Are People Staying at Home? Capturing the Effects of Lockdown Policies on Mobility in the US and China

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Abstract

We study mobility data gathered by Google and Baidu to measure the direct effect that the lockdown policies had on social distancing in the United States and China. In the United States, we specifically examine the different responses to stay-at-home orders in counties based on their voting share (Democratic/Republican) in the 2016 election. We find that stay-at-home orders do not have a different direct effect on more Democratic or Republican counties, though Democratic counties were more likely to decrease mobility before the orders were issued. The direct effect of the stay-at-home order on mobility ranges from 2% to 13% depending on the type of district (parks, transit, grocery, retail, workplaces, residential), only increasing in residential areas. In China, we study the mobility changes due to different levels of lockdowns implemented across the country, ranging from complete shutdown, like in the Hubei province, to quarantine zones and other mobility restrictions. The initial complete lockdown was responsible for about a 70% decrease in mobile people as a percentage of the population, and subsequent less-stringent lockdown measures did not significantly drop mobility further. Both of these results are robust up to controls on demographics, time, and coronavirus case numbers.

1 Introduction

When the Chinese CDC reported on the human-to-human transmission capability of the novel coronavirus (COVID-19) on January 20th 2020, the Chinese government soon after imposed a lockdown on the city of Wuhan as well as its surrounding cities in the Hubei province in an attempt to halt the spread of the highly infectious virus. Many other countries also implemented policies that restrict mobility as cases introduced by international travelers begin to emerge and increase rapidly within countries such as the United States, Italy, Korea, to name just a few. As countries' leaders around the world responded differently to their situations, we observe that citizens also varied in their responses to the mobility-restricting policies enacted by their respective governments. In Wuhan, the amount of people traveling within the city had decreased as much as 99% shortly after the lockdown had been imposed. While in New York

City, mobility had only dropped to about half of the city's baseline after a state-wide stay-at-home order had been issued. Such disparity in mobility change is especially alarming given that the confirmed count of cases in New York City alone is more than 4 times the total amount of confirmed cases in Wuhan, and the confirmed death count in New York City exceeds that of Wuhan by more than 5 times. Since it is empirically challenging to both obtain granular human mobility and disease occurrence data, and quantify the impact of human mobility on the spread of the virus (Fang et al., 2020), we focus instead on quantifying the impact of stay-at-home orders across the US and lockdown measures across China on human mobility.

The reasoning behind our objective is two-fold. First, it is important for a government to understand and evaluate the effectiveness of an enacted policy. In this particular context, understanding the response to a mobility-restricting policy helps inform government decision-making and policy-making when new waves of COVID-19 cases, or new strains of virus, emerge in the future. Second, we choose to focus on comparatively analyzing the coronavirus responses of United States and China because of their dichotomy in political systems and the roles they have played amid the global pandemic.

In this paper, we exploit mobility data from Google and Baidu (top search engine in China) to estimate the effect of stay-at-home orders and lockdown policies on mobility change across US counties and Chinese cities. Using double-LASSO to select control variables for our fixed effects models, we find that in the United States, stay-at-home orders account for about a 4-13% drop in mobility in parks, transit, retail, grocery, and workplace areas, while it is responsible for a 2% increase in mobility in residential areas. We also provide upper bounds for our estimate of these mobility effects in the case of our assumptions failing, though we argue that our measures are robust. And, we find that we cannot conclude with confidence that Republican counties are more or less affected by stay-at-home orders than Democratic counties, though Democratic counties experience higher mobility decreases with or without the orders. In China, using the same type of fixed effects model, we provide a large range of 47% to 73% for the mobility decrease caused by the most strict type of lockdown in the Hubei province, though we argue that the true parameter is closer to 70%, which is in line with current estimates in the literature. Even more so, we conclude that less stringent lockdown procedures like quarantine zones or partial shutdowns do not significantly decrease mobility after the complete shutdown was initiated, as our estimates were not statistically different from 0.

Stay-at-home orders in the US

On March 16th, 2020, with almost 600 cases and over a dozen deaths reported in California, seven Bay Area counties in California became the firsts to officially declare a shelter-in-place order. Three days later, California became the first state to urge all residents to stay at home, except to obtain food, prescriptions, healthcare and those who are essential personnel working in one of the 16 critical infrastructure (e.g. healthcare, transportation, communications, etc.) during COVID-19 identified by the Cybersecurity & Infrastructure Security Agency (CISA). Other states soon followed suit as reported cases had begun to spread rapidly throughout the country. By mid-April, 42 states had issued statewide stay-at-home (shelter-in-place, on PAUSE) orders, while three states had partial stay-at-home orders declared in populous counties and cities. In addition to residents sheltering-in-place, schools were closed, large-scale events cancelled and public gatherings forbidden.

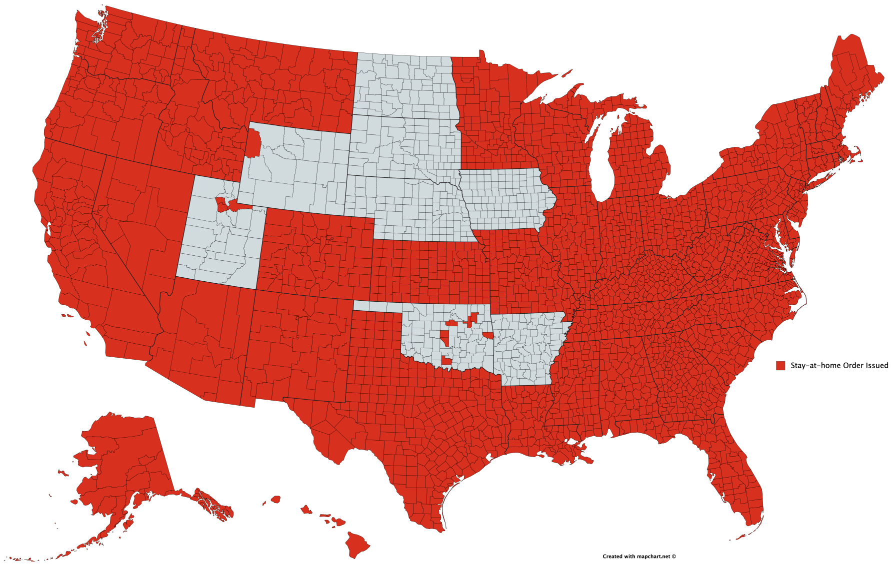

Figure 1: Stay-at-home orders issued across US Counties by mid-April

Lockdown Policies in China

On January 23rd 2020, airports, train stations, buses were shut down as the entire city of Wuhan went into complete lockdown. Roads and highways were also closed to private vehicles as residents were forbidden to leave the city. Entrances of all the residential compounds were also tightly controlled as outsiders were prohibited. Over the course of next two days, major cities surrounding Wuhan and all populous cities within Hubei province also went into complete lockdown. In the next week, seven other cities surrounding the Hubei province enacted partial lockdowns, where highway toll stations, within-city metro and buses were suspended, and residents' trips out of their homes were limited to once every two days. For the next week and a half, major cities around the country began implementing similar measures of establishing checkpoints and quarantine zones in public venues and residential compounds as cases trickled in. Public transportation and commercial buildings remained in operation as residents' temperature and health status were recorded.

