Gov 50: 7. Randomized Experiments

Matthew Blackwell

Harvard University

- 1. Randomized experiments
- 2. Calculating effects



POLITICAL SCIENCE

Durably reducing transphobia: A field experiment on door-to-door canvassing

David Broockman¹⁸ and Joshua Kalla²

Latistic research depicts intergroup projudices as deeply ingrained, requiring interse intervention to lastingly reduce. Here, we show that a angle opposing the USE of the conversation encouraging activity lishing the perspective of others can marked by encouraging activity lishing the perspective of the second by a conversation of the encouraging activity angle of the declines in homophobia, transphobia remains persains. For the intervention, 56 conversation of the encouraging activity angle on the declines in homophobia remains persains. For the intervention, 56 conversation of the other encouraging activity encourage in the enclines of transphobia, with decreases greater than Americanni average decreases in homophobia from 1998 to 2021. These decises persisted in a month, and boht transgender and nontransgrander convases were effective. The intervention volters to contergraments.

· Can canvassers change minds about topics like transgender rights?



POLITICAL SCIENCE

Durably reducing transphobia: A field experiment on door-to-door canvassing

David Broockman¹⁺ and Joshua Kalla²

Latistic research depicts intergroup projudices as deeply ingrained, requiring interse intervention to lastingly reduce. Here, we show that a angle opposing the USE of the conversation encouraging activity lishing the perspective of others can marked by encouraging activity lishing the perspective of the second by a conversation of the encouraging activity angle of the declines in homophobia, transphobia remains persains. For the intervention, 56 conversation of the encouraging activity angle on the declines in homophobia remains persains. For the intervention, 56 conversation of the other encouraging activity encourage in the enclines of transphobia, with decreases greater than Americanni average decreases in homophobia from 1998 to 2021. These decises persisted in a month, and boht transgender and nontransgrander convases were effective. The intervention volters to contergraments.

- Can canvassers change minds about topics like transgender rights?
- Experimental setting:



POLITICAL SCIENCE

Durably reducing transphobia: A field experiment on door-to-door canvassing

David Broockman¹⁺ and Joshua Kalla²

Latistic research depicts intergroup projudices as deeply ingrained, requiring interse intervention to lastingly reduce. Here, we show that a angle opposing the USE of the conversation encouraging activity lishing the perspective of others can marked by encouraging activity lishing the perspective of the second by a conversation of the encouraging activity angle of the declines in homophobia, transphobia remains persains. For the intervention, 56 conversation of the encouraging activity angle on the declines in homophobia remains persains. For the intervention, 56 conversation of the other encouraging activity encourage in the enclines of transphobia, with decreases greater than Americanni average decreases in homophobia from 1998 to 2021. These decises persisted in a month, and boht transgender and nontransgrander convases were effective. The intervention volters to contergraments.

- · Can canvassers change minds about topics like transgender rights?
- Experimental setting:
 - Randomly assign canvassers to have a conversation about transgender right or a conversation about recycling.



POLITICAL SCIENCE

Durably reducing transphobia: A field experiment on door-to-door canvassing

David Broockman¹⁺ and Joshua Kalla²

Latistic research depicts intergroup projudices as deeply ingrained, requiring interse intervention to lastingly reduce. Here, we show that a angle opposing the USE of the conversation encouraging activity lishing the perspective of others can marked by encouraging activity lishing the perspective of the second by a conversation of the encouraging activity angle of the declines in homophobia, transphobia remains persains. For the intervention, 56 conversation of the encouraging activity angle on the declines in homophobia remains persains. For the intervention, 56 conversation of the other encouraging activity encourage in the enclines of transphobia, with decreases greater than Americanni average decreases in homophobia from 1998 to 2021. These decises persisted in a month, and boht transgender and nontransgrander convases were effective. The intervention volters to contergraments.

