THE DUKE UNIVERSITY ECONOMICS REVIEW

EQUILIBRIA

BRINGING YOU A DIFFERENT PERSPECTIVE ON THE ECONOMICS WORLD

Profit Benefits of Sustainability

The Externalities of Traffic Congestion

Public Health and Demand During COVID-19

Economic Growth and Universities

Spring 2021

EDITOR'S NOTE

The rigorous economics curriculum at Duke provides proper knowledge and tools that can be utilized in an attempt to quantify human behavior and relationships. But original research requires building on this foundation with creativity and willingness to uncover insights. The COVID-19 pandemic has presented a unique opportunity to develop solutions to unprecedented issues. As such, we have attempted to synthesize research in this rapidly changing environment into this creative composition. This journal contains innovative inquiries into public health government policy, green energy profit benefits, traffic congestion effects, Ukrainian economic development, altruistic demand, and universities and growth relationships. Our team strives to educate readers by providing well-argued practical pieces regarding issues and trends in economics and business, expand knowledge with regard to foreign nations, and motivate new explorations utilizing contemporary techniques.

Sincerely,

Kelly Gourrier

DUKE ECONOMICS REVIEW

EQUILIBRIA: The Duke University Economics Review is the official publication of the Economics Student Union at Duke University. This magazine is dedicated to presenting select research and opinions by university scholars on all fields relevant to economics.

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Regarding questions on submissions and other suggestions, please contact *Equilibria: The Duke University Economics Review* at dukeequilibria@gmail.com.

Diminishing Returns to Public Health?: Mask Use, Stay-At-Home Order Effectiveness, and COVID-19 Incidence

By: Hunter Treschl

Grinnell College

On May 1st, 2020, a group of armed protesters entered Michigan's capitol building. They stood on balconies overlooking state legislators, demanding an end to Michigan's emergency stay-at-home orders, put in place to prevent the spread of COVID-19. Outside the statehouse, a larger group of protestors, also armed, chanted and disrupted traffic (IANS 2020).

Michigan is not an isolated case: other protests against stay-at-home orders and mandatory business closures have swept the United States. Protestors' concerns are often valid: many have lost their jobs or had their businesses close as a result of stay-at-home orders. Estimates suggest that each week of stay-at-home orders increases state-level unemployment claims by nearly 2% (Baek, McCrory, Messer, and Mui 2020).

The costs of imposing stay-at-home orders are drastic. However, stay-at-home orders and government action decrease COVID-19 caseload and prevent hospitals from being overwhelmed, limiting deaths. It is therefore imperative that policy makers have as much information as possible about the effects of implementing (or not implementing) stay-at-home orders.

Luckily, a growing body of literature estimates the effect of stay-at-home orders and other policies on COVID-19 caseload. This research has focused primarily on the effectiveness of stay-at-home orders and other government countermeasures in isolation. However, in reality, countermeasures like stay-at-home orders are not deployed in isolation but as part of broader policy aimed at slowing the spread of SARS-CoV-2. I attempt to fill this gap in the literature by estimating the effect of stay-at-home orders in the presence of another common countermeasure: masks. I find that stay-at-home orders are nearly twice as effective in counties with relatively low mask usage. In counties with high mask usage, stay-at-home orders are much less effective. Moreover, in all counties except those with low mask usage, advisories are as effective as mandatory orders.

My results have broad policy implications. I suggest that, given the decreasing effectiveness of stay-at-home orders with increasing mask usage, rewarding consistent mask usage by lifting or easing stay-at-home orders could be an effective tool to incentivize mask-wearing. Additionally, advisories are as effective as mandatory orders except in low mask-use counties. Policymakers can achieve the results of a mandatory order while seeming less extreme to constituents who disagree with the principle of stay-at-home orders by enacting advisories in medium and high mask-usage counties. However, advisories are ineffective in low mask-use counties and policy makers there should enact and enforce mandatory stay-at-home orders to lower COVID-19 spread

I. Literature Review

Michigan is not an isolated case: other protests against stay-at-home orThere seems to be new research on the effects of COVID-19 that comes out every day, and my work is neither the first that attempts to measure the effects of government policies nor the first to leverage exogenous policy implementation to estimate a difference-in-differences (DD) model. For example, Van Dyke et al. (2020) leverage exogenous determination of mask mandates in Kansas to estimate the effect of mask mandates on case growth. The Kansas study employs a similar difference-in-difference as I; however, their analysis is limited to mask usage. Xu et al. (2020) use time series discontinuity to evaluate the effect of face-masking and stay-at-home orders. Their use of regression discontinuity likely yields results that are less accurate than other, more common difference-in-difference models, although they do find that government countermeasures are effective.

Abouk and Heydari (2020) also leverage exogenous policy implementation and prove a mechanism whereby government stay-at-home orders reduce the spread of COVID-19. They do so by measuring the effect of six government policies, including stay-at-home orders, on people's movement. Government policies generally seek to slow the spread of COVID-19 by restricting movement and minimizing contact. By estimating the effects of these policies on people's movement, the authors provide a measure of whether the six studied policies are working as intended. They find that "strong" policies like stay-at-home orders and non-essential business closures are highly effective at restricting movement, while more lenient policies are less effective. Their work supports my research and research by other authors looking directly at the effect of policy on case growth by showing that stay-at-home orders encourage social distancing and limit movement, which slows the spread of COVID-19.

In the research most closely related to my own, Courtemanche et al. (2020) employ an event study design to measure the effect of government policies in a series of time frames after they are implemented. Their model is essentially a difference-in-difference approach and is in many ways similar to my study. They estimate the effects on case growth of stay-at-home orders, school closures, bans on large gatherings, and bar and gym closures. However, their study has limitations. They do not discriminate between mandatory stay-at-home orders and advisories. More importantly, they do not measure the effect of government policies when other policies are or are not in place. For example, they do not provide different estimates for the effect of school closures in counties that do or do not also ban large gatherings. This is especially problematic because policies are often implemented in tandem, and there are very few counties that implement only one public health measure. I attempt to fill this gap in the literature by estimating the interaction between masking and stay-at-home orders.

II. Data

Data on COVID-19 cases and deaths come from *The New York Times*, who aggregate data from county and state daily reports (The New York Times 2020). Data on deaths includes probable COVID-19 deaths. COVID-19 case and death data are available for starting from the earliest confirmed cases in February through the present. I limit my analysis to the period before May 1st, 2020 since I am estimating the effect of stay-at-home orders implemented in late March and early April.

Data on mask usage come from a New York Times survey conducted between July 2nd and July 14th, 2020. The survey was conducted across the US and has 250,000 responses. Participants were asked "How often do you wear a mask in public when you expect to be within six feet of another person?" and could respond "Never," "Rarely," "Sometimes," "Frequently," or "Always." Despite these data being collected after the period I study, the difference in dates is unlikely to bias my estimates because my analysis relies only on relative mask usage. Since I rely on relative mask usage, my results are unaffected by general trends in mask usage (e.g. an increase in usage across all response categories).

Data on stay-at-home orders come from The Centers for Disease Control and Prevention (CDC) (Moreland et al. 2020). The CDC collects data on stay-at-home orders from local governments, classifies them, and aggregates the data as a county-date panel. They include four levels of stay-at-home orders with varying degrees of severity: no order, advisory, mandatory for at-risk individuals, and mandatory for everyone. In my analysis, I compare new COVID-19 cases in counties before and after they implement mandatory stay-at-home orders and stay-at-home advisories to counties that never implement stay-at-home orders during the studied period.

I limit my observations only to counties with active COVID-19 cases prior to the implementation of stay-at-home orders. I do so because stay-at home orders were often instituted at the state level. State-level orders led to many counties with no COVID-19 cases — and therefore little to no local spread — to be under stay-at home orders. In these counties, observed lack of local transmission after the implementation of stay-at-home orders is not due to the order but rather to a lack of initial cases. Dropping counties with no active cases leaves 1,450 counties in the dataset.

Table 1: Descriptive Statistics

	Mean	SD	Count	Min	Max
Relative Date	2.639	6.99	61564	-14	14
Cases	139.9	1023.5	61564	0	59905
New Cases	7.46	47.6	60878	-47	2174
Log(Daily New Cases)	.648	1.152	61564	0	7.68
After Mandatory Stay-At-Home	.652	.476	61564	0	1
After Stay-At-Home Advisory	.057	.233	61564	0	1
% of Time Mask Worn	.782	.092	61564	.354	.961
Low Mask Use	.311	.463	61564	0	1
High Mask Use	.298	.457	61564	0	1
Observations	61564				

Descriptive statistics. The mean of relative date is not zero because some counties are treated more than once; fixed effects standardize later treatments.

III. Empirical Strategy

I measure the effect of stay-at-home orders on new COVID-19 cases in counties with varying severity of stay-at-home orders using a difference-in-differences approach. I match counties by lining up the day they institute stay-at-home orders and comparing daily case change in those counties by the type of order they institute. Counties that never institute stay-at-home orders serve as the baseline. My model to measure the effect of different order types is a traditional difference-in-differences model with fixed effects (1).

To understand how mask wearing alters the effectiveness of stay-at-home orders, I turn to a triple-differenced model that adds an interaction term between the DD estimate for each treatment group and a dummy variable for mask-usage. In these models, the interaction term measures the additional effect of stay-at-home orders in counties with the specified level of mask-usage. The net effect of stay-at-home orders in these high or low mask-usage counties is the baseline DD effect added to the triple-differenced estimate. In these triple-differenced models, effects by level of mask-usage are relative to all other counties. For instance, in (3), my preferred specification, mask-usage estimates are relative to the middle 40% of counties.

I run three variations of the triple-differenced model to measure the effect of stay-at-home orders in counties with high and low mask-usage. low mask-usage. First, I run two triple-differenced models with an interaction term for high mask-usage counties and low mask-usage counties, respectively. The final model includes interactions for both high and low mask-usage counties (3). This combined model is my preferred specification and is necessary to ensure high mask-usage counties don't bias the baseline coefficient in the low mask-usage model and vice versa. In other words, if stay-at-home orders are most effective in high mask-usage counties and they remain in the base group, stay-at-home orders will appear to be more effective for all counties.

(1) $NewCases_{c,t} = \beta_0 + \beta_1 Mandatory_c Post_t + \beta_2 Advisory_c Post_t + \beta_3 Post_t + \gamma_c + \rho_t + \epsilon_{c,t}$

(2) $NewCases_{c,t} = \beta_0 + \beta_1 Mandatory_c Post_t + \beta_2 Advisory_c Post_t + \beta_3 Post_t + Mask(\beta_4 Mandatory_c Post_t + \beta_5 Advisory_c Post_t) + \gamma_c + \rho_t + \epsilon_{c,t}$

(3) $NewCases_{c,t} = \beta_0 + \beta_1 Mandatory_c Post_t + \beta_2 Advisory_c Post_t + \beta_3 Post_t + LowMask(\beta_4 Mandatory_c Post_t + \beta_5 Advisory_c Post_t) + HighMask(\beta_6 Mandatory_c Post_t + \beta_7 Advisory_c Post_t) + \gamma_c + \rho_t + \epsilon_{c,t}$

In these models, *NewCases* is the natural log of daily new cases; *Mandatory* is a dummy for whether a county received a mandatory stay-at-home order; *Advisory* is a dummy for whether a county received a stay-at-home advisory; *Post* is a dummy variable for the period after stay-at-home orders have been implemented; the interaction between the *Post* and *Mandatory* or *Advisory* is the DD term; *Mask* is a dummy variable of whether a county is a low or high mask-use county where *MaskHigh* and *MaskLow* refer to counties in the top 30% and bottom 30% of mask-usage, respectively; γ are county fixed effects and ρ are date fixed effects. All es-

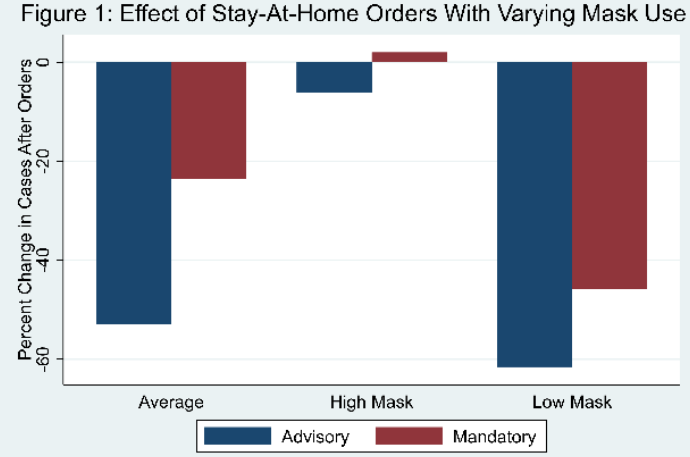
estimates are clustered at the county level.