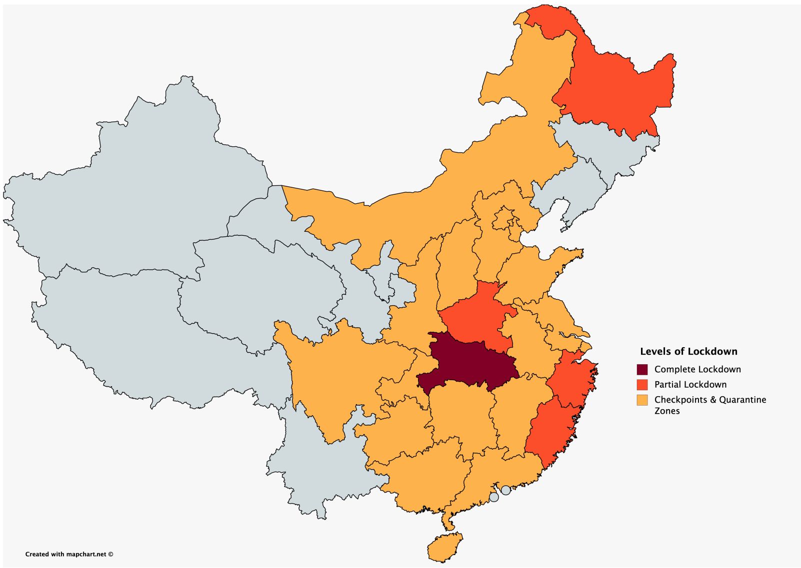

Figure 2: levels of lockdown across China

2 Literature Review

Even though our question is a recent and ongoing subject matter, there is a collection of literature that analyzes how people respond to quarantine measures in both US and China. In "Staying at Home: Mobility Effects of COVID-19" (Engle et al., 2020), the authors construct a simple model of individual decision to travel based on estimated perception of risk and utility cost of contracting the virus. The authors estimate at the baseline that a restriction order reduces mobility by 7.87% of a county with median demographic characteristics (Republican voter share, age structure, population density), as well as median perceived risk of contracting the virus estimated with COVID-19 prevalence in local and neighboring counties and county-level population demographics. They also find that perturbing the demographic characteristics one at a time by one standard deviation yields different magnitudes of the effects of the restriction order on mobility. This study is similar to ours in that we are also interested in how a specific demographic characteristic such as voter share determines the amount of impact a stay-at-home order has on mobility. However, we aim to select demographic features by using double-LASSO, instead of assuming how each of them function in an individual decision model.

In "Political Beliefs affect Compliance with COVID-19 Social Distancing Orders" (Painter et al., 2020), the authors include county and date fixed effects to study how political beliefs heterogeneously affect compliance with social distancing orders. They estimate that the change in proportion of people who completely stay at home is 1.4 percentage points higher in counties with a state-level policy relative to those without. Furthermore, they find that people in Republican counties respond less to social distancing orders relative to Democratic counties; specifically, one standard deviation increase in county-level voter share for Donald Trump in 2016 is associated with a 0.6 percentage point decrease of people who stay at home after an order relative to an average county. This study is similar in nature to ours in that we are also interested in isolating the effects of a stay-at-home order on mobility and how partisan differences impact compliance. However, we aim to also consider the effect of the declaration of national emergency on the heterogeneity of stay-at-home orders response across counties with varying levels of democratic voter share.

For China, a paper that provides insight on the causal impact of lockdown in Wuhan is "Human Mobility Restrictions and the Spread of the Novel Coronavirus (2019-nCov) In China" (Fang et al., 2020). The authors use the difference-in-differences method to disentangle the causal effect of the lockdown on mobility as well as on the spread of confirmed cases from confounding factors the virus deterrence effects (curtailed human movement even in absence of a lockdown). To isolate the lockdown effect from the virus effect, the authors include data from seven other cities in Hubei that had not gone into lockdown until February 2nd and February 4th as a control group. They find that the lockdown in Wuhan caused a 54.16% decrease in within-city movements. Our study is similar in that we also look to quantify and isolate the the causal impact of the lockdown effects by having more control variables included in our fixed effects model across cities and dates. But we also look to analyze the responses of different levels of lockdown policies in other cities across China, and assume that our demographic characteristics and features selected could account for people's perception of risk associated with in-city movements.

3 Data Description

United States

We construct a county-level panel data for the United States with dates ranging from February 15th to May 25th. It includes the following information:

Google COVID-19 Community Mobility Reports

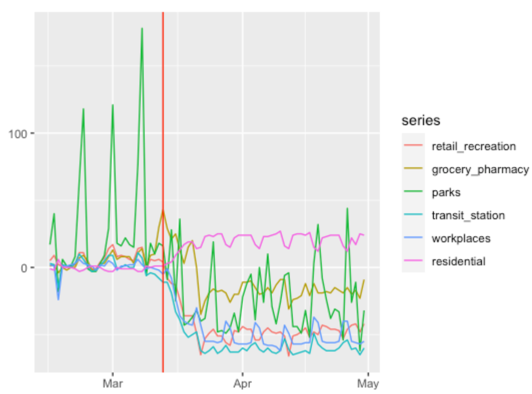

Figure 3: Mobility in Cook County, Illinois at the Onset of the US National Emergency (Red)

Google aggregates anonymous location history on individual user's mobile devices and publishes daily, county-level mobility percentage change from the baseline of that weekday in different categories of locations, including residential, workplaces, groceries and pharmacies, transportation, retail, and parks.

MIT Election Data Science Lab County Presidential Election Returns

We obtain county-level demographic information including age structure, racial distribution, election results, gender distribution, income, education and unemployment from this dataset.

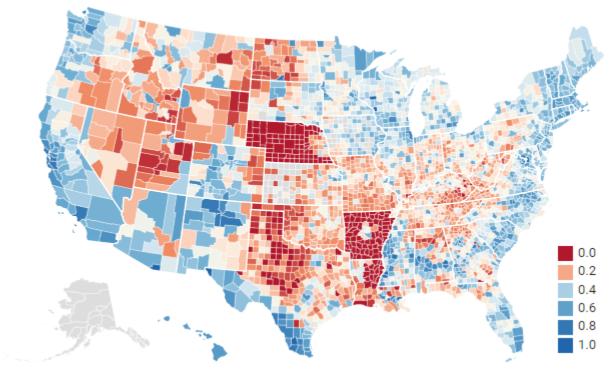

Figure 4: 2016 Democratic Voter Share across US Counties

USDA Demographic Data

We use more data from the USDA to supplement our county-level information from the MIT Election Lab. This data includes more information on population, income, poverty, and other controls.

COVID-19 Cases by County

We use a dataset from the Center for Disease Control that has information of each county's number of confirmed cases, deaths, suspects, and recovered.

Stay-At-Home Orders

We use a compiled dataset from the New York Times that has information on whether a county has an enacted a stay-at-home order and when that order is enacted.

China

We construct a city-level panel data for China with dates ranging from January 10th to March 15th, including the following information:

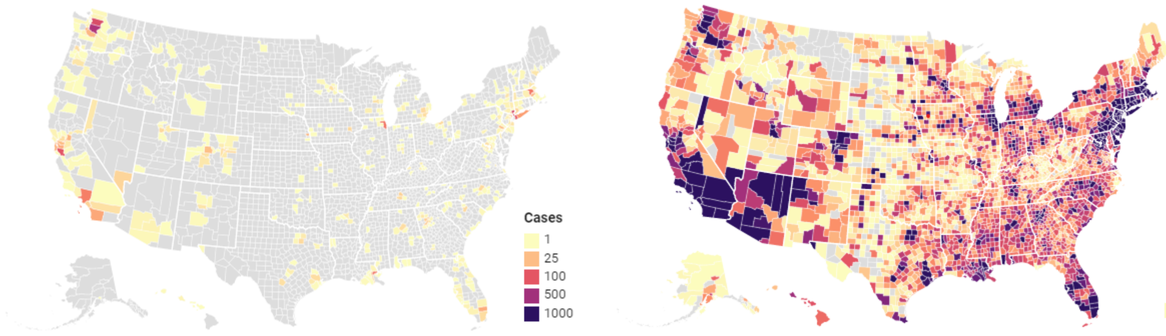

Figure 5: County-Wide Cumulative Case Counts on March 15th (left) and June 1st (right)

Baidu Qianxi (Mobility)

Baidu Maps releases annual Spring Festival migration data of population outflow, inflow and within-city movements of major cities in China using location data of individual users' mobile devices. For our purposes, we scraped within-city Movement Intensity Index, calculated by $\log(\frac{\text{mobile people in the city}}{\text{population of the city}})$ of around 50 major cities in China from January 10th to March 15th. This dataset serves as our human mobility observations.

