Estimating the Causal Effect of Mobility on COVID-19 Related Mortality *

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Abstract

This paper estimates the effect of mobility (social distancing) on future COVID-19 mortality in the United States. By using mobility indices that directly track cellphones, we can estimate the effect of a standard deviation increase in mobility on future mortality without using proxies for mobility. To solve omitted variable bias and measurement error issues we use rainfall as an instrumental variable; to find how far in the future mobility affects mortality we use LASSO. Finally, we decompose the bias in naive OLS into measurement error and omitted variable bias by considering two mobility indices (by Descartes Lab and Unacast). Using both datasets, we estimate two statistically similar effects. A one standard deviation spike in mobility is associated with a spike of 7.34 (15.1) deaths per million people 3 weeks in the future using the Descartes Lab (Unacast) datasets. These numbers are large in magnitude, as COVID-19 resulted in 96.7 deaths per million in the week of April 20, the last week of our data. Finally, our bias decomposition shows that measurement error is a much greater concern than omitted variable bias. This suggests that we should be careful in interpreting regression results using cellphone data that do not consider measurement error.

keywords: COVID-19, Coronavirus, IV, Measurement Error, LASSO, Mobility Data, Social Distancing

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1 Introduction

Does social distancing reduce the spread of viral outbreaks? And how long do we have to wait until we can see the effects of social distancing? Anecdotal evidence and preliminary research investigating the coronavirus outbreak suggest that social distancing works after 2-3 weeks. However, few studies have a robust claim to causality because social distancing is a choice variable based on how dangerous it is outside (unknown bias) and because social distancing indices have a lot of measurement error (attenuation bias). Our paper attempts to remedy the endogeneity and measurement error issue through an instrumental variables (IV) approach and explicitly estimates the appropriate lagged effect with LASSO. By using multiple mobility indices and a relevant & exogeneous instrument, we can decompose the bias in the endogeneous linear regression into attenuation bias and omitted variable bias. We find that measurement error is a much greater concern than omitted variable bias.

Our paper draws from a number of literatures that estimate the effect of mobility on virus spread. Papers investigating this causal effect tend to use either an instrumental variables or differences-in-differences approach. Mobility tends to be measured directly by tracking cell phones or proxied by government-imposed mobility restrictions.

The foundational paper investigating the causal effects of social distancing on virus spread is Adda (2016). Utilizing weekly spatial data on the spread of multiple viruses in France in the 20th century, they estimate the effect of social distancing policies (such as school closures) on outbreak levels. They estimate the spread of a virus within and between cities using a linear version of the susceptible-infected-recovered (SIR) model that includes lagged infection rates. They use weather as an instrumental variable to solve for potential serial correlation and measurement error introduced by using lagged infection rates. Variations on this model have been incorporated into research estimating the effect of social distancing policies on the coronavirus outbreak. For example, Qiu et al. (2020) study Chinese city-level outbreak data and consider multiple lagged climate variables as instruments and proxy mobility with weighted sums of infection rates in nearby cities. Our paper incorporates many aspects of this model, including weather as an instrumental variable. Our paper differs from this research in that we use mobility data rather than a proxy for mobility and use US outbreak data.

Research into the coronavirus more commonly estimates causal effects through the differences-in-differences design (DiD) or regression discontinuity design (RDD) frameworks. Most relevant to our paper, Dave et al. (2020) use DiD and event study techniques to estimate the effect of shelter-in-place orders on COVID-19 cases through its effect on mobility. They estimate a 5-10% increase in the rates of individuals staying home full-time, leading to a 44% decline in cumulative cases three weeks after the adoption of such orders. Villas-Boas et al. (2020) do the same with only a DiD design. They estimate a 5% reduction in average distance traveled, 6% reduction in non-essential visits, and 23% reduction in human interactions immediately after the adoption of such orders. This decline in mobility reduced mortality by 0.5 deaths per million residents per day. Some other papers using DiD and RDD on coronavirus related topics are Brodeur et al. (2020) (effect of lockdowns on psychological factors), He et al. (2020) (effect of lockdowns in China on pollution), and Tucker and Yu (2020) (relationship between in-person dining bans and retail traffic).

The broader literature investigates the relationship between social distancing policies, mobility measures, and various outcomes. Some papers investigate the effectiveness of social distancing policies, or the effect of policy implementation on mobility reductions. For example, Engle et al. (2020) examine the relationships of stay-at-home orders and two-week lagged COVID-19 infection rates on future mobility. They estimate a 7.87% reduction in mobility after the implementation of stay-at-home orders, but also measure heterogeneity across a number of factors. Wellenius et al. (2020) estimate the effect of various social distancing policies on mobility in the next week. They estimate a 10% reduction in time spent away from home after state-of-emergency declarations, with cumulative effects as more policies are adopted. Other papers consider health, economic, or political effects. Liautaud et al. (2020) and Zhang et al. (2020) use lagged change in mobility to estimate effects on fever incidence rates in the US after the start of the coronavirus pandemic. Alexander and Karger (2020) and Baker et al. (2020) look at the relationship between stay-at-home orders and consumer spending, mediated through mobility. Both measure heterogeneity in mobility changes across various measures. Blais et al. (2020) estimate the relationship between coronavirus lockdowns in Europe and changes in voting intentions, trust in government, and support for democracy. Building off the observed heterogeneity in mobility responses, we choose to include state fixed effects in our main regression specifications. This allows us to control for any state-level heterogeneity that is fixed over short periods of time.

Our paper is structured as follows. In Section 2, we discuss our empirical model. In Section 3, we discuss our data sources. In Section 4, we argue why our instrumental variable, rainfall, satisfies the relevance and exclusion restriction requirements. In Section 5, we use the LASSO to determine the correct number of lags for mobility to include in our empirical model. In Section 6, we analyze our IV regression results. In Section 7, we decompose the bias from a naive OLS into measurement error and omitted variable bias. Section 8 concludes.

2 Model

2.1 Empirical Model

We are interested in investigating the causal effect of changes in mobility on changes in COVID-19 related mortality. We consider the following regression model:

$$\Delta D_{it} = \beta_0 + \sum_{j=1}^{J} \beta_j \Delta MB_{i,t-1+j} + \zeta_j \Delta D_{i,t-2+j} + \eta_1 \mathbf{P}_{i,t} + \lambda_i \chi_i + \gamma_i \chi_i \cdot t + \epsilon_{i,t} \tag{1}$$

where i and t subscripts denote state and week respectively. $\Delta D_{i,t}$ denotes the change in COVID-19 related deaths per capita, $\Delta MB_{i,t}$ denotes the change in social distancing, $\mathbf{P}_{i,t}$ are various policy indicators, χ_i are state indicators, and $\chi_i \cdot t$ capture linear trends for each state.

Existing literature indicates significant heterogeneity in COVID-19 mortality rates across many demographic variables. However, these variables, such as population, proportion of people aged 65 and older, or political support, will stay approximately constant over the 3-month panel. As a result, they are absorbed into the time-invariant state level fixed effects. As this paper does not investigate demographic heterogeneity, we choose not to include individual controls for these variables. Further, including these controls would introduce significant collinearity with the state level fixed effects, unnecessarily increasing the variance of our estimates. This is of notable concern given the small number of observations in our sample.

Our model also includes lagged effects of changes in social distancing on changes in COVID-19 outcomes. This is because the decision to socially distance today is expected to affect virus outcomes for possibly several weeks into the future. However, it is difficult to know how many lagged terms (J) to include. We apply the LASSO approach to choose the J most relevant lags for predicting COVID-19 outcomes. We detail this approach in Section 5.