- · Can canvassers change minds about topics like transgender rights?
- Experimental setting:
 - Randomly assign canvassers to have a conversation about transgender right or a conversation about recycling.
 - · Trans rights conversations focused on "perspective taking"



POLITICAL SCIENCE

Durably reducing transphobia: A field experiment on door-to-door canvassing

David Broockman1* and Joshua Kalla²

Latisting research depicts intergroup projudices as deeply ingrained, requiring interess intervention to lastingly reduce. Here, we show that a single approximately U-minute conversation encouraging activity listing the perspective of others can enabled provide the single state of the single single single single single single single declines in homophobia, transphobia remains persains. For the intervention, 56 conversation of theorem of the other perspective. Based with 501 writers enclosed and the single single single single single single single single declines in homophobia, transphobia remains persains. For the intervention, 56 conversation of theorem of the single single single single single single reduced transphobia, with decreases greater than Americanni average decreases in homophobia from 1996 a local. These decises perside single average both transgender and nontransgeneric converses where the conversion of the single single single single single single single single where the output single single. These single single where the conversion of the single single single where the conversion of the single single single where the conversion single where the conversion single where the conversion single s

- Can canvassers change minds about topics like transgender rights?
- Experimental setting:
 - Randomly assign canvassers to have a conversation about transgender right or a conversation about recycling.
 - · Trans rights conversations focused on "perspective taking"
- Outcome of interest: support for trans rights policies.

• What does " T_i causes Y_i " mean? \rightsquigarrow counterfactuals, "what if"

- What does " T_i causes Y_i " mean? \rightsquigarrow counterfactuals, "what if"
- Would respondent change their support based on the conversation?

- What does " T_i causes Y_i " mean? \rightsquigarrow counterfactuals, "what if"
- Would respondent change their support based on the conversation?
- Two potential outcomes:

- What does " T_i causes Y_i " mean? \rightsquigarrow counterfactuals, "what if"
- Would respondent change their support based on the conversation?
- Two potential outcomes:
 - *Y_i*(1): would respondent *i* support ND laws if they had trans rights script?

- What does " T_i causes Y_i " mean? \rightsquigarrow counterfactuals, "what if"
- Would respondent change their support based on the conversation?
- Two potential outcomes:
 - *Y_i*(1): would respondent *i* support ND laws if they had trans rights script?
 - $Y_i(0)$: would respondent *i* support ND laws if they had recycling script?

- What does " T_i causes Y_i " mean? \rightsquigarrow counterfactuals, "what if"
- Would respondent change their support based on the conversation?
- Two potential outcomes:
 - *Y_i*(1): would respondent *i* support ND laws if they had trans rights script?
 - $Y_i(0)$: would respondent *i* support ND laws if they had recycling script?
- Causal effect: $Y_i(1) Y_i(0)$

- What does "*T_i* causes *Y_i*" mean? → **counterfactuals**, "what if"
- Would respondent change their support based on the conversation?
- Two potential outcomes:
 - *Y_i*(1): would respondent *i* support ND laws if they had trans rights script?
 - $Y_i(0)$: would respondent *i* support ND laws if they had recycling script?
- Causal effect: $Y_i(1) Y_i(0)$
 - $Y_i(1) Y_i(0) = 0 \rightsquigarrow$ script has no effect on policy views

- What does "*T_i* causes *Y_i*" mean? → **counterfactuals**, "what if"
- Would respondent change their support based on the conversation?
- Two potential outcomes:
 - *Y_i*(1): would respondent *i* support ND laws if they had trans rights script?
 - $Y_i(0)$: would respondent *i* support ND laws if they had recycling script?
- Causal effect: $Y_i(1) Y_i(0)$
 - $Y_i(1) Y_i(0) = 0 \rightsquigarrow$ script has no effect on policy views
 - $Y_i(1) Y_i(0) = -1 \rightsquigarrow$ trans rights script lower support for laws

- What does " T_i causes Y_i " mean? \rightsquigarrow counterfactuals, "what if"
- Would respondent change their support based on the conversation?
- Two potential outcomes:
 - *Y_i*(1): would respondent *i* support ND laws if they had trans rights script?
 - $Y_i(0)$: would respondent *i* support ND laws if they had recycling script?
- Causal effect: $Y_i(1) Y_i(0)$
 - $Y_i(1) Y_i(0) = 0 \rightsquigarrow$ script has no effect on policy views
 - $Y_i(1) Y_i(0) = -1 \rightsquigarrow$ trans rights script lower support for laws
 - $Y_i(1) Y_i(0) = +1 \rightsquigarrow$ trans rights script increases support for laws

Potential outcomes

i	T_i	Y_i	$Y_i(1)$	$Y_i(0)$
Respondent 1	0	0	???	0
Respondent 2	1	1	1	???