For proper comparison purposes to the Google Mobility dataset, we computed the percentage change in mobility from the baseline of January 1 - January 15, before the epidemic settled in. The percentages are calculated controlling for the day of the week.

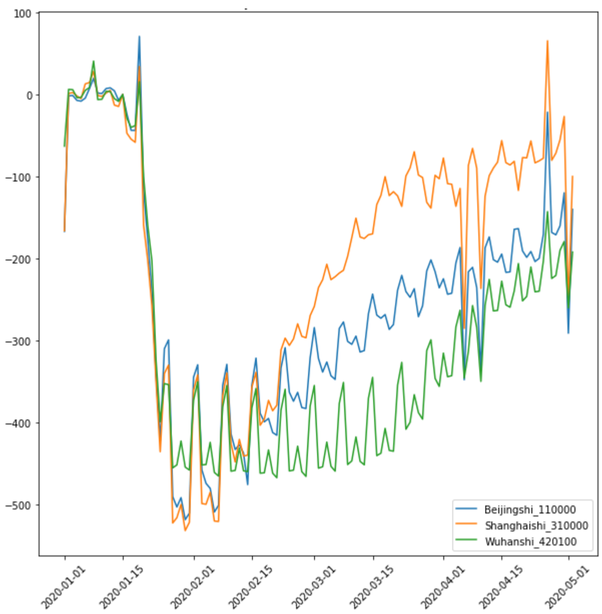

Figure 6: Mobility Differences in Beijing, Shanghai, and Wuhan

Population by City

As we would have liked to obtain official China Census data that includes a much wider array of demographic characteristics, we were unable to given the resources we had. Thus, we were only able to obtain 2019 population data by city from UN World Population Prospects.

Levels of Quarantine Measures in Various Cities

We obtain a compiled dataset from (Fang et al., 2020) that includes all the cities across China that has enacted one of the three levels of restriction measures and the date of which it became effective.

COVID-19 Cases by City

We use data from the Center for Disease Control (China) that includes city-level COVID-19 confirmed cases, deaths, suspects and recovered.

4 Research Design

Many factors contribute to the heterogeneity of how people adhere to quarantine policies. The stringency of the policies, the political and social structure behind the regions they are enacted in, and the perceived risk or severity of the stay-at-home phenomenon are all examples of the elements that contribute to the population's response. In analyzing the impact of quarantine orders in the United States, there are many potential confounding variables at play that are potentially related to the stay at home order, mobility, or both. In fact, some of these variables are inherently interesting with respect to the mobility response after stay at home orders. The share of Democratic vs. Republican votes in a county, for example, seems to tell us a little more about how a county responds to a stay at home order. In China, control variables designating the tier of a lock-down tell us far more about the mobility response than a standard stay-at-home indicator variable.

Our first goal is to narrow down the factors at play when analyzing the impact of stay-at-home orders. To do this, we gathered a variety of data on mobility, census demographics, the progression of the coronavirus, government policies, election results, and population densities at a county level across time. More details on this data can be found in section 3. Using this set of control variables, along with interaction terms and the stay-at-home indicator variable, we utilize the double-LASSO method to identify which covariates are empirically relevant for inclusion in our model, and to prevent the potential over-selection of colinear covariates.

Next, to address the panel nature of the data on the county and date levels, we use the control variables that were not dropped in the double-LASSO method in a Fixed Effects Model on dates and counties in the US, with the goal of identifying the significance and directions of the coefficients of the treatment variable, the stay at home order indicator, and a subset of the chosen control variables that are of interest.

Double LASSO

LASSO (Least Absolute Shrinkage and Selection Operator) regression solves the following minimization problem:

$$\min_{\beta}\left[\sum_{i}^{n}(Y_i - \beta_0 - \beta_1 D_i - \beta_2 X_{1i} - ... - \beta_{n+1} X_{ni})^2 + \lambda \sum_{k} |\beta_k|\right]$$

Where Y_i is the dependent/response variable, D is the treatment variable of interest, and $X_1, ..., X_n$ are the control variables. The notable difference between LASSO regression and linear regression is the inclusion of the regularization term, $\lambda \sum_k |\beta_k|$. This regularization term is the product of a regularization parameter, λ, and the L1 Norm, and functions as a "penalty" for large magnitudes of β_i. Due to the nature of the L1 Norm, combined with a proper regularization parameter, LASSO effectively helps shrink regression coefficients towards zero if they do not contribute much towards predicting the response variable. This is an enticing way to help us achieve a model as parsimonious as possible and to remove potential multicollinearity in our dataset.

However, pure LASSO regression has some setbacks. LASSO emphasizes prediction over causal inference, and as a consequence of minimizing a loss function that penalizes large regression coefficients, some of the covariates that have small to moderate sized true coefficients may end up with a LASSO coefficient of zero. This is especially the case when the covariance between the dropped regressor and the treatment variable is non-negligible. This introduces omitted variable bias in the regression that potentially erroneously assigns less weight to the control and more to the treatment variable - not because the true coefficient is insignificant, but because the magnitude of the coefficient is penalized and the predictive power of the variable is misassigned to the treatment variable.

To resolve this, we employ the method for double-LASSO described in "Inference On Treatment Effects After Selection Amongst High-Dimensional Controls" (Belloni, et. al., 2013) for deciding which covariates are empirically reasonable for inclusion in our model. This is done in two steps:

1. Fitting a LASSO regression regressing the treatment variable, the stay-at-home indicator SAH, and a set of covariates, Γ, on the dependent variable, $Mobility$.

$$Mobility_i = \alpha_{M0} + \alpha SAH_i + \theta_M \Gamma_i + \epsilon_i$$

2. Fitting a second LASSO regression regressing the set of covariates, Γ, on the treatment variable, SAH.

$$SAH_i = \alpha_{S0} + \theta_S \Gamma_i + v_i$$

Then, we retrieve the union of the control variables that are not dropped in step 1 and step 2 for inclusion in our model. In other words, we only drop the variables that have a zero coefficient in both steps of the double LASSO process. We can see that this relieves the problem posed earlier - if the first regression drops a particular covariate that is highly correlated with the treatment variable SAH, then it would likely not be dropped in the second step.

We performed this variable selection routine using our data (and some intuitive interaction terms, interpreted in section 5) for both the United States and China, which we separate into two model specifications in section 5, and for the district categories of retail, grocery, parks, transit, residential, and workplaces. Due to the scale-invariant nature of LASSO's penalty term, the regressors are all normalized before running the regression. The penalty parameter is chosen through 5-fold Cross Validation for each step in the process. The results for residential and workplace mobility data are shown in `Table 1`, with the rest depicted in `Appendix A`.