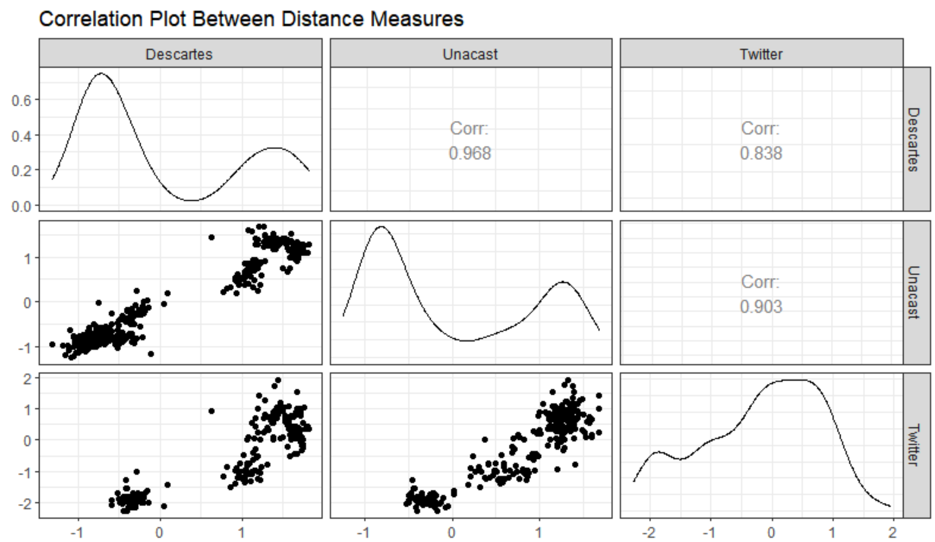
We also include a lagged term for COVID-19 death in our regression because we believe a spike in deaths could induce people to stay indoors and because it also shows what part of the infection phase we are on.

3 Data

Our outcome of interest is changes in COVID-19 mortality per capita at the state level. A daily time series of this data comes from Science and at Johns Hopkins University (2020). Data for our endogenous regressor, mobility, are gathered from three sources: Descartes Lab (Warren and Skillman (2020)), Unacast (Unacast (2020)), and Twitter (Xu et al. (2020)). Data for our instrumental variable, rainfall, are collected from Community Collaborative Rain (n.d.). Finally, data for our policy controls are collected from Fullman et al. (2020). We convert all variables into change in level by week except for the policy indicators to fit our regression specification.

Descartes Lab and Twitter report levels of mobility at a daily and weekly frequency, respectively, while Unacast reports percent change in mobility from baseline at a daily frequency. To make the Unacast data comparable to the Descartes Lab and Twitter data, we convert it to levels by adding back the baseline for each state and day. Once we have all three datasets in levels, we average the Descartes Lab and Unacast data at the weekly level. This is done to make them comparable to the Twitter data (this has the additional benefits of reducing noise from the rainfall data and removing the need to include day-of-week controls). Because in the Unacast data the baseline is set to 1 for each state, we cannot compare mobility across states. Given this constraint, we normalize all three datasets at the state level once they have been averaged by week. Once the measures have been properly adjusted, particular measurements can be interpreted as standard deviations from average state-level mobility. Figure 1 shows and visualizes the correlations between each mobility metric. The top right of the plot displays the correlations, the diagonal displays the estimated density, and the bottom left displays the cross scatterplots.

Figure 1: Comparison of Distance Measures



The high correlation among the three different mobility indices indicate that these are correctly measuring the similar population mobility number.

All other data is at the daily level. Rainfall data is given as average daily rainfall by state in inches, which we then average by week. Policy data is reported as the date enacted for each policy by state. To convert this into a weekly panel data setting, we set each policy to 0 for all weeks prior to enactment and 1 for all other weeks. In our specification, we use Emergency Declarations and Gathering Restrictions as policy controls because these were implemented by every state, whereas many other policies were not.

We include time series trends of all variables in Texas below. Figure 2 shows the weekly trend of changes in these variables with vertical lines at policy implementation weeks. Figure 3 shows the daily time series of mobility levels with vertical lines at policy implementation dates.

Figure 2: Time Series of Variables in Texas

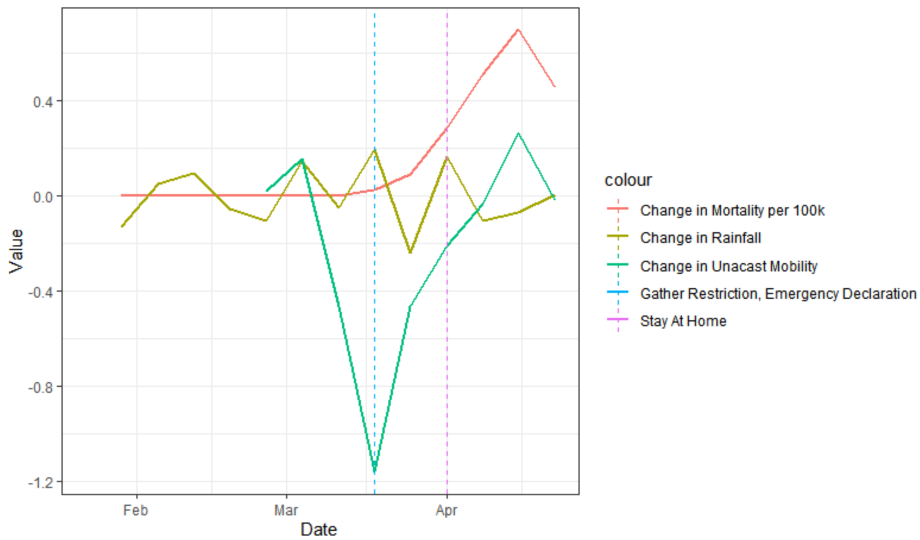
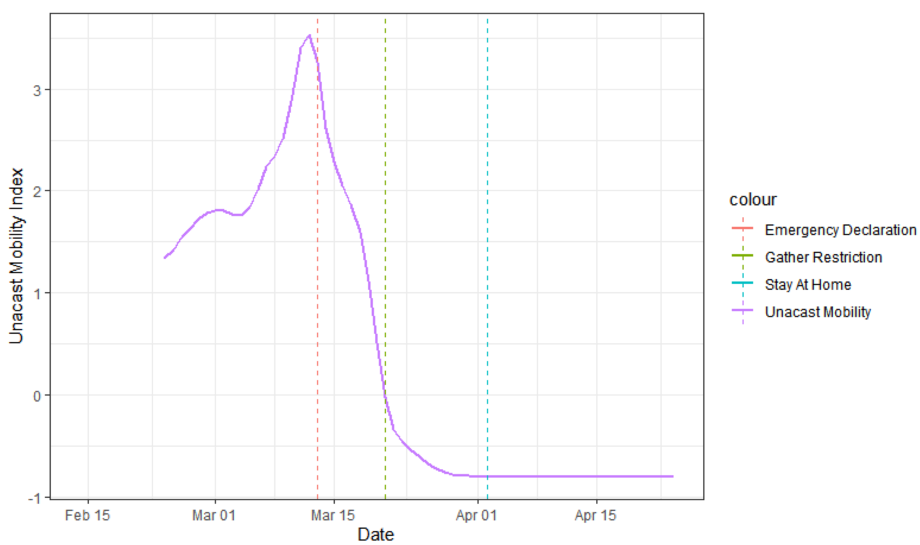


Figure 3: Time Series of Mobility Level in Texas



4 IV Regression

We choose to use an IV approach over DiD because we believe DiD does not satisfy the parallel trends assumption. For states that enact mobility restricting policies such as Texas, mobility often spikes prior to policy implementation (visualized in Figure 3). These spikes happen because of an anticipation effect, where people rush to grocery stores to buy food and toilet paper. Even if there are parallel pre-trends (i.e. a spike in mobility for states that didn't enact such policies), they would be for completely different reasons. As a result, we wouldn't expect the parallel trend to continue in the future.