• Fundamental problem of causal inference:

Potential outcomes

i	T_i	Y_i	$Y_i(1)$	$Y_i(0)$
Respondent 1	0	0	???	0
Respondent 2	1	1	1	???

- Fundamental problem of causal inference:
 - We only observe one of the two potential outcomes.

i	T_i	Y _i	$Y_i(1)$	$Y_{i}(0)$
Respondent 1	0	0	???	0
Respondent 2	1	1	1	???

• Fundamental problem of causal inference:

- We only observe one of the two potential outcomes.
- Observe $Y_i = Y_i(1)$ if $T_i = 1$ or $Y_i = Y_i(0)$ if $T_i = 0$

i	T_i	Y_i	$Y_i(1)$	$Y_i(0)$
Respondent 1	0	0	???	0
Respondent 2	1	1	1	???

• Fundamental problem of causal inference:

- We only observe one of the two potential outcomes.
- Observe $Y_i = Y_i(1)$ if $T_i = 1$ or $Y_i = Y_i(0)$ if $T_i = 0$
- To infer causal effect, we need to infer the missing counterfactuals!

1/ Randomized experiments



• **Randomized control trial**: each unit's treatment assignment is determined by chance.



- **Randomized control trial**: each unit's treatment assignment is determined by chance.
 - Flip a coin; draw red and blue chips from a hat; etc



- **Randomized control trial**: each unit's treatment assignment is determined by chance.
 - Flip a coin; draw red and blue chips from a hat; etc
- Randomization ensures balance between treatment and control group.



- **Randomized control trial**: each unit's treatment assignment is determined by chance.
 - Flip a coin; draw red and blue chips from a hat; etc
- Randomization ensures **balance** between treatment and control group.
 - Treatment and control group are identical **on average**



- **Randomized control trial**: each unit's treatment assignment is determined by chance.
 - Flip a coin; draw red and blue chips from a hat; etc
- Randomization ensures **balance** between treatment and control group.
 - Treatment and control group are identical **on average**
 - Similar on both observable and unobservable characteristics.

• We will often refer to the **sample size** (number of units) as *n*.

- We will often refer to the **sample size** (number of units) as *n*.
- We often have *n* measurements of some variable: $(Y_1, Y_2, ..., Y_n)$

- We will often refer to the **sample size** (number of units) as *n*.
- We often have *n* measurements of some variable: $(Y_1, Y_2, ..., Y_n)$
- How many in our sample support nondiscrimination laws?

$$Y_1 + Y_2 + Y_3 + \dots + Y_n$$

- We will often refer to the **sample size** (number of units) as *n*.
- We often have *n* measurements of some variable: $(Y_1, Y_2, ..., Y_n)$
- How many in our sample support nondiscrimination laws?

$$Y_1 + Y_2 + Y_3 + \dots + Y_n$$

• Notation is a bit clunky, so we often use the Sigma notation:

$$\sum_{i=1}^{n} Y_{i} = Y_{1} + Y_{2} + Y_{3} + \dots + Y_{n}$$

- We will often refer to the **sample size** (number of units) as *n*.
- We often have *n* measurements of some variable: $(Y_1, Y_2, ..., Y_n)$
- How many in our sample support nondiscrimination laws?

$$Y_1 + Y_2 + Y_3 + \dots + Y_n$$

• Notation is a bit clunky, so we often use the Sigma notation:

$$\sum_{i=1}^{n} Y_{i} = Y_{1} + Y_{2} + Y_{3} + \dots + Y_{n}$$

• $\Sigma_{i=1}^{n}$ means sum each value from Y_1 to Y_n



• The **sample average** or **sample mean** is simply the sum of all values divided by the number of values.