For my model to be valid, the trajectory in COVID-19 case growth must be the same in the treatment and control groups. I am confident this assumption holds in most instances. Date and county fixed effects control for constant variation across county and date. Thus, the primary concern are time-variant effects that change the number of cases in the treatment and control groups. There are certainly concerns, though: one potential problem is testing availability. Testing capacity is time variant and directly affects the number of reported cases. This is a problem because places initially severely affected by COVID-19 did not have the testing capacity to confirm all existing COVID-19 cases prior to instituting stay-at-home orders. However, as tests became available, more people were tested, and more cases were reported. Thus, my models likely underestimate the effect of mandatory stay-at-home orders at the beginning of the pandemic because of limited testing capacity in the early months of the pandemic. The effect of advisories is less biased by limited testing because advisories were almost never instituted in counties with very high initial caseloads. Instead, politicians in counties with high initial caseloads immediately instituted mandatory stay-at-home orders.

IV. Results

My models yield a series of interesting results. The simple DD estimates for the effect of different stay-at-home orders suggest that, not surprisingly, new daily cases are 21% (95% CI: 14% to 28%) lower in counties with mandatory stay-at-home orders and 33% (95% CI: 24% to 44%) lower in counties with stay-at-home advisories than in counties with no orders. While it may be surprising that there is no statistically significant difference between advisories and mandatory orders, my results echo evidence from Sweden that suggests voluntary measures to slow the spread of COVID-19 can be as effective mandatory measures (Kamerlin and Kasson 2020). In all of my models, my results indicate that advisories and mandatory stay-at-home orders are equally effective in decreasing new COVID-19 cases, with the notable exception of low mask-usage counties.

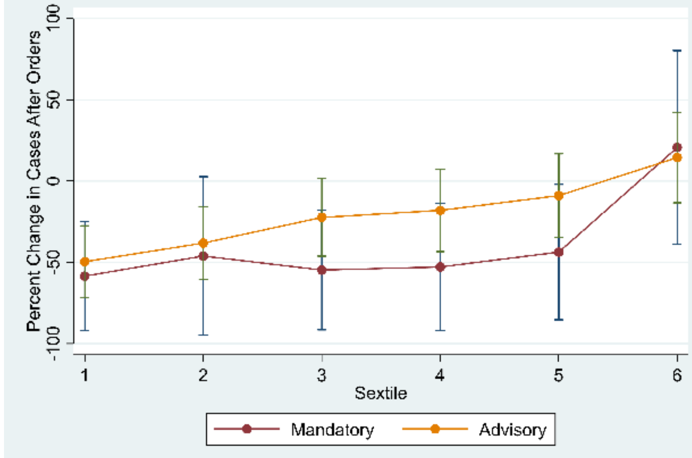
Figure 1: Effect of Stay-At-Home Orders With Varying Mask Use



Note: Low refers to bottom 30%; high to top 30%

The more interesting results come from the triple-differenced models. The first model measures the additional effect of stay-at-home orders on counties with low mask usage. In counties with low mask usage, mandatory stay-at-home mandates reduce daily new cases by 34.4% on top of a 15% reduction across all counties for a net 49% reduction. Advisories show an additional 26% decrease on top of a 32% decrease across all counties. In other words, stay-at-home policies, mandatory or simply advisories, are roughly twice as effective at slowing COVID-19 spread in low mask-use counties as they are in other counties.

Figure 2: Effect of Stay-At-Home Orders by Mask-Use Sextile



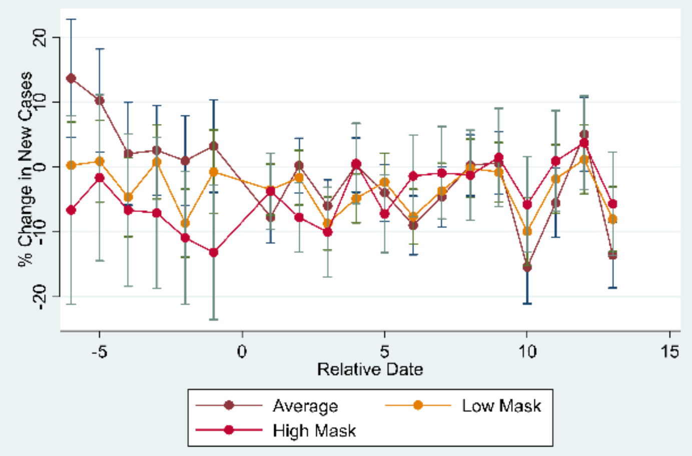
In high mask-usage counties, on the other hand, stay-at-home orders have a net effect of essentially zero. The net effect of advisories in high mask-use counties is a 6% reduction in new cases. For mandatory orders, the net effect is a 1.8% increase in new cases, which seems to contradict basic epidemiology theory. However, these results are likely biased. Counties in the northeast that instituted mandatory lockdowns and where mask usage was relatively common experienced runaway case growth after mandatory stay-at-home orders relative to the fixed effects and the pre-trend controls. Fixed effects are unable to control for early case growth because these counties were the first to be affected by the COVID-19 pandemic in the US, and testing was not widespread enough to allow for the documentation of all cases. In other words, there is likely an omitted variable for testing availability that biases the results and cannot be controlled by pre-trends or the date and county fixed effects since testing capacity is time-variant.

Table 2: The Effect of Stay-At-Home Orders on Case Growth	(1) DD	(2) DDD Low Mask Use	(3) DDD High Mask Use	(4) DDD All Mask Levels
After Mandatory Stay-At-Home	-0.216*** (0.0734)	-0.128* (0.0740)	-0.340*** (0.0762)	-0.246*** (0.0792)
After Stay-At-Home Advisory	-0.339*** (0.0950)	-0.300*** (0.105)	-0.559*** (0.0834)	-0.540*** (0.0926)
After any mandate	0.219*** (0.0766)	0.251*** (0.0760)	0.249*** (0.0775)	0.262*** (0.0768)
After Mandatory; Low Mask		-0.329*** (0.0317)		-0.221*** (0.0353)
After Advisory; Low Mask		-0.314*** (0.0779)		-0.0855 (0.0621)
After Mandatory; Mask High			0.360*** (0.0460)	0.259*** (0.0512)
After Advisory; Mask High			0.496*** (0.142)	0.468*** (0.149)

Note: Regression table for the effect of stay-at-home orders on the log of new COVID-19 cases. Coefficients should be interpreted as the percentage change in new cases. In (4), DDD estimates are relative to the average mask use counties (middle 40%); in (2) and (3) DDD estimates are relative to all other counties.
$^{*}p<.1$, $^{**}p<.05$, $^{***}p<.01$

This model also reveals a shortcoming of the mask data: since the survey was conducted in July, mask usage is likely influenced by the spread of COVID-19 in April, the period I study, and May. Thus, there is likely a reverse causality problem: mask usage is high in some counties because they had very high case growth in March. I expect some of this problem to be mitigated by the fact that my models rely on relative mask usage — how common mask usage is in one county relative to another — that removes bias from general trends such as a likely increase in mask usage across all counties from March to July. I also expect this problem to be somewhat mitigated by the politicization of mask-wearing. Since politics heavily influences people's decisions about how to respond to the virus

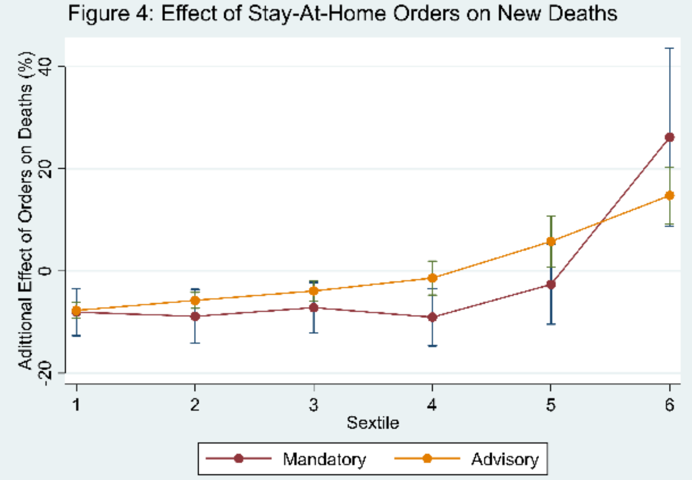
Figure 3: Effect of Mandatory Stay-At-Home Orders on Cases by Day



(Adolph et al. 2020), for example by wearing a mask, mask usage in July is less dependent on COVID-19 incidence in March and April. However, relative use and the politicization of masks do not entirely mitigate the reverse causality problem.

The final model gives similar results to the first three — in significance and magnitude — with one notable exception. In low mask-use counties, advisories no longer have a significant effect on new cases. This result is not entirely surprising: if people living in a county do not often wear masks, they are also not likely to respond to other voluntary public health measures such as stay-at-home advisories.

Figure 4: Effect of Stay-At-Home Orders on New Deaths



V. Robustness Checks

There are two primary concerns with my results. The first is that testing availability cannot be controlled adequately by fixed effects since it is both time- and county-variant.

Controlling for testing availability is straightforward. I do so by using deaths, rather than cases, as the response variable. Deaths related to COVID-19 are still a measure of spread in local populations but are not affected by the availability of local tests. This is because my data include probable COVID-19 deaths as well as

Table 3: The Effect of Stay-at-Home Orders on Daily Deaths	(1)	(2)	(3)	(4)
	DD	DDD Low Mask Use	DDD High Mask Use	DDD All Mask Levels
After Mandatory Stay-At-Home	-0.0878** (0.0383)	-0.0655* (0.0386)	-0.130*** (0.0391)	-0.113*** (0.0400)
After Stay-At-Home Advisory	-0.0741* (0.0439)	-0.0609 (0.0469)	-0.153*** (0.0385)	-0.153*** (0.0385)
After Any Order	0.00593 (0.0371)	0.0141 (0.0368)	0.0171 (0.0373)	0.0194 (0.0372)
After Mandatory; Low Mask		-0.0833*** (0.00942)		-0.0394*** (0.00801)
After Advisory; Low Mask		-0.0929*** (0.0274)		-0.00492 (0.00389)
After Mandatory; Mask High			0.122*** (0.0201)	0.104*** (0.0213)
After Advisory; Mask High			0.181*** (0.0539)	0.179*** (0.0540)

Note: Regression table for the effect of stay-at-home orders on the log of new COVID-19 deaths. High Mask refers to counties in the top 30% of mask use. Low mask refers to the bottom 30%. Coefficients should be interpreted as the percentage change in new cases. In (4), DDD estimates are relative to the average mask use counties (middle 40%); in (2) and (3) DDD estimates are relative to all other counties.
* $p < .1$, ** $p < .05$, *** $p < .01$

deaths confirmed by testing, so testing is not a necessary condition to declare that a death was caused by COVID-19.

I re-run each of my models using deaths as the response variable. The results from the new specification agree with the previous results using cases as the response variable, although the magnitude of the effect is lower. I suspect that the magnitude is lower here because deaths are a lagging indicator of community spread, and stay-at-home orders are slower to reduce deaths than to reduce cases. As before, I find that stay-at-home orders still decrease new deaths and that the effectiveness of stay-at-home orders decreases as mask usage increases. Consistent with my earlier findings, I also find that advisories do not have any additional effect in low mask-use counties while mandatory stay-at-home orders are more effective.

