power[1]

- N = 1000
- Y0 = Normal (mean=0, sd=1)
- Taus: 0.1 to 0.7 by 0.05
- $\alpha = 0.5$

Table 1: Power Analysis

	Hypothesized Effect Among Selectors												
	0.1	0.15	0.2	0.25	0.3	0.35	0.4	0.45	0.5	0.55	0.6	0.65	0.7
ATE	0.05	0.04	0.05	0.04	0.06	0.06	0.07	0.05	0.03	0.05	0.06	0.06	0.07
ACTE-Select	0.12	0.13	0.27	0.40	0.47	0.64	0.72	0.78	0.87	0.92	0.97	0.98	0.99
ACTE-Avoid	0.08	0.12	0.22	0.28	0.33	0.50	0.56	0.63	0.76	0.81	0.86	0.91	0.92
CACTE-Treatment	0.09	0.15	0.28	0.40	0.46	0.59	0.73	0.82	0.91	0.95	0.97	0.99	0.99
CACTE-Alternative	0.10	0.17	0.27	0.35	0.49	0.60	0.72	0.82	0.92	0.94	0.96	0.99	1.00

[1] The lower power for the ACTE among selectors is lower here in part because the variances are adjusted to reflect the additional weighting of the avoiders assigned to control.

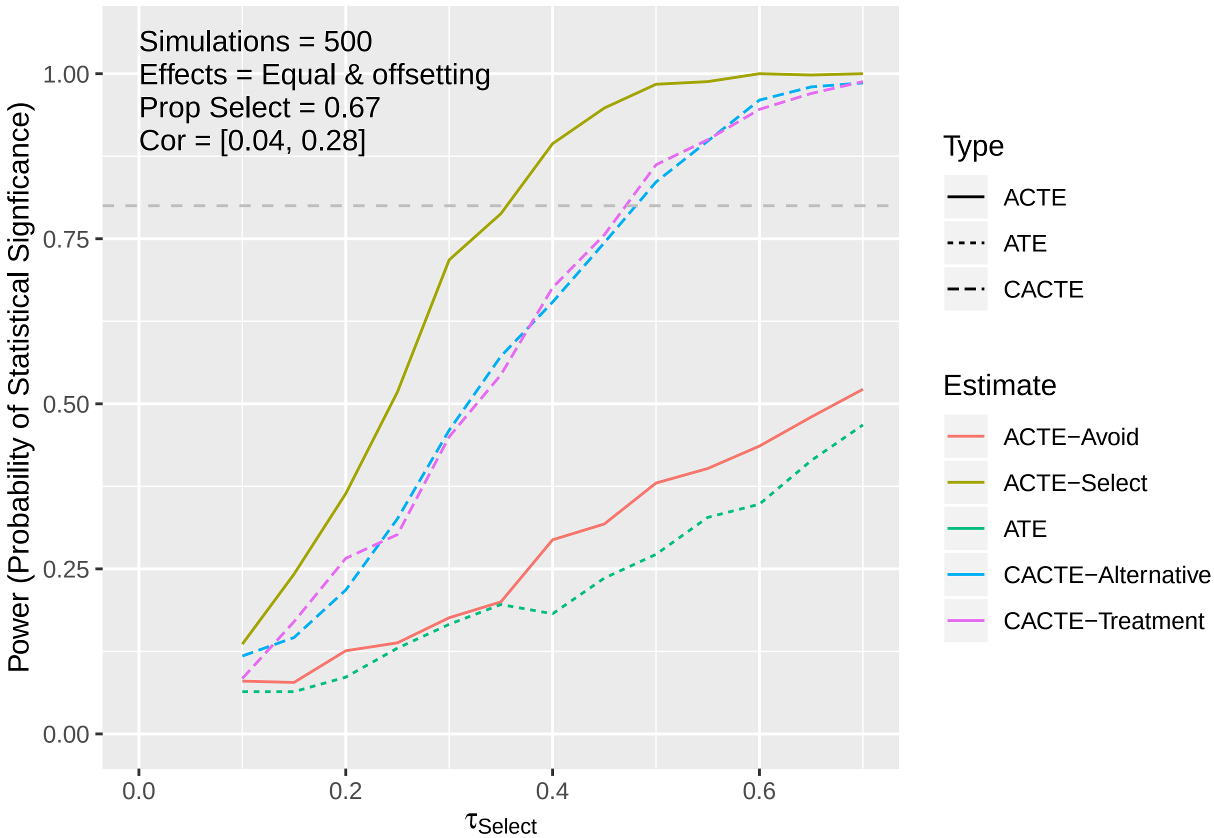


Figure 5

More Selectors than Avoiders, Equal and Offsetting Effects

Assuming about two-thirds of subjects will select the treatment, the ACTE among selectors can be expected to detect an effect of about 0.35 with about 80 percent power. The conditional ACTE among avoiders can be expected to detect an effect of about 0.45 with about 80 percent power, while the maximum power for ACTE among avoiders for an effect of 0.7 is 50 percent.

- N = 1000
- Y0 = Normal (mean=0, sd=1)
- Taus: 0.1 to 0.7 by 0.05
- $\alpha = 0.66$

More Selectors than Avoiders, Equal and Offsetting Effects, Increasing Correlation between Selection and Outcomes

Increasing the positive correlation between the selection and outcome increases the power the ACTE among selectors and decreases the the power of the ACTE among avoiders, while the power of the conditional ACTEs among avoiders are largely unchanged.

