Causal Reasoning on Published Paper ‘Public transit and urban redevelopment: The effect of light rail transit on land use in Minneapolis, Minnesota’

Final Project | Final Report v2

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This final report is structured into 5 sections:

1. Introduction
2. Summary of the Paper
3. Experiment Motivation and Experiment Procedure
4. Experiment Results and Analysis
5. Conclusion

Section 1: Introduction

The Paper in Summary

The paper is named: “Public transit and urban redevelopment: The effect of light rail transit on land use in Minneapolis, Minnesota” and is written by Needham B. Hurst and Sarah E. West. Broadly, the paper looks at the impact of transit development on land redevelopment. More specifically, the paper estimates the effect of introducing the METRO Blue Line LRT has on potential land use changes (as determined by parcel data and aerial photographs). The paper answers 2 questions when measuring the causal effect:

1. Did the LRT introduction incite land use change that is different from land use changes happening in the rest of the city?
2. How does the LRT introduction affect land use change spatially within the LRT corridor?

The Weakness in Focus

With difference-in-difference, parallel trends must hold. The parallel trends assumption state that the counterfactual trend for the treated group should match the trend of the control group. We were concerned that the treated group (the land within ½ mile of the LRT) would experience different changes even without the LRT when compared to the rest of Minneapolis (the land outside ½ mile of the LRT). The paper gave some context to the LRT corridor - it currently includes both an LRT system, freight rail, and a highway, and connects major landmarks within Minneapolis. Therefore, we were confident that the parallel trends wouldn’t hold given this specific track of land has special importance compared to the rest of the city and would likely change differently.

We decided that political will was a variable that should be accounted for. In the problem model, we added political will to our simulated data. Contextually, we believe that the political will (both incentivizing land use change and accelerating/delaying regulatory approval) differs per area depending on its political importance. Therefore, we wanted to investigate whether this specific transportation corridor was affected by special political treatment. If it was, then the counterfactual of this corridor would likely not match the trend of the control group (rest of the city).

Our Results

We were able to replicate similar results to the paper: land parcels near the LRT were slightly more likely to change in use from operation to construction.

Running both the baseline model and the problem model displays that when political will (a variable not accounted for by the paper) is added (to the problem model), the parallel trends assumption fails. In Section 3, we dive into how the trend of the treated group's counterfactual does not match the trend of the control group. We prove this with both different structural equation models (SEMs) and with plots.

Section 2: Summary of the Paper

The Causal Relationship

As mentioned earlier, the paper explores whether being in proximity to an LRT station would make a land parcel more likely to change in use.

More specifically, the causal relationship is as follows:

- Treatment: Is the parcel (land) within ½ mile of the LRT station?
  - 0: No
  - 1: Yes
- Outcome: Does the land use change?
  - 0: No
  - 1: Yes

This was measured over 3 time periods:

- Period 1: Pre-construction (1997-2000)
- Period 2: Construction (2000-2005)
- Period 3: Operations/Post-construction (2005-2010)

Identification Problems

There are several potential identification problems with this study. We dived into whether the parallel trends assumption holds when additional variables were accounted for in a model. We decided that political will was a relevant variable that could influence whether a land use changes.

The parallel trends assumption states that the trend of the treated group's counterfactual matches the trend of the control group - this is key for difference in difference. If the parallel trends assumption is not met, we cannot accurately measure causal effect without bias. The treated group should be randomized and resemble the control group except receiving treatment after a specific time.

As mentioned above, we recognized that the transportation corridor that the LRT was built in (which includes a pre-existing highway, freight rail, and connected major landmarks within the city) held special political importance and thus would experience different land use changes than the rest of the city. We believe that governments can influence land use changes and also can accelerate/delay the regulatory process of approving a submitted change proposal. Governments may want the transportation corridor (even without the LRT - counterfactual) to transform in a certain way compared to the rest of the city.

How Causal Effect was Estimated

The paper primarily used difference-in-difference (DD) to measure the causal effect. The paper is looking for the intervention (treatment) effect that a land being in proximity to an LRT station (within ½ miles) has on the outcome (whether its use type changes). The paper calculated 3 main DDs for their main model (model III). Table 7 shows the first difference, and table 6 shows the second difference results. In summary, here are the 3 main DDs approaches:

1. Second Difference between Periods 2-3: (Period3Treated - Period2Treated) - (Period3Control - Period3Control)
2. Second Difference between Periods 1-3: (Period3Treated - Period1Treated) - (Period3Control - Period1Control)
3. Second Difference between Periods 1-2: (Period2Treated - Period1Treated) - (Period2Control - Period1Control)

The number used for calculating DDs is the average adjusted probability of change in land use type (for each of the 3 time periods, it takes the average of 5 different original land types).

Propensity-score-matching was also used to check the accuracy of difference-in-difference.

The Paper's Main Result

While individual results per model varied, the paper's main result was that there was a slight increase in the likelihood of a land use change for land parcels within a ½ mile distance from an LRT station relative to construction and operation. However, the paper found that neither construction (period 2) nor operation (period 3) of the LRT appeared to change land use relative to pre-construction (period 1). One could hypothesize that land owners and developers acted before the LRT system was announced and before construction began instead of during or after construction.

Specifically, the results for the 3 DD approaches are as follows:

1. Second Difference between Periods 2-3: (0.0337–0.0187)–(0.0146–0.0135) = 0.0139 (slightly positive)
2. Second Difference between Periods 1-3: (0.0337–0.0384)–(0.0146–0.0174) = -0.0019 (slightly negative - negligible)
3. Second Difference between Periods 1-2: (0.0187–0.0384)–(0.0135–0.0174) = -0.0158 (slightly negative)

Within the corridor itself, land use changes were likely for specific land types when in proximity to an LRT station.

Section 3: Experiment Motivation

The Baseline Model

In the paper, models I to III increase in complexity as the authors add additional controls, while model IV answered the question of how land use changes differed spatially within the corridor. We decided to recreate model I given it captured the main causal effect (without additional controls). Model I's structural equation model is as follows:

$$Y_{it} = \alpha + \beta_0 H_i + \beta_1 T_t + \beta_2 H_i T_t + \varepsilon_{it}$$

- Y_it - Land use change
  - 0 - not changed
  - 1 - changed
- H_i - Parcel (land) in ½ mile of station
  - 0 - no
  - 1 - yes
- T_t - Time periods
  - 1 - before construction (1997-2000)
  - 2 - construction (2000-2005)

   - 3 - after construction (2005-2010)

- Alpha - Baseline average
- Beta0 - Difference between 2 groups pre-intervention (initial difference - unobserved unit fixed effects)
- Beta1 - Time trend (unobserved time fixed effects)
- Beta2 - Average treatment effects (DD)

As mentioned above, H_i is the treatment variable, while Y_it is the outcome variable. Our model includes these variables along with U_t, which represents unobserved time-fixed effects. Given that we aren't using the paper's data set (The Metropolitan Council's Generalized Land Use Survey (GLUS) and the City of Minneapolis' parcel data set), our results won't exactly match the papers, although the correlations and relationships remain the same. In addition, we decided to not make the outcome variable (Y_it) binary, and instead show the likelihood of a land use change given we do not have the exact same dataset.