Estimating Equation 1 through OLS will have omitted variable bias and attenuation bias. This is because we expect decisions about social distancing to be correlated with unobservables related to COVID-19 mortality rates. For example, people in places where there has been a greater increase in the concentration of COVID-19 may choose to have a greater increase in social distancing to reduce risk of transmission. Furthermore, we believe that $MB_{i,t}$ is measured with significant error. To avoid the endogeneity and measurement error issue, we use rainfall as an instrument for mobility. This identification strategy requires that rainfall satisfies is relevant, exogeneous, and uncorrelated to the measurement errors in $MB_{i,t}$.

4.1 Instrument Relevance

Rainfall satisfies instrument relevance by making travel more inconvenient, reducing mobility. Looking at the data, Figure 4 shows the weekly time series of change in mobility and change in rainfall in Texas. The left plot shows the raw variables and the right plot shows the raw variables residualized from our controls. Although the relationship is not clear in the raw data, the residualized data show that an increase in rainfall (conditioning on policy and trend) is associated with a contemporaneous decrease in mobility. Residualizing from the controls is important because Texas (along with many other states) saw large drops in mobility after declaring a state of emergency and implementing gathering restrictions.

Figure 4: Time Series of Mobility Change and Rainfall Change in Texas

We can more formally evaluate instrument relevance through a first stage regression. We run the following regression to examine the effect of rainfall on social distancing measures:

$$MB_{i,t} = \beta R_{i,t} + P'_{i,t}\eta + \Delta D_{i,t-1} + \lambda_i \chi_i + \gamma_i \chi_i t$$

Table Table 1 shows the first stage results.

Table 1: First Stage: Change in Mobility

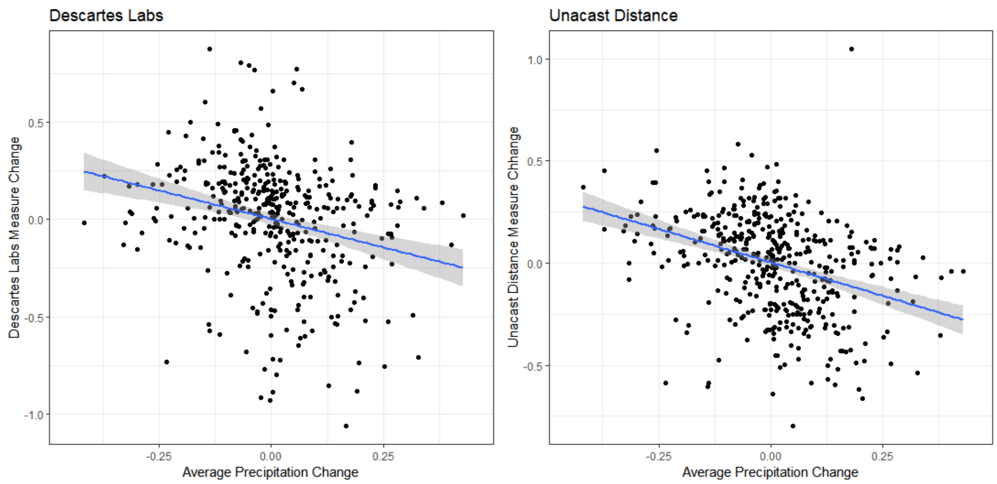
	Descartes Lab				Unacast				Twitter			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
ΔAvg. Precipitation$_t$	−0.679***	−0.684***	−0.791***	−0.622***	−0.683***	−0.685***	−0.714***	−0.726***	−0.433	−0.462	0.104	0.642
	(0.122)	(0.131)	(0.112)	(0.154)	(0.077)	(0.082)	(0.076)	(0.103)	(0.421)	(0.493)	(0.635)	(1.826)
Emergency Declaration$_t$			−0.643***	−1.379***			−0.253***	−0.432***			−0.805***	−0.655
			(0.090)	(0.143)			(0.041)	(0.076)			(0.129)	(0.684)
Gathering Restriction (Any)$_t$			−0.082	−0.928***			0.017	−0.431***			−0.235	−0.794
			(0.100)	(0.161)			(0.046)	(0.093)			(0.141)	(0.645)
ΔCOVID Mortality Per Capita$_{t-1}$			5,959.519***	−1.39e+04***			4,243.694***	−4155.311			3.03e+05***	2.32e+06
			(2,196.567)	(5,050.702)			(1,371.104)	(3,141.586)			(1.08e+05)	(3.57e+06)
Constant	−0.225***	−0.195***	0.407***	−6.665***	−0.234***	−0.229***	−0.058**	−0.602	−0.633***	−0.735***	−0.230***	3.802
	(0.005)	(0.001)	(0.035)	(2.066)	(0.002)	(0.002)	(0.024)	(1.030)	(0.014)	(0.025)	(0.061)	(19.351)
N	400	400	400	400	450	450	450	450	260	260	200	200
R^2	0.027	0.031	0.196	0.727	0.065	0.067	0.192	0.591	0.005	0.027	0.391	0.714
Adj. R^2	0.025	−0.108	0.073	0.557	0.063	−0.050	0.084	0.379	0.002	−0.218	0.170	−0.238
State FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes
State · Week	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes
F-stat	31.2	27.4	49.9	16.4	79.0	69.9	87.2	49.9	1.05	.877	.027	0.124

Robust standard errors in parentheses. Standard errors clustered by state.

Both for Descartes Lab data and Unacast data, we find that an increase of 1 inch of rain is associated with about a 0.7 standard deviation decrease in mobility using columns (3) and (4). Both of these coefficients are very significantly different from zero and have F statistics above 10. This is consistent with instrument relevance. In the Twitter data, we do not find a significant relationship between rainfall and mobility, so we do not proceed with this mobility measurement. We believe this is caused by the slightly different methodologies used by the Twitter data. Twitter location data comes from Twitter posts. Since people are more likely to tweet when indoors (e.g. at home or at work), Twitter captures movement that people must do regardless of how rainy it is outside. As a result, Twitter has a strong (but somewhat weaker) correlation to Descartes Lab and Unacast but no correlation to rainfall.

Figure 5 shows the relationship between social distancing measures and rainfall from the first stage results, residualizing for our controls.

Figure 5: First Stage: Residualized Rainfall Change versus Residualized Social Distancing Change



These figures show the strong negative correlation between the residualized social distancing measures and residualized rainfall. This gives a visual representation of the relevance of our instrument.

4.2 Exclusion Restriction

There is limited scientific literature about the relationship between COVID-19 and meterological variables since we are so early in the pandemic. Li et al. (2020) study the effects of temperature and many other variables on COVID-19 outcomes, but do not have an identification strategy for showing causality. Tosepu et al. (n.d.) specifically study the effect of weather on COVID-19 in Indonesia. They find that rainfall is not significantly correlated with transmission or outcomes, but that temperature is. They also cite several other studies that find a correlation between temperature & humidity and basic reproduction number, transmission, and mortality of COVID-19. However, the authors admit that their study and many others do not account for many omitted factors such as population mobility, population endurance, and virus resistance.

Due to the limitations of available scientific research on COVID-19, we also consider other virology research. In a literature review, Pica and Bouvier (2012) claim that the empirical relationship between precipitation and transmission of respiratory syncytial viruses is largely inconclusive and country (even province) dependent. They do note that there generally is a relationship between influenza and rainfall; however, unlike the relationships for humidity and temperature, the authors do not discuss known mechanisms that explain the relationship between virus transmission and rainfall. Lowen and Steel (2014), who discuss weather-based predictors for influenza transmissions, argue that temperature and relative humidity are the primary drivers of the relationship between the rainy season and the influenza virus. They find in a guinea pig experiment that temperature and relative humidity affect virus transmission and review several scientific models through which these factors affect virus transmission. Such mechanisms include virus-based ones (e.g. stability of the virus shell) and human-based ones (e.g. human immune response to viruses).