Table 2: Power Analysis

	Hypothesized Effect Among Selectors												
	0.1	0.15	0.2	0.25	0.3	0.35	0.4	0.45	0.5	0.55	0.6	0.65	0.7
ATE	0.06	0.06	0.09	0.13	0.17	0.20	0.18	0.24	0.27	0.33	0.35	0.41	0.47
ACTE-Select	0.14	0.24	0.36	0.52	0.72	0.79	0.89	0.95	0.98	0.99	1.00	1.00	1.00
ACTE-Avoid	0.08	0.08	0.13	0.14	0.18	0.20	0.29	0.32	0.38	0.40	0.44	0.48	0.52
CACTE-Treatment	0.08	0.17	0.27	0.30	0.45	0.54	0.68	0.76	0.86	0.90	0.95	0.97	0.99
CACTE-Alternative	0.12	0.15	0.22	0.33	0.46	0.57	0.65	0.74	0.84	0.90	0.96	0.98	0.99

- N = 1000
- Y0 = Normal (mean=0, sd=1)
- Taus: 0.1 to 0.7 by 0.1
- $\alpha = 0.66$
- Selection effect = .5

Table 3: Power Analysis

	Hypothesized Effect Among Selectors												
	0.1	0.15	0.2	0.25	0.3	0.35	0.4	0.45	0.5	0.55	0.6	0.65	0.7
ATE	0.07	0.08	0.10	0.12	0.14	0.15	0.18	0.18	0.23	0.24	0.34	0.33	0.41
ACTE-Select	0.11	0.21	0.38	0.50	0.59	0.77	0.87	0.92	0.96	0.98	1.00	1.00	1.00
ACTE-Avoid	0.03	0.08	0.11	0.14	0.17	0.20	0.22	0.28	0.33	0.36	0.39	0.42	0.43
CACTE-Treatment	0.09	0.17	0.24	0.34	0.42	0.54	0.65	0.79	0.86	0.93	0.96	0.98	0.99
CACTE-Alternative	0.12	0.16	0.20	0.31	0.43	0.59	0.63	0.78	0.84	0.90	0.96	0.97	0.99

Appendix

Code for Power Simulations

```
#### Overview #####

### Setup ###

# Load some defaults (Not necessary)
if (!require("pacman")){ install.packages("pacman") }
pacman::p_load("knitr","tidyverse","data.table","grid","gridExtra",
               "reshape2","haven","readxl","janitor","scales",
               "foreign","devtools","Hmisc",
               "texreg","stargazer","xtable","GGally","kableExtra",
```