Capturing the Causal Assumptions Made in the Paper

To measure the causal effect being in proximity (within ½ mile) to an LRT station has on the likelihood of a land use change, the paper made several assumptions. These include:

1) Time period Assumptions

A key complexity that arises with a time dimension is leads and lags. The paper chooses to measure 3 periods from 1997 to 2010:

- Period 1: Pre-construction (1997-2000)
- Period 2: Construction (2000-2005)
- Period 3: Operations/Post-construction (2005-2010)

The paper's authors argue that it only became apparent that an LRT system was going to be constructed post-1997 due to mixed signals from the Ventura administration and the federal government, and therefore any lead effect would not precede 1997. The paper's authors also argue that the post-2010 period sees little changes and thus 2005-2010 would be a good time frame for the operational period.

While our group feels that any lag effect would be reasonably captured within the 3 time periods in the paper, we feel that the pre-construction period may not have captured the lead effect completely. The paper mentions that the government was looking at several transit options (including express bus lines, parkway, etc.) since the 1950s. While there were funding concerns and no consensus pre-1997, one could argue that land developers could act differently for land parcels near this corridor compared to the rest of the city. In addition, the counterfactual of this transportation corridor may include an alternative transit option chosen (like a bus express route) instead of an LRT, which could have influenced land use changes differently from the land use changes in the rest of the city (thus the parallel trends assumption may not hold). Therefore there could be relevant variables that were observed pre-1997 (leads).

2) Treatment Assumptions

The paper assumes that land within ½ mile of an LRT station is treated, while land outside of this boundary is not treated. However, reality may not be as static or fixed as this assumption the paper made. The paper mentions the highest probability of land use change occurs in land less than 200 meters from the station or only a fraction of the ½ mile measurement (Figures 9 and 11). One could argue therefore that ½ mile is too broad or too narrow of a measurement (which the authors acknowledged). Therefore, the paper's authors made a key assumption in how treatment was measured.

3) Treated/Control Group Assumptions

As a reminder, the treated group included land parcels within this specific transportation corridor (that includes the LRT, freight rail, and highway). The paper mentions that they initially wanted to find a similar transportation corridor (without LRT) within Minneapolis for the control group, but no such similar corridor exists. Therefore, the paper made the rest of Minneapolis the control group instead. This assumption possibly breaks the parallel trends assumption. As previously mentioned, this corridor likely holds special importance and therefore would experience different land use changes compared to the rest of the city. In other words, the counterfactual of the treated group may not match the trend of the control group.

4) Outcome Assumptions

Broadly, the paper investigated any relationships between transit and urban redevelopment. The paper's authors chose land use changes as how the outcome would be measured. This however excludes certain types of redevelopment. More specifically, same-land use redevelopment was not measured. If a developer decided to tear down a 5-storey multi-family complex and build a 30-storey multi-family complex, the land use would remain the same (multi-family). However, it is clear that significant redevelopment occurred.

In-Depth Explanation of the Baseline Model

We began by creating simulate_data with the following properties:

- N_units = 93457, the # of entries in the paper's dataset
- T = 3, as the paper used 3 periods
- treatment_period = 2, given the paper found the only positive DD value occurring between periods 2-3

The following variables were created:

- i: an id of the properties
- t: time period (1, 2, or 3)
- H_i: normal distribution to be used by ever_treated (the treatment variable), that reflects % of land parcels within proximity to an LRT station
- U_t: unobserved time fixed effects
- ever_treated: treatment variable (0 or 1), parcel (land) in ½ mile of LRT station
- post_treatment: whether treatment already occurred (0 or 1)

The Problem Model

Weakness Being Explored

As previously mentioned, the baseline model was based on this SEM:

$$Y_{it} = \alpha + \beta_0 H_i + \beta_1 T_t + \beta_2 H_i T_t + \varepsilon_{it}$$

However, we feel that political will was a variable not captured by the paper that can best highlight the differences this corridor (treated group) has compared to the rest of the control group. Due to special political importance, the counterfactual of this transportation corridor may not match the control group's trend. We feel that the treated group will experience different land use changes even without an LRT compared to the rest of the city. This breaks the parallel trends assumption. Therefore, we based the problem model on this SEM, where Beta3 captures the political influence governments can have on treatment:

$$Y_{it} = \alpha + \beta_0 H_i + \beta_1 T_t + \beta_2 H_i T_t + \beta_3 H_i T_t + \varepsilon_{it}$$

The parallel trends assumption is broken if the treated group has significant political will supporting land use change while the other does not:

$$Y_{it}^{0} = \alpha + \beta_0 H_i + \beta_1 T_t + \beta_2 H_i T_t + \varepsilon_{it}$$
$$Y_{it}^{1} = \alpha + \beta_0 H_i + \beta_1 T_t + \beta_2 H_i T_t + \beta_3 H_i T_t + \varepsilon_{it}$$

Plausibility of Critique

In the paper, the authors mentioned that this specific transportation corridor held special importance as it connected several important landmarks (including the Mall of America, the downtown district, etc.) within the city of Minneapolis. Prior to the LRT, the corridor was home to a freight rail and an Interstate freeway already, highlighting its importance as a transportation corridor. The authors acknowledged its uniqueness when they discussed how they couldn't find a similar transportation corridor in the city. Their choice to use the rest of the city as the control group is where the parallel trends assumption will likely fail. The land use changes in the rest of the city will likely not match the trend of the corridor's counterfactual. The government already saw the corridor's importance when they chose to add freight rail and the Interstate freeway to the corridor. Our group feels it is likely therefore that the government would go further to influence land owners and developers to pursue specific land uses in the land around this corridor, while the rest of the city would not experience this special treatment from the government.

Our group is confident that political will is a significant variable that will challenge the parallel trends. The government yields significant control over zoning rules and regulations. This and its relationships with large land developers likely give the government the power to influence land use changes.

In-Depth Explanation of the Changes in the Problem Model

The modification in the problem model is the inclusion of P_it, which represents political will that could influence or accelerate/delay a land use change. P_it is incorporated into the calculation of Y0 - or the outcome of units not treated. Therefore, within the simulate_data function, the lack of political will is incorporated into the control group. Our group is specifically looking for a different trend for the treated group's counterfactual compared to the trend of the control group.