Despite the known relationships between weather and the spread of other viruses, for the coronavirus specifically, it is too early to determine the causal link between weather and transmission. This is to be expected as the literature is only a few months old. For other viruses, there seems to be a well documented causal relationship between temperature & humidity and virus transmission. In contrast, the relationship between other virus transmission and rainfall seems non-robust. This could show that there are no biological factors affected by rainfall (because we expect biological factors to stay consistent across countries), and the effects are only driven by human behavior reacting to rainfall (our first stage story). As a result, their non-robust estimates may be a sign of no true relationship and primarily measuring omitted variable bias.

4.3 Identifying Assumptions

Since we have panel data, we need to consider the identifying assumptions that are required for consistent estimates in panel regressions. Specifically, we need strict exogeneity and no serial correlation. Strict exogeneity implies the following:

$$E[\epsilon_{it}|X_{i1},\ldots,X_{iT},\lambda_i]=0 \quad \forall t$$

where X_{it} is a matrix of controls and λ_i is the state fixed effect. No serial correlation implies the following:

$$Corr(\epsilon_{it},\epsilon_{is}|X_i,\lambda_i)=0 \quad \forall t\neq s$$

Adda (2016) discuss this issue for infection data. They note that while serial correlation may occur, this can be remedied through the use of instrumental variables. Qiu et al. (2020) note that any linear equation relating changes in coronavirus rates over time will be subject to two forms of serial correlation. First, in general the number of new infections for most diseases increases, peaks, then decreases over time. Second, local clusters that cause spikes in infection rates will lead to persistent effects on error terms. They propose following the framework developed in Adda (2016) and using past weather as an instrumental variable.

To show strict exogeneity, we will argue that the weather IV's exclusion restriction applies for all weeks, not just the contemporaneous week. There is no lagged dependence concern because we select the appropriate lag using our LASSO procedure. Additionally, Pica and Bouvier (2012) conducts a literature review including articles that aggregate precipitation over long periods (one month) without finding a conclusive relationship between precipitation and respiratory virus infection. While exogeneity for one month does not imply exogeneity for all time, we do not expect rainfall to effect virus transmission more than one month in the future or past. This agrees with our intuition that the virus cannot act on weather that has not happened, nor does it alter its behavior today based on past weather.

5 LASSO Implementation: Variable Selection

As mentioned before, it is difficult to determine how many (J) or which relevant lagged terms to include in our regression. To preclude an arbitrary choice of lags, we use the data to determine the optimal lags to include. We do this using LASSO, a shrinkage method that selects covariates by introducing a constraint. Running LASSO directly on the empirical model has two problems: first, it could deselect an important control and cause omitted variable bias; second, $MB_{i,t}$ is endogeneous without first applying the instrument. In our LASSO procedure, we solve the following minimization problem:

$$\min_{\beta} \sum_{i=1}^{N} \left(D_{i,t} - \beta_0 - \sum_{j=1}^{J} \beta_j MB_{i,t-1+j} - C'_{i,t}\eta \right)^2$$
$$\text{subject to } \sum_{j=1}^{P} |\beta_j| \leq t$$

where C are the controls explained in Section 2.1. The coefficients on the controls, η, are not in the constraint because we do not want to regularize them. We can represent the LASSO problem equivalently in terms of the following Lagrangian form:

$$\min_{\beta} \sum_{i=1}^{N} \left(D_{i,t} - \sum_{j=1}^{J} \beta_j MB_{i,t-1+j} - C'_{i,t}\eta \right)^2 + \lambda \sum_{j=1}^{P} |\beta_j|$$

where we have a one-to-one mapping between t and λ. λ can be interpreted as the Lagrangian multiplier associated with the constraint, or the level of parsimony we would like the linear model to have. By the Frisch-Waugh theorem, we can residualize $MB_{i,t}$ and $D_{i,t}$ from $C_{i,t}$ to get equivalent results:

$$\min_{\beta} \sum_{i=1}^{N} \left(\tilde{D}_{i,t} - \sum_{j=1}^{J} \beta_j M\tilde{B}_{i,t-1+j} \right)^2 + \lambda \sum_{j=1}^{P} |\beta_j|$$

where $\tilde{D}_{i,t}$ are the residuals of an OLS regression of $D_{i,t}$ on $C_{i,t}$ and $M\tilde{B}_{i,t-1+j}$ are the residuals of an OLS regression of $MB_{i,t-1+j}$ on $C_{i,t}$. To resolve the endogeneity issue, we convert MB to $M\hat{B}$, the fitted values of MB from the IV first stage. Finally, to ensure that LASSO doesn't over-weight high variance variables we normalize all variables to be mean 0 and standard deviation 1. Mathematically, we are running the following regression:

$$M_C D_{i,t} = M_C M\hat{B}\beta + \lambda|\beta|$$

where $M_C = 1 - C(C'C)^{-1}C'$. In summary, we apply LASSO to mobility projected onto the instrument described in Section 3 and residualized from the controls. Projecting onto the instrument ensures LASSO picks an exogeneous lag, and residualizing from the controls ensures that LASSO keeps all of the controls. We then run 2SLS using the lags selected by LASSO and the controls.

In general, LASSO estimates are biased, which is why we use it only to select the correct lagged terms. Empirical evidence shows that LASSO finds correct variables under a sparse data generating process. Here, we believe that mobility today will affect virus mortality only up to a certain point in the future. Moreover, mobility today is unlikely to affect COVID-19 related mortality in the near future due to the incubation period. Therefore, it seems reasonable to conclude we have a sufficiently sparse data generating process to effectively use LASSO for variable selection.

5.1 LASSO Implementation

We use cross-validation to find the λ that gives us the least out-of-sample MSE. The LASSO implementation procedure can be summarized as the following:

1) Split the data into in-sample and out-of-sample data.

2) Using the in-sample data, generate $M_C D_{i,t}$ and $M_C MB_{i,t}$. These are residuals from an OLS regression of $D_{i,t}$ and $MB_{i,t}$ on $C_{i,t}$, respectively.

3) Using the in-sample data, regress $M_C MB_{i,t}$ on $R_{i,t}$ and store the predicted $\widehat{M_C M}B_{i,t}$. This is equivalent to the predicted values from the first stage of an IV regression.

4) Consider a list of possible λ values that can be used for the LASSO regression.

5) For each λ, using the in-sample data, run the following LASSO Regression:

$$M_C D_{i,t} = \widehat{M_C M}B_{i,t}\beta + \lambda|\beta|$$

6) Using the out-of-sample data, calculate the MSE based on our estimates from (5). Store the MSE value.

7) Repeat (5) - (6) for the range of λ considered in (4). Choose the λ that gives the lowest MSE, plus one standard error.

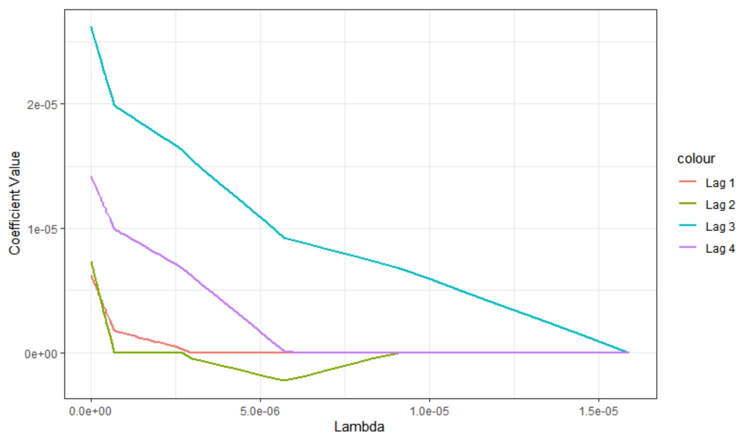
8) Given this optimal λ parameter, find the lagged variables where $\beta_i > 0$.