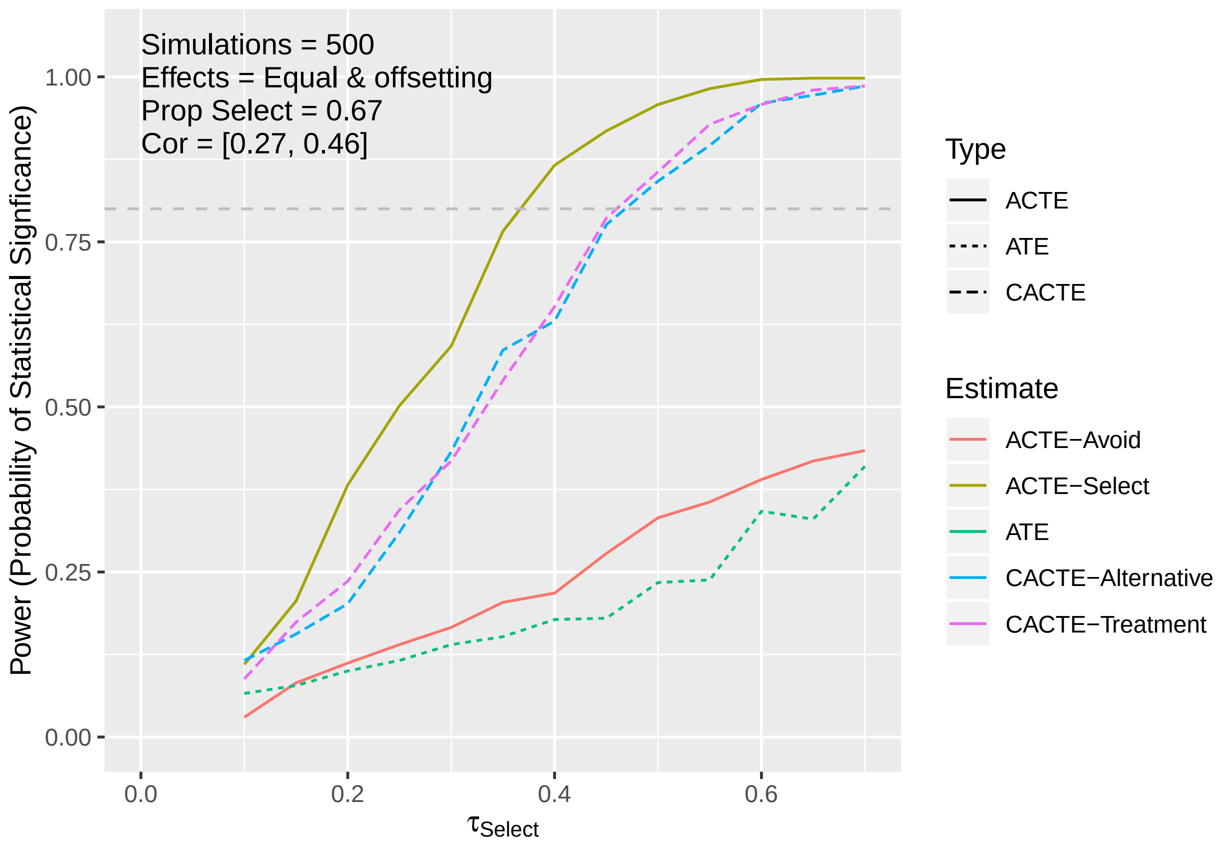

Simulations = 500
Effects = Equal & offsetting
Prop Select = 0.67
Cor = [0.27, 0.46]
Power (Probability of Statistical Signficance)
1.00
0.75
0.50
0.25
0.00
0.0
0.2
0.4
0.6
τSelect
Type
ACTE
ATE
CACTE
Estimate
ACTE−Avoid
ACTE−Select
ATE
CACTE−Alternative
CACTE−Treatment