Section 3: Experiment Procedure

Baseline Model

```
simulate_data <- function(N_units=93457,T=3,treatment_period=2,delta=5){ # 93457 properties in dataset

  N_obs <- N_units*T

  # Create index of i and t values
  tibble(
    i = rep(1:N_units,times=1,each=T), t =
    rep(1:T,times=N_units,each=1),
  ) %>%

  # Create unit-level variables
  group_by(i) %>%
  mutate(
    H_i = rnorm(1,-0.34845, 0.25),
  ) %>%

  # Create period-level variables
  group_by(t)  %>%
```

```
mutate(
U_t = rnorm(1,(t/10),0.1), treatment_period =
        treatment_period,
    ) %>%

    # Create remaining variables
ungroup() %>% mutate(
ever_treated = as.integer(H_i>0),
post_treatment  =  as.integer(t>=treatment_period), D =

        ever_treated*post_treatment,

Y0 = H_i + U_t + rnorm(N_obs,0,1), Y1 = Y0 +
        H_i*((t-1.5)/3),

Y = D*Y1 + (1-D)*Y0
    )
 }

simulate_data()
```

```
## # A tibble: 280,371 x 11
##        i     t     H_i     U_t  treat~1 ever_~2 post_~3     D     Y0     Y1      Y
##    <int> <int>   <dbl>   <dbl>    <dbl>   <int>   <int> <int>  <dbl>  <dbl>  <dbl>
##  1     1     1 -0.658  0.206          2       0       0     0  0.735  0.845  0.735
##  2     1     2 -0.658  0.0374         2       0       1     0 -0.656 -0.765 -0.656
##  3     1     3 -0.658  0.107          2       0       1     0 -0.474 -0.804 -0.474
##  4     2     1 -0.410  0.206          2       0       0     0 -0.412 -0.344 -0.412
##  5     2     2 -0.410  0.0374         2       0       1     0 -1.01  -1.08  -1.01
##  6     2     3 -0.410  0.107          2       0       1     0 -0.690 -0.894 -0.690
##  7     3     1 -0.579  0.206          2       0       0     0  0.133  0.230  0.133
##  8     3     2 -0.579  0.0374         2       0       1     0  0.663  0.567  0.663
##  9     3     3 -0.579  0.107          2       0       1     0 -2.20  -2.49  -2.20
## 10     4     1 -0.0586 0.206          2       0       0     0  0.337  0.346  0.337
## # ... with 280,361 more rows, and abbreviated variable names ## #        1:
treatment_period, 2: ever_treated, 3: post_treatment
```

```
tbl_summary(simulate_data())
```

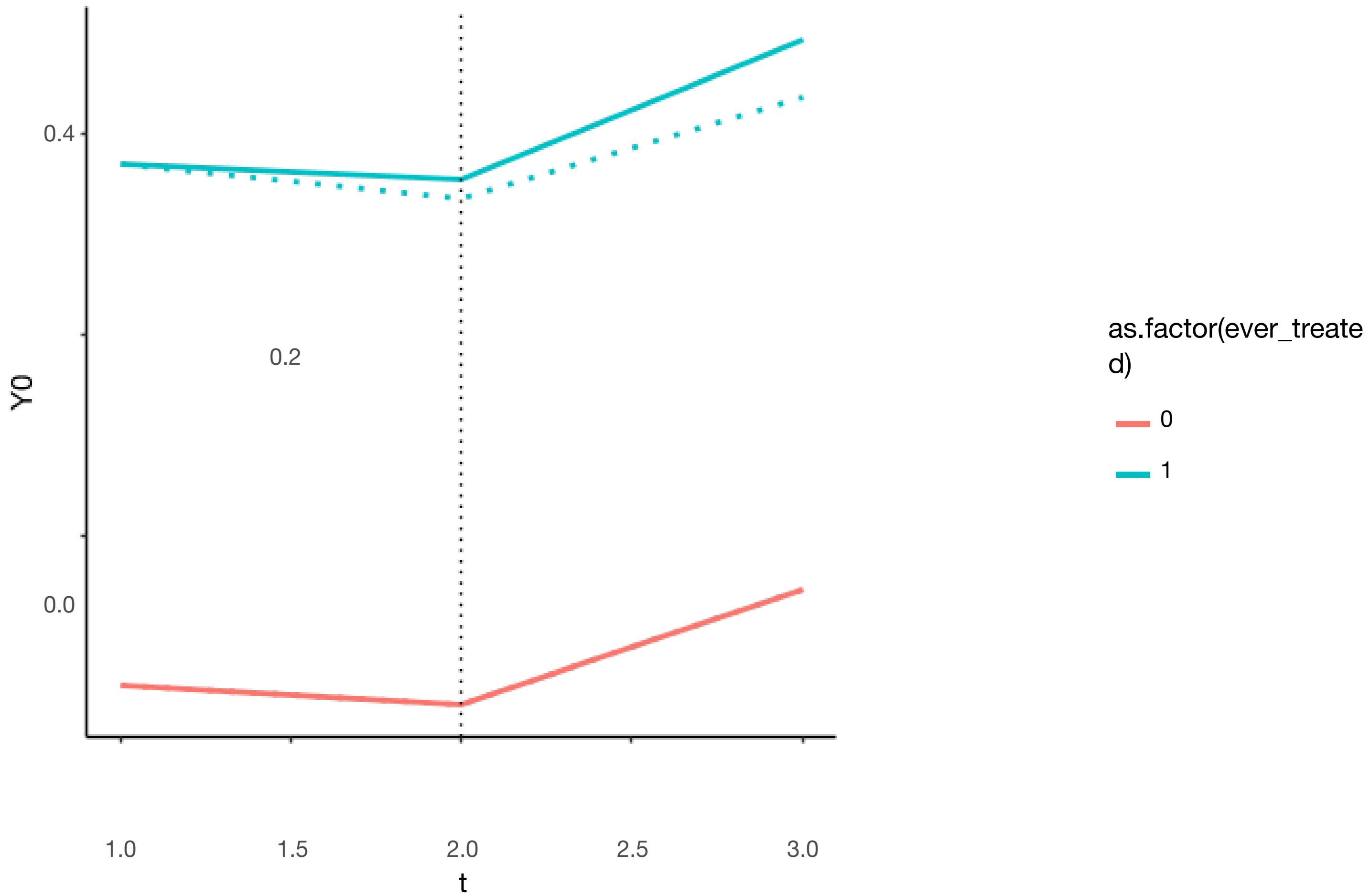
```
## Table printed with ‘knitr::kable()‘, not {gt}. Learn why at
##   https://www.danieldsjoberg.com/gtsummary/articles/rmarkdown.html
## To suppress this message, include ‘message = FALSE‘ in code chunk header.
```

Characteristic	**N = 280,371**
i	46,729 (23,365, 70,093)
1	93,457 (33%)
2	93,457 (33%)
3	93,457 (33%)
t	