- The **sample average** or **sample mean** is simply the sum of all values divided by the number of values.
- Sigma notation allows us to write this in a compact way:

$$\overline{Y} = \frac{1}{n} \sum_{i=1}^{n} Y_i$$



- The **sample average** or **sample mean** is simply the sum of all values divided by the number of values.
- Sigma notation allows us to write this in a compact way:

$$\overline{Y} = \frac{1}{n} \sum_{i=1}^{n} Y_i$$

• Suppose we surveyed 6 people and 3 supported nondiscrim. laws:

$$\overline{Y} = \frac{1}{6} \left(1 + 1 + 1 + 0 + 0 + 0 \right) = 0.5$$

Quantity of interest

• We want to estimate the average causal effects over all units:

Sample Average Treatment Effect (SATE) =
$$\frac{1}{n} \sum_{i=1}^{n} \{Y_i(1) - Y_i(0)\}$$

= $\frac{1}{n} \sum_{i=1}^{n} Y_i(1) - \frac{1}{n} \sum_{i=1}^{n} Y_i(0)$

Quantity of interest

• We want to estimate the average causal effects over all units:

Sample Average Treatment Effect (SATE) =
$$\frac{1}{n} \sum_{i=1}^{n} \{Y_i(1) - Y_i(0)\}$$

= $\frac{1}{n} \sum_{i=1}^{n} Y_i(1) - \frac{1}{n} \sum_{i=1}^{n} Y_i(0)$

• Why can't we just calculate this quantity directly?

• We want to estimate the average causal effects over all units:

Sample Average Treatment Effect (SATE) =
$$\frac{1}{n} \sum_{i=1}^{n} \{Y_i(1) - Y_i(0)\}$$

= $\frac{1}{n} \sum_{i=1}^{n} Y_i(1) - \frac{1}{n} \sum_{i=1}^{n} Y_i(0)$

- Why can't we just calculate this quantity directly?
- What we can estimate instead:

Difference in means =
$$\overline{Y}_{\text{treated}} - \overline{Y}_{\text{control}}$$

• We want to estimate the average causal effects over all units:

Sample Average Treatment Effect (SATE) =
$$\frac{1}{n} \sum_{i=1}^{n} \{Y_i(1) - Y_i(0)\}$$

= $\frac{1}{n} \sum_{i=1}^{n} Y_i(1) - \frac{1}{n} \sum_{i=1}^{n} Y_i(0)$

- Why can't we just calculate this quantity directly?
- What we can estimate instead:

Difference in means =
$$\overline{Y}_{\text{treated}} - \overline{Y}_{\text{control}}$$

• $\overline{Y}_{\text{treated}}$: sample average outcome for treated group

• We want to estimate the average causal effects over all units:

Sample Average Treatment Effect (SATE) =
$$\frac{1}{n} \sum_{i=1}^{n} \{Y_i(1) - Y_i(0)\}$$

= $\frac{1}{n} \sum_{i=1}^{n} Y_i(1) - \frac{1}{n} \sum_{i=1}^{n} Y_i(0)$

- Why can't we just calculate this quantity directly?
- What we can estimate instead:

Difference in means =
$$\overline{Y}_{\text{treated}} - \overline{Y}_{\text{control}}$$

- $\overline{Y}_{\text{treated}}$: sample average outcome for treated group
- $\overline{Y}_{control}$: sample average outcome for control group

• We want to estimate the average causal effects over all units:

Sample Average Treatment Effect (SATE) =
$$\frac{1}{n} \sum_{i=1}^{n} \{Y_i(1) - Y_i(0)\}$$

= $\frac{1}{n} \sum_{i=1}^{n} Y_i(1) - \frac{1}{n} \sum_{i=1}^{n} Y_i(0)$

- Why can't we just calculate this quantity directly?
- What we can estimate instead:

Difference in means =
$$\overline{Y}_{treated} - \overline{Y}_{control}$$

- $\overline{Y}_{\text{treated}}$: sample average outcome for treated group
- $\overline{Y}_{control}$: sample average outcome for control group
- When will the difference-in-means is a good estimate of the SATE?

• Under an RCT, treatment and control groups are random samples.