Figure 6

```r
               "broom","AER","car","arm","MASS",
               "lmtest")

options(knitr.table.format = "latex")

# Set working directory
wd <- "."

setwd(wd)

### Helper labels
c_ate <- c("Control","Treatment")
c_acte_s <- c("Control","Selection")
c_acte_a <- c("Selection","Treatment")

ates <- list(c_ate)
actes <- list(c_acte_s,c_acte_a)


#### Simulate Data function #####

data_fn <- function(N=1000,
                    sigma = 2,
                    p_treat=.5,
                    prop_select=.5,
                    p_treat_select = c(.25,.5,.25),
                    tau_st = .5,
                    tau_at = -.5,
                    tau_aa = .5,
                    select_effect = 0,
                    ...
){
  select <- rbinom(n=N, size=1,prob = prop_select)
  avoid <- as.numeric(select!=1)

  # U1 <- rnorm(n=N, mean = 0, sd = 1) +  2*select
```

```r
# U2 <- rnorm(n=N, mean = 0, sd = 1) + -2*select
# U3 <- rnorm(n=N, mean = 0, sd = 1) + .5*select

# Baseline
Y0 <- rnorm(n=N, mean = 0, sd = sigma) + select_effect * select

# Condition
C <- sample(c("Choice","Experiment"),size=N,
            prob = c(prop_select, 1- prop_select),replace = T)
n_exp <- sum(C=="Experiment")
n_ch_a <- sum(C=="Choice" & avoid==1)

# Treatment status in experimental arm
D_exp <- rep("Selection",N)
D_exp[C=="Experiment"] <- sample(c("Treatment","Control"),size=n_exp,
                                 prob = c(p_treat,1-p_treat), replace = T)


D_ch <- rep("Experiment",N)
D_ch[C=="Choice" & avoid==1] <- sample(c("Treatment","Control", "Alternative"),size=n_ch_a,
                                       prob = p_treat_select,replace = T)
n_choice <- sum(C == "Choice")
n_select <- sum(C=="Choice" & avoid==0)
n_avoid <- sum(C=="Choice" & avoid==1)
n_control <- sum(D_ch == "Control")
weights <- rep(1,N)
weights[C=="Choice" & avoid==0 & D_ch == "Control"] <-1/(n_select/n_choice)
weights[C=="Choice" & avoid==1 & D_ch == "Control"] <-1/(n_control/n_avoid)

# Potential outcome is conditional on preferences
Y1 <- Y0 + tau_st*(select == 1 & D_exp == "Treatment") + tau_at*(avoid == 1 & D_exp == "Treatment" ) +
  tau_st*(select == 1 & C == "Choice")+
  tau_at*(avoid == 1 & D_ch == "Treatment" ) + tau_aa*(avoid == 1 & D_ch == "Alternative" )




# Observed Outcome
```

```
  Y <- rep(NA,N)
  Y[C=="Experiment"] <- Y0[C=="Experiment"]*(D_exp[C=="Experiment"]=="Control") +
    Y1[C=="Experiment"]*(D_exp[C=="Experiment"]=="Treatment")
  Y[C=="Choice" & select == 1] <- Y1[C=="Choice" & select == 1]
  Y[C=="Choice" & select == 0 & D_ch == "Control"] <- Y0[C=="Choice" & select == 0 & D_ch == "Control"]
  Y[C=="Choice" & select == 0 & D_ch == "Treatment"] <- Y1[C=="Choice" & select == 0 & D_ch == "Treatme
  Y[C=="Choice" & select == 0 & D_ch == "Alternative"] <- Y1[C=="Choice" & select == 0 & D_ch == "Alter


  treatment = rep(NA, N)
  treatment[C=="Experiment" & D_exp == "Treatment"] <- "Treatment"
  treatment[C=="Experiment" & D_exp == "Control"] <- "Control"