Characteristic	N = 280,371
H_i	-0.35 (-0.51, -0.18)
U_t	
0.155579169058195	93,457 (33%)
0.188892642166294	93,457 (33%)
0.214935059077543	93,457 (33%)
treatment_period	
2	280,371 (100%)
ever_treated	22,761 (8.1%)
post_treatment	186,914 (67%)
D	15,174 (5.4%)
Y0	-0.16 (-0.86, 0.53)
Y1	-0.22 (-0.93, 0.49)
Y	-0.16 (-0.86, 0.53)

```
data <- simulate_data()
```

```
data %>% group_by(ever_treated,t)
  %>% summarise_all(mean) %>%
ggplot(aes(x=t,color=as.factor(ever_treated))) +
  geom_line(aes(y=Y0),linetype='dotted',size=1) +
  geom_line(aes(y=Y),size=1) + geom_vline(xintercept =
  2,linetype='dotted') + theme_classic()
```




```
data %>%
lm(Y ~ ever_treated*post_treatment,.) %>%
  summary()
```

```
##
## Call:

## Residuals:
##      Min      1Q  Median      3Q     Max
## -5.0176 -0.6899  0.0030  0.6918  4.4632
##
## Coefficients:
##                              Estimate Std. Error t value Pr(>|t|)
## (Intercept)                 -0.147296   0.003493 -42.165   <2e-16 ***
## ever_treated                 0.515555   0.012249  42.089   <2e-16 ***
## post_treatment               0.037722   0.004278   8.817   <2e-16 ***
## ever_treated:post_treatment  0.017014   0.015002   1.134    0.257
## ---
## Signif. codes:  0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1
##
## Residual standard error: 1.024 on 280367 degrees of freedom
```

```
##  Multiple R-squared:  0.01973,  Adjusted R-squared:  0.01972
##  F-statistic:  1881 on 3 and 280367 DF,  p-value: < 2.2e-16
## lm(formula = Y ~ ever_treated * post_treatment, data = .) ##
```

```
feols(Y ~ ever_treated*post_treatment,cluster=~i,data=data)
```

```
## OLS estimation, Dep. Var.: Y
## Observations: 280,371
## Standard-errors: Clustered (i)
##                                 Estimate Std. Error   t value   Pr(>|t|)
## (Intercept)                    -0.147296   0.003487 -42.24185 < 2.2e-16 ***
## ever_treated                    0.515555   0.012090  42.64402 < 2.2e-16 ***
## post_treatment                  0.037722   0.004183   9.01774 < 2.2e-16 ***
## ever_treated:post_treatment     0.017014   0.014690   1.15825   0.24676
## ---
## Signif. codes: 0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1
## RMSE: 1.02357   Adj. R2: 0.01972
```

```
mc_estimate <- function(s){

  # Simulate data
sample_data <- simulate_data()

  estimate <- sample_data %>%
    filter(ever_treated==1) %>% summarize(

      # Compute ATT
sample_ATT = mean(Y1 - Y0)
    )

  # Return the estimate
estimate
}

 mc_estimate()
```

```
## # A tibble: 1 x 1 ##
sample_ATT
##       <dbl>
## 1     0.0191
```

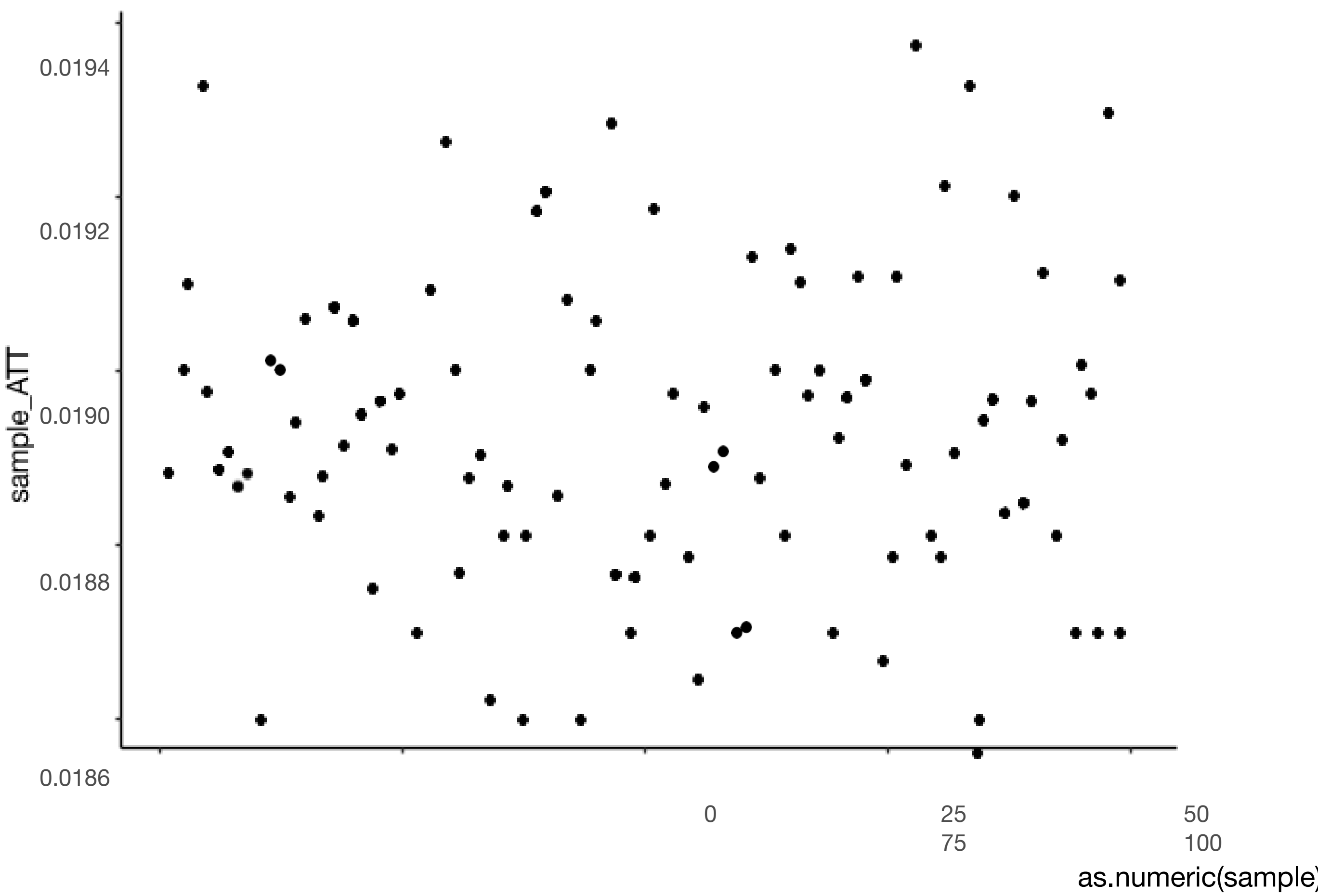
```
mc_estimates <- 1:100 %>%
  map_df(mc_estimate,.id='sample')

head(mc_estimates,10)
```

```
## # A tibble: 10 x 2 ##
     sample sample_ATT
##    <chr>        <dbl>
##  1 1           0.0189
##  2 2           0.0190
##  3 3           0.0191
##  4 4           0.0194
##  5 5           0.0190
##  6 6           0.0189
```