We used the function by Friedman et al. (2010) (glmnet R library) to pick λ via cross validation and run the LASSO regression.

5.2 LASSO Results

The following plot represents the coefficient estimates for a range of λ.

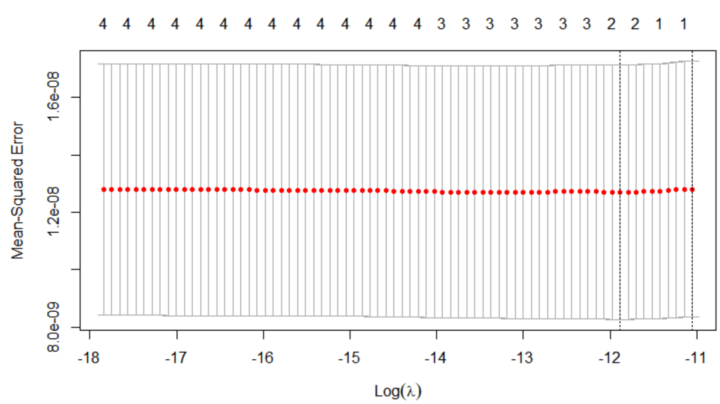
Figure 6: Coefficient Estimates for a range of λ



Based on theory, we expect less coefficients to be selected as λ increases. This is consistent with our graph in Figure 6. As we can see, mobility lagged by three weeks is very different from zero and will be selected from the LASSO regression.

The optimal λ was selected using cross validation and comparing MSE for a range of λ:

Figure 7: MSE for Range of λ



The optimal λ is not clear from Figure 7. We use the λ value that is one standard error away from the minimum MSE, as is usually done with LASSO to reduce overfit. This one standard error value chooses exactly one coefficient different from zero. Therefore, we use only the social distancing measure lagged by three weeks as an independent variable. This choice of lag is consistent with the broader literature: Qiu et al. (2020) include climate variables as instruments lagged at both three and four weeks based on the widely known incubation period of around 2 weeks; Dave et al. (2020) assumes there is a 3-week lagged effect between the adoption of shelter-in-place orders and changes in COVID-19 mortality rates; and Zhang et al. (2020) find there is a 3-week effect of changes in mobility on fever rates.

6 Regression Results

Our second stage IV regression results can be seen in Table 2:

Table 2: Second Stage: Change in COVID Mortality Per Capita (lag 3)

	Descartes Lab				Unacast				Twitter			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
ΔMobility$_{t-3}$	1.72e-05***	1.04e-05**	7.34e-06*	2.90e-06	2.20e-05**	2.35e-05**	1.51e-05*	3.52e-06	–9.47e-05	–9.09e-05	1.86e-04	1.71e-05
	(6.21e-06)	(4.32e-06)	(4.15e-06)	(3.66e-06)	(9.58e-06)	(1.02e-05)	(8.38e-06)	(2.84e-06)	(9.47e-05)	(8.76e-05)	(9.81e-04)	(3.67e-05)
Emergency Declaration$_{t-3}$			–1.22e-06	–8.53e-07			6.70e-06***	–7.93e-06***			1.53e-04	6.40e-06
			(4.95e-06)	(4.49e-06)			(2.16e-06)	(3.02e-06)			(7.84e-04)	(2.74e-05)
Gathering Restriction (Any)$_{t-3}$			2.36e-05***	–2.74e-06			2.25e-05***	–1.90e-06			5.89e-05	6.16e-06
			(7.10e-06)	(2.86e-06)			(7.42e-06)	(2.05e-06)			(2.31e-04)	(1.31e-05)
ΔCOVID Mortality Per Capita$_{t-4}$			4.24e-01	–6.80e+00***			9.19e-01**	–5.82e+00***			–5.21e+01	–8.34e+00
			(4.37e-01)	(1.61e+00)			(4.01e-01)	(1.29e+00)			(2.97e+02)	(2.80e+01)
Constant	2.80e-05***	1.27e-05***	–1.85e-06	–7.39e-05***	2.50e-05***	1.52e-05***	–3.67e-06	–7.79e-05***	–5.21e-05	–6.11e-05	4.25e-05	–1.01e-04
	(6.35e-06)	(1.87e-06)	(2.44e-06)	(1.64e-05)	(6.46e-06)	(3.64e-06)	(2.32e-06)	(1.68e-05)	(5.95e-05)	(6.24e-05)	(2.32e-04)	(7.33e-05)
N	250	250	250	250	300	300	300	300	260	260	200	200
R^2	.	0.626	0.706	0.970	.	0.434	0.611	0.958	.	.	.	0.827
Adj. R^2	.	0.531	0.626	0.949	.	0.321	0.527	0.936	.	.	.	0.641
State FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes	Yes
State · Week	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes

Robust standard errors in parentheses. Standard errors clustered by state.

Our regression results imply that we expect a positive relationship between mobility and COVID-19 related mortality. We will use column (3) and column (7) to interpret our results. Column (4) includes an additional control of state times week interactions. This will add an additional 50 coefficients to estimate. However, for each state, we have only five weeks, or five observations, and thus we do not expect to get significant estimates. On average, we estimate a 7.34×10^{-6} per capita increase from a one standard deviation increase in the difference of mobility measurement by Descrates Lab in a week, controlling for emergency declaration and gathering restriction policies. Similarly, we estimate a 15.1×10^{-6} per capita increase from a one standard deviation increase in the difference of mobility measurement by Unacast in a week, controlling for emergency declaration and gathering restriction policies. The average COVID-19 related mortality per capita is 96.7×10^{-6} for the week of April 20 in our data. Therefore, our estimates indicate that the change in mobility levels have a substantial effect on COVID-19 related mortality.

We note that the IV estimates on mobility measured by Descartes Lab and Unacast are similar with overlap in the 95% confidence interval. If we assume that mobility has a homogeneous effect on COVID-19 related deaths, we believe these results indicate we have a good instrument that is able to purge away the mobility measure from the measurement error component. Moreover, we run a test with a null hypothesis that the estimates on mobility are the same from Descartes and Unacast Lab. Then, we have a p-value of 0.4090, and thus we cannot reject the null hypothesis.

Our coefficient estimates on emergency declaration and gather restriction policies are positive and significant. However, we would expect to see negative coefficients, as these policies are implemented in the hopes of reducing COVID-19 mortality. We believe these results arise from endogeneity problems. For example, states that believe they are at higher risk from COVID-19 are likely to have stricter policy responses and at earlier dates. Our instrument, rainfall, only allows us to estimate the exogenous effect of mobility on COVID-19. Since we do not have other instruments, our policy controls will be biased. While the policy controls are biased, it is still important to include them in our regression model because they are correlated with mobility.

7 Measurement Error Bias

We believe that mobility measures are contaminated by measurement error for two reasons. First, mobility measures are sourced from a limited number of phones. Therefore, these measures suffer from sampling noise. Second, mobility is measured differently by each source. Therefore, errors may arise directly from the technology used for data collection or indirectly through the methodology used to convert the raw data into mobility measures. Without correcting for measurement error, a simple OLS regression will recover coefficients that are biased towards zero (attenuation bias). In this section, we investigate how much of the bias on the estimates of the causal effect of mobility on COVID-19 mortality comes from measurement error and endogeneity problems following the methodology proposed by Acemoglu et al. (2001).