  treatment[C=="Choice"] <- "Selection"
  treatment[C=="Choice" & D_ch == "Treatment" ] <- NA
  treatment[C=="Choice" & D_ch == "Alternative" ] <- NA

  avoid01 <- NA
  avoid01[C == "Choice" & avoid == 1] <- 1
  avoid01[C == "Choice" & avoid == 0] <- 0

  select01 <- NA
  select01[C == "Choice" & avoid == 0] <- 1
  select01[C == "Choice" & avoid == 1] <- 0

  df <- data.frame(Y0,Y1,Y,true_diff = Y1-Y0,C,treatment, select, avoid,select01,avoid01, D_exp, D_ch, w
  return(df)
}

#### ATE Function ####


ate_fn <- function(the_data, dv1="Y",c,weights=F,...){
  #frm<-paste(dv1,"~  age+age_sq+is_female+is_nonwhite+is_democrat+is_moderate+is_liberal+open+consc+ex
  #df[,dv1] <- resid(lm(frm,df,na.action="na.exclude"))
```

```
  df <- the_data
  # Weights
  tmp <- as.data.frame(df[df$treatment%in%c, ])
  if(weights==F){
    mu1 <- with(tmp, mean(tmp[treatment==c[1], dv1],na.rm=T))
    mu2 <- with(tmp, mean(tmp[treatment==c[2], dv1],na.rm=T))
  }
  if(weights==T){
    mu1 <- with(tmp, Hmisc::wtd.mean(tmp[treatment==c[1], dv1],na.rm=T,weights=tmp[treatment==c[1],"wei
    mu2 <- with(tmp, Hmisc::wtd.mean(tmp[treatment==c[2], dv1],na.rm=T,weights=tmp[treatment==c[2],"wei
  }
  diff <- mu2-mu1
  if(weights==F){
    sd1 <- with(tmp, sd(tmp[treatment==c[1], dv1],na.rm=T))
    sd2 <- with(tmp, sd(tmp[treatment==c[2], dv1],na.rm=T))
  }
  if(weights==T){
    sd1 <- sqrt(with(tmp, Hmisc::wtd.var(tmp[treatment==c[1], dv1],na.rm=T,weights = tmp[treatment==c[1
    sd2 <- sqrt(with(tmp, Hmisc::wtd.var(tmp[treatment==c[2], dv1],na.rm=T,weights = tmp[treatment==c[2
  }
  n1 <- with(tmp, sum(!is.na(tmp[treatment==c[1], dv1])*tmp[treatment==c[1],"weights"]))
  n2 <- with(tmp, sum(!is.na(tmp[treatment==c[2], dv1])*tmp[treatment==c[2],"weights"]))
  se <- sqrt( sd1^2/n1 + sd2^2/n2)
  the_df <- (sd1^2/n1+sd2^2/n2)^2/((sd1^4)/(n1^2*(n1-1))+ (sd2^4)/(n2^2*(n2-1)))
  ll <- diff - qt(.975,the_df)*se
  ul <- diff + qt(.975,the_df)*se
  stat <- diff/se
  pval = 2 * pt(-abs(stat),the_df)
  result <- c(Estimate = diff, SE = se, ll = ll, ul = ul, pval = pval)
  return(result)
}

#### ACTE function ####

acte_fn <- function(dat,dv2="Y",z,w,...){
  df <- dat
```

```
  tmp <- ate_fn(the_data=dat, dv1=dv2,c=z, weights=w)
  x <- as.numeric(tmp["Estimate"])
  se_x <- as.numeric(tmp["SE"])
  if(z[1]=="Control"){
    y <- summary(lm(select01~1,df[df$C=="Choice",]))$coef[1,1]
    se_y <- summary(lm(select01~1,df[df$C=="Choice",]))$coef[1,2]}
  if(z[2]=="Treatment"){
    y <- summary(lm(avoid01~1,df[df$C=="Choice",]))$coef[1,1]
    se_y <- summary(lm(avoid01~1,df[df$C=="Choice",]))$coef[1,2]}
  mvec <- c(x=x, y= y)
  V <- diag(c(se_x,se_y)^2)
  est <- car::deltaMethod(mvec,"x/y",V,level=.95)
  #print(y)
  stat <- as.numeric(est[1])/as.numeric(est[2])
  result <- c(Estimate = as.numeric(est[1]), SE = as.numeric(est[2]), ll = as.numeric(est[3]), ul = as.
              pval = 2 * pnorm(-abs(stat)))
  return(result)
}