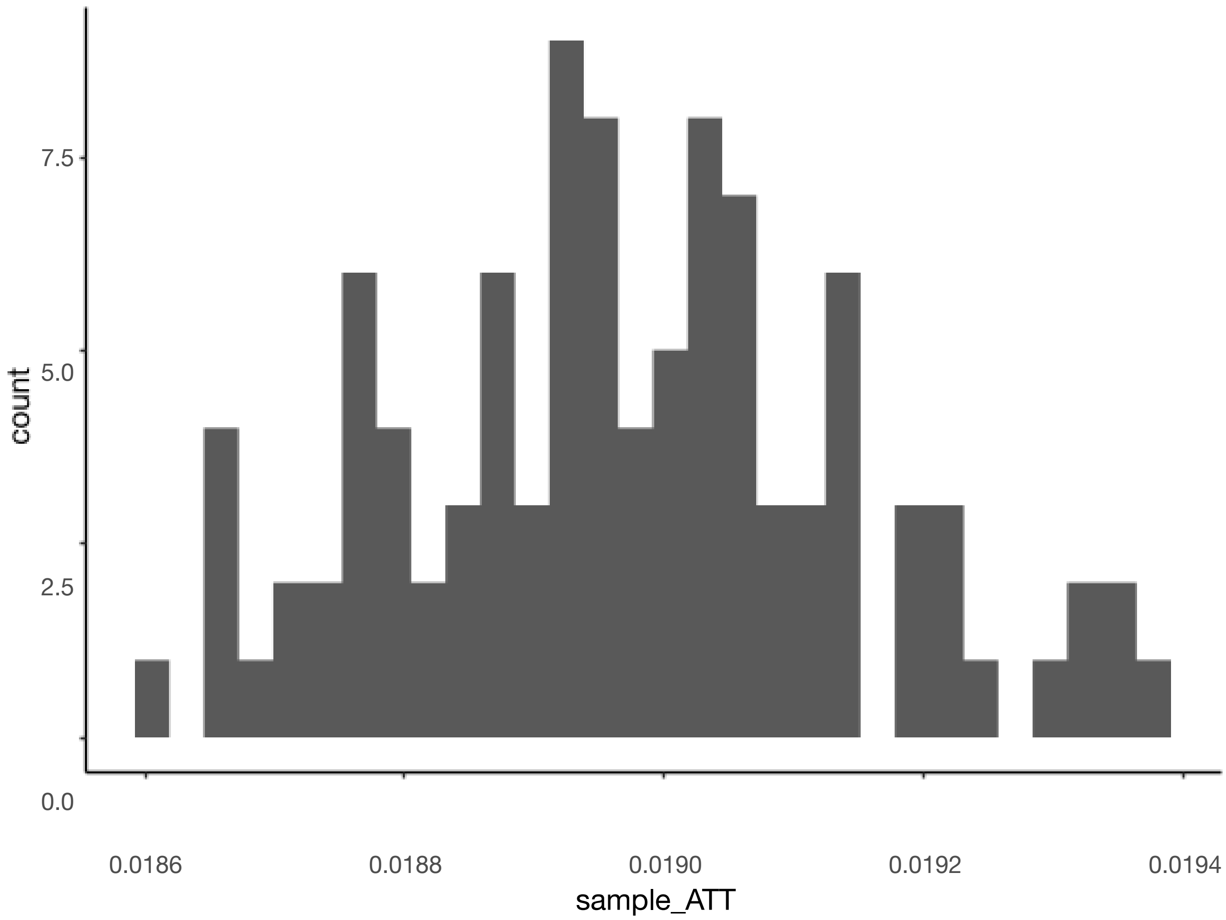
```
##     7 7                0.0190
##     8 8                0.0189
##     9 9                0.0189
## 10 10                  0.0187
```

```
# Results in Scatter Plot
mc_estimates %>%
  ggplot(aes(x=as.numeric(sample),y=sample_ATT)) +
  geom_point() +
theme_classic()
```




```
# Results in Histogram
mc_estimates %>%
  ggplot(aes(sample_ATT)) +
  geom_histogram() +
  theme_classic()
```

```
## 'stat_bin()' using 'bins = 30'. Pick better value with 'binwidth'.
```



Problem Model

```
simulate_data2 <- function(N_units=93457,T=3,treatment_period=2,delta=5){ # 93457 properties in citywid

  N_obs <- N_units*T

  # Create index of i and t values
  tibble(
    i = rep(1:N_units,times=1,each=T), t =
    rep(1:T,times=N_units,each=1),
  ) %>%

  # Create unit-level variables
  group_by(i) %>%
  mutate(
    H_i = rnorm(1,-0.34845, 0.25),
  ) %>%

  # Create period-level variables
  group_by(t) %>%
  mutate(
    U_t = rnorm(1,(t/10),0.1), treatment_period =
    treatment_period,
  ) %>%
```

```
# Create remaining variables
```

```
ungroup() %>% mutate(
P_it = rnorm(N_obs,0,10), ever_treated =
        as.integer((H_i)>0),
post_treatment  =  as.integer(t>=treatment_period), D =

        ever_treated*post_treatment,

Y0 = H_i*P_it + U_t + rnorm(N_obs,0,1), Y1 = Y0 +
        (H_i)*((t-1.5)/3),

Y = D*Y1 + (1-D)*Y0
    )
 }

simulate_data2()
```

```
## # A tibble: 280,371 x 12
##          i     t    H_i     U_t treat~1   P_it ever_~2 post_~3     D     Y0     Y1
##      <int> <int>  <dbl>   <dbl>   <dbl>  <dbl>   <int>   <int> <int>  <dbl>  <dbl>
##  1       1     1 -0.554 -0.0443       2   5.61       0       0     0 -0.785 -0.693
##  2       1     2 -0.554  0.206        2  -1.71       0       1     0  3.44   3.35
##  3       1     3 -0.554  0.468        2 -14.2        0       1     0  8.57   8.29
##  4       2     1 -0.197 -0.0443       2   4.58       0       0     0 -2.72  -2.69
##  5       2     2 -0.197  0.206        2  -2.58       0       1     0  1.08   1.05
##  6       2     3 -0.197  0.468        2  -9.13       0       1     0  1.15   1.05
##  7       3     1 -0.230 -0.0443       2 -26.9        0       0     0  4.42   4.46
##  8       3     2 -0.230  0.206        2  -1.49       0       1     0  0.765  0.727
##  9       3     3 -0.230  0.468        2   9.01       0       1     0 -1.81  -1.92
## 10       4     1 -0.286 -0.0443       2  -9.53       0       0     0  3.46   3.51
## # ... with 280,361 more rows, 1 more variable: Y <dbl>, and abbreviated
## #   variable names 1: treatment_period, 2: ever_treated, 3: post_treatment
```

```
tbl_summary(simulate_data2())
```