The other concern with my initial models is that there is reverse causality between case growth and mask usage. The reverse

Table 4: The Effect of Cases on Masks	(1)
	Mask Use
Mean Log of New Cases	0.0383*** (0.00174)

Regression table for the log of new daily cases on mask use.
* $p < .1$, ** $p < .05$, *** $p < .01$

causality comes from the fact that data on mask usage was colleted in July 2020 while case data comes from April and May. This is a problem because I assume that COVID-19 spread in the spring affected mask usage in the summer. If spread in the spring affected mask usage in the summer, my estimates for the effect of lockdowns in high mask-usage areas will be biased upwards, which is a particular problem because the primary result — that stay-at-home orders are less effective in high mask-use counties — is based on that coefficient being high.

However, the reverse causality is less of a problem that it may seem. I test the effect of case growth in March and April on mask usage in July using a simple OLS regression of new cases on mask usage. I find a statistically significant positive correlation between new cases in March and mask usage in July. However, the estimated coefficient suggests that a 100% increase in mean daily new cases

Table 5: Robustness Checks	(1) 28-day window	(2) 7-day window	(3) Mandatory Orders Only	(4) 3-Day Moving Average
After Stay-At-Home Advisory	-0.577^{***} (0.0997)	-0.340^{***} (0.0942)		-0.477^{***} (0.0871)
After Mandatory Stay-At-Home	-0.305^{***} (0.0868)	-0.183^{**} (0.0814)	-0.239^{***} (0.0831)	-0.198^{***} (0.0733)
After Mandatory; Mask High	0.286^{***} (0.0524)	0.149^{***} (0.0486)	0.259^{***} (0.0514)	0.226^{***} (0.0481)
After Mandatory; Low Mask	-0.221^{***} (0.0353)	-0.195^{***} (0.0353)	-0.226^{***} (0.0351)	-0.211^{***} (0.0336)
After Advisory; Mask High	0.534^{***} (0.169)	0.401^{***} (0.138)		0.437^{***} (0.138)
After Advisory; Low Mask	-0.118^{*} (0.0648)	-0.110^{*} (0.0608)		-0.0846 (0.0587)
After any mandate	0.286^{***} (0.0843)	0.193^{**} (0.0801)	0.263^{***} (0.0810)	0.181^{**} (0.0719)

Regression table for the effect of stay-at-home orders on the log of new COVID-19 cases. High Mask refers to counties in the top 30% of mask use. Low mask refers to the bottom 30%. All triple difference estimates relative to average mask use counties (middle 40%).
$^{*}p < .1$, $^{**}p < .05$, $^{***}p < .01$

only leads to a 3% increase in mask usage. So, case growth in March has a limited effect on mask usage in July. My results corroborate other research. Adolph et al. (2020) suggest that the response to COVID-19 is primarily rooted in politics. Given both my estimation and other literature, while mask usage in July is likely somewhat impacted by COVID-19 spread in March, this does not seriously hinder the robustness of my initial results. Given the high magnitude of my original estimates, I am not concerned that case growth in March had enough of an effect on July mask usage to make any of my estimates insignificant.

Finally, I confirm my results are robust with regards to stay-at-home order type and the length of the pre- and post-periods. I also confirm that they are not biased by any possible drastic, one-day variation in new cases. I re-run my preferred specification (3) and alter the initial 14-day post-period to 28 days and 7 days.

I find that changing the time frame has no effect on my results. Stay-at-home orders still increase in effectiveness as mask usage decreases. I also drop all counties that implemented advisories as an initial response and drop observations occurring after a county shifted from a mandatory order to an advisory. My results remain the same when I consider only mandatory stay-at-home orders. Finally, I use the 3-day moving average of new cases as the response variable to control for any single-day case spikes. Again, my results remain robust. The only change to my initial results is that advisories do have an additional impact in low mask-usage counties in the two models that change time frames, although both effects are significant only at the .1 level. As in my original results, advisories are less effective than mandatory orders in low mask-usage counties.

VI. Discussion

The results of my study broadly validate two important findings. First, mandatory stay-at-home orders and stay-at-home advisories significantly reduce daily COVID-19 case growth. This result supports other work showing that public health directives can greatly reduce COVID-19 incidence. Second, the results show that stay-at-home orders (mandatory or advisory) reduce new daily cases by almost twice as much in counties with low mask usage as they do in other counties. On the other hand, in high mask-usage counties, both types of stay-at-home orders are much less effective at reducing new cases. In fact, the effect of stay-at-home orders in high mask-usage counties is essentially negligible. These results are intuitive: stay-at-home policies are less effective when people consistently wear masks and more effective when they do not. Broadly, the extent to which people adopt voluntary measures like mask-wearing affects the marginal decrease of new cases from stay-at-home orders.

Voluntary measures, if commonly followed, have the potential to be as effective as stay-at-home orders in limiting the spread of COVID-19, which has far-reaching policy applications. Voluntary measures like face-masking and social distancing require less sacrifice for local businesses than stay-at-home orders or government-mandated business closures do. This is because businesses can remain open and generate some revenues with voluntary measures but not with stay-at-home orders or mandated closures in place. Policymakers trying to balance the benefits of stay-at-home orders with economic costs may find the benefits of stay-at-home orders outweigh the costs in counties where people are unwilling to wear masks and/or embrace other voluntary measures, but the benefits do not outweigh the costs in places where mask usage is common.

Policy makers can consider incentivizing mask usage and voluntary prevention measures by using the prospect of lifting stay-at-home-orders as a potential reward. They can do so because stay-at-home orders have no additional effect when people properly wear masks and socially distance. Assuming the public reacts to the incentive of lifted stay-at-home orders by increasing voluntary countermeasures, policy makers can create a mutually advantageous scenario where businesses can reopen and people can venture from their homes without an associated spike in cases.

Another somewhat intuitive result with far-reaching policy consequences is that advisories have no additional effect in counties with low mask usage while mandatory orders reduce cases by an additional 30%. This result is fairly easy to explain by way of a question: are people in counties where mask usage is not common likely to listen to government advisories? Advisories have no additional impact in these counties because people simply choose to ignore them. Policy makers, especially those trying to control the spread of COVID-19 in counties where mask usage is uncommon, should note that advisories will likely not be effective.

VII. Conclusions

There is no question that stay-at-home orders, mandatory or otherwise, helped to reduce COVID-19 caseload in late March and April. Results in this paper support many other findings that stay-at-home orders were effective. Across all counties, mandatory stay-at-home orders and advisories reduced new daily cases by 21% and by 33%, respectively.

However, these stay-at home orders are not equally effective across all counties. I find the effect of stay-at-home orders is highly dependent on the level of participation in voluntary measures like consistent mask-wearing. I find mandatory stay-at-home orders are nearly twice as effective in low mask-use counties as they are in average mask-use counties. However, advisories have no additional effect in low mask-use counties, likely because the same people unwilling to follow guidance on masks are also unwilling to respond to a stay-at-home advisory. On the other hand, I find that stay-at-home orders are much less effective in high mask-use counties than in other counties: mandatory orders have no effect, and advisories only reduce new daily cases by 8%.

My results have broad implications for policymakers, especially those in counties where the public has a strong track record of mask usage or does not often wear masks. In counties where people voluntarily wear masks, the marginal benefit of a stay-at-home order is minimal and may not be in the best interest of the county. However, in counties where people do not often wear masks, the marginal benefit of stay-at home orders, particularly lockdowns, is much higher than in other counties.

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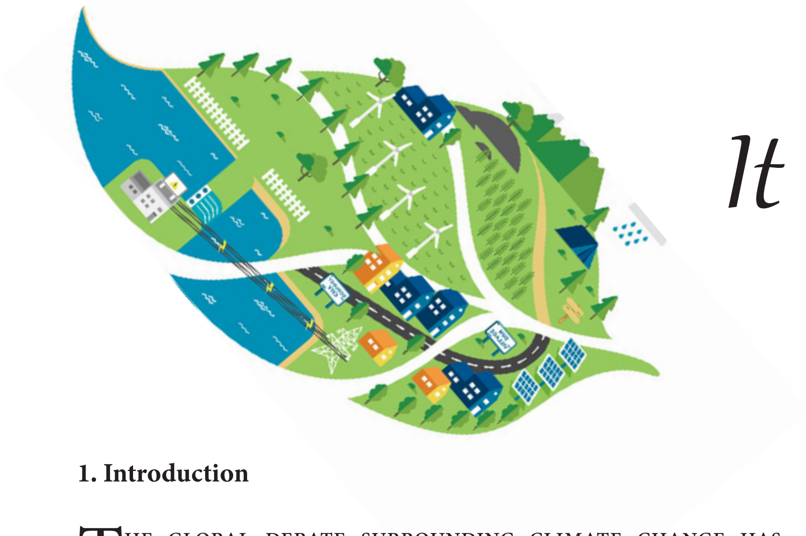

It Pays To Be Green

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1. Introduction

The global debate surrounding climate change has prompted investors, policy-makers, and corporations to consider using buildings as a means to achieve sustainability. The built environment and sustainability are undoubtedly intertwined. For example, it is reported that buildings account for approximately 40% of the consumption of raw materials and energy, while their associated construction activity accounts for at least 30% of world greenhouse gas emissions (Royal Institution of Chartered Surveyors 2005). Thus, the design and operation of real estate can play an important role in energy conservation. Buildings are increasingly being touted as vehicles for achieving energy efficiency, carbon abatement, and corporate social responsibility (Waddock and Graves 1997). This shift in the perception and use of buildings is gradually moving commercial property markets to seek highly coveted "green" certifications. This paper seeks to explore the financial consequences of building "green" by empirically evaluating the impact "green" certifications have on rents commanded by commercial office properties in Manhattan, New York.

Investments in energy efficiency at the time of construction or renovation are fiscally prudent. Some benefits of building "green" include: saving current resources expended on energy, water, waste disposal, and other operating costs; insuring against future energy price increases; and decreasing greenhouse gas emissions. Additionally, improved environmental quality inside of "green" buildings could result in higher employee productivity. There is a popular discussion of the presumed health and productivity costs that are imposed by poor interior quality in commercial buildings, and thus tenants may be willing to pay a higher rent for buildings in which indoor environmental quality is better. Moreover, locating corporate activities in a "green" building may affect the corporate image of tenants since leasing space in a "green" building may send a concrete signal of corporate social responsibility. Favorable reputations may enable firms to charge premium prices (Klein and Leffler 1981), attract a better workforce (Turban and Greening 1997), and attract investors (Milgrom and Roberts 1986). As a result, tenants may be willing to pay higher rents for "green" buildings. Finally, if tenants would prefer sustainable buildings, then sustainable buildings could have longer economic lives than conventional buildings. If the economic benefits of building "green" for commercial property are indeed reflected in tenants' willingness to pay premiums on rent for "green" spaces, this would enable investors to offset the higher initial investment required for sustainable buildings.

Image 1: The 4,969 distinct office buildings identified in CompStak

In the United States, there are two major programs that encourage the development of energy efficient and sustainable buildings through systems of ratings that designate and publicize exemplary buildings. The Energy Star program is jointly sponsored by two federal agencies, the US Environmental Protection Agency and the US Department of Energy. Energy Star began in 1992 as a voluntary labeling program designed to identify and promote energy-efficient products in order to reduce greenhouse gas emissions. The Energy Star rating was extended to commercial buildings in 1995, and the labeling program for these buildings began in 1999. Non-residential buildings can receive an Energy Star certification if the source energy use of the building (that is, the total of all energy used in the building), as certified by a professional engineer, achieves certain specified benchmark levels. The benchmark is chosen so that the rating is awarded to the top quarter of all comparable buildings, ranked in terms of source energy efficiency. The Energy Star rating is marketed as a commitment to conservation and environmental stewardship; it is also touted as a vehicle for reducing building costs and for demonstrating superior management skill.