Since we have two noisy measurements of mobility, we can use one as an instrument for the other. We have instrument relevance because they both measure the same true value (population mobility). However, this IV does not solve for possible endogeneity, because mobility still remains a choice variable that is likely correlated with unobservables that affect COVID-19 related mortality. In contrast, an instrument, such as rainfall, that is relevant, exogenous, and uncorrelated with the measurement errors can resolve both the measurement error and endogeneity problems. By projecting mobility onto our instrument, rainfall, we can purge the measurement error from the mobility and focus solely on the effects of exogenous variation in mobility on COVID-19 related mortality. Acemoglu et al. (2001) describes a process that uses estimates from both instruments to measure the bias from measurement error and endogeneity.

We are interested in estimating the causal effect of mobility on COVID-19 mortality. We apply Frisch-Waugh Theorem to our empirical model in Section 2.1 as the following:

$$D_{it} = \beta_1 MB_{i,t-2} + \eta_1 T_{i,t-2} + \eta_2 \mathbf{P}_{i,t-2} + \lambda_i + \epsilon_{i,t}$$
$$D_{it} = \beta_1 MB_{i,t-2} + \eta C_{i,t-2} + \epsilon_{i,t}$$
$$\Leftrightarrow M_C D_{it} = \beta_1 M_C MB_{i,t-2} + \epsilon_{i,t}$$

where $M_C D_{it}$ and $M_C D_{i,t-2}$ are residuals from an OLS regression of $D_{i,t}$ and $MB_{i,t-2}$ on controls. In our setting, we have two measurements of mobility MB and an instrument rainfall z:

- $\tilde{MB}_1$: a random variable that measures MB with error, so that

$$\tilde{MB}_1 = MB + u_1$$
$$\Leftrightarrow M_C \tilde{MB}_1 = M_C MB + M_C u_1$$

- $\tilde{MB}_2$: another random variable that measures MB with error, so that

$$\tilde{MB}_2 = MB + u_2$$
$$\Leftrightarrow M_C \tilde{MB}_2 = M_C MB + M_C u_2$$

- Rainfall z: a good (relevant and exogenous) instrument for mobility

We make five reasonable assumptions about the measurement errors. First, we believe that the measurement errors on mobility indices are uncorrelated with individuals' decision to social distance because individuals generally do not know or care about their phone being tracked. Therefore, given a constant matrix M_C, we have that:

$$\begin{aligned} Cov(MB, u_1) = Cov(MB, u_2) = 0 \\ \Rightarrow Cov(M_C MB, M_C u_1) = M_C(MB, u_1)M_C' = 0 \\ \Rightarrow Cov(M_C MB, M_C u_2) = M_C(MB, u_2)M_C' = 0 \end{aligned} \tag{Assumption 1}$$

Second, it's reasonable to assume that different companies using similar methodologies collect data from different phones. Therefore, the measurement error from one source is uncorrelated with the measurement error from the other source:

$$Cov(u_1, u_2) = 0 \tag{Assumption 2}$$

Third, we believe that the measurement errors on mobility indices are not correlated with the controls included in the empirical model. This is because we believe the measurement errors are random errors and are not systematic errors that depend on changes in the controls. Therefore, we have $Cov(C, u_1) = Cov(C, u_2) = 0$. This implies the following:

$$\begin{aligned} M_C u_1 &= u_1 \\ M_C u_2 &= u_2 \end{aligned} \tag{Assumption 3}$$

We require two more assumptions about the measurement errors in relation to the instrument rainfall and error term in the empirical model:

$$\begin{aligned} Cov(\epsilon, u_1) = Cov(\epsilon, u_2) = 0 \\ Cov(z, u_1) = Cov(z, u_2) = 0 \end{aligned} \tag{Assumption 4}$$

Here, we believe that these are reasonable assumptions because our proposed sources of measurement error (choice of phones to track and measurement methodology) are uncorrelated with rainfall and COVID-19 mortality. Moreover, our second stage IV regression results in Section 6 imply that we have $Cov(Z, u_1) = Cov(z, u_2)$ because the coefficient on mobility from the two IV regressions using Descartes Lab and Unacast are similar and overlap in the 95% confidence interval. In other words, our instrument was successful in purging out the measurement error component from mobility.

7.1 OLS Regression

Let's examine the β_1^{OLS} coefficient on $\tilde{M}B_1$ for the regular OLS regression. We can express β_1^{OLS} as the following using the Frisch-Waugh Theorem:

$$\begin{aligned} \beta_1^{OLS} &= \frac{Cov(M_C D, M_C \tilde{M}B_1)}{Var(M_C \tilde{M}B_1)} \\ &= \frac{Cov(M_C D, M_C MB + M_C u_1)}{Var(M_C \tilde{M}B_1)} \\ &= \frac{Cov(M_C D, M_C MB)}{Var(M_C \tilde{M}B)} + \frac{Cov(M_C D, u_1)}{Var(M_C \tilde{M}B_1)} \\ &= \frac{Cov(\beta_1 M_C MB + \epsilon, M_C MB)}{Var(M_C \tilde{M}B_1)} + \frac{Cov(M_C D, u_1)}{Var(M_C \tilde{M}B_1)} \\ &= \beta_1 \frac{Var(M_C MB)}{Var(M_C \tilde{M}B)} + \frac{Cov(\epsilon, M_C MB)}{Var(M_C \tilde{M}B_1)} + \frac{Cov(M_C D, u_1)}{Var(M_C \tilde{M}B_1)} \end{aligned}$$

Here, we have the following:

$$\frac{Cov(M_C D, u_1)}{Var(M_C \tilde{MB}_1)} = \frac{Cov(\beta_1 M_C MB + \epsilon, u_1)}{Var(M_C \tilde{MB}_1)}$$
$$= \beta_1 \frac{Cov(M_C MB, u_1)}{Var(M_C \tilde{MB}_1)} + \frac{Cov(\epsilon, u_1)}{Var(M_C \tilde{MB}_1)}$$

Assuming that $Cov(M_C MB, u_1) = 0$ and $Cov(\epsilon, u_1) = 0$ from (Assumption 1) and (Assumption 4), we get:

$$\beta_1^{OLS} = \beta_1 \frac{Var(M_C MB)}{Var(M_C \tilde{MB}_1)} + \frac{Cov(\epsilon, M_C MB)}{Var(M_C \tilde{MB}_1)}$$

7.2 IV regression with z as instrument

Now, consider the IV estimator $\beta_{1,z}^{IV}$ of $\tilde{MB}$ using rainfall z as an instrument. By Frisch-Waugh we can express $\beta_{1,z}^{IV}$ as:

$$\beta_{1,z}^{IV} = \frac{Cov(M_C D, z)}{Cov(M_C \tilde{MB}_1, z)} = \frac{Cov(\beta_1 M_C MB + \epsilon, z)}{Cov(M_C MB + M_C u_1, z)} = \frac{Cov(\beta_1 M_C MB + \epsilon, z)}{Cov(M_C MB + u_1, z)}$$

Here, we assume that $Cov(z, u_1) = 0$ and $Cov(z, \epsilon) = 0$ from (Assumption 4). Then, we have the following:

$$\beta_{1,z}^{IV} = \frac{Cov(\beta_1 M_C MB + \epsilon, z)}{Cov(M_C MB + u_1, z)} = \beta_1$$