The results of our double LASSO implementation help point us towards our eventual fixed effects model specification. Notably, the covariate and interaction terms associated with democratic voter share in each county are important in each district, which came initially as a surprising result and helped guide our analysis into partisan behavior. Because of this, we chose to focus substantially on the partisan effects on mobility and its interactions with the stay at home order as a possible key insight into the mobility responses. We can also see that roughly half of the original covariates from `Table 1` are dropped, with results varying slightly between different districts. For the sake of comparison, we chose a uniform set of control variables that have an effect on and are not dropped for most of the districts, so that we can compare results between across mobility types. These variables include democratic voter share, the Coronavirus case count, the interaction between voter share and the stay at home indicator, the foreign born population percentage, age 29 and under percentage, age 65 and older percentage, Civilian Labor Force unemployment percentage, percentage of education less than high school and college, and the rural percentage for the population in counties across the United States. Interestingly, many of the variables dealing with race, income, and gender were dropped by the double LASSO model, with the exception of the Residential and Parks mobility districts. We found that for the parks and residential neighborhoods, there were a lot fewer dropped covariates than for the retail, workplace, travel, and grocery districts, an intuitive result following the sharper drop in mobility for the latter districts due to Coronavirus factors/quarantine policies. These two districts, particularly Residential, responded the least in terms of mobility to the stay at home orders, and pose the outliers in our uniformity assumption. The percentage of female citizens, for example, seems to help predict residential mobility, as does the poverty level and the age group percentage statistics. In parks, where mobility has decreased the least, the only dropped variables are the percentage of non-whites and the percentage of citizens with less than a college education (Results can be found in `Table 6`). These results are interesting with respect to these

Table 1: USA Model: Variable Selection

	Residential		Workplaces	
	(1)	(2)	(1)	(2)
SAH	6.11		−21.3	
VS	−1.86279	−0.87595	4.98380	−0.87595
income	−0.00000	−0.00000	−0.00000	−0.00000
pop	−0.00000	−0.00000	0.00000	−0.00000
pctpov	0.04363	−0.00000	−0.00000	−0.00000
cases	0.00003	−0.00000	−0.00005	−0.00000
VSxSAH	13.28257	1.84466	−24.72960	1.84466
VSxcases	0.00000	−0.00000	−0.00005	−0.00000
popxpctpov	−0.00000	−0.00000	0.00000	−0.00000
SAHxpop	0.00000	−0.00000	−0.00000	−0.00000
pctwhite	0.00000	0.00000	−0.00000	0.00000
pctblack	0.00000	−0.00000	0.00000	−0.00000
pcthispanic	0.00000	−0.00000	0.00000	−0.00000
pctnonwhite	−0.00000	−0.00000	0.00000	−0.00000
pctforeignborn	0.09150	−0.00000	−0.04562	−0.00000
pctfemale	0.09474	−0.00000	−0.00000	−0.00000
pctage29andunder	0.01794	−0.00000	0.00000	−0.00000
pctage65andolder	−0.01709	0.00000	0.00000	0.00000
medianhhinc	0.00006	−0.00000	−0.00002	−0.00000
pctclfunemploy	−0.21587	−0.00000	0.02238	−0.00000
pctlesshs	−0.11508	−0.00000	0.06003	−0.00000
pctlesscollege	−0.03386	0.00000	0.25771	0.00000
pctlesshswhites	0.18301	0.00000	−0.10400	0.00000
pctlesscollegewhites	−0.01618	0.00000	0.00000	0.00000
pctrural	0.01477	0.00000	−0.00606	0.00000
ruralurbancc	−0.39910	0.00000	0.77431	0.00000

Double LASSO Variable Selection for Residential and Workplace Neighborhoods in the US.

specific areas, but are less generalizable for our comparisons in section 5. Therefore, we chose to focus on the aforementioned set of control variables that are significant for our fixed effects model. We will briefly return to these "outlier" selection results for parks in our analysis of the results in section 5.

To compare our results globally through a separate model for China, we repeated the double LASSO method above using our China dataset. Unfortunately, census demographic, political, and economic data pertaining to regions in China was much more difficult to obtain, so our variables under consideration are restricted to a much smaller set. We identify in `Table 8` that potential candidates for our China model specification include the quarantine Policy Type (A: Complete Shutdown, B: Partial Shutdown, or C: Quarantine Zones), their interaction with the stay at home indicator, as well as population and Coronavirus case counts in each Chinese city. We chose the interaction variables for our final model specification as they are intuitively better than the standard stay at home indicator variable since they also include information about the quarantine policy type.

Fixed Effects Model

Now that we have picked out our relevant control variables, we proceed with a fixed effects model on county and date in the US and on city and date in China to obtain the direct effect of lockdown policies in both areas. In the United States specifically, we include county and date fixed effects to determine the mobility response directly motivated by the enactment of a stay-at-home order and not by a heightened caution of the coronavirus pandemic. As we cannot assume homogeneity of county response to the coronavirus, given different demographics, exposure to the virus, individual and group decision making, and county-level orders, we use a county-level fixed effects to try to better estimate the stay-at-home order response across the country. Including date fixed effects in the US model is a bit more complicated, however. On March 13, 2020, a national emergency was issued, and on March 16, 2020, the California governor placed a number of California counties in lockdown, triggering a massive decline in mobility across the nation before stay-at-home orders were issued. On top of our date fixed effects, to better detect the effect of the orders on mobility without this large nationwide movement, we also run a fixed effect models on dates only after these events occurred in our "Post Nat Emergency" column that we describe in the next section. In this way, we generate a more robust estimate for the stay-at-home order effect that does not include the spillover effect from initial response and caution to the disease.

In China, we also run a city and date fixed effects model, as different cities and provinces experienced lockdowns at different times throughout 2020. However, China experienced a massive country-wide mobility shock once the initial Hubei complete lockdown was implemented, and only after were less strict lockdown policies issued across the country in different provinces. As a result, our results using the date fixed effects underestimate the true parameter for the effect of the lockdown order in a way that is more difficult to parse than in the United States. Specifically, both the initial lockdown and country-wide national response and awareness occurred at the same time in China, but not in the United States. So, we also include a fixed effects model without date as a factor to obtain a range that the true lockdown effect had on mobility. The resulting models, assumptions, and results are explained in more detail in section 5.

5 Model and Results

We analyze the effect that different implementations of stay-at-home and other lockdown orders had on the mobility of different populations. Specifically, we use our mobility data spanning from January to May gathered by Google and Baidu to extract the quantitative drop in mobility directly attributable to these orders. In both regions, we use a fixed effects model to account for heterogeneity in county or city and to identify time-varying responses to the pandemic, most easily seen beginning mid-March in the United States after a national emergency was issued and widespread information about the coronavirus dropped mobility without any direct orders. In doing so, we try to determine the direct effect of stay-at-home orders in the context of a heightened perception and worry about the virus in both the United States and China. In particular, in the United States we study how political leanings affect mobility responses and are able to capture the effect of stay-at-home orders with respect to partisan leanings. In China, we compare the effectiveness of varying levels of quarantine stringency across different provinces.

United States

General Specification

We use the following specification to determine the drop in mobility attributable to stay-at-home orders in the United States:

$$Mobility^i_{c,t} = \beta SAH_{c,t} + \gamma \log(\text{cases})_{c,t} + \delta' \Gamma_c$$

where $Mobility^i_{c,t}$ is a measure of mobility percentage changes to baseline movement in a particular county in a date, with i representing the type of mobility (residential, workplace, retail, grocery, transit, and parks). $SAH_{c,t}$ is an indicator for whether a stay-at-home order was enacted in a county on a date. We also include a coefficient that measures change in movement to the number of cases that we use as a proxy for general perception of the virus. And, we include a number of county-based controls, Γ_c, as determined by our LASSO in section 4, that include information on county demographics. We include county and date fixed effects as mentioned before. In doing so, we cluster the standard errors by county. So, β would be our coefficient that denotes the percentage drop in mobility due to orders.