The US Green Building Council (USGBC), a private nonprofit organization, has developed the LEED ("Leadership in Energy and Environmental Design") "green" building rating system to encourage the "adoption of sustainable green building and development practices." Since its adoption in 1999, separate standards have been applied to new buildings and to existing structures. The requirements for certification of LEED buildings are substantially more complex than those for an Energy Star rating. It is claimed that LEED-rated buildings have lower operating costs, increased asset values, and provide healthier and safer environments for

Dependent Variable	
log(EFF_RENT)	Log of effective rent per square foot
Explanatory Variables	
LEED	=1 if LEED rated at time of transaction
E_STAR	=1 if Energy Star rated at time of transaction
GREEN	= 1 if LEED or Energy Star rated at time of transaction
TRANSACT_SIZE	Square footage of rental area
REN	=1 building was renovated prior to time of transaction
AGE010	=1 if the building is 0 to 10 years old
AGE1120	=1 if the building is 11 to 20 years old
AGE2130	=1 if the building is 21 to 30 years old
AGE3140	=1 if the building is 31 to 40 years old
AGE40	=1 if the building is over 40 years old
AGE_UNKNOWN	=1 if the building age is unknown
LOW_RISE	=1 if the building is less than 21 storeys tall
MID_RISE	=1 if the building is between 21 and 35 storeys tall
HIGH_RISE	=1 if the building age is greater than 35 storeys tall
RISE_UNKNOWN	=1 if the building height is unknown
CLASS_A	=1 if the building is considered to be a Class A office space
CLASS_B	=1 if the building is considered to be a Class B office space
CLASS_C	=1 if the building is considered to be a Class C office space
FREE_RENT	Months of free rent granted by the lease
LEASE_MONTHS	Length of lease in months
YR_QTR	Vector of 38 time dummies reflecting the quarter and year in which the rental contract transacted
LOC	Vector of either 24 or 30 location dummies reflecting the sub-markets or 0.25 square mile GIS clusters, respectively

Table 1: Variable Explanations

occupants. It is also noted that the award of a LEED designation "demonstrates an owner's commitment to environmental stewardship and social responsibility." LEED ratings come in four distinct levels - Certified, Silver, Gold, and Platinum - reflecting varying degrees of energy performance between awarded buildings.

To persuade property owners, developers, and investors in the global marketplace of the benefits of "green" investment, the payoff from investment needs to be identified. Prior published literature devoted to this task generally indicates a positive relationship between environmental certification and financial outcomes in the marketplace. Eichholtz, Kok, and Quigley (2010) document significant and positive effects on market rents and selling prices following environmental certification of office buildings in the United States. Relative to a control sample of conventional office buildings, LEED or Energy Star labeled office buildings achieve rents that are about 2% higher and selling price premiums as high as 16%. Other studies confirm these findings (Fuerst and McAllister 2011a; Miller, Spivey, and Florance 2008). Within the London commercial office market, premiums for certified buildings are approximately 19.7% for rental transactions and 14.7% for sales transactions, relative to non-certified buildings in the same location cluster (Chegut, Eichholtz, and Kok 2013).

This paper seeks to corroborate prior literature and provide analysis on the impact of environmentally sustainable building practices upon economic outcomes as measured in the marketplace. I concentrate on commercial office properties in Manhattan and investigate the relationship LEED and Energy Star ratings have with the effective rents (rents adjusted for building occupancy levels) commanded by these properties during the 2003 to 2016 period, relative to comparable control buildings in a similar location. My findings suggest that there is a rental premium of approximately 4% for buildings with a "green" certification.

2. Data

This paper uses data from 28,432 rental contracts for commercial office buildings in Manhattan between 2003 and 2016. The rental contract data was obtained from CompStak, a crowd-sourced real estate data platform. The data is cross-sectional at the contract level and contains the following information about each building: street address, transaction year, transaction quarter, construction year, renovation year, sub-market, building class (building quality), transaction size, effective rent (equal to contract rent multiplied by the occupancy rate), lease term, and free rent period. There are a total of 4,969 distinct commercial office buildings in the dataset.

LEED and Energy Star-rated buildings were identified by street address on Green Building Information Gateway (GBIG). GBIG is a data platform launched by the U.S. Green Building Council to provide greater transparency of the built environment's "green" dimension. The data is cross-sectional at the rating level and contains the following information about each rating: street address, type of rating (LEED or Energy Star), subcategory of rating, and rating date. I merged the rating data from GBIG with the office

Variable	Obs	Mean	Std. Dev.	Min	Max
EFF_RENT	17,003	68.01	133.41	1	6696
log(EFF_RENT)	17,003	3.96	.56	0	8.81
LEED	17,003	.03	.17	0	1
E_STAR	17,003	.02	.15	0	1
GREEN	17,003	.03	.18	0	1
TRANSACT_SIZE	17,003	17,320.11	48,358.59	7	1,869,752
REN	17,003	.58	.49	0	1
AGE010	17,003	.01	.10	0	1
AGE1120	17,003	.03	.17	0	1
AGE2130	17,003	.03	.18	0	1
AGE3140	17,003	.09	.29	0	1
AGE40	17,003	.83	.38	0	1
AGE_UNKNOWN	17,003	.01	.09	0	1
LOW_RISE	17,003	.33	.47	0	1
MID_RISE	17,003	.31	.46	0	1
HIGH_RISE	17,003	.32	.47	0	1
RISE_UNKNOWN	17,003	.05	.22	0	1
CLASS_A	17,003	.53	.50	0	1
CLASS_B	17,003	.39	.49	0	1
CLASS_C	17,003	.07	.26	0	1
FREE_RENT	17,003	3.43	3.75	0	50
LEASE_MONTHS	17,003	90.51	56.11	12	2256

Table 2: Summary Statistics

building rental contracts identified in CompStak based on street address and rating date. Thus, a rating was "added" to a transaction if the rating was awarded prior to the quarter in which the rental contract transacted. I assume that a building has no rating prior to its earliest rating date. A total of 242 LEED or Energy Star ratings have been distributed to 49 distinct commercial office buildings in New York City. However, only 30 of these rated buildings had rental contracts transacted between 2003 and 2016.

Image 1 depicts all 4,969 distinct commercial office buildings identified in the CompStak database projected onto a map of Manhattan. The 30 distinct buildings which received a LEED or Energy Star rating prior to at least one rental transaction appear as green circles while the remaining, unrated buildings appear as yellow circles.

Based upon the latitude and longitude of each rated building, I used geographic information system (GIS) techniques to identify all other office buildings in the CompStak database within a radius of one quarter mile. In this way, I created 30 clusters of nearby office buildings. Image 2 depicts the same buildings as Image 1, but additionally shows the radius of one quarter mile surrounding each rated building. Although the original CompStak dataset included information about sub-markets, the definition of sub-market varies from the very general "Upper East Side" to the very specific "World Trade Center." These sub-markets are widely accepted Manhattan neighborhood delineations. A total of 24 distinct sub-markets exist in this dataset and include: Madison/Fifth Avenue, Grand Central, Columbus Circle, Gramercy Park/Union Square, Sixth Avenue, World Trade Center, NoHo/Greenwich Village, Financial District, Hudson Square, Midtown Eastside, Murray Hill, Penn Station, Park Avenue, City Hall Insurance, Times Square South, Times Square, SoHo, Hudson Yards, UN Plaza, Upper Eastside, Chelsea, Upper Westside, North Manhattan, and Tribeca. Location is arguably the single most important factor influencing rental price. Therefore, I did not feel it was adequate to only control for sub-market given the variation in location quality that exists in larger sub-markets (a building on 59th Street may rent for a very different price point than a building on 96th Street, yet they would both be within the "Upper East Side" sub-market). As a result, some of my regressions control for location using these 24 sub-markets while others control for location using the 30 one quarter mile GIS clusters.

Variable explanations are presented in Table 1, and summary statistics for relevant variables are presented in Table 2. Further augmentation to the data set included the creation of dummy variables reflecting the age of the building, renovation status, building height, and building class. Additionally, 2,225 observations were missing data for the dependent variable, EFF_RENT, and were dropped from the data set. Moreover, 2,894 observations were dropped due to missing information regarding the building's class. Finally, since the first "green" rating in my dataset occurred in quarter three of 2007, the 6,310 observations that transacted prior to this date were dropped from the data set.

The shortcomings of this data primarily resided in crucial data, such as effective rent and building class, missing from observations. Less crucial missing data, such as building age and height, were dealt with by the introduction of AGE_UNKNOWN and RISE_UNKNOWN variables. However, there was no appropriate way to estimate effective rent or building class. As a result, observations missing either of these dimensions had to be dropped from the dataset. Additionally, the dataset did not include information regarding building amenities, a characteristic that other papers found to be significant in explaining variation in rental prices. Furthermore, it is possible that not all rental transactions or ratings were reported. If true, and if this omission is not randomly distributed throughout the dataset, the analysis could be unknowingly skewed. As a general comment, commercial real estate data is incredibly fragmented due to the industry's minimal adoption of technology, asymmetric sharing of information, and extreme lengths of time between transactions (a commercial office building may only transact once every 20 years).

Dependent Variable: log(EFF_RENT)	(1)	(2)	(3)	(4)	(5)	(6)
GREEN	0.039**		0.016	0.0480**		0.032*
	(0.018)		(0.018)	(0.019)		(0.019)
LEED		-0.018			-0.001	
		(0.026)			(0.027)	
E_STAR		0.056**			0.056*	
		(0.029)			(0.030)	
TRANSACT_SIZE	-0.000***	-0.000***	-0.000***	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
REN	0.032***	0.032***	0.023***	0.042***	0.041***	0.034***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
AGE010	0.414***	0.413***	0.368***	0.424***	0.423***	0.372***
	(0.028)	(0.028)	(0.028)	(0.029)	(0.029)	(0.030)
AGE1120	0.221***	0.221***	0.190***	0.195***	0.195***	0.167***
	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.023)
AGE2130	0.153***	0.154***	0.112***	0.154***	0.155***	0.116***
	(0.023)	(0.023)	(0.023)	(0.024)	(0.025)	(0.025)
AGE3140	0.063***	0.062***	0.038***	0.026*	0.025*	-0.005
	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)
AGE_UNKNOWN	-0.073	-0.074	-0.078	-0.096	-0.096	-0.116*
	(0.060)	(0.060)	(0.060)	(0.061)	(0.061)	(0.062)
MID_RISE	0.086***	0.086***	0.037***	0.063***	0.064***	0.008
	(0.011)	(0.011)	(0.012)	(.010)	(.010)	(.012)
HIGH_RISE	0.179***	0.180***	0.109***	0.163***	0.164***	0.083***
	(0.012)	(0.012)	(0.013)	(0.012)	(0.012)	(0.013)
RISE_UNKNOWN	0.171***	0.170***	0.180***	0.194***	0.194***	0.196***
	(0.030)	(0.029)	(0.029)	(0.032)	(0.032)	(0.032)
FREE_RENT	-0.030***	-0.030***	-0.031***	-0.031***	-0.031***	-0.032***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
LEASE_MONTHS	0.003***	0.003***	0.003***	0.003***	0.003***	0.003 ***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
CLASS_A			0.156***			0.108***
			(0.023)			(0.024)
CLASS_B			0.023			-0.041**
			(0.020)			(0.021)
constant	3.672***	3.468***	3.458***	3.732***	3.732***	3.750***
	(0.115)	(0.115)	(0.042)	(0.042)	(0.042)	(0.045)
Sub-Market Location Dummies	Y	Y	Y	N	N	N
GIS Cluster Location Dummies	N	N	N	Y	Y	Y
R-squared	0.328	0.328	0.287	0.287	0.287	0.294
Number of Observations	17,003	17,003	17,003	17,003	17,003	17,003

Table 3: Robust Regression Results

Robust standard errors in parentheses below respective coefficient. *, ** and *** represent statistical significance at the 90%, 95%, and 99% confidence level. Storeys medium and high are relative to low-storey buildings, age factors are relative to buildings older than 40 years in age, and building classes are relative to Class C buildings.

3. Methods and Results

To investigate how "green" certification influences the effective rent of commercial office buildings, the sample of "green"-rated office buildings and the control sample consisting of nearby, non-rated office buildings are used to estimate a semi-log equation relating office rents per square foot to the hedonic characteristics of the buildings, location, and transacting date of each building. As such, the regression equation we wish to estimate is:

$$log(EFF\_RENT) = \alpha + \delta * G + \beta * X + \gamma * LOC + \eta * YR\_QTR + \epsilon$$

This regression equation was estimated using robust linear regression. I chose to run a robust regression because of the likely presence of outliers and heteroskedasticity in the dataset. Least squares estimates for regression models are highly sensitive to outliers. An outlier that results from non-normal measurement error or some other violation of standard ordinary least squares assumptions compromises the validity of the regression results if a non-robust regression technique is used. Given the nature of measurement error in commercial real estate data, I opted to move forth assuming the presence of outliers.