#### Sequential ACTE function ####


s_acte_fn <- function(d= data_fn(), dv3="Y", z, w2=F){
  df <- d
  tmp <- as.data.frame(df[df$avoid01 == 1, ])
  tmp$treatment <- tmp$D_ch
  # Treatment effect among avoiders
  at_ate <- ate_fn(tmp, c=c("Control","Treatment"),weights=w2)

  # Alternative treatment effect
  aa_ate <- ate_fn(tmp, c=c("Control","Alternative"),weights=w2)
  res <- rbind(at_ate,aa_ate)
  return(res)



}
```

```
#### Treatment Effects function ####

te_fn <- function(the_dv,...){
  # if(grepl("exp",the_dv)){
  #   ates <- ates_s
  #   actes <- actes_s
  # }else{
  #   ates <- ates_g
  #   actes <- actes_g
  # }

  the_ates <- as.data.frame(do.call(rbind,lapply(ates,function(x)ate_fn(dv1=the_dv,c=x,...))))
  the_actes <- as.data.frame(do.call(rbind,lapply(actes,function(x)acte_fn(dv2=the_dv,z=x,...))))
  results<- rbind(the_ates,the_actes)
  rownames(results) <- c("Treatment - Control","Select Treatment","Avoid Treatment")
  return(results)


}

#### Plot Treatment Effects ####

plot_fn <- function(to_plot="dv_fair_black",caption=NULL){
  tmp <- te_fn(the_dv = to_plot)
  tmp$Effect <- paste(c("ATE","ACTE","ACTE"),rownames(tmp))
  tmp$Effect <- factor(tmp$Effect,levels=tmp$Effect)
  tmp$Treatment <- factor(c("ATE","ACTE","ACTE"),levels=c("ATE","ACTE"))
  p <- ggplot(tmp, aes(Effect, Estimate,col=Treatment))+
    geom_point()+
    geom_errorbar(aes(ymin=ll,ymax=ul),width=.2)+
    geom_hline(yintercept = 0,linetype="dashed")+
    theme(axis.text.x = element_text(
      angle = 90, hjust = 1)
    )+
    labs(title=caption)
```

```
  return(p)

}

#### Power Function #####

power_fn <- function(sims,
                     p_N=1000,
                     p_sigma = 1,
                     p_p_treat=.5,
                     p_prop_select=.5,
                     p_p_treat_select = c(.25,.5,.25),
                     p_tau_st = .5,
                     p_tau_at = -.5,
                     p_tau_aa = .5,
                     p_select_effect = 0,

                     ...){

  ate <- rep(NA,sims)
  acte_s <- rep(NA,sims)
  acte_a <- rep(NA,sims)
  acte_at <- rep(NA,sims)
  acte_aa <- rep(NA,sims)

  sig_ate <- rep(NA,sims)
  sig_acte_s <- rep(NA,sims)
  sig_acte_a <- rep(NA,sims)
  sig_acte_at <- rep(NA,sims)
  sig_acte_aa <- rep(NA,sims)
  cor_select <- rep(NA,sims)


  for(i in 1:sims){
    df <- data_fn(N=p_N,sigma= p_sigma ,
                  p_treat=p_p_treat,
```

```
                  prop_select = p_prop_select,
                  p_treat_select = p_p_treat_select ,
                  tau_st= p_tau_st ,
                  tau_at= p_tau_at ,
                  tau_aa= p_tau_aa ,
                  select_effect= p_select_effect )
  sig_ate[i] <- ate_fn(df,dv1="Y",c=c("Control","Treatment"),weights = T)["pval"]
  sig_acte_s[i] <- acte_fn(df,z=c_acte_s, w=T)["pval"]
  sig_acte_a[i] <- acte_fn(df,z=c_acte_a, w=T)["pval"]
  sig_acte_at[i] <- s_acte_fn(df)[1,"pval"]
  sig_acte_aa[i] <- s_acte_fn(df)[2,"pval"]