7.3 IV regression with $\tilde{MB}$ as instrument

Consider using $\tilde{MB}_2$ as an instrument for $\tilde{MB}_1$. In this setting, we have relevance because they are both measurements of the same population mobility number. However, we do not fulfill the exclusion restriction requirement. We can express $\beta^{IV}_{1,\tilde{MB}_2}$ as the following using the Frisch-Waugh Theorem:[1]

$$\begin{aligned}
\beta^{IV}_{1,\tilde{MB}_2} &= \frac{Cov(M_C D, M_C \tilde{MB}_2)}{Cov(M_C \tilde{MB}_1, M_C \tilde{MB}_2)} \\
&= \frac{Cov(\beta_1 M_C MB + \epsilon, M_C MB + u_2)}{Cov(M_C MB + u_1, M_C MB + u_2)} \\
&= \frac{\beta_1 Cov(M_C MB, M_C MB) + \beta_1 Cov(M_C MB, u_2) + Cov(\epsilon, M_C MB) + Cov(\epsilon, u_2)}{Cov(M_C MB, M_C MB) + Cov(M_C MB, u_1) + Cov(M_C MB, u_2) + Cov(u_1, u_2)}
\end{aligned}$$

Here, we assume that $Cov(M_C MB, M_C u_1) = Cov(M_C MB, u_1) = 0$, $Cov(M_C MB, M_C u_2) = Cov(M_C MB, u_2) = 0$, $Cov(\epsilon, u_2) = Cov(u_1, u_2) = 0$ from (Assumption 1) and (Assumption 4). Then, we have the following:

$$\beta^{IV}_{1,\tilde{MB}_2} = \beta_1 + \frac{Cov(\epsilon, M_C MB)}{Var(M_C MB)}$$

7.4 Identification

Given the assumptions and coefficient results from Section 7.1, Section 7.2, and Section 7.3, let's identify the unobserved quantities: β_1, $Cov(\epsilon, M_C MB)$, $Var(M_C MB)$, and $Var(u_1)$. We have the following:

$$\beta^{OLS}_1 = \beta_1 \frac{Var(M_C MB)}{Var(M_C \tilde{MB}_1)} + \frac{Cov(\epsilon, M_C MB)}{Var(M_C \tilde{MB}_1)} \tag{7.4.1}$$

$$\beta^{IV}_{1,z} = \beta_1 \tag{7.4.2}$$

$$\beta^{IV}_{1,\tilde{x}_2} = \beta_1 + \frac{Cov(\epsilon, M_C MB)}{Var(M_C MB)} \tag{7.4.3}$$

First, we assume that $Cov(M_C MB, M_C u_1) = Cov(M_C MB, u_1) = 0$ from (Assumption 1). Therefore, we have the following:

$$Var(M_C \tilde{MB}_1) = Var(M_C MB + M_C u_1) = Var(M_C MB) + Var(u_1) \tag{7.4.4}$$

[1] Here, for algebraic simplicity, we use $M_C \tilde{MB}_2$ instead of $\tilde{MB}_2$. We know that using either instruments yields the same coefficient $\beta^{IV}_{1,\tilde{MB}_2}$. We have the following regression:

$$D_{it} = \beta_1 MB_{i,t-2} + \eta C_{i,t-2} + \epsilon_{i,t}$$

Using 2SLS, we first get $\hat{MB}_{i,t-2}$ using either $\tilde{MB}_2$ or $M_C \tilde{MB}_2$. Here, the predicted $\hat{MB}_{i,t-2}$ can be calculated as the following:

$$M = \xi_0 + \xi_1 \tilde{MB}_2 + \xi_2 C + \mu$$
$$M = \alpha_0 + \alpha_1 M_C \tilde{MB}_2 + \alpha_2 C + \nu$$

Applying Frisch-Waugh Theorem, we have the following:

$$M = \xi_0 + \xi_1 M_C \tilde{MB}_2 + \mu$$
$$M = \alpha_0 + \alpha_1 M_C \tilde{MB}_2 + \nu$$

because $M_C M_C \tilde{MB}_2 = M_C \tilde{MB}_2$. So, we have the same predicted $\hat{MB}_{i,t-2}$ using either instruments, $\tilde{MB}_2$ or $M_C \tilde{MB}_2$. This will lead to the same coefficient $\beta^{IV}_{1,\tilde{MB}_2}$.

By substituting the expression for $Var(M_C\tilde{M}B_1)$ into (7.4.1), we have the following:

$$\beta_1^{OLS}(Var(M_CMB) + Var(u_1)) = \beta_1 Var(M_CMB) + Cov(\epsilon, M_CMB) \tag{7.4.5}$$

From expression (7.4.3), we have the following:

$$\beta_{1,\tilde{x}_2}^{IV} Var(M_CMB) = \beta_1 Var(M_CMB) + Cov(\epsilon, M_CMB) \tag{7.4.6}$$

Combining (7.4.4) and (7.4.5), we have the following:

$$\beta_1^{OLS}(Var(M_CMB) + Var(u_1)) = \beta_{1,\tilde{x}_2}^{IV} Var(M_CMB) \tag{7.4.7}$$

Here, if we substitute the expression of (7.4.4), we have the following:

$$Var(M_CMB) = \frac{\beta_1^{OLS}}{\beta_{1,\tilde{x}_2}^{IV}} Var(M_C\tilde{M}B_1) \tag{7.4.8}$$

From (7.4.4), substituting the expression for $Var(M_CMB)$, we have the following:

$$Var(u_1) = \left(1 - \frac{\beta_1^{OLS}}{\beta_{1,\tilde{x}_2}^{IV}}\right) Var(M_C\tilde{M}B_1)$$

By substituting the expression for $Var(M_CMB)$ and β into (7.4.3), we have the following:

$$Cov(\epsilon, M_CMB) = \frac{\beta_1^{OLS}}{\beta_{1,\tilde{x}_2}^{IV}}(\beta_{1,\tilde{x}_2}^{IV} - \beta_{1,z}^{IV}) Var(M_C\tilde{M}B_1)$$

7.5 Bias Calculation

Given the expressions of the unobserved quantities in Section 7.4, we can now measure the bias from measurement error and endogeneity. From the naive OLS regression, we have the following:

$$\beta_1^{OLS} = \beta_1 \frac{Var(M_CMB)}{Var(M_C\tilde{M}B_1)} + \frac{Cov(\epsilon, M_CMB)}{Var(M_C\tilde{M}B_1)}$$

Therefore, we have the following:

- The omitted variable bias if you observed M_CMB and used it in the initial regression can be represented as the following:

$$\frac{Cov(\epsilon, M_CMB)}{Var(M_CMB)} = \beta_{\tilde{x}_2}^{IV} - \beta_z^{IV}$$

- The bias due to the omitted variables in β^{OLS} can be represented as the following:

$$\frac{Cov(\epsilon, M_C MB)}{Var(M_C \tilde{M}B_1)} = \frac{\beta^{OLS}}{\beta^{IV}_{\tilde{x}_2}} \left(\beta^{IV}_{\tilde{x}_2} - \beta^{IV}_z\right)$$

- The bias due to measurement error in β^{OLS} can be represented as the following:

$$\frac{Var(M_C MB)}{Var(M_C \tilde{M}B_1)} = \frac{\beta^{OLS}}{\beta^{IV}_{\tilde{x}_2}}$$

The following are relevant regressions necessary to calculate the bias coming from measurement error and endogeneity.