Image 2: Quarter mile clusters surrounding each of the 30 rated buildings

Regression results are presented in Table 3. G is the set of "green" characteristics for a given transaction. In regressions (1), (3), (4), and (6), G contains only the variable GREEN. In regressions (2) and (5), G contains the variables *LEED* and *E_STAR*. *X* is the set of hedonic characteristics of the transacting building. In regressions (1), (2), (4), and (5), the hedonic characteristics in X include: *TRANSACT_SIZE, REN, AGE010, AGE1120, AGE2130, AGE3140, AGE_UNKNOWN, MID_RISE, HIGH_RISE, RISE_UNKNOWN, FREE_RENT,* and *LEASE_MONTHS.* In regressions (3) and (6), *X* is modified to also include *CLASS_A* and *CLASS_B*. To control for location effects, I include a set of location dummy variables. In regressions (1), (2), and (3), LOC contains a dummy for each of the 24 Manhattan sub-markets. These 24 dummy variables are exhaustive and mutually exclusive. In regressions (4), (5), and (6), *LOC* contains a dummy for each of the 30 one quarter mile GIS clusters. These GIS dummies are not exhaustive nor mutually exclusive — many buildings appear in more than one cluster while some buildings do not appear in any. Finally, I include a set of time dummies, *YR_QTR*, to control for the year and quarter in which the rental transaction occurred. α, δ, β, γ, and η are estimated coefficients, and ε is an error term. The coefficient on G, δ, can be interpreted as the effective rent percentage premium paid for buildings with a "green" certification. Column (1) shows that a commercial office building with a "green" rating (that is, *LEED* or Energy Star) rents for a 3.9% premium on average. This coefficient is statistically significant at the 95% level. The sign and strength of this finding suggests that, relative to similar office buildings in a given sub-market, tenants are willing to pay more for "green" buildings. The direction, magnitude, and significance of this coefficient corroborates prior literature.

In column (2), G is modified to include both *LEED* and *E_STAR* so as to estimate the effect these awards individually have on the effective rent commanded by office buildings. The estimated coefficient for the *LEED* rating indicates a discount of 1.8% for commercial effective rents, but this coefficient is statistically insignificant. The Energy Star rating is associated with premium rents of 5.6% and is statistically significant at the 95% level. Interestingly, these findings suggest that tenants may value an Energy Star rating over a LEED rating. This contradicts prior literature and my own hypothesis given that requirements for LEED certification are more stringent than that of Energy Star. One study notes "that the attributes of sustainability rated in the LEED certification process have a substantial effect on the effective rents commanded by office buildings" (Eichholtz, Kok, and Quigley 2013). A possible explanation for my results could be that LEED-rated buildings in my sample consisted primarily of sustainability attributes not valued by the buildings' respective tenants. Column (3) shows that once building class is factored into the hedonic regression, "green" certification is no longer statistically significant. Building class, while an important industry categorization, is a subjective determinant of building quality. Classification standards "vary by market" and are often referred to as "an art rather than a science" (Golden 2013). A potential explanation of the sudden insignificance of "green" certifications could be attributed to the fact that the high-quality infrastructure of Class A buildings overshadows the added benefits of "green" certification.

In column (4), we see statistically significant rental premiums of 4.8% for certified buildings. This regression, while similar to column (1)'s regression, now controls for location by using GIS clusters of one quarter mile as opposed to sub-market delineations. As a result, the buildings are being compared to more "like" buildings with regard to location, and the resulting importance of obtaining a "green" certification increases when measured by rental rate premiums. Column (5) again distinguishes between the LEED and Energy Star ratings. Similar to column (2), buildings with an Energy Star rating achieved a rental premium of 5.6% on average. This coefficient is significant at the 90% level. Interestingly, the estimated coefficient for the LEED rating indicates a statistically insignificant rental discount of only 0.1%. Finally, column (6) shows a positive, statistically significant coefficient for *G* when building class is included in the hedonic regression and location is controlled for using GIS clustering. The coefficient indicates that premiums of 3.2% are paid for "green" certified buildings, even when infrastructure quality is controlled for via building class.

4. Conclusion

Growing global concern about climate change is increasingly affecting the preferences of consumers and investors. In addition, international, national, and local governmental institutions are expanding the scope of environmental regulation, affecting commercial real estate assets. Similar to other product markets, a voluntary environmental certification system for new buildings and renovations has emerged in most real estate markets. Despite the publicity and promotion, the voluntarily certified sector is minuscule in terms of the current total commercial real estate stock (as evident by there being only 49 distinct buildings with a LEED or Energy Star rating in all of Manhattan). However, it is likely that "green" certification of commercial buildings will become progressively more important.

The idea that "green" certified buildings should obtain a rental price premium is *a priori*. It is expected that investors' holding costs should be lower due to attractiveness to tenants associated with business performance, image, fiscal incentives, corporate social responsibility, and lower operating costs. This can lead to a rental premium and/or lower vacancy rates. The results of the empirical analysis confirm these expectations. The hedonic regressions suggest that there is a rental premium of approximately 4% for "green" certification.

It is important to note that there are a number of caveats attached to the interpretation of this and similar empirical studies of price differentials in commercial real estate. First, the controls for inherent heterogeneity between certified and non-certified buildings are bound to be imperfect even when applying a comprehensive set of variables in the hedonic model. For example, it is possible that the "green" certification process is only one element of additional investment to create a market-leading building. To control for all unique facets of a building is virtually impossible in the framework of a hedonic model. Second, it could be the case that intangible preferences play a role in determining the value of real estate. Why is it that comparable buildings across the street from one another rent for different prices? Is it because of the building's architecture, the view, the way that sunlight filters through the windows, or some other obscure feature that is not easily quantifiable? Finally, these empirical studies provide a cross-sectional snapshot of price differentials for a specific sample in a specific time period. It is expected that price differentials for certified buildings should vary over time and between buildings. Although the results of this paper are in line with the findings of the majority of studies on rental premiums of certified buildings, this is a study of a niche market with relatively small sample sizes. As the quantity, detail, and accuracy of data is likely to improve over time, future research will be able to address a number of more specific issues such as the individual contributions of tenant image benefits, higher productivity, or lower operating costs to the "green" premium.

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IMPACTS OF HUMAN MOBILITY

Quantifying the Externalities of Traffic Congestion

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Abstract

The COVID-19 pandemic is one of the greatest public health crises of the last century, forcing governments to impose strict lockdowns to curb the spread of the virus. Significant decreases in economic activity have enabled further study on the environmental impacts of human mobility, including more detailed analyses of anthropogenic sources of air pollution. This study compares traffic congestion, air pollution, and key events during the pandemic with data from 2017–2020 to determine the association between human mobility and NO_x, CO, and PM_{10} concentrations in eight US cities. Although there is considerable variation across cities, NO_x and CO have positive relationships with human mobility, with NO_x as a strong example of a traffic-based air pollution externality. Further, government lockdowns resulted in a significant decrease in NO_x concentrations.

I. Introduction

A. Background on COVID

SARS-CoV-2, a type of coronavirus that causes the COVID-19 disease, was first identified in December 2019 in China. Common symptoms include fever, cough, fatigue, and shortness of breath, and the virus most often spreads through close contact between individuals (Centers for Disease Control and Prevention 2021). As of March 2021, the virus is responsible for nearly three million deaths and 130 million confirmed cases worldwide (Johns Hopkins 2021). The pandemic is one of the greatest global health crises in human history, forcing governments to take unprecedented measures to prevent its spread, with significant impacts on the global economy.

To curtail the spread of the virus within and between borders, countries imposed heavy mobility restrictions: banning foreign arrivals, closing "non-essential" businesses, limiting gatherings, and enacting "stay-at-home" orders. The combined effects of these policies contributed to the largest global recession since the Great Depression, with the most severely affected industries including tourism, passenger transport, and entertainment (Suneson 2020). The global economic downturn presents new opportunities for research on the interaction between human activity and the environment. Papers studying this pandemic can provide important insights into environmental policy, economics, human behavior, and public health, among others.

B. Pollution and Traffic

The COVID-19 pandemic has presented a unique opportunity to more closely study the effects of human activity on the environment. One of the earliest environmental effects of the pandemic was observed in mainland China, where significant reductions in air pollution were recorded after the enforcement of COVID-19 lockdowns (Wang and Su 2020). Satellite data confirmed that nitrogen dioxide (NO_2) levels in China were already dropping by mid-January of 2020, and as the virus spread internationally, the same trends appeared in other countries. In almost all country-level studies, COVID-related lockdowns are associated with significant decreases in air pollution (Dang and Trinh 2020). The decrease in mobility as a result of government lockdowns allows for an exploration of the pathways by which air pollution decreased during the pandemic, with further insights into the specific ways humans contribute to air pollution.

A natural consequence of government-mandated mobility restrictions is a decrease in both public transportation use and traffic congestion. The closure of "non-essential" businesses, curfews, and social gathering restrictions reduced incentives for people to leave their homes, which reduced traffic congestion. Taiwan, on the other hand, did not enact any COVID-related mobility restrictions. Air pollutants associated with automobile transportation actually increased 5-12% during the pandemic, which suggests that strong government restrictions reduce traffic more than individuals' concern for the virus if allowed to conduct business as usual (Chang, Meyerhoefer, and Yang 2020).

C. Literature Review

Air pollution is a textbook example of a negative externality since consumers tend to overlook the social costs of their energy consumption such as the resulting air pollution. For instance, Clay and Muller (2019) estimate that economic losses in the United States due to premature deaths from air pollution amount to at least $89 billion, while outdoor air pollution causes approximately 3.4 million deaths annually (Chang, Meyerhoefer, and Yang 2020). This study focuses on three pollutants often associated with vehicle emissions: nitrogen oxides, carbon monoxide, and large particulate matter.

Nitrogen oxides (NO_x) are a group of compounds that contribute to smog and the production of ground-level ozone, which is harmful to the respiratory system. NO_x emissions also have serious environmental effects such as eutrophication, which can cause dangerous algal blooms in bodies of water that severely threaten marine animals. Approximately 50% of NO_x emissions can be attributed to transportation with smaller proportions from electricity production and manufacturing (EPA 1999).

Carbon monoxide (CO) is known for its acute effects in the form of carbon monoxide poisoning, but it also has harmful health effects during prolonged exposure to lower concentrations. CO inhalation displaces oxygen in the blood, which can cause permanent damage to the heart and brain. One of the most widespread sources of carbon monoxide is the internal combustion engine, which is present in all gas-powered automobiles (Occupational Safety and Health Administration 2012).

Greater concentrations of particulates ($PM_{2.5}$/PM_{10}) increase susceptibility to respiratory infections such as influenza and COVID-19 (Graff Zivin et al. 2020, Wu et al. 2020). Ebenstein et al. (2017) estimate that a 10 microgram-per-meter cubed (μg/m^3) increase in local PM_{10} concentration reduces life expectancy by .64 years. Previous studies on the effects of traffic congestion and PM_{10} suggest that automobile emissions contribute somewhat to particulate matter concentrations, but the wide array of PM_{10} sources implies a weaker relationship relative to traffic's effects on NO_x or CO (Cheng and Li 2010).

Traffic congestion increases air pollution through multiple processes. High congestion increases per-vehicle emissions exposure over time, reduces dispersion of pollutants, and is characterized by frequent start-stop driving patterns that increase emissions (Zhang and Batterman 2013). Traffic congestion is responsible for up to 50% of $PM_{2.5}$ pollution in large cities. A primary driver of increased air pollution in expanding cities is the addition of more automobiles and therefore more congestion (Lu et al. 2020). Previous attempts to reduce traffic have included various taxation structures, such as gasoline taxes and carbon taxes, with mixed results (Knittel and Sandler 2013, Williams 2016). Strict government traffic controls in a non-pandemic setting contribute to significant reductions in air pollution. However, if policies are not stringent enough, congestion remains the same but is distributed more evenly throughout the day, leading to trivial reductions in overall air pollution (Chen et al. 2011). Meanwhile, congestion pricing on heavy-use roads during peak commuting hours have had an immediate impact on traffic volume in Singapore and European countries that have enacted similar policies (Chin 1996). COVID-19 lockdowns have also reduced traffic congestion, particularly in urban areas (Parr et al. 2020). Finding effective policy solutions to reduce traffic-related air pollution is a necessary goal in light of increasing global urbanization.