As mentioned by (Painter, et. al., 2020), there is some room for endogeneity when considering our fixed effects regression. Mainly, we see endogeneity problems arise when stay-at-home orders and mobility responses are both affected by an unobserved "perception" parameter, or how a county/state views the intensity of the COVID-19 infection. As they argue, there are variations in the level of quarantine measures in different states as well as timing of enactment of the order which could be endogenous to how state politicians and government leaders assess the threat of the virus. For example, in seeing that mobility has not decreased enough or that the infection rate continues to climb, only then will a governor enact an order, possibly to a selected number of counties or districts that are most vulnerable. By including our coefficient on the number of cases in the county and accounting for national response through our date fixed effects, we hope to reduce the effect of endogeneity on the accuracy of the model. Other sources of potential endogeneity issues also include the different new sources consumed by counties, possibly affecting response to movement and adherence to stay-at-home orders, as well as pressure to enact, maintain, or remove an order. Unfortunately, we could not find accessible data on networks watched or internet sources consumed in our model, though work by (Barrios, et. al., 2020) shows a strong correlation between internet searches of coronavirus and related terms and Trump vote shares in that county in the 2016 election. And, we do not have information on the relative strengths of the stay-at-home orders across different states and counties, unlike our data in China. As a result, we assume that these sources of endogeneity do not influence the model in a sizable way, though we also try to account for differences in general perception about the virus in another model that studies the partisan effect on mobility.

Ultimately, we find that a stay-at-home order issued accounts for about a 4% drop in mobility in workplaces and a 2% increase in mobility in residential areas, among the other measures of mobility provided by the Google data. Drops in movement are significant at the lowest α levels for all measures of mobility. We highlight changes in mobility in workplaces and residential areas in our figures in the Results section, though all of the different types of mobility movements bring a slightly different issue of endogeneity (workplaces could be disproportionately affected by county-specific exposure to industry, transit is related to changes in workplace movement, etc.). We assume that these do not affect our results greatly given our controls and fixed effects specifications. As expected, movement in residential areas is the only measure of mobility to increase with the enactment of a stay-at-home order.

Partisan Differences

As mentioned before, one potential endogeneity problem stems from the effects of unobserved perception on movement and the issuance of stay-at-home orders, which is strongly related to political leanings according to existing literature. And, studying the difference in response to stay-at-home orders by a more Republican or Democratic county is especially interesting to policy makers. As a result, we include the variable VS in our model, which is a measure of the democratic vote shares of the county in the 2016 election. We normalize this value to have mean 0 and standard deviation 1 to better interpret our coefficients from our fixed effects regression. By including its interaction with our terms in our existing specification, we determine how much political leaning affects mobility as perception of the pandemic grows and as stay-at-home orders are implemented. We have the following specification:

$$Mobility^i_{c,t} = \beta_0 SAH_{c,t} + \beta_1 VS_c + \beta_2(SAH_{c,t} \times VS_c) + \gamma_0 \log(\text{cases})_{c,t} + \gamma_1(\log(\text{cases})_{c,t} \times VS_c) + \delta'\Gamma_c$$

The existing regressors are the same, and we include their interactions with our partisan measure VS. In this model, we focus on β_0, β_1, and β_2 to obtain the interaction of partisan leaning with order implementation. Again, we cluster standard errors by county.

However, there are still a few issues that are not addressed by including a measure of political leanings. Specifically, the party of the governor and state officials in charge of implementing a stay-at-home order is influenced by the voter shares of the county, though we do not included it in our regression. And counties that have the same political leaning could be exposed to different levels of media exposure and other endogenous factors that would influence movement. But, we believe that including a political bias measure reduces our endogeneity problems from our general specification, and as a result, our new partisan specification also should not have a significant endogeneity problem. Again, we also include results from another fixed effects regression only using dates after the national emergency in the United States.

We obtain results for our β_0 and γ_0 parameters that are consistent with the results from the general specification. We also find a statistically significant result that Democratic counties are more likely to see a drop in mobility even before a stay-at-home order was issued. However, our interaction terms of the number of cases and SAH with VS shows a more complicated result, pointing to different effects in our regression using all dates and in our regression using dates after the national emergency. We cannot say with confidence that the stay-at-home orders affected Democratic counties more than Republican counties, even though these counties had different changes in mobility to factors that were not the stay-at-home orders.

Results

We highlight our results on the mobility changes in workplaces and residential areas. We will also discuss results on parks, transit, grocery, and retail areas, with more details offered in `Appendix A`. We focus here on residential areas, as they were the only sector to see an increase in movement, as expected, as more people spend time at home during quarantine. `Table 2` shows our results for workplaces.

All of the coefficients represent percentage changes to mobility. Columns (1) and (3) contain the results for our general specification fixed effects model in our two different date settings, and columns (2), (4), and (5) contain the results for our partisan differences model. We include column (5) as a fixed effects regression without using date as a fixed effect as a reference to obtain an upper bound for the stay-at-home order effect. Specifically, while not controlling for nationwide time varying response after the national emergency was issued, we attribute any change in mobility caused by caution around the coronavirus after the initial big drop in

Table 2: Stay At Home Order Mobility Responses (US, Workplaces)

	All Dates		Post Nat Emergency		
	(1)	(2)	(3)	(4)	(5)
SAH	-4.72^{***}	-4.59^{***}	-3.509^{***}	-3.237^{***}	-8.699^{***}
	(0.133)	(0.130)	(0.122)	(0.115)	(0.183)
log(cases)	-2.23^{***}	-1.842^{***}	-1.282^{***}	-1.224^{***}	1.013^{***}
	(0.059)	(0.059)	(0.073)	(0.072)	(0.063)
VS		-0.694^{***}		-2.468^{***}	-1.639^{***}
		(0.187)		(0.252)	(0.285)
SAH $\times$ VS		-0.452^{**}		0.298^{*}	-0.453^{*}
		(0.170)		(0.140)	(0.193)
log(cases) $\times$ VS		-0.272^{***}		-0.050	-0.170^{**}
		(0.039)		(0.048)	(0.065)
Observations	214,944	214,941	152,985	148,924	148,992
County FE	Y	Y	Y	Y	Y
Date FE	Y	Y	Y	Y	N

mobility to the stay-at-home order. Obviously, this assumption is not accurate, but it allows us to obtain an upper bound on the effect that the stay-at-home orders had on mobility of a 8.7% drop in mobility in workplaces. We use dates after the national emergency to obtain a better estimate of the true parameter, and we can consider this an upper bound in the event that our exogeneity assumptions are inaccurate in the real world. The other coefficients in column (5) don't give us a similar intuition.

In all of our regressions, our stay-at-home parameter ranges from a 3.3% to a 4.7% drop in mobility in workplaces. As expected, an increase in the number of cases also sees a decreased mobility, resulting in about a 1.3% drop in mobility with each log increment of cases, using data only after the national emergency and California lockdowns.

As mentioned before, our results on the effects of political leanings are especially interesting. Using all dates in our dataset, we find that a county that is one standard deviation more Democratic (in 2016 vote shares), sees a statistically significant 0.7% drop in mobility outside of the order, and a 0.45% mobility drop once the order is issued. However, when we only use dates after mid-March in our regression, the mobility change due to political leaning independent of the orders increases to a 2.5% with each standard deviation more Democratic a county is, and the effect of the stay-at-home order interaction is now statistically significant in the other direction. Here, we have a 0.298% increase in mobility with a standard deviation more Democratic after the order enactment, or a drop in mobility for more Republican counties. These two results suggest that we cannot conclude that stay-at-home orders more directly affected Democratic or

Republican counties. Rather, we conclude that more Democratic counties were more likely to decrease mobility before stay-at-home orders were issued, and the actual implementation of the order affected everyone in a way that is not significantly affected by political bias. Even more so, our case number interaction regressor, log(cases) $\times VS$, in column (4) is not statistically different from 0, indicating again that Democratic and Republican leaning counties respond about the same to an increase in case numbers, which is our proxy for county-wide awareness of the disease. So, the differences in mobility response attributable to differences in political leanings must stem from another factor that is not the stay-at-home order or county-specific case number. We also highlight our results on residential areas in `Table 3`.