  cor_select[i] <- cor(df$Y,df$select)

  ate[i] <- ate_fn(df,dv1="Y",c=c("Control","Treatment"))["Estimate"]
  acte_s[i] <- acte_fn(df,z=c_acte_s, w=T)["Estimate"]
  acte_a[i] <- acte_fn(df,z=c_acte_a,w = T)["Estimate"]
  acte_at[i] <- s_acte_fn(df)[1,"Estimate"]
  acte_aa[i] <- s_acte_fn(df)[2,"Estimate"]
}

pow_ate <- mean(sig_ate<.05)
pow_acte_s <- mean(sig_acte_s<.05)
pow_acte_a <- mean(sig_acte_a<.05)
pow_acte_at <-  mean(sig_acte_at<.05)
pow_acte_aa <- mean(sig_acte_aa<.05)
mn_cor_select <- mean(cor_select)

mn_ate <- mean(ate)
mn_acte_s <- mean(acte_s)
mn_acte_a <- mean(acte_a)
mn_acte_at <-  mean(acte_at)
mn_acte_aa <- mean(acte_aa)



res <- rbind(
  c(mn_ate,
```

```
      mn_acte_s,
      mn_acte_a,
      mn_acte_at,
      mn_acte_aa,mn_cor_select),

    c(pow_ate,
      pow_acte_s,
      pow_acte_a,
      pow_acte_at,
      pow_acte_aa,
      NA
    ))

  return(res)

}

#### Simulate Power ####



sim_power_fn <- function(
                         s_sims = 1000,
                         s_N=1000,
                         s_sigma = 1,
                         s_p_treat=.5,
                         s_prop_select=.5,
                         s_p_treat_select = c(.25,.5,.25),
                         s_tau_st = .5,
                         s_tau_at = -.5,
                         s_tau_aa = .5,
                         s_select_effect = 0




) {
  # Create matrix to store values
```

```r
power_mat <- matrix(NA,nrow=5, ncol = length(s_tau_st),
                    dimnames = list(c("ATE","ACTE-Select","ACTE-Avoid","CACTE-Treatment","CACTE-Alte
                                    s_tau_st
                    )
                   )
tmp <- c()
ave_cor <- c()
tmp_df <- data.frame(Estimate=NULL,Tau=NULL)
df <- data.frame(Estimate= NULL,
                 Tau_Select = NULL,
                 Tau_Avoid = NULL ,
                 Tau_Alt = NULL ,
                 Power = NULL )
# Loop over possible values
for(i in 1:length(s_tau_st)){
  tmp <- power_fn(sims = s_sims,
                           p_N = s_N,
                           p_sigma  = s_sigma,
                           p_p_treat = s_p_treat,
                           p_prop_select = s_prop_select,
                           p_p_treat_select  = s_p_treat_select,
                           p_tau_st  = s_tau_st[i],
                           p_tau_at  = s_tau_at[i],
                           p_tau_aa  = s_tau_aa[i],
                           p_select_effect  = s_select_effect
  )
  ave_cor[i] <- tmp[1,6]
  power_mat[,i] <- tmp[2,1:5]
  tmp2 <- data.frame(Estimate=c("ATE","ACTE-Select","ACTE-Avoid","CACTE-Treatment","CACTE-Alternative
                    Type = c("ATE","ACTE","ACTE","CACTE","CACTE"),
                 Tau_Select = rep(s_tau_st[i],5),
                 Tau_Avoid = rep(s_tau_at[i],5),
                 Tau_Alt = rep(s_tau_aa[i],5),
                 Power = tmp[2,1:5]
                 )
  df <- rbind(tmp2,df)
```

```
  }
  return(list(df,power_mat,ave_cor))