- Under an RCT, treatment and control groups are random samples.
- Average in the treatment group will be similar to average if all treated:

$$\overline{Y}_{ ext{treated}} pprox rac{1}{n} \sum_{i=1}^{n} Y_i(1)$$

- Under an RCT, treatment and control groups are random samples.
- Average in the treatment group will be similar to average if all treated:

$$\overline{Y}_{ ext{treated}} pprox rac{1}{n} \sum_{i=1}^{n} Y_i(1)$$

• Average in the control group will be similar to average if all untreated:

$$\overline{Y}_{\text{control}} \approx \frac{1}{n} \sum_{i=1}^{n} Y_i(0)$$

- Under an RCT, treatment and control groups are random samples.
- Average in the treatment group will be similar to average if all treated:

$$\overline{Y}_{ ext{treated}} pprox rac{1}{n} \sum_{i=1}^{n} Y_i(1)$$

• Average in the control group will be similar to average if all untreated:

$$\overline{Y}_{\text{control}} \approx \frac{1}{n} \sum_{i=1}^{n} Y_i(0)$$

• Implies difference-in-means should be close to SATE:

$$\overline{Y}_{\text{treated}} - \overline{Y}_{\text{control}} \approx \frac{1}{n} \sum_{i=1}^{n} Y_i(1) - \frac{1}{n} \sum_{i=1}^{n} Y_i(0) = \frac{1}{n} \sum_{i=1}^{n} \{Y_i(1) - Y_i(0)\} = \text{SATE}$$

Placebo effects:

- Placebo effects:
 - Respondents will be affected by any intervention, even if they shouldn't have any effect.

- Placebo effects:
 - Respondents will be affected by any intervention, even if they shouldn't have any effect.
 - · Reason to have control group be recycling script

- Placebo effects:
 - Respondents will be affected by any intervention, even if they shouldn't have any effect.
 - Reason to have control group be recycling script
- Hawthorne effects:

- Placebo effects:
 - Respondents will be affected by any intervention, even if they shouldn't have any effect.
 - Reason to have control group be recycling script
- Hawthorne effects:
 - Respondents act differently just knowing that they are under study.

• Can we determine if randomization "worked"?

- Can we determine if randomization "worked"?
- If it did, we shouldn't see large differences between treatment and control group on **pretreatment variable**.

- Can we determine if randomization "worked"?
- If it did, we shouldn't see large differences between treatment and control group on **pretreatment variable**.
 - Pretreatment variable are those that are unaffected by treatment.

- Can we determine if randomization "worked"?
- If it did, we shouldn't see large differences between treatment and control group on **pretreatment variable**.
 - Pretreatment variable are those that are unaffected by treatment.
- We can check in the actual data for some pretreatment variable X

- Can we determine if randomization "worked"?
- If it did, we shouldn't see large differences between treatment and control group on **pretreatment variable**.
 - Pretreatment variable are those that are unaffected by treatment.
- We can check in the actual data for some pretreatment variable X
 - $\overline{X}_{\text{treated}}$: average value of variable for treated group.

- Can we determine if randomization "worked"?
- If it did, we shouldn't see large differences between treatment and control group on **pretreatment variable**.
 - Pretreatment variable are those that are unaffected by treatment.
- We can check in the actual data for some pretreatment variable X
 - $\overline{X}_{\text{treated}}$: average value of variable for treated group.
 - $\overline{X}_{control}$: average value of variable for control group.

- Can we determine if randomization "worked"?
- If it did, we shouldn't see large differences between treatment and control group on **pretreatment variable**.
 - Pretreatment variable are those that are unaffected by treatment.
- We can check in the actual data for some pretreatment variable X
 - $\overline{X}_{\text{treated}}$: average value of variable for treated group.
 - $\overline{X}_{control}$: average value of variable for control group.
 - Under randomization, $\overline{X}_{\text{treated}} \overline{X}_{\text{control}} \approx 0$

• Instead of 1 treatment, we might have multiple **treatment arms**:

- Instead of 1 treatment, we might have multiple **treatment arms**:
 - Control condition

- Instead of 1 treatment, we might have multiple treatment arms:
 - Control condition
 - Treatment A

- Instead of 1 treatment, we might have multiple treatment arms:
 - Control condition
 - Treatment A
 - Treatment B

- Instead of 1 treatment, we might have multiple **treatment arms**:
 - Control condition
 - Treatment A
 - Treatment B
 - Treatment C, etc

- Instead of 1 treatment, we might have multiple **treatment arms**:
 - Control condition
 - Treatment A
 - Treatment B
 - Treatment C, etc
- In this case, we will look at multiple comparisons:

- Instead of 1 treatment, we might have multiple treatment arms:
 - Control condition
 - Treatment A
 - Treatment B
 - Treatment C, etc
- In this case, we will look at multiple comparisons:

•
$$\overline{Y}_{\text{treated, A}} - \overline{Y}_{\text{control}}$$

- Instead of 1 treatment, we might have multiple **treatment arms**:
 - Control condition
 - Treatment A
 - Treatment B
 - Treatment C, etc
- In this case, we will look at multiple comparisons:

•
$$\overline{Y}_{\text{treated, A}} - \overline{Y}_{\text{contro}}$$

• $\overline{Y}_{\text{treated, B}} - \overline{Y}_{\text{control}}$

- Instead of 1 treatment, we might have multiple **treatment arms**:
 - Control condition
 - Treatment A
 - Treatment B
 - Treatment C, etc
- In this case, we will look at multiple comparisons:

•
$$\overline{Y}_{\text{treated, A}} - \overline{Y}_{\text{control}}$$

• $\overline{Y}_{\text{treated, A}} - \overline{Y}_{\text{treated, B}}$

- Instead of 1 treatment, we might have multiple treatment arms:
 - Control condition
 - Treatment A
 - Treatment B
 - Treatment C, etc
- In this case, we will look at multiple comparisons:

•
$$\overline{Y}_{\text{treated, A}} - \overline{Y}_{\text{control}}$$

•
$$\overline{Y}_{\text{treated, B}} - \overline{Y}_{\text{control}}$$

- $\overline{Y}_{\text{treated, A}} \overline{Y}_{\text{treated, B}}$
- If treatment arms are randomly assigned, these differences will be good estimators for each causal contrast.

2/ Calculating effects

reinstall gov50data if necessary
library(gov50data)

Variable Name	Description		
age	Age of the R in years		
female	1=R marked "Female" on voter reg., 0 otherwise		
voted_gen_14	1 if R voted in the 2014 general election		
vote_gen_12	1 if R voted in the 2012 general election		
treat_ind	1 if R assigned to trans rights script, 0 for recycling		
racename	name of racial identity indicated on voter file		
democrat	1 if R is a registered Democrat		
nondiscrim_pre	1 if R supports nondiscrim. law at baseline		
nondiscrim_post	1 if R supports nondiscrim. law after 3 months		

trans

##	# A	tibbl	le: 565	x 9			
##		age	female	voted_gen_14	voted_gen_12	treat_ind	racename
##		<dbl></dbl>	<dbl></dbl>	<dbl></dbl>	<dbl></dbl>	<dbl></dbl>	<chr></chr>
##	1	29	Θ	Θ	1	Θ	African~
##	2	59	1	1	Θ	1	African~
##	3	35	1	1	1	1	African~
##	4	63	1	1	1	1	African~
##	5	65	Θ	1	1	1	African~
##	6	51	1	1	1	Θ	Caucasi~
##	7	26	1	1	1	Θ	African~
##	8	62	1	1	1	1	African~
##	9	37	Θ	1	1	Θ	Caucasi~
##	10	51	1	1	1	Θ	Caucasi~
##	# i	555 n	nore row	VS			
##	# i	3 moi	re varia	ables: democra	at <dbl>, nond</dbl>	discrim_pre	e <dbl>,</dbl>
##	#	nondi	iscrim_µ	oost <dbl></dbl>			

Calculate the average outcomes in each group

```
treat_mean <- trans |>
  filter(treat_ind == 1) |>
  summarize(nondiscrim_mean = mean(nondiscrim_post))
treat_mean
```

```
## # A tibble: 1 x 1
## nondiscrim_mean
## <dbl>
## 1 0.687
```

Calculate the average outcomes in each group

```
treat mean <- trans |>
  filter(treat ind == 1) |>
  summarize(nondiscrim_mean = mean(nondiscrim_post))
treat mean
## # A tibble: 1 x 1
##
    nondiscrim mean
               < db1 >
##
               0.687
## 1
control mean <- trans |>
  filter(treat ind == 0) |>
  summarize(nondiscrim mean = mean(nondiscrim post))
control mean
```

A tibble: 1 x 1
nondiscrim_mean
<dbl>
1 0.648

Calculating the difference in means

treat_mean - control_mean

nondiscrim_mean

1 0.039

We'll see more ways to do this throughout the semester.