Table 3: Stay At Home Order Mobility Responses (US, Residential)

	All Dates		Post Nat Emergency		
	(1)	(2)	(3)	(4)	(5)
SAH	1.914***	1.797***	1.495***	1.456***	4.003***
	(0.075)	(0.081)	(0.059)	(0.065)	(0.140)
log(cases)	1.259***	0.868***	0.634***	0.509***	−0.879***
	(0.042)	(0.043)	(0.050)	(0.050)	(0.043)
VS		0.392***		1.190***	1.161***
		(0.098)		(0.152)	(0.169)
SAH × VS		0.270**		−0.111	0.195
		(0.091)		(0.091)	(0.148)
log(cases) × VS		0.184***		0.110***	0.151***
		(0.022)		(0.029)	(0.043)
Observations	104,099	104,096	67,914	67,911	67,978
County FE	Y	Y	Y	Y	Y
Date FE	Y	Y	Y	Y	N

We obtain similar, complementary results as those from the workplace data in our residential mobility data. Our increase in movement in residential areas ranges from 1.4% to 1.9% increases in mobility due to the stay-at-home order, with an upper bound of 4.0%, using the same reasoning as we did before. Again, the other coefficients in column (5) do not give us much meaning. And, we also conclude that more Democratic counties are more likely to shift mobility to residential areas independent of the issuance of stay-at-home orders. Interestingly, we are also unable to determine with confidence that stay-at-home orders affected the movement of more Democratic/Republican counties in different magnitudes, using data after mid-March. This effect is not specific to workplace or residential data. In all of our mobility measures, the coefficient of our $SAH \times VS$ regressor is not statistically different from 0 or switches sign in either the dataset with all dates or the one with dates only after mid-March. As a result, we can conclude that the stay-at-home order itself did not have a partisan bias in its effect on mobility. Any changes in mobility attributable to differences in political leaning are not systemically related to the implementation of a stay-at-home order. Differences in mobility due to partisan leanings, then, are most likely explained by a different initial response to the increased awareness about coronavirus in March.

We obtain similar results in our data for transit, retail, grocery, and parks, shown in `Tables 9,10,11,12`, with stay-at-home coefficients ranging from 3% to 9%. As a result, we argue that our results on the effect of stay-at-home orders for each area of mobility is robust, only varying by about 1% when using different dates, controls, and interactions. However, our results for parks are especially interesting, as they are the most variable, having the highest standard errors and coefficients, and they also challenge the patterns noticed from the other mobility data. Specifically, the stay-at-home coefficient has the highest magnitude and standard error of any sector, reaching up to -13%, and the VS coefficient is about 20%, which breaks the usual trend of more Democratic counties having a lower mobility throughout the dates. A few explanations for this phenomenon include more Democratic counties shifting mobility toward parks, more Republican counties shifting mobility away from parks, or incomplete data on how parks are distributed through counties, and how they interact with voting shares. The results for parks hint at possible endogeneity issues that the model fails to address and reveals a particularly interesting and significant consequence of stay-at-home orders on movement.

China

Specification

We use a similar specification and design for our China data as well. The main difference, however, is that we have data on different levels of lockdown in China, but not a lot of demographic information for cities and provinces as well as the existence of a regressor like political leanings to fix some endogeneity issues. In particular, there are three types of lockdowns: type A, which is a complete shutdown like that experienced in Hubei, type B, which is a partial shutdown, and type C, which institutes quarantine zones in a city. So, we have the model

$$Mobility_{c,t} = \beta_A SAH^A_{c,t} + \beta_B SAH^B_{c,t} + \beta_C SAH^C_{c,t} + \delta' \Gamma_c$$

Again, Γ_c represents city level control information that we used from our LASSO. We don't run into the multicollinearity trap by using indicators SAH^A, SAH^B, SAH^C for the three different lockdown policies, as there are times when none of these policies are instituted, like before January 23, 2020, the date of the first Hubei lockdown. As we determined from our double LASSO that the case count does not significantly impact our regressors and regressand, we still include case count in our controls but are not specifically interested in its coefficient. We will eventually see that the quarantine measures in China have a much greater impact on mobility, and the other control variables don't affect the regression in a major way. So, we argue that we do not require the same amount of demographic, county-level control data as we did in the US model, and our results are not skewed because of our choice of controls. We use city fixed effects, and also add date fixed effects later, as the first wave of lockdown orders were issued within days of each other, so including date fixed effects immediately would obscure the effect of the lockdown orders. Given the difference in how stay-at-home orders were implemented in the US and China, our fixed effects model must also be tweaked to obtain a value closer to the true parameter. We also cluster standard errors by city.

Our mobility variable is measured a bit differently from the one in our United States specification, as the Baidu mobility data gives a CMI (City Movement Intensity) number for each date and city, which is calculated as the $\log(\frac{\text{mobile people in the city}}{\text{population of the city}})$. Using this information, we convert the mobility data into a percent change from baseline of the percentage of mobile people in the city (which is not necessarily bounded by 100%).

One interesting feature of the enactment of the quarantine orders is that complete lockdown orders (type A) were issued in the Hubei province on January 23, 2020, which prompted a decrease in mobility everywhere in China. Subsequently, type B and C orders were issued over the next couple months, so these orders were made in the context of the initial widespread type A lockdown orders. As a result, as we will see, these orders did not reduce mobility in a significant way, only acting as additional small drops in mobility for cities that enacted these policies.

Our dataset for China is substantially smaller than that for our US model, as we have both quarantine level data and mobility data for select cities in the country. And, it is a bit difficult to directly compare our results from China to our results from the US model, as we have two different types of lockdown strategies and two different measures of mobility. And, just like our assumptions with the US model, there are areas where endogeneity can arise, including public perception about the virus and its effect on both mobility and the stay-at-home order implementation. However, we argue that the effect of potential endogeneity with the China dataset is much smaller than that of the US dataset, as there is more agency on part of the central government in China to enact lockdown orders in different provinces, which is what happened country-wide at the end of January. And, the effect of the initial lockdown was so substantial that we believe that the effect of endogenous variables would be small compared to it. As a result, we argue that endogeneity and our relatively small dataset are not major problems in obtaining the true effects of the lockdown orders.

Results

We have the following results for our specification on the different levels of orders in China in `Table 4`.

Table 4: Stay At Home Order Mobility Responses (China, By Type of Order)

	All Dates	
	(1)	(2)
SAH^A	-73.42^{***}	-47.435^{*}
	(5.967)	(23.293)
SAH^B	-4.812	-2.280
	(3.879)	(3.872)
SAH^C	-0.470	1.120
	(1.311)	(1.025)
Observations	2,093	2,093
City FE	Y	Y
Date FE	N	Y

The coefficients represent percentage changes in mobility with respect to the indicator variable. In column (1), we don't include the date fixed effects, while we include it in column (2). In these results, we are ultimately restricted by our sample size, as we only get a statistically significant level of 5% when including date fixed effects, even though the drop in mobility is especially steep. However, this issue is most likely due to the method of how the orders were implemented, as most type A orders were enacted within days of each other and mobility dropped across the country in that time span, so some of the true effect of the orders on cities would be absorbed into the date specific parameters. On the other hand, when we don't include the date fixed effects, we obtain a 73% drop in mobility after the type A complete lockdown orders were enacted. It is difficult to pinpoint the exact measure of the mobility change attributable to the stay-at-home order, but we argue that the true value of the stay-at-home parameter is closer to -73%, as many of the most stringent lockdown orders were implemented very close to each other, which is not the case in the United States. Our other two parameters, SAH^B, SAH^C are not statistically different from 0 in both regressions. The minor effect of the lower levels of quarantine policy is an interesting result for policy makers, as it suggests that additional measures are not that effective in decreasing mobility when the whole country has engaged in a strict lockdown discipline, even if it is not legally implemented everywhere.