Studies of the reduction in air pollutants following COVID-19 lockdowns point to multiple pathways by which human activity contributes to air pollution. Aside from reductions in traffic congestion, industrial activity was also severely impacted by lockdowns, reducing demand for electricity (Wang and Su 2020). Studies in Europe confirm the same phenomenon, where the effects of large-scale lockdowns further support the intuitive and well-studied relationship between traffic and air pollution (Baldasano 2020). The aim of this study is not only to corroborate the negative effects of traffic congestion on air quality, but also to attempt to quantify the relationship between a percent increase in city-wide travel times and air pollution concentrations.

D. Overview

This paper explores the relationship between human mobility and air pollution with an emphasis on the effects of traffic congestion. It consists of two primary analyses: the effects of reduced traffic congestion on air pollution at the start of the pandemic and the effects of stay-at-home orders on air pollution.

This study focuses on eight geographically and demographically diverse cities in the United States: Atlanta, Boston, Cincinnati, Los Angeles, Miami, Pittsburgh, Seattle, and Washington, D.C. These cities have different traffic regimes, weather patterns, average air pollution, and responses to the pandemic, allowing for a generalized analysis on the effects of human mobility on air pollution. The data concern the years 2017–2020, with the onset of the pandemic in early 2020 as a source of exogenous variation.

Daily pollutant data was collected from public Environmental Protection Agency (EPA) data for each city. To match the timescale of publicly provided Uber Movement data, the pollutant data were aggregated into monthly averages. Precipitation and average daily temperature data were also included to account for some of the variation in pollutant concentrations. Air pollution is typically the most severe during colder months, whereas precipitation tends to "wash out" pollutants from the air and decrease airborne concentrations. The addition of this data helps explain the seasonal variation in pollution, enabling a more direct analysis of traffic congestion's effects on pollution.

There are empirical challenges to measuring aggregate traffic congestion across an entire city. Traffic congestion data are typically collected through three main methods: point sensors, GPS/floating car data, and toll data (Bickel et al. 2007). Direct sensor methods are naturally very accurate at measuring traffic at specific locations. GPS origin-destination data are the most suitable for measuring and predicting aggregate city-wide traffic flows, but this method requires many voluntary participants to collect reliable data.

This study uses public Uber Movement data, an anonymized aggregate dataset of mean Uber travel times between "zone pairs" in each city. Uber data have been supported as a low-cost alternative for accurately estimating automobile travel times in American cities (Y. Sun, Ren, and X. Sun 2020). Its data are strongly correlated with the aforementioned traditional traffic measurement methods, with an added advantage of measuring aggregate traffic in a similar way to GPS-based floating car data (Vieira and Haddad 2020). The sheer volume of Uber rides addresses one of the main caveats of traditional floating car data, which is the need for a large number of voluntarily tracked vehicles to produce a reliable, large-scale measure of travel times.

All-day (including weekdays and weekends) travel times decreased 6.0% between January 1st–March 31st 2017–2019 and the same interval in 2020, while weekday travel times decreased 6.4%. This is consistent with other studies on the pandemic's effect on human mobility and supports the claim that Uber Movement data can be used as a low-cost estimate of traffic congestion. Across all eight cities, NO_x, CO, and PM_{10} concentrations dropped 7.6%, 5%, and 4.2%, respectively, between the same two periods. However, there is significant variation between individual cities' changes in pollution concentrations during this timeframe. Five of the eight cities had insignificant changes in traffic congestion after COVID's arrival. Potential explanations for these variations are discussed, although the topic requires further research.

The second part of this analysis looks at the effects of government lockdowns on air pollution in the United States. All eight cities and/or their respective states enacted stay-at-home orders by early April and did so within two weeks of one another. These orders closed "non-essential" business and restricted travel. Controlling for weather effects, NO_x and CO concentrations dropped in all eight cities after their respective lockdowns. On the other hand, PM_{10} concentrations increased in all but one city. Both analyses suggest that NO_x concentrations are more responsive to short-term changes in human mobility than CO and that

PM_{10} is largely unaffected by traffic congestion.

The trends in air pollution following the "arrival" of COVID-19 to a city's state are also noteworthy. In contrast to the nearly simultaneous imposition of lockdowns, the dates of the first confirmed COVID-19 case in each of the eight cities' respective states varied quite extensively. The earliest first confirmed case in the sample was in Washington (Seattle) on January 21st, 2020. The latest first confirmed case was in Ohio (Cincinnati) on March 9th, 2020. The trends in air pollution *after* the first confirmed case but *before* government-imposed lockdowns may shed light on human behavior as the data mirrored the observations in Taiwan. Without restrictions, fears of COVID-19 alone did not seriously change economic activity enough to produce significant changes in air pollution.

II. Data

A. Pollution

Pollution data were gathered from the EPA's daily outdoor air quality database. Air pollution data were taken from one monitoring station per city if possible. Some cities had different stations record data for different pollutants, but all monitoring stations were within the boundaries of each city. Pollutant concentrations were recorded on a daily basis. NO_x concentration is measured as the maximum daily one-hour concentration in ppb. CO is measured as the maximum daily eight-hour concentration in parts-per-million (ppm). PM_{10} concentration is measured as the daily mean concentration in $\mu g/m^3$. For the Uber Movement analysis, pollution concentrations were aggregated into monthly means to match the timespan of the Uber data. For the lockdown analysis, these concentrations were aggregated into weekly means instead.

B. Traffic Congestion

Uber Movement data were used to generate an aggregate estimate of relative city-wide traffic congestion on a monthly basis. It is a public dataset of mean travel times between every "zone pair" within each city, starting in 2016. The raw datasets were initially organized by quarter, with three months per set. The average mean travel time of all the cities' zone pairs can be used to compare relative traffic congestion, and the traffic analysis uses the first quarter of the years 2017 through 2020, with February/March 2020 representing the start of the pandemic.

Some cities had over 2000 "zones." Naturally, some zone pairs did not have enough Uber trips between them to produce average travel time data. For instance, two suburban zones on geographically opposite sides of a city probably have very little, if any, Uber volume. Although not every possible zone pair had travel time data, the largest city-quarter datasets had over one million zone pairs, suggesting that a change over *all* hundreds of thousands of zone pairs represents a significant change in the aggregate traffic level of a city. Zone pairs that only had one mean in a three-month block (e.g. there was data for February but not January or March) were eliminated to strengthen the comparability of overall traffic congestion between months.

Uber publishes both all-day and weekday-only data. Both were used in estimating traffic, although weekday-only data are a more accurate estimate since recurring congestion patterns are more closely associated with weekday commuting. The datasets record monthly averages, which helps ensure a sufficient sample size of trips for analysis, but this also requires the other variables to be aggregated into monthly means.

C. Weather Data

Daily precipitation and average temperature were collected from the National Oceanic and Atmospheric Administration's (NOAA) online climate database. Data came from one station for each city, with airports giving the most consistent and frequent measurements. As with pollution data, weather was aggregated into monthly means for the traffic analysis and weekly means for the lockdown analysis.

D. Important Data

The dates for the first "local" confirmed case and beginning of the lockdown were recorded for the lockdown analysis. "First confirmed case" is defined as the date of the first confirmed COVID-19 case in each cities' respective state, so it may not have occurred within the city itself. It is a general indicator of the "relevance" of the virus's spread to a city's population. These dates were taken from local news sources for each city, and the first commonly agreed-upon date is used. The first confirmed case date for each state is quite consistent across local media outlets.

III. Analysis

A. Uber Movement/Traffic

Since the causal impact of traffic congestion on pollution is both intuitive and well-documented in previous literature, the following linear-log specification is used to estimate the relationship between traffic time and each of the three pollutants. I use a linear-log model because the direct impacts of additional traffic congestion on pollution diminish with greater traffic volumes:

$$(1)\ Y_{ijt} = \alpha + \beta_1(\text{ln\_traffic}_{ijt}) + \beta_2(\text{ln\_traffic}_{ijt} * \text{covid}_{ijt}) + \delta X_{ijt} + \mu_i + \pi_t + \varepsilon_{ijt}$$

Y_{ijt} is the dependent variable of air pollution concentration. i, j, and t represent city, month, and year, respectively. α is the constant, β_1 and β_2 are the parameters of interest, and δ measures the effects of time-variant weather variables associated with the dependent variable. β_1 estimates the relationship between traffic and air pollution, while β_2 addresses the interaction term of traffic and *covid* after the start of the COVID-19 pandemic. *covid* is a dummy variable that is 1 if it is February or March of 2020 and 0 otherwise. μ_i and π_t denote city and year fixed effects, and ε_{ijt} is the error term. City fixed effects are included to account for the inherent pollution differences between cities due to factors such as population, urban density, and overall climate patterns that are relatively constant over the four years of data. Year fixed effects absorb the impacts of unobserved time-based trends in air pollution. Traffic is measured as the mean Uber Movement travel time across all zone pairs.

The within-city relationship between travel times and covid is estimated by:

$$(2)\ Y_{ijt} = \alpha_i + \beta_1(\text{covid}_{ijt}) + \delta X_{ijt} + \varepsilon_{ijt}$$

where α_i is the within-city constant, β_1 is the parameter of interest, δ measures the effects of time-variant weather variables on travel times, and ε_{ijt} is the error term.

B. COVID-19 and Lockdowns

The within-city relationship between local stay-at-home orders and air pollution is estimated by the following log-linear specification with the dependent variable transformed into a natural logarithm. A log-linear model allows for a generalized estimate of lockdown effects across cities given the variation in pre-existing pollutant concentrations between cities:

$$(3)\ \mathit{ln}(Y_{ijt}) = \alpha_i + \beta_i(\text{lockdown}_{ijt}) + \delta_i X_{ijt} + \pi_t + \varepsilon_{ijt}$$

lockdown is a dummy variable that indicates the presence of a stay-at-home order in a city; 1 when a stay-at-home order is in effect, and 0 otherwise. Since the first case was in Washington state, *lockdown* and *covid* begin in the same week for the city of Seattle. As in (1), Y_{ijt} is air pollution concentration. Since this analysis does not use the Uber Movement data, j represents weeks instead of months, while i and t represent city and year, respectively. αi is the within-city constant, and β_i is the within-city relationship. δ measures the effects of time-variant weather variables associated with the dependent variable. π_t denotes year fixed effects, and ε_{ijt} is the error term. Fixed effects are included for the same reasons as (1).

Estimating the effects of covid, firstcase, and lockdown on air pollution is estimated by a log-linear specification, which is also used to more closely estimate percentage changes in pollution following key COVID-19 events:

$$(4)\ \mathit{ln}(Y_{ijt}) = \alpha + \beta_1(\mathit{covid}_{ijt}) + \beta_2(\mathit{firstcase}_{ijt}) + \beta_3(\mathit{lockdown}_{ijt}) + \delta X_{ijt} + \mu_i + \pi_t + \varepsilon_{ijt}$$

where Y_{ijt}, δ, π_t, and ε_{ijt} represent the same parameters as (3). α is the across-cities constant. The earliest confirmed case of COVID in the United States was on January 19th, 2020 (Holshue et al. 2020). *covid* is a dummy variable that indicates whether or not COVID-19 was confirmed to be present in the United States; 1 after week two of 2020, and 0 otherwise. *firstcase* is a dummy variable that is 1 from the week of the first confirmed case in a city's state onwards, and 0 otherwise. μ_i represents city fixed effects.