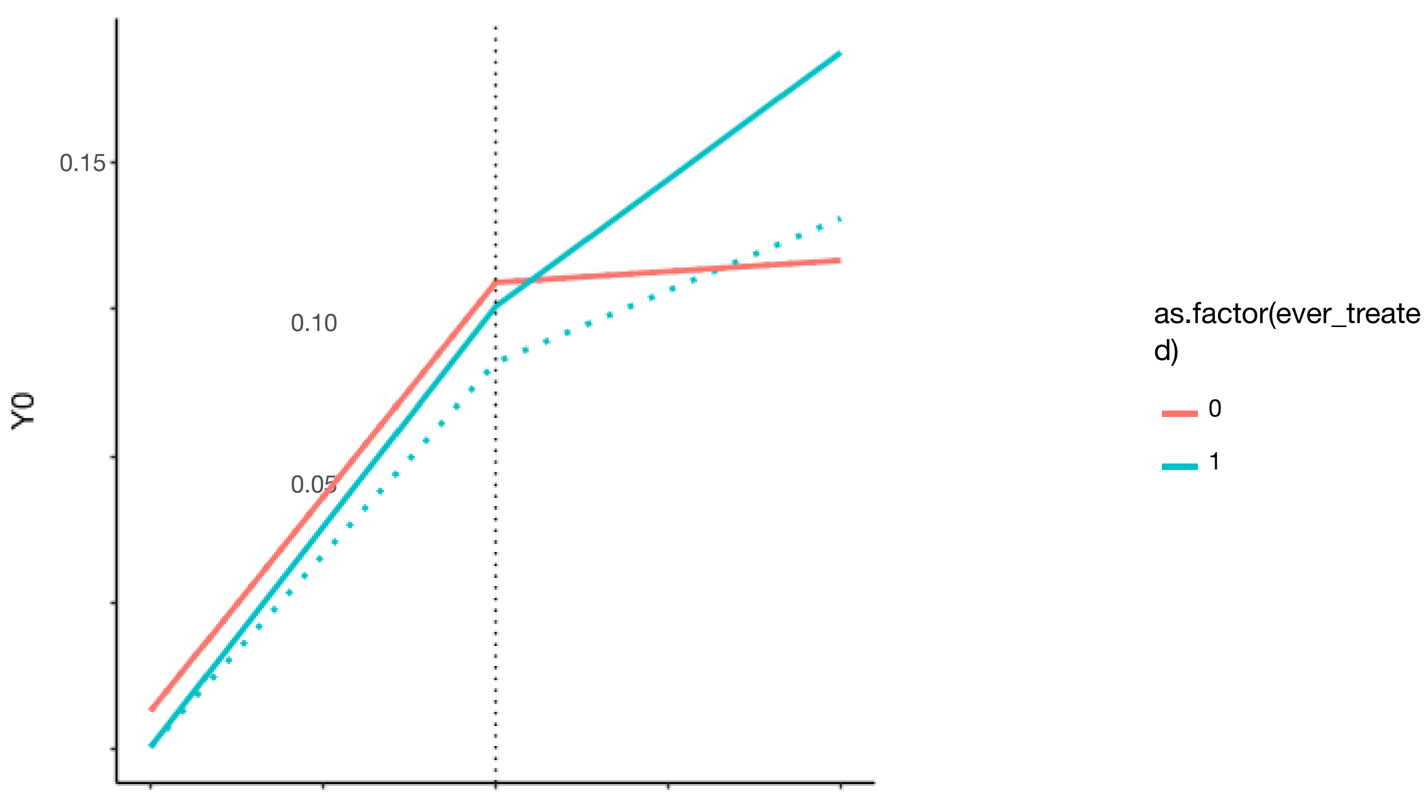
```
## Table printed with ‘knitr::kable()‘, not {gt}. Learn why at
##   https://www.danieldsjoberg.com/gtsummary/articles/rmarkdown.html
## To suppress this message, include ‘message = FALSE‘ in code chunk header.
```

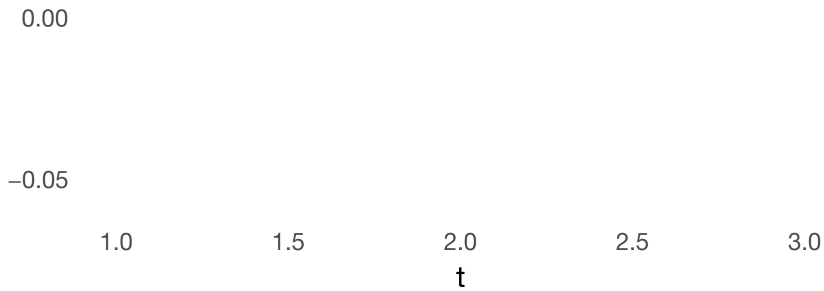
Characteristic	**N = 280,371**
i	46,729 (23,365, 70,093)
t	
1	93,457 (33%)
2	93,457 (33%)
3	93,457 (33%)
H_i	-0.35 (-0.52, -0.18)
U_t	
-0.0849838265831788	93,457 (33%)
0.230270509465736	93,457 (33%)
0.283145294697318	93,457 (33%)

Characteristic	N = 280,371
treatment_period	
2	280,371 (100%)
P_it	0 (-7, 7)
ever_treated	22,713 (8.1%)
post_treatment	186,914 (67%)
D	15,142 (5.4%)
Y0	0.1 (-2.0, 2.3)
Y1	0.1 (-2.0, 2.2)
Y	0.1 (-2.0, 2.3)

```
data2 <- simulate_data2()
```

```
data2 %>% group_by(ever_treated,t)
   %>% summarise_all(mean) %>%
ggplot(aes(x=t,color=as.factor(ever_treated))) +
    geom_line(aes(y=Y0),linetype='dotted',size=1) +
    geom_line(aes(y=Y),size=1) + geom_vline(xintercept =
    2,linetype='dotted') + theme_classic()
```




0.00
−0.05
1.0
1.5
2.0
2.5
3.0
t

```
data2  %>%
lm(Y ~ ever_treated*post_treatment,.) %>%
   summary()
```

```
##
## Call:
## lm(formula = Y ~ ever_treated * post_treatment, data = .) ##
##  Residuals:
##        Min        1Q   Median        3Q       Max
## -34.984     -2.090     0.011      2.096     34.138
##
##  Coefficients:
##                                    Estimate Std. Error t value Pr(>|t|)
##  (Intercept)                       -0.03703     0.01509    -2.454     0.0141 *
##  ever_treated                      -0.01269     0.05275    -0.241     0.8099
##  post_treatment                     0.14982     0.01848     8.106 5.26e-16 ***
##  ever_treated:post_treatment        0.04432     0.06461     0.686     0.4927
##  ---
##  Signif. codes:       0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1
##
##  Residual standard error: 4.421 on 280367 degrees of freedom
##  Multiple  R-squared:       0.0002704,     Adjusted R-squared:       0.0002598
##  F-statistic: 25.28 on 3 and 280367 DF,               p-value: 2.399e-16
```

```
feols(Y ~ ever_treated*post_treatment,cluster=~i,data=data2)
```

```
## OLS estimation, Dep. Var.: Y ##
Observations: 280,371
## Standard-errors: Clustered (i)
##                                     Estimate Std. Error          t value       Pr(>|t|)
##  (Intercept)                       -0.037029      0.015634     -2.368500  1.7862e-02 *
##  ever_treated                      -0.012693      0.025800     -0.491969  6.2274e-01
##  post_treatment                     0.149821      0.019184      7.809614  5.7955e-15 ***
##  ever_treated:post_treatment        0.044318      0.031635      1.400906  1.6125e-01
##  ---
##  Signif. codes:       0 '***' 0.001 '**' 0.01 '*' 0.05                    '.' 0.1 ' ' 1
##  RMSE: 4.42068        Adj. R2: 2.598e-4
```

```
mc_estimate2 <- function(s){

      # Simulate data
sample_data <- simulate_data2()

      estimate <- sample_data %>%
         filter(ever_treated==1) %>% summarize(

            # Compute ATT
sample_ATT = mean(Y1 - Y0)
         )
```

```
        # Return the estimate
estimate
 }

mc_estimate2()
```