6 Discussion

We use fixed effects models on stay-at-home indicators along with interaction terms to generate robust estimates on the direct effect these orders had on mobility in both the United States and China. In the United States, we had five different sectors of mobility that experienced drops in mobility that was relatively constant throughout changes in controls and dates, and one sector, residential areas, that experienced an increase in mobility as expected. And, we also included these mobility measures' interaction with partisan leanings to conclude that more Democratic counties were more likely to decrease mobility even before the orders were issued, and the effect of the order on Democratic vs Republican counties was not statistically different from 0. This conclusion is consistent with current research on partisan differences in social distancing, as shown by Painter et. al. in `Figure 7`. Here, Democratic counties engaged in more social mobility, which the authors describe as the percentage of people who stayed in their homes using different geolocation data. Though the measures of mobility are different, our results support this hypothesis throughout all types of mobility, which has not been addressed in literature yet. We also argue that the potential for endogeneity concerns has minimal significance to the results of the model, especially when accounting for partisan differences our second US specification. However, in the case our exogeneity assumptions fail, we also provide upper bounds on the magnitude of the effect stay-at-home orders have on mobility in our workplaces and residential area mobility tables, `Tables 2,3`. Future plans to reduce endogeneity concerns in our fixed effects models would be to include more controls for measuring the perception of the coronavirus among residents of a county as in (Barrios et. al., 2020), as well as including data on county and state exposure to different industries, which may see varying effects on order enactment and mobility.

Our conclusions on the mobility effects on China are perhaps less robust compared to our US model, but provide an important intuition for enacting different levels of quarantine in a country, which is information we could not find for the United States. We estimate the drop in the mobile percentage of the population in complete lockdown in provinces like Hubei to be near 73%, though our lower bound for the magnitude of the drop is 47%. This number is consistent with research by Fang et. al., where they estimate a decrease in mobility of about 54% in Wuhan after the quarantine policies were issued. And, in the context of the initial complete lockdown in January, the other, less-stringent, types of lockdown were not statistically effective in decreasing mobility across the country. This is not to say that partial lockdowns are not effective on their own, but rather they do not deter mobility significantly once a complete lockdown in another province is implemented and a country-wide awareness on the virus decrease mobility across cities. We see this type of effect in the United States in mid-March to a lesser extent, but the additional stay-at-home orders decrease mobility in a sizable way. Of course, we cannot immediately compare our results of the US and China as they have different measures of mobility, but they give us insights into areas of future research: we can further analyze different levels of quarantine and stay-at-home orders in different states in the United States as well as try to find a similar regressor to voting shares in China that reduces our endogeneity concerns and gives a more dynamic understanding of population mobility changes.

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A Appendix

Table 5: USA Model: Variable Selection for Retail and Grocery Districts

	Retail		Grocery	
	(1)	(2)	(1)	(2)
SAH	−12.28		−2.38	
VS	0.00000	−0.87595	−0.00000	−0.87595
income	−0.00000	−0.00000	−0.00000	−0.00000
pop	−0.00000	−0.00000	−0.00000	−0.00000
pctpov	0.00000	−0.00000	0.00000	−0.00000
cases	−0.00005	−0.00000	0.00000	−0.00000
VSxSAH	−40.22760	1.84466	−25.44179	1.84466
VSxcases	−0.00000	−0.00000	0.00000	−0.00000
popxpctpov	−0.00000	−0.00000	−0.00000	−0.00000
SAHxpop	−0.00000	−0.00000	−0.00000	−0.00000
pctwhite	−0.02274	0.00000	−0.00000	0.00000
pctblack	0.02951	−0.00000	0.00000	−0.00000
pcthispanic	0.00000	−0.00000	−0.00000	−0.00000
pctnonwhite	0.00111	−0.00000	0.00000	−0.00000
pctforeignborn	−0.18701	−0.00000	−0.10053	−0.00000
pctfemale	0.00000	−0.00000	0.00000	−0.00000
pctage29andunder	0.06282	−0.00000	0.03979	−0.00000
pctage65andolder	−0.18360	0.00000	−0.18991	0.00000
medianhhinc	−0.00000	−0.00000	−0.00001	−0.00000
pctclfunemploy	0.40431	−0.00000	0.34873	−0.00000
pctlesshs	0.00000	−0.00000	0.00000	−0.00000
pctlesscollege	0.17856	0.00000	0.11488	0.00000
pctlesshswhites	0.00000	0.00000	0.00000	0.00000
pctlesscollegewhites	0.00000	0.00000	0.00000	0.00000
pctrural	0.00505	0.00000	−0.00000	0.00000
ruralurbancc	0.00000	0.00000	0.00000	0.00000

Table 6: USA Model: Variable Selection for Parks and Transit Districts

	Parks		Transit	
	(1)	(2)	(1)	(2)
SAH	16.7		−2.84	
VS	47.91034	−0.87595	−0.00000	−0.87595
income	−0.00035	−0.00000	−0.00000	−0.00000
pop	0.00000	−0.00000	−0.00000	−0.00000
pctpov	−0.87808	−0.00000	0.09985	−0.00000
cases	0.00539	−0.00000	−0.00000	−0.00000
VSxSAH	−40.00596	1.84466	−54.50642	1.84466
VSxcases	−0.00549	−0.00000	−0.00000	−0.00000
popxpctpov	−0.00000	−0.00000	−0.00000	−0.00000
SAHxpop	−0.00001	−0.00000	−0.00000	−0.00000
pctwhite	0.54423	0.00000	−0.00000	0.00000
pctblack	−0.38064	−0.00000	0.00000	−0.00000
pcthispanic	−0.50009	−0.00000	0.00000	−0.00000
pctnonwhite	−0.00000	−0.00000	0.00000	−0.00000
pctforeignborn	−0.78772	−0.00000	−0.27775	−0.00000
pctfemale	1.85490	−0.00000	0.00000	−0.00000
pctage29andunder	0.38023	−0.00000	0.12503	−0.00000
pctage65andolder	−1.66323	0.00000	−0.00000	0.00000
medianhhinc	0.00046	−0.00000	−0.00002	−0.00000
pctclfunemploy	0.87429	−0.00000	0.00000	−0.00000
pctlesshs	1.37923	−0.00000	0.10617	−0.00000
pctlesscollege	0.00000	0.00000	0.23847	0.00000
pctlesshswhites	−1.55897	0.00000	−0.00000	0.00000
pctlesscollegewhites	0.54144	0.00000	0.00000	0.00000
pctrural	−0.39183	0.00000	0.00062	0.00000
ruralurbancc	−1.27617	0.00000	0.00000	0.00000

Table 7: USA Model: Variable Selection for Average Mobility Across all Districts

	Average Mobility Percentage Change from Baseline	
	(1)	(2)
SAH	1.13	
VS	0.00000	−0.87595
income	0.00000	−0.00000
pop	−0.00000	−0.00000
pctpov	−0.00000	−0.00000
cases	0.00011	−0.00000
VSxSAH	−25.92678	1.84466
VSxcases	0.00000	−0.00000
popxpctpov	−0.00000	−0.00000
SAHxpop	−0.00000	−0.00000
pctwhite	0.02182	0.00000
pctblack	−0.00000	−0.00000
pcthispanic	−0.00000	−0.00000
pctnonwhite	−0.00973	−0.00000
pctforeignborn	−0.21639	−0.00000
pctfemale	0.22979	−0.00000
pctage29andunder	0.00272	−0.00000
pctage65andolder	−0.32747	0.00000
medianhhinc	0.00000	−0.00000
pctclfunemploy	0.03153	−0.00000
pctlesshs	0.00000	−0.00000
pctlesscollege	0.00000	0.00000
pctlesshswhites	−0.00000	0.00000
pctlesscollegewhites	0.10802	0.00000
pctrural	−0.02838	0.00000
ruralurbancc	−0.00000	0.00000

Table 8: China Model: Variable Selection

	Mobility Percentage Change from Baseline	
	(1)	(2)
SAH	1.34	
A	-0.00000	0
B	0.00000	0.00000
C	0.00000	-0.05
Pop	$-3.28e{-08}$	-0.00000
Cases	$-2.12e{-05}$	0.00000
SAHxA	-0.15	0.087
SAHxB	0.00000	0.86
SAHxC	0.12	0.92
SAHxCases	-0.00000	0.00000
Pop Density	-0.00000	-0.00000