IV. Results

A. Traffic Analysis

Table 1.1: Summary Statistics

	(1) Full sample	(2) Pre-COVID	(3) COVID	(4) Difference (3) – (2)
NO_x (ppb)	27.14	27.58	24.94	-2.64
	(6.485)	(6.463)	(6.339)	[1.742]
CO (ppm)	0.42	0.43	0.37	-0.06
	(0.171)	(0.174)	(0.154)	[0.095]
PM_{10} (μg/m^3)	14.37	14.56	13.45	-1.11
	(4.847)	(4.786)	(5.190)	[1.404]
Traffic (WD)	1324.08	1338.30	1252.94	-85.36
	(239.1)	(243.0)	(211.5)	[59.44]
Traffic (All)	1286.12	1299.05	1221.48	-77.57
	(221.3)	(224.9)	(196.2)	[55.12]
N	96	80	16	

Mean coefficients; standard deviations in parentheses. Standard error of difference-in-means in brackets. Uses monthly averages to coincide with Uber data. Comparison across Q1 2017–2020.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.2: Traffic (weekday) Regressions by City

VARIABLES	(1) Atlanta	(2) Boston	(3) Cincinnati	(4) LA	(5) Miami	(6) Pittsburgh	(7) Seattle	(8) DC
COVID	-61.60	-39.69	**45.61***	**-161.76***	-124.78	30.41	-109.81	**-135.44****
	(100.79)	(67.92)	**(21.04)**	**(80.63)**	(97.28)	(26.53)	(98.63)	**(43.83)**
Precipitation	-92.50	-384.22	315.67	-782.63*	128.31	280.05	41.14	994.27**
	(376.36)	(585.51)	(193.32)	(359.81)	(605.66)	(241.79)	(104.63)	(328.13)
Temperature	-1.02	-6.52	-4.21*	-21.90***	1.42	-4.39*	-1.12	-2.53
	(4.35)	(6.19)	(2.07)	(6.51)	(8.20)	(2.00)	(5.47)	(3.15)
Constant	1,609.15***	1,598.89***	1,136.47***	2,894.43***	1,564.85**	1,127.43***	1,213.99***	1,419.36***
	(242.72)	(241.26)	(60.87)	(402.17)	(573.14)	(62.42)	(249.44)	(138.84)
Observations	12	12	12	12	12	12	12	12
R-squared	0.13	0.21	0.34	0.56	0.38	0.30	0.37	0.56

Robust standard errors in parentheses. Significant β on COVID is bolded.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 1.3: Regressions

VARIABLES	(1) NO_x (ppb)	(2) CO (ppm)	(3) PM_{10}
Traffic (ln)	11.05	0.13	-5.59
	(8.53)	(0.18)	(6.91)
Traffic (ln) x COVID	-0.04	0.00	-0.15
	(0.25)	(0.01)	(0.21)
Precipitation	-16.52***	-0.17	-25.89***
	(5.63)	(0.12)	(4.78)
Temperature	0.14	0.01**	0.15
	(0.12)	(0.00)	(0.09)
Constant	-56.56	-0.77	50.49
	(62.56)	(1.30)	(50.70)
Observations	96	96	93
R-squared	0.78	0.86	0.74

Standard errors in parentheses. Traffic variables are ln. Uses city fixed effects.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Uber travel times, as well as pollutant concentrations, were lower in the COVID-19 period (February/March 2020) than before. NO_x, CO, and PM_{10} concentrations decreased 9.6%, 14.0%, and 7.6% between the two periods, respectively. Weekday travel times dropped 6.4%, while all-day travel times dropped 6.0%. Weekday travel times were higher than all-day travel times for both time periods, which supports the notion that recurring traffic congestion is determined primarily by commuting volume. Over the whole eight city sample, the decreases are not statistically significant, but the high standard deviations are partly due to inherent variation between cities. These city fixed effects are accounted for in Table 1.3: Regressions.

Table 1.2 summarizes the change in weekday travel times associated with the arrival of COVID-19 into the United States by city. Significant coefficients on the covid dummy variable are in bold. Of the eight cities, only Los Angeles and Washington, D.C.'s weekday travel times can be significantly, and negatively, associated with the presence of the pandemic in the United States at a .1 significance level. Interestingly, travel times in Cincinnati have a positive, significant association with the *covid* variable. The other five cities have insignificant associations with *covid*.

Table 1.3 contains the regressions of interest between traffic and pollutants. As expected from the literature, there is a positive coefficient of travel times with both NO_x and CO, and there is a negative coefficient for PM_{10}. A 1% increase in travel time is associated with a .1105 ppb increase in NO_x, a .0013 ppm increase in CO, and a .0559 μg/m^3 decrease in PM_{10}, controlling for weather and city/

year fixed effects. None of these coefficients are significant at a .05 significance level. Likewise, the coefficient on the interaction term between travel times and covid is small and insignificant.

Table 2.1: Summary Statistics

	(1) Full sample	(2) Pre-COVID	(3) COVID	(4) FIRSTCASE	(5) LOCKDOWN
NO_x (ppb)	23.12	23.83	20.65	20.34	19.15
	(8.298)	(8.387)	(7.479)	(7.514)	(6.761)
CO (ppm)	0.38	0.39	0.34	0.34	0.33
	(0.181)	(0.185)	(0.155)	(0.158)	(0.144)
PM_{10} ($\mu g/m^3$)	16.18	16.10	16.43	16.71	17.05
	(8.019)	(8.080)	(7.805)	(8.057)	(8.010)
N	1631	1264	367	331	292

Standard deviations in parentheses.

Table 2.2: Pre-COVID and LOCKDOWN Difference-in-Means

	(1) Pre-COVID	(2) LOCKDOWN	(3) Difference (2)-(1)
NO_x (ppb)	23.83	19.15	-4.68
	(8.387)	(6.761)	[.046]
CO (ppm)	0.39	0.33	-0.06
	(0.185)	(0.144)	[0.01]
PM_{10} ($\mu g/m^3$)	16.10	17.05	0.95
	(8.080)	(8.010)	[.052]
N	1264	292	

Standard deviations in parentheses. Standard error of difference-in-means in brackets.

Table 3.1: NO_x by City

VARIABLES	(1) Atlanta	(2) Boston	(3) Cincinnati	(4) LA	(5) Miami	(6) Pittsburgh	(7) Seattle	(8) DC
COVID	-0.28	-0.18	-0.18	-0.33	-0.06	-0.12	-0.12	**-0.29***
	(0.21)	(0.16)	(0.16)	(0.24)	(0.13)	(0.16)	(0.13)	**(0.16)**
Precipitation	-0.56***	0.11	-0.35***	-0.13	0.00	-0.15	-0.07	-0.06
	(0.12)	(0.16)	(0.11)	(0.33)	(0.05)	(0.13)	(0.10)	(0.10)
Temperature	-0.01***	-0.01***	-0.00**	-0.01***	-0.02***	-0.01***	0.00	-0.02***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Constant	3.74***	3.81***	3.14***	4.33***	4.54***	3.46***	3.49***	4.11***
	(0.12)	(0.07)	(0.07)	(0.31)	(0.20)	(0.06)	(0.09)	(0.08)
Observations	206	200	200	196	180	204	195	182
R-squared	0.29	0.55	0.12	0.10	0.26	0.42	0.21	0.65

Standard errors in parentheses. Dependent variable is $\ln(NO_x)$. Includes year fixed effects.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 3.2: CO by City

VARIABLES	(1) Atlanta	(2) Boston	(3) Cincinnati	(4) LA	(5) Miami	(6) Pittsburgh	(7) Seattle	(8) DC
COVID	-0.19	-0.14	-0.07	**-0.68****	-0.08	-0.36	0.13	**-0.31***
	(0.19)	(0.15)	(0.17)	**(0.31)**	(0.17)	(0.25)	(0.14)	**(0.17)**
Precipitation	-0.37***	-0.01	-0.19*	0.50	0.14**	-0.50**	-0.19*	-0.05
	(0.11)	(0.14)	(0.11)	(0.42)	(0.07)	(0.21)	(0.11)	(0.11)
Temperature	-0.01***	-0.01***	-0.00***	-0.02***	-0.02***	-0.00**	-0.00**	-0.01***
	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
Constant	-0.43***	-0.81***	-1.07***	0.55	0.11	-0.74***	-0.41***	-0.44***
	(0.11)	(0.06)	(0.07)	(0.40)	(0.24)	(0.10)	(0.09)	(0.08)
Observations	203	200	205	195	190	197	176	182
R-squared	0.14	0.31	0.16	0.16	0.34	0.11	0.16	0.37

Standard errors in parentheses. Dependent variable is ln(CO). Includes year fixed effects.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 3.3: PM_{10} by City

VARIABLES	(1) Atlanta	(2) Boston	(3) Cincinnati	(4) LA	(5) Miami	(6) Pittsburgh	(7) Seattle	(8) DC
COVID	0.15	-0.02	**0.35****	-0.23	-0.01	0.05	0.52	-0.11
	(0.16)	(0.26)	**(0.17)**	(0.29)	(0.24)	(0.19)	(0.35)	(0.18)
Precipitation	-0.23**	-0.90***	-0.30***	-2.27***	-0.15	-0.83***	-1.21***	-0.48***
	(0.09)	(0.27)	(0.11)	(0.45)	(0.10)	(0.15)	(0.33)	(0.11)
Temperature	0.01***	0.01***	0.00***	0.02***	0.01**	0.01***	0.01**	0.00
	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
Constant	2.06***	1.98***	2.52***	2.08***	1.76***	2.33***	1.72***	2.80***
	(0.09)	(0.12)	(0.07)	(0.39)	(0.34)	(0.07)	(0.27)	(0.08)
Observations	206	188	208	168	193	208	117	181
R-squared	0.35	0.26	0.17	0.30	0.18	0.31	0.24	0.15

Standard errors in parentheses. Dependent variable is $\ln(PM_{10})$. Includes year fixed effects.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 4.1: Regressions

VARIABLES	(1) NO_x (ppb)	(2) CO (ppm)	(3) PM_{10} ($\mu g/m^3$)
COVID	-0.12	-0.13	0.06
	(0.08)	(0.09)	(0.10)
FIRSTCASE	0.09	-0.02	-0.09
	(0.06)	(0.07)	(0.08)
LOCKDOWN	-0.18***	-0.06	0.15**
	(0.04)	(0.05)	(0.06)
Precipitation	-0.21***	-0.11**	-0.48***
	(0.04)	(0.05)	(0.05)
Temperature	-0.01***	-0.01***	0.01***
	(0.00)	(0.00)	(0.00)
Constant	3.74***	-0.62***	2.30***
	(0.03)	(0.04)	(0.04)
Observations	1,563	1,548	1,469
R-squared	0.52	0.54	0.45

Standard errors in parentheses. Includes city and year fixed effects. Dependent variables are in ln.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 2.1 displays downward trends in NO_x and CO concentrations as the pandemic progressed. NO_x concentrations following local lockdowns were 19.6% lower than pre-COVID levels, while CO concentrations fell 15.4%. On the other hand, PM_{10} concentrations post-lockdown were 5.9% higher with an upward trend after COVID-19's arrival in the United States, but this is not significant. Table 2.2 shows that the differences in means between these two time frames are statistically significant for all three pollutants at a .01 significance level.

The estimated relationships between *covid* and each of three pollutants are summarized by city in Tables 3.1–3.3. Similar to the traffic analysis, the coefficients of *covid* on pollutant concentrations are insignificant for most cities. Controlling for weather and year fixed effects, the relationship between *covid* and both NO_x/CO is significantly negative only for Washington, D.C. For PM_{10}, the only significant coefficient is for Cincinnati, where it is positive.

Table 4.1 shows the main regression of interest. The coefficients of *covid* and *firstcase* predict decreases in all three pollutants after the first local confirmed case but before the imposition of local lockdowns. However, the coefficients on these two events for all three pollutants are not significant at a .05 significance level. There is a significant negative coefficient on *lockdown* for NO_x and a significant positive coefficient for PM_{10}.

V. Discussion and Conclusion

A. Traffic

The decreases across the whole sample in both pollutants and travel times between Q1 (2017–2019) and Q1 (2020) are consistent with previous literature regarding the effects of the pandemic's onset on these variables, although analysis at the city level reveals insignificant changes in traffic for five of eight cities. Since the pollution data are taken directly from measuring stations, there is little concern about the validity and accuracy of these measurements; other studies use the same data.

Naturally, there are legitimate objections to using Uber Movement data to estimate relative traffic congestion. There may be changes in travel priorities (e.g. a shift from visiting all businesses to only "essential" businesses) which raises concerns of confounding variables in the Uber Movement analysis. For instance, perhaps people took shorter Uber trips as COVID-19 spread or took fewer Ubers over concerns of possible transmission between drivers and passengers. There are many plausible shifts in trip priorities that could affect the distribution of Uber volume across zone pairs. However, comparing only zone pairs with multiple months of data addresses this problem as it generally controls for inherent zone pair differences. This allows each respective zone pair's trips to be compared over different months, regardless of volume shifts. Removing non-recurring zone pair data also reduces the sample to higher-volume pairs that tend to be concentrated in the city center, which refines the Uber Movement estimate of urban traffic congestion.