Checking balance on numeric covariates

We can use group_by to see how the mean of covariates varies by group:

```
trans |>
  group_by(treat_ind) |>
  summarize(age_mean = mean(age))
```

```
## # A tibble: 2 x 2
## treat_ind age_mean
## <dbl> <dbl>
## 1 0 48.2
## 2 1 48.3
```

Checking balance on categorical covariates

Or we can group by treatment and a categorical control:

```
trans |>
  group_by(treat_ind, racename) |>
  summarize(n = n())
```

##	#	A tibble:	9 x 3		
##	#	Groups:	treat_ind [2]		
##		treat_ind	racename	n	
##		<dbl></dbl>	<chr></chr>	<int></int>	
##	1	Θ	African American	58	
##	2	Θ	Asian	2	
##	3	Θ	Caucasian	77	
##	4	Θ	Hispanic	150	
##	5	1	African American	68	
##	6	1	Asian	4	
##	7	1	Caucasian	75	
##	8	1	Hispanic	130	
##	9	1	Native American	1	

Hard to read!

pivot_wider() takes data from a single column and moves it into multiple columns based on a grouping variable:

```
trans |>
 group_by(treat_ind, racename) |>
 summarize(n = n()) |>
 pivot_wider(
    names_from = treat_ind,
    values_from = n
)
```

pivot_wider() takes data from a single column and moves it into multiple columns based on a grouping variable:



names_from tells us what variable will map onto the columns
values_from tell us what values should be mapped into those columns

```
trans |>
 group_by(treat_ind, racename) |>
 summarize(n = n()) |>
 pivot_wider(
   names_from = treat_ind,
   values_from = n
)
```

##	#	A tibble: 5 x 3		
##		racename	`0`	`1`
##		<chr></chr>	<int></int>	<int></int>
##	1	African American	58	68
##	2	Asian	2	4
##	3	Caucasian	77	75
##	4	Hispanic	150	130
##	5	Native American	NA	1

Calculating diff-in-means by group

```
trans |>
 mutate(
    treat ind = if else(treat ind == 1, "Treated", "Control"),
    party = if else(democrat == 1, "Democrat", "Non-Democrat")
  group by(treat ind, party) |>
  summarize(nondiscrim mean = mean(nondiscrim post)) |>
 pivot wider(
   names from = treat ind,
    values from = nondiscrim mean
 mutate(
   diff in means = Treated - Control
```

```
## # A tibble: 2 x 4
## party Control Treated diff_in_means
## <chr> <dbl> <dbl> <dbl> <dbl>
## 1 Democrat 0.704 0.754 0.0498
## 2 Non-Democrat 0.605 0.628 0.0234
```

Creating more complicated groups with case_when

```
trans |>
  mutate(
    age_group = case_when(
        age < 25 ~ "Under 25",
        age >=25 & age < 65 ~ "Bewteen 25 and 65",
        age >= 65 ~ "Older than 65"
    )
    ) |>
    count(age_group)
```

##	#	A tibble: 3 x 2	
##		age_group	n
##		<chr></chr>	<int></int>
##	1	Bewteen 25 and 65	369
##	2	Older than 65	116
##	3	Under 25	80

Calculating ATE by age group

```
trans |>
 mutate(
    treat_ind = if_else(treat_ind == 1, "Treated", "Control"),
    age group = case when(
      age < 25 \sim "Under 25",
      age >=25 & age < 65 \sim "Bewteen 25 and 65",
      age \geq 65 ~ "Older than 65"
  group_by(treat_ind, age_group) |>
  summarize(nondiscrim mean = mean(nondiscrim post)) |>
 pivot wider(
   names_from = treat_ind,
    values from = nondiscrim mean
 mutate(
   diff in means = Treated - Control
```

##	#	A tibble: 3 x 4			
##		age_group	Control	Treated	diff_in_means
##		<chr></chr>	<dbl></dbl>	<dbl></dbl>	<dbl></dbl>
##	1	Bewteen 25 and 65 $$	0.694	0.683	-0.0112
##	2	Older than 65	0.576	0.614	0.0378
##	3	Under 25	0.556	0.829	0.273