Table 9: Stay At Home Order Mobility Responses (US, Retail)

	All Dates		Post Nat Emergency	
	(1)	(2)	(3)	(4)
SAH	-6.859^{***}	-6.654^{***}	-4.456^{***}	-4.241^{***}
	(0.324)	(0.322)	(0.332)	(0.307)
log(cases)	-2.593^{***}	-1.296^{***}	-1.775^{***}	-1.265^{***}
	(0.122)	(0.136)	(0.175)	(0.171)
VS		-1.063^{**}		-2.892^{***}
		(0.361)		(0.484)
SAH $\times$ VS		-1.827^{***}		0.355
		(0.331)		(0.315)
log(cases) $\times$ VS		-0.615^{***}		-0.795^{***}
		(0.079)		(0.102)
Observations	165,442	165,439	101,270	101,267
County FE	Y	Y	Y	Y
Date FE	Y	Y	Y	Y

Table 10: Stay At Home Order Mobility Responses (US, Grocery)

	All Dates		Post Nat Emergency	
	(1)	(2)	(3)	(4)
SAH	-8.759^{***}	-8.629^{***}	-7.726^{***}	-7.598^{***}
	(0.281)	(0.285)	(0.282)	(0.286)
log(cases)	-2.155^{***}	-1.570^{***}	-1.360^{***}	-1.186^{***}
	(0.113)	(0.144)	(0.173)	(0.189)
VS		-0.422		-1.412^{*}
		(0.449)		(0.561)
SAH $\times$ VS		-1.305^{***}		0.201
		(0.301)		(0.245)
log(cases) $\times$ VS		-1.891^{**}		-0.230^{**}
		(0.067)		(0.084)
Observations	159,108	159,105	95,435	95,432
County FE	*Y*	*Y*	*Y*	*Y*
Date FE	*Y*	*Y*	*Y*	*Y*

Table 11: Stay At Home Order Mobility Responses (US, Transit)

	All Dates		Post Nat Emergency	
	(1)	(2)	(3)	(4)
SAH	-3.835^{***}	-3.545^{***}	-3.451^{***}	-3.646^{***}
	(0.514)	(0.581)	(0.484)	(0.562)
log(cases)	-3.693^{***}	-1.774^{***}	-1.091^{**}	-0.591
	(0.227)	(0.256)	(0.362)	(0.335)
VS		-0.574		-4.194^{**}
		(1.127)		(1.449)
SAH $\times$ VS		-0.985		1.067
		(0.650)		(0.608)
log(cases) $\times$ VS		-0.992^{***}		-0.544^{**}
		(0.136)		(0.181)
Observations	86,856	86,853	60,289	60,286
County FE	*Y*	*Y*	*Y*	*Y*
Date FE	*Y*	*Y*	*Y*	*Y*

Table 12: Stay At Home Order Mobility Responses (US, Parks)

	All Dates		Post Nat Emergency	
	(1)	(2)	(3)	(4)
SAH	-7.442^{***}	-9.986^{***}	-9.327^{***}	-13.63^{***}
	(1.223)	(1.513)	(1.337)	(1.714)
log(cases)	1.431^{*}	4.634^{***}	2.666	3.380^{*}
	(0.644)	(0.809)	(1.387)	(1.386)
VS		17.38^{***}		20.01^{***}
		(3.380)		(4.550)
SAH $\times$ VS		2.096		3.733^{*}
		(1.450)		(1.462)
log(cases) $\times$ VS		-2.028^{***}		-1.986^{***}
		(0.341)		(0.484)
Observations	57,807	57,804	39,025	39,022
County FE	Y	Y	Y	Y
Date FE	Y	Y	Y	Y

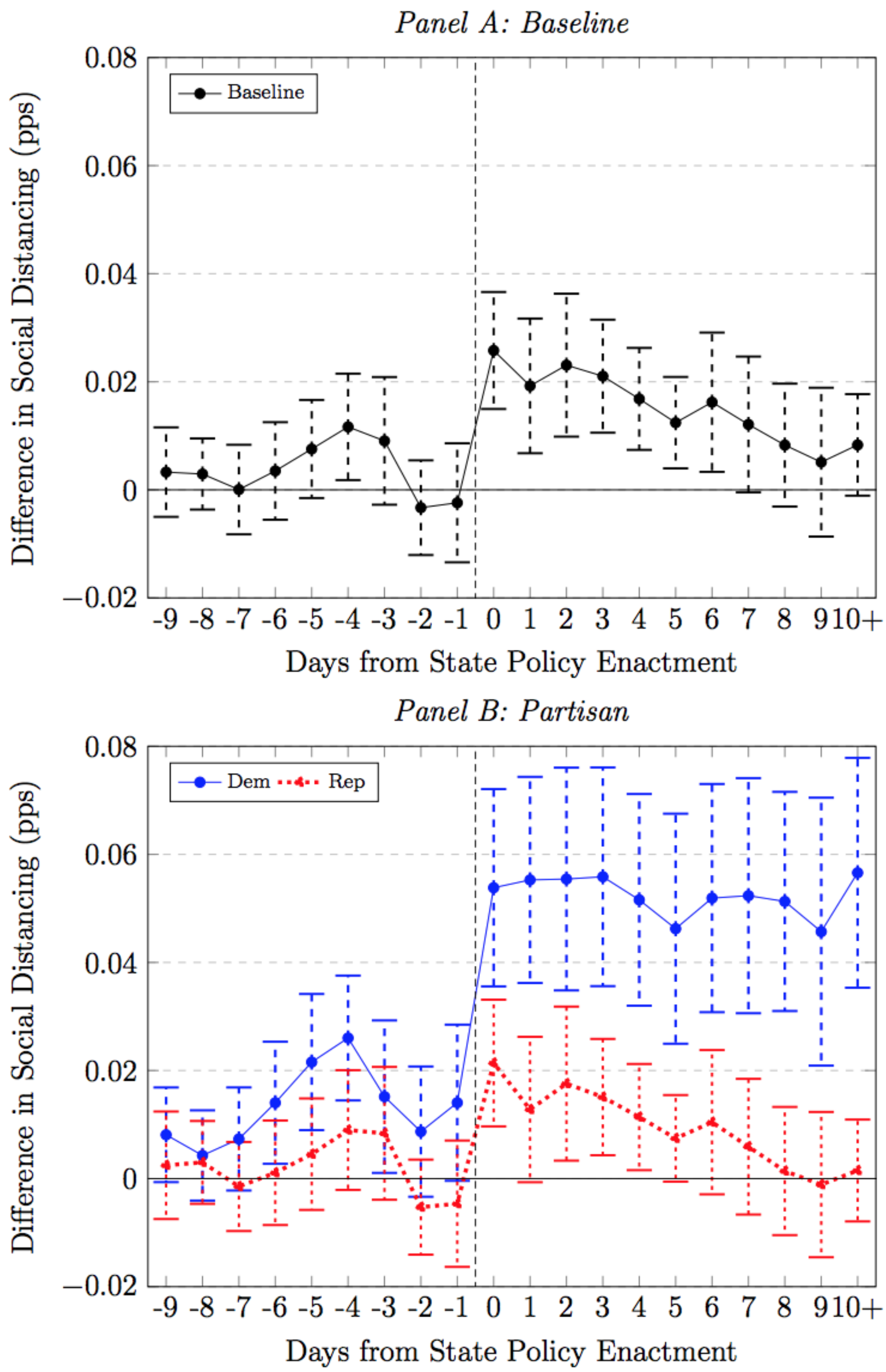

Figure 7: Partisan Difference in Social Distancing (Painter et. al., 2020)