}



#### Plot Power Simulations ####



plot_power_sim_fn <- function(
  p_s_sims = 1000,
  p_s_N=1000,
  p_s_sigma = 1,
  p_s_p_treat=.4,
  p_s_prop_select=.5,
  p_s_p_treat_select = c(.25,.5,.25),
  p_s_tau_st = .5,
  p_s_tau_at = -.5,
  p_s_tau_aa = .5,
  p_s_select_effect = 0,
  lab_effects = "Effects = Equal & offsetting"

){
  pow <- sim_power_fn(
    s_sims = p_s_sims,
    s_N= p_s_N,
    s_sigma = p_s_sigma,
    s_p_treat= p_s_p_treat,
    s_prop_select= p_s_prop_select,
    s_p_treat_select = p_s_p_treat_select,
    s_tau_st = p_s_tau_st,
    s_tau_at = p_s_tau_at,
    s_tau_aa = p_s_tau_aa,
    s_select_effect = p_s_select_effect
```

```
)
rhos.min <- round(range(pow[[3]]),2) [1]
rhos.max <- round(range(pow[[3]]),2) [2]

p <- pow[[1]] %>%
  ggplot(aes(Tau_Select,Power, col=Estimate,linetype=Type))+
  geom_line()+
  ylim(0,1.05) +
  xlim(0,.7) +
  geom_hline(yintercept = .8,linetype = "dashed",col="grey")+
  xlab(expression(tau[Select]))+
  ylab("Power (Probability of Statistical Signficance)")+
  annotate(geom = "text",
           hjust = 0,
           y = 1.05,
           x = 0,
           label = paste("Simulations =",scales::comma(p_s_sims))
  )+
  annotate(geom = "text",
           hjust = 0,
           y = 1,
           x = 0,
           label = lab_effects
  )+
  annotate(geom = "text",
           hjust = 0,
           y = .95,
           x = 0,
           label =paste("Prop Select =", round(p_s_prop_select,2))
  ) +
  annotate(geom = "text",
           hjust = 0,
           y = .90,
           x = 0,
           label = paste("Cor = [",rhos.min,", ",rhos.max,"]",sep="")
  )
```

```
  tab <- kable(pow[[2]],
               caption = "Power Analysis",
               format = "latex",
               booktabs=T,
               linesep = "",
               digits=2) %>%
    add_header_above(c("","Hypothesized Effect Among Selectors"=dim(pow[[2]])[2])) %>%
    kable_styling(latex_options = c("hold_position",font_size=10))

  return(list(p,tab))


}

### Power Simulations Above (Not Run) ###

# pow1 <- plot_power_sim_fn(p_s_sims = 500,
#                   p_s_tau_st = seq(.1,.7,by=.05),
#                   p_s_tau_at = seq(-.1,-.7,by=-.05),
#                   p_s_tau_aa = seq(.1,.7,by=.05)
#
#                   )
#
# pow2 <- plot_power_sim_fn(p_s_sims = 500,
#                           p_s_prop_select = 2/3,
#                   p_s_tau_st = seq(.1,.7,by=.05),
#                   p_s_tau_at = seq(-.1,-.7,by=-.05),
#                   p_s_tau_aa = seq(.1,.7,by=.05)
#
#                   )
#
# pow3 <- plot_power_sim_fn(p_s_sims = 500,
#                           p_s_prop_select = 2/3,
#                           p_s_select_effect = 0.5,
#                   p_s_tau_st = seq(.1,.7,by=.05),
#                   p_s_tau_at = seq(-.1,-.7,by=-.05),
```

```
#                   p_s_tau_aa = seq(.1,.7,by=.05)
#
#                   )
```

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