Table 3: Second Stage: Change in COVID Mortality Per Capita

	OLS	2SLS	Measurement Error
	(1)	(2)	(3)
ΔMobility$_{t-3}$	–2.65e-06	1.51e-05*	–1.12e-06
	(2.51e-06)	(8.38e-06)	(2.67e-06)
Emergency Declaration$_{t-3}$	1.98e-06	6.70e-06***	–6.53e-06
	(2.01e-06)	(2.16e-06)	(4.31e-06)
Gathering Restriction (Any)$_{t-3}$	1.83e-05***	2.25e-05***	1.96e-05***
	(6.42e-06)	(7.42e-06)	(6.34e-06)
ΔCOVID Mortality Per Capita$_{t-4}$	1.66e+00***	9.19e-01**	9.00e-01
	(6.14e-01)	(4.01e-01)	(5.79e-01)
Constant	–4.96e-06*	–3.67e-06	9.37e-07
	(2.67e-06)	(2.32e-06)	(1.48e-06)
N	306	300	250
R^2	0.630	0.611	0.714
Adj. R^2	0.551	0.527	0.636
State FE	Yes	Yes	Yes

Robust standard errors in parentheses. Standard errors clustered by state.

With these estimates, we can calculate the bias as the following:

- The omitted variable bias if you observed $M_C MB$ and used it in the initial regression can be represented as the following:

$$\frac{Cov(\epsilon, M_C MB)}{Var(M_C MB)} = \beta^{IV}_{\tilde{x}_2} - \beta^{IV}_z = -1.12 \times 10^{-6} - 1.51 \times 10^{-5} \approx -1.62 \times 10^{-5}$$

- The bias due to the omitted variables in β^{OLS} can be represented as the following:

$$\frac{Cov(\epsilon, M_C MB)}{Var(M_C \tilde{M}B_1)} = \frac{\beta^{OLS}}{\beta^{IV}_{\tilde{x}_2}} \left(\beta^{IV}_{\tilde{x}_2} - \beta^{IV}_z\right) = \frac{-2.65 \times 10^{-6}}{-1.12 \times 10^{-6}}(-1.12 \times 10^{-6} - 1.51 \times 10^{-5}) \approx -3.84 \times 10^{-5}$$

- The bias due to measurement error in β^{OLS} can be represented as the following:

$$\frac{Var(M_C MB)}{Var(M_C \tilde{M}B_1)} = \frac{\beta^{OLS}}{\beta^{IV}_{\tilde{x}_2}} = \frac{-2.65 \times 10^{-6}}{-1.12 \times 10^{-6}} \approx 2.37$$

The above results show us that the attenuation bias from measurement error is much larger than the bias from endogeneity. This suggests that researchers should be much more concerned about bias from measurement error than from endogeneity when using mobility data. Moreover, we must be very skeptical when interpreting results that use mobility measurements but do not correct for measurement error.

8 Conclusion

This paper investigates the causal effect of mobility on COVID-19 related mortality. Many papers evaluate the effectiveness of government policy, such as emergency declarations and stay at home orders, on COVID-19 related mortality. Politicians expect these policies to reduce mortality through their effect on mobility, making it very important to evaluate the causal relationship between mobility and COVID-19 mortality. However, we believe that mobility measurements are contaminated by measurement error. Additionally, population mobility is a choice variable that is correlated with unobservables such as perceived risk from COVID-19. To resolve measurement error and endogeneity, we instrument mobility with rainfall, which is uncorrelated with the measurement error. To find the partial effects of measurement error and endogeneity, we use mobility indices from two different organizations.

Our IV regression results show a positive relationship between mobility and COVID-19 related mortality. Specifically, we find that a one standard deviation increase in the weekly change in mobility is associated with a 7.34×10^{-6} per capita (7.34 per million) increase in mortality 3 weeks in the future. This is a substantial effect considering that the average COVID-19 related mortality for the week of April 20 in our data is 96.7×10^{-6} (96.7 per million). We also follow the strategy used by Acemoglu et al. (2001) to examine the bias coming from measurement error and endogeneity. We note that the bias from measurement error is much larger than that from endogeneity.

We could extend this research by estimating a more nonlinear model that reflects the fact that mobility reductions would save more lives if a lot of people are currently infected. Mathematically, the regression would look like:

$$D_{it} = \beta_0 + \beta_1 \cdot MB_{i,t-3} + \beta_2 \cdot MB_{i,t-3} \cdot I_{i,t-3} + \eta_1 \mathbf{P}_{i,t} + \lambda_i \chi_{\text{State}} + \gamma_i \chi_{\text{State}} \cdot t + \epsilon_{i,t}$$

which is our original empirical model with an added interaction term between mobility ($MB_{i,t-3}$) and infection rate ($I_{i,t-3}$). In this model, a downward spike in mobility at time t would reduce the death rate by $\beta_1 + \beta_2 \cdot I_{i,t}$. The estimates β_1 and β_2 decompose this effect into the intrinsic effect of mobility versus the effect related to the number of people already infected (which part of the "curve" we are on). If we want to make the above interpretation, we can't re-run the IV regression with this new empirical model because mobility and infection rate are both endogeneous. To resolve this, we would need another relevant and exogeneous instrument for $MB_{i,t} \cdot I_{i,t}$. Without an additional instrument, we cannot correctly interpret β_1 as the intrinsic effect of mobility on death rates after controlling for the amplification effect. However, finding an instrument that satisfies both relevance and the exclusion restriction for the interaction term $MB_{i,t} \cdot I_{i,t}$ could be difficult. Instead, it is sufficient to find a good instrument for infection rates alone.[2]

Ultimately, while our finding of positive effects of social distancing on COVID-19 related mortality is consistent with past research, we have contributed to existing literature by empirically validating that social distancing has a three week lagged effect through LASSO and quantifying the effect of measurement error from mobility data. Our measurement error results indicate that while past research may overstate the effects of social distancing on COVID-19 related mortality, even corrected estimates are large enough to justify social distancing measures.

[2]Consider the following regression:

$$D_{it} = \beta_1 MB_{i,t-2} + \beta_2 MB_{i,t-2} \cdot I_{i,t-2} + \eta C_{i,t-2} + \epsilon_{i,t}$$
$$\Leftrightarrow M_C D_{it} = \beta_1 M_C MB_{i,t-2} + \beta_2 M_C MB_{i,t-2} \cdot I_{i,t-2} + \epsilon_{i,t}$$

In order to have a consistent estimate of β_2, we need the following:

$$E[M_C(MB_{i,t-2} \cdot I_{i,t-2}) \cdot \epsilon_{i,t}] = 0$$

When running the IV regression, we need the following where $\hat{MB}_{i,t-2}$ comes from the first stage:

$$E[\widehat{M_C MB}_{i,t-2} \cdot I_{i,t-2} \cdot \epsilon_{i,t}] = 0$$
$$\Leftrightarrow E[I \cdot E[\widehat{M_C MB} \cdot \epsilon | I]] = E[I \cdot \epsilon \cdot E[\widehat{M_C MB} | I, \epsilon]]$$

where $\widehat{M_C MB}_{i,t-2} = \hat{\gamma}_0 + \hat{\gamma}_1 M_C z$. However, this expression is not equal to zero because we believe that $Cov(M_C z, I) \neq 0$ as rainfall and infection rate may be correlated and thus $E[\widehat{M_C MB} | I, \epsilon] \neq 0$. So, we cannot get a consistent estimator for β_2. Instead, we can find an instrument for I as well. In this case, we have the following:

$$E[\widehat{M_C MB}_{i,t-2} \cdot \hat{I}_{i,t-2} \cdot \epsilon_{i,t}] = E[\epsilon \cdot E[\widehat{M_C MB}_{i,t-2} \cdot \hat{I}_{i,t-2} | \epsilon]] = 0$$

where $\hat{I}_{i,t-2} = \hat{\alpha}_0 + \hat{\alpha}_1 z$. This holds because $\widehat{M_C MB}_{i,t-2}$ and $\hat{I}_{i,t-2}$ are mean independent of ϵ by construction of a good instrument.

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