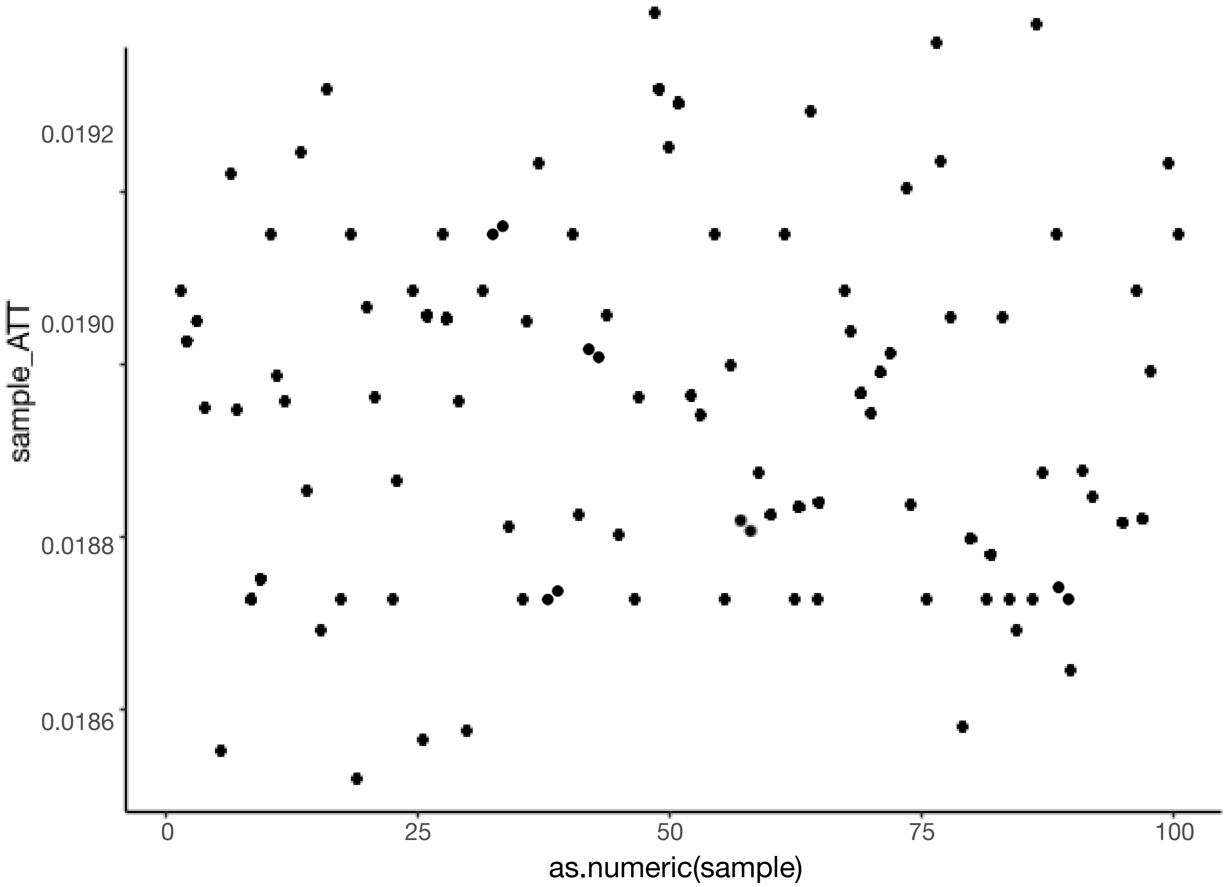
```
## # A tibble: 1 x 1 ##
sample_ATT
##              <dbl>
## 1          0.0190
```

```
mc_estimates2 <- 1:100 %>%
   map_df(mc_estimate2,.id='sample')

head(mc_estimates2,10)
```

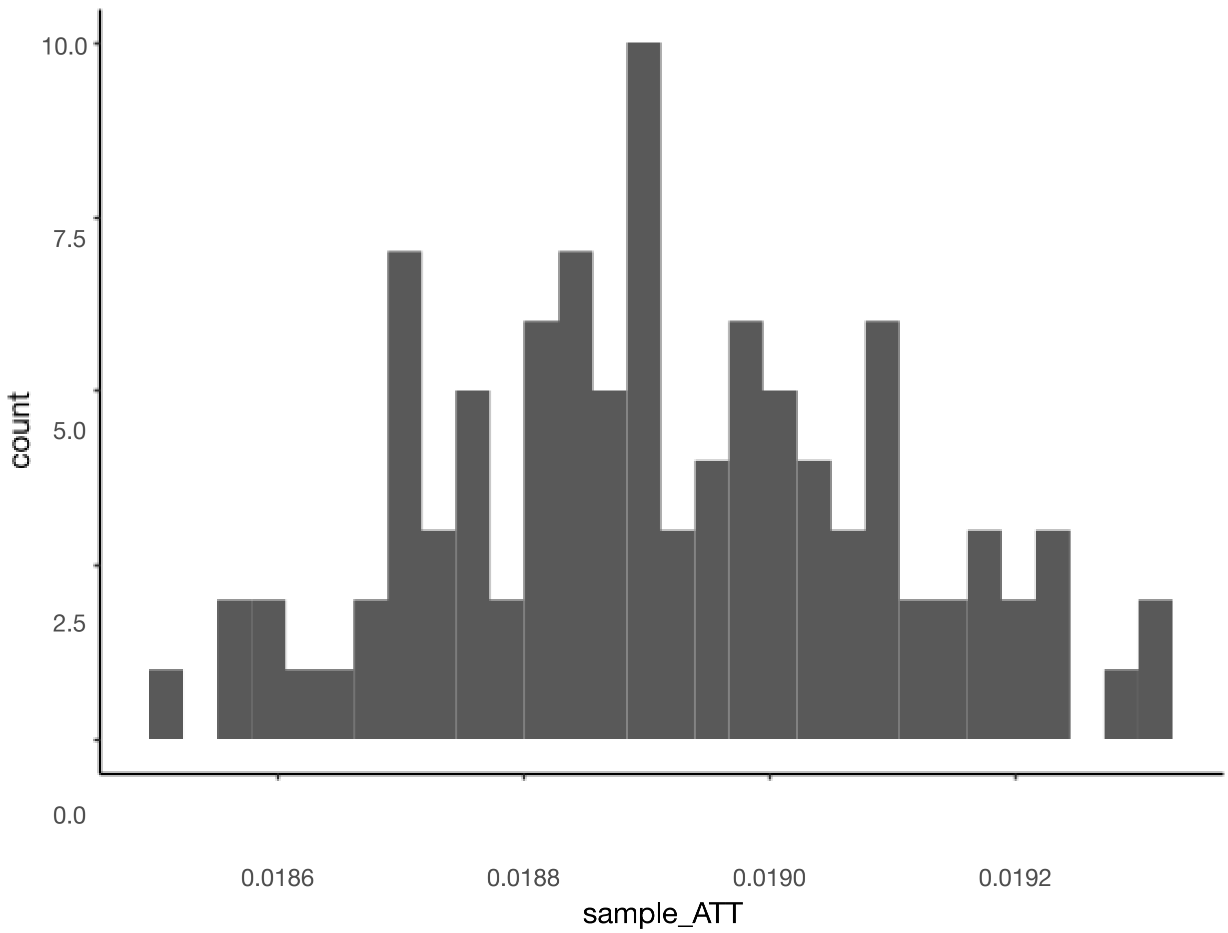
```
##  # A tibble: 10 x 2
##       sample sample_ATT
##       <chr>            <dbl>
##    1  1                0.0190
##    2  2                0.0190
##    3  3                0.0190
##    4  4                0.0189
##    5  5                0.0186
##    6  6                0.0191
##    7  7                0.0189
##    8  8                0.0187
##    9  9                0.0187
##   10  10               0.0191
```

```
# Results in Scatter Plot
mc_estimates2 %>%
   ggplot(aes(x=as.numeric(sample),y=sample_ATT))  +
   geom_point() +
theme_classic()
```




```
# Results in Histogram
mc_estimates2 %>%
  ggplot(aes(sample_ATT)) +
  geom_histogram() +
  theme_classic()
```

```
## 'stat_bin()' using 'bins = 30'. Pick better value with 'binwidth'.
```



Section 4: Experiment Results and Analysis

The Baseline Model Results

The baseline model has the following results:

1. Plot - Parallel Trends Assumption
2. 2 Approaches in Computing the DD Estimate (OLS and 2-way FE estimator)
3. Monte Carlo Methods
4. Plot - Parallel Trends Assumption

Like the model in the paper, the parallel trends assumption holds, as we see that the treated group's counterfactual (dotted blue line) matches the trend of the control group (solid red line). This is bound to change once political will is incorporated into the problem model.

2. 2 Approaches in Computing the DD Estimate (OLS and 2-way FE estimator)

Both OLS and the 2-way FE estimator estimated DD to be 0.033639 in one run. The OLS standard error was 0.014904 while the 2-way FE estimator standard error was 0.014693, a small marginal difference. These results are close to the paper's DD estimate relative to periods 2-3 (the stimulate_data applied intervention after period 2) which is 0.0139. Therefore the baseline model arrived at the same result as the paper - between periods 2-3, there is a small positive effect that being in proximity to an LRT station has on the likelihood of a land use change.

3. Monte Carlo Methods

Due to limitations, our group only ran 100 Monte Carlo simulations. In those simulations, the sample ATT (calculated by subtracting the mean of Y1 from the mean of Y0 for the treated group) hovered around 0.019, which is even closer to the paper's estimate of 0.0139. See the scatter plot and histogram of the 100 Monte Carlo simulations for more detail.

The Problem Model Results

Like the baseline model, the problem model has the following results:

1. Plot - Parallel Trends Assumption
2. 2 Approaches in Computing the DD Estimate (OLS and 2-way FE estimator)
3. Monte Carlo Methods
4. Plot - Parallel Trends Assumption

The weakness our group was exploring was the possibility that the parallel trends assumption was not met (due to the political will variable). As seen in the Structural Equation Models (SEM), Y1 and Y0 did not match, meaning Y1's counterfactual will not match Y0's trend. The plot further proves this as the treated group's counterfactual (dotted blue line) did not match the trend of the control group (solid red line). In many runs, the counterfactual line converged and intersected the control group line.

This violation of the parallel trend assumption means that the paper's estimate of the causal effect is biased. This helps confirm our group's hypothesis that political will was a significant and relevant variable that should be included. The background behind the LRT's development made it clear that the land near the transportation corridor would experience different land use changes than the rest of Minneapolis.

2. 2 Approaches in Computing the DD Estimate (OLS and 2-way FE estimator)

Both OLS and the 2-way FE estimator estimated DD to be 0.054986 in one run. The OLS standard error was 0.064279 while the 2-way FE estimator standard error was 0.031427, a noticeable difference. Compared to the baseline model, these results are farther from the paper's DD estimate relative to periods 2-3 (the stimulate_data applied intervention after period 2) which is 0.0139. Therefore the problem model shows the paper's results will change if additional variables were included. Nevertheless, we now know that this estimate is biased as the parallel trends assumption does not hold.

3. Monte Carlo Methods

Due to limitations, our group only ran 100 Monte Carlo simulations. In those simulations, the sample ATT (calculated by subtracting the mean of Y1 from the mean of Y0 for the treated group) hovered around 0.0189, which is slightly higher than the baseline model's sample ATT estimate (with Monte Carlo methods). This puts it slightly higher compared to the paper's estimate of 0.0139. See the scatter plot and histogram of the 100 Monte Carlo simulations for more detail. But once again, we now know that this estimate is biased as the parallel trends assumption does not hold.

Implications from the Results

Our group's experiment demonstrates the difficulty in truly meeting the parallel trends assumption. Ran- domization is key to any experiment. Unfortunately, the treated and control groups were out of the author's control. Governments would only consider constructing a major transit system like an LRT in areas that need it. Governments likely would not construct an LRT system in a random corridor in Minneapolis that does not connect enough key communities and landmarks as it is a costly investment that could ruin the incumbent party's re-election. So by this logic, it is difficult for the authors to measure the causal effect that transit has on urban redevelopment. Transit would always be placed strategically in key areas, which raises the complexity in measuring the causal effect installing an LRT system in a random area truly has.

There are several things the paper's authors could do moving forward. The authors should consider including other relevant variables like political will in their model. Adding instrumental variables (IVs) is one way to estimate causal effects even with bias present. The authors should also consider using important corridors in nearby cities (like Chicago or Milwaukee) that lack an LRT system (or substitute transit system) as the control group instead of the rest of Minneapolis, as that would be closer to randomization.

Section 5: Conclusion

Major Learnings of the Paper

Our group had several learnings from the paper:

1. Real-Life Challenges in Estimating Causal Effect

The paper has taught us a lot about the challenges in estimating a causal effect outside of a controlled environment (like coding in R). With many factors outside of the researchers' control, it became harder to eliminate bias and have a randomized sample. This means that tools like Instrumental Variables (IVs) and controlling or adjusting variables are more key, as confounders and bias likely are plentiful in many real-world situations.

2. Determining Measurement

While the paper seemed to take land use change and redevelopment as the same thing, it became clear to our group that some cases of redevelopment may not include a land use change. We would believe that a 5-storey residential complex being torn down to make way for a 20-storey residential complex counts as redevelopment even though it did not constitute a land use change. Our group also identified that the results of this study would likely change if we adjusted how the treatment was measured. We observed that in cities like Toronto, transit projects incentivize landowners within a block or two to redevelop their land (and change land use). However, most of the time, a block or 2 falls short of the ½ mile measurement the paper's authors set. Therefore, the causal effect we estimate varies depending on how we measure it.

3. Evaluating Political Wins

Many politicians champion large infrastructure/transit projects completed under their term (or started by them) as instrumental in redeveloping and improving a specific area within their jurisdiction. However, this paper has taught us that such land areas primed for infrastructure/transit are likely in the running for a reason (importance) and therefore would experience different changes from the average land parcel. Like Minneapolis (where planners wanted a transit system since the 1950s), many infrastructure/transit projects are called for long before a politician in power decides to build it. When you consider the counterfactual (perhaps the opposing party wins and enters office instead), you realize that if a project is demanded by consensus, it likely will be built, regardless of the specific politician in power. The only question that remains is when the project will be undertaken.

Potential Weaknesses Shown

Our group demonstrated that the paper had a major weakness: it likely did not meet the parallel trends assumption, which means bias is likely present within the results. The transportation corridor in Minneapolis holds significance and political importance that the average land in Minnesota likely does not receive, which means the counterfactual of this land (without an LRT system) would likely not match the trend of the rest of Minneapolis when it comes to land use changes. Our experiment shows that when the political will variable is added, it becomes clear that the parallel trends assumption does not hold given the treated group's special properties. This can be seen from the plot and different structural equation models (SEMs). Therefore, the paper likely had some bias within its estimates given the parallel trends assumption does not hold.

Implications

The analysis our group conducted affect how the results of the study should be interpreted. Knowing that the parallel trends assumption is likely broken means that the paper's results likely are biased. The inclusion of an additional relevant variable (political will) that highlighted the differences the treated group (and its counterfactual) had from the control group proves the parallel trends assumption is likely broken. This means that we should take the results with a grain of salt and ideally build on the researchers' work and build a model that both accounts for more relevant variables and meet the parallel trends assumption.