Another concern with the Uber data is that rideshare services themselves may be contributing to traffic congestion. When an Uber has passengers and is en route to a destination, the Uber vehicle is often a 1-for-1 replacement for a private vehicle that would have otherwise been used. "Active" Ubers do not substantially increase or decrease traffic congestion. However, the primary concern is in regard to Ubers that are *not* in use — when they are "idling" between trips. Empty rideshare vehicles roaming around a city add additional cars to the road without transporting additional people or goods. During these instances, there is a valid argument for rideshares contributing to traffic congestion.

Uber travel volume fell by 80% in April 2020. Thus, the distorting effect of Ubers on private vehicle travel times would decrease over the course of the pandemic (Siddiqui 2020). A wide array of factors can influence traffic patterns, including Ubers. However, this analysis is not seeking to isolate Uber-independent traffic because the relationship in question concerns overall traffic — regardless of cause — and pollution.

Within-city regressions (Table 1.2) reveal considerable variation in traffic changes with respect to *covid*. Although weekday travel times decreased over the whole sample, there is an insignificant association between *covid* and travel times for five of the eight cities. Likewise, although all three pollutant concentrations decreased between the pre-*covid* and *covid* periods (Table 1.1), the decreases were also not significant. One possible explanation is the limitations of the Uber Movement data. Uber published data up to March 31st, 2020, leaving only the very beginning of the pandemic and its related effects available for the traffic analysis. By the end of March, COVID-19 lockdowns were only two weeks old at most, whereas *covid* represents the arrival of the virus to the United States two months earlier. The lack of significant coefficients for most cities supports the hypothesis that individual concern about the virus without reinforcement from government restrictions did not greatly influence human activity and consequently traffic congestion.

Of the eight cities, only Los Angeles and Washington, D.C. have significant negative coefficients of *covid* on traffic congestion. At first glance, it is difficult to determine any particular similarities between the two cities. Los Angeles' first confirmed case was on January 26th, 2020, while Washington, D.C.'s was six weeks later on March 7th, 2020. They are geographically distinct and have very different climates. However, one potential similarity is the political lean of these two cities' regions. California and the District of Columbia are the two most liberal states/regions in the sample based on the percentage of votes for President Joe Biden in the 2020 election (New York Times 2020). Individuals who identify as Democrats are more likely to follow social distancing and mask-wearing COVID-19 guidelines than Republicans (Leventhal et al. 2020). Although greater COVID-19 caution in Democratic states presents one potential explanation for the significant drops in traffic under *covid* in these two cities, this area clearly requires more research.

Over the whole sample, the primary regressions (Table 1.3) do not strongly support or oppose previous findings on the causal relationship between traffic and NO_x/CO concentrations. The coefficient of *ln(traffic)* is positive, although insignificant, for both of these pollutants. This may be also attributed to the limitations of the Uber data. Having more data during the pandemic may have captured the longer-term effects of government lockdowns, given only two months out of twelve sample months are classified as *covid* months.

However, the negative coefficient of *ln(traffic)* on PM_{10} is noteworthy. According to previous studies, PM_{10} is not particularly influenced by traffic congestion, although prevailing estimates suggest a positive relationship. PM_{10} concentrations are not highly correlated with short-term time-variant traffic conditions, but there is evidence that over longer timespans, recurring traffic congestion raises ambient PM_{10} concentrations (Cheng and Li 2010). According to Table 1.3, a 1% increase in travel times is correlated with a .0559 μg/m^3 decrease in PM_{10}. A negative coefficient on the traffic x *covid* interaction term suggests the relationship is even more negative during the pandemic, which is an interesting finding. However, the coefficients are not significant, so further study is required.

The within-city regressions on PM_{10} show that the effects of covid vary between cities, and apart from one city, the coefficients are insignificant (Table 3.3). This supports previous conclusions that PM10 concentrations are not influenced as strongly by traffic congestion in the short-term as NO_x or CO. Given PM_{10}'s detrimental effects on respiratory health, more detailed analyses of the sources of PM_{10} are imperative to developing effective pollution-abatement policy.

The traffic analysis does not strongly support previous literature on traffic-based pollution externalities, although data limitations may have contributed to the insignificant coefficients.

B. Lockdown

Table 2.1 shows a downward trend in NO_x and CO concentrations as the pandemic progressed and an upward trend in PM_{10}

concentrations. Although the traffic analysis indicated a drop in PM_{10} concentrations between the pre-*covid* and *covid* periods, that analysis' covid variable represented a cruder and shorter timeframe than the lockdown analysis. Whereas average PM_{10} concentrations were lower between February/March 2020 and the first quarters of the previous three years, the lockdown analysis contains data up to the end of 2020, revealing an upward trend in PM_{10} towards the middle and late months of 2020. In fact, PM_{10} concentrations increased 6.0% between the pre-*covid* and post-*lockdown* periods. The lockdown analysis provides more fruitful findings regarding how pollutant concentrations responded to decreases in human activity given that the concentrations for all three pollutants were significantly different between the pre-COVID and post-lockdown periods (Table 2.2).

Within-city regressions on NO_x and CO (Table 3.1/3.2) display negative coefficients on *covid* for all eight cities except for CO concentrations in Seattle. Although most of the coefficients are insignificant, Washington D.C.'s are significant for both pollutants, and Los Angeles' is for CO. Across the entire sample, NO_x concentrations were 7.3% lower after *lockdown* compared to all observations under *covid*, while CO concentrations dropped 2.9%. NO_x concentrations appear to be more responsive to the decrease in human mobility. The traffic analysis also demonstrates this trend, with NO_x concentrations having a greater proportional decrease associated with traffic.

Most within-city regressions show significant coefficients on the weather variables, which tend to be negative for both temperature and precipitation. The coefficients on the weather variables in Table 3.3 support the "washing out" process of airborne PM_{10} by precipitation and the tendency of PM_{10} pollution to be the worst during summer months. The coefficient of precipitation is significantly negative, while the coefficient of temperature is significantly positive for most, if not all, eight cities.

firstcase is included to determine if individual behavior, independent of COVID-19 lockdowns, may have shifted economic activity enough to produce a noticeable effect on air pollution. Studies in Taiwan, which did not have stringent stay-at-home restrictions over the duration of COVID, support the notion that unrestricted individuals do not significantly change their behavior even in the face of a public health crisis. The coefficients on *firstcase* for all three pollutants are smaller in magnitude than *covid* or *lockdown*. Assuming that NO_x is more responsive to changes in traffic congestion, the insignificant coefficient of *firstcase* for NO_x concentration suggests that economic activity did not substantially decrease in the period between COVID-19's arrival and local lockdowns. This may be because people did not yet understand the severity of the pandemic or valued social gatherings and "non-essential" business highly. Within the context of COVID-19's presence in the United States, the local arrival of the virus, denoted by *firstcase*, does not appear to be heavily associated with decreases in air pollution.

Aside from the PM_{10} data, the event regressions (Table 4.1) support other literature on the effects of lockdowns on pollution. *lockdown* reinforces the pre-existing relationship with *covid* as the coefficients match for each pollutant: both negative for NOx and CO, and both positive for PM_{10}. Studies of COVID-19 lockdowns in other countries yield the same result that lockdowns decrease air pollution in part by decreasing human mobility and transportation-related emissions. For instance, the coefficient of *lockdown* on NO_x is negative and significant, estimating a .18 ppb decrease associated with local lockdowns after incorporating both the effects of COVID-19's arrival in the United States and the first confirmed local case. As evidenced in the traffic analysis and the covid-only regressions, CO concentrations do not appear to be as responsive to lockdowns, but they still decreased significantly over the course of the pandemic.

C. Conclusion

Of the three pollutants studied, decreases in NO_x have the greatest correlation with tightening mobility restrictions. The majority of airborne nitrous oxides come from anthropogenic sources (EPA 2018). While both CO and NO_x concentrations significantly decreased between the pre-COVID and post-*lockdown* time frames, NO_x dropped by 19.6%, while CO fell 15.4%. Over the whole sample, NO_x has a positive, albeit insignificant correlation with Uber Movement travel times.

Putting both analyses together, the greatest decreases in NOx concentrations are in cities that also have the largest drops in travel times after *covid*. Since ground transportation is a major source of NO_x emissions and NO_x concentrations appear to be the most responsive to decreases in human mobility, NO_x is a strong example of a traffic-based air pollution externality, whose social costs are not internalized by commuters.

Modeling NO_x emissions with *covid*, *firstcase*, and *lockdown* events reveals that lockdown has a significant negative effect on NO_x concentrations. COVID-19 lockdowns are the most stringent restrictions on human mobility, as pre-lockdown data show smaller, insignificant changes in both travel times and air pollution. In the absence of strict government lockdowns, air pollution concentrations do not diverge heavily from pre-*covid* measurements.

Many of the trends in NO_x concentrations can also be seen with CO. Concentrations of both of these pollutants decreased over the course of the pandemic, although the coefficients of all three events are insignificantly negative for CO. Of the eight cities, Washington, D.C. had significant drops in traffic, NO_x, and CO associated with *covid*. Like NO_x, the cities with the largest decreases in travel times tended to also have the largest decreases in CO concentrations, especially Washington, D.C. and Los Angeles. Both traffic and lockdown analysis reveal a decrease in CO as human mobility decreased.

While CO concentrations appear to be positively associated with traffic congestion, the sharpest event-based decrease is associated with the *covid* variable rather than *lockdown*. Although CO concentrations dropped 15.4% from pre-*covid* levels, less of the decrease comes from *lockdown* in comparison to NO_x, suggesting that there are unaccounted-for non-mobility related factors influencing carbon monoxide concentrations. This may imply that although CO emissions are also a result of increased economic activity, they are not determined as strongly by vehicle emissions and that CO concentrations are also influenced by other sources that were less negatively impacted by government lockdowns.

PM_{10} has a wide array of detrimental health impacts and is one of the most straightforward examples of pollution externalities. However, the PM_{10} data in both analyses run counter to previous assumptions of traffic's impact on particulate matter. While PM_{10} decreased between the first quarters of 2017–2019 to 2020, controlling for weather and city effects reveals a negative association with traffic congestion. The negative coefficient from the traffic analysis may in part be attributed to the general upward trend in PM_{10} concentrations over the course of the pandemic. As traffic started to decrease after the arrival of COVID-19, PM_{10} concentrations began increasing, suggesting an increase of activity from other sources of particulate matter. As mentioned before, traffic congestion is not a major source of PM_{10}. Particulate matter, unlike NO_x or CO, comes from a diverse group of both anthropogenic and natural sources since PM_{10} includes pollen, smog, smoke, suspended liquid droplets, and inorganic ions (California ARB 2021).

The data from the lockdown analyses suggest that human mobility does not have a significant positive relationship with PM_{10}. *covid* and *lockdown* are positively associated with PM_{10} concentrations, and the significant coefficient on *lockdown* suggests that limiting human mobility may have exacerbated other sources of PM_{10} as more people stayed at home. The data are consistent with the notion that transportation is not a significant source of PM_{10}. Thus, PM_{10} is likely not a pollutant of concern strictly within the context of traffic-based pollution externalities. However, the harmful health effects of particulate matter are still an important issue that demand attention from urban planners and regulators.

In conclusion, the two analyses support a larger role of government in reducing pollution-based externalities from human mobility. The insignificant coefficients on *firstcase* imply that individuals are far more incentivized to reduce their activity from government coercion than the possibility of viral transmission. This suggests that other social costs of human mobility, such as pollution, are also largely overlooked by commuters. In light of this, several countries have already adopted various methods to reduce traffic and pollution since both have evident economic and health effects. As the United States grows more urbanized, it is of utmost importance that legislators develop sound policy and infrastructure improvements that address the detrimental effects of traffic congestion while still maintaining economic stability and growth.

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Economic Development in Ukraine

Policies for Modernization

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Ukraine has the potential to emerge as a major player in the European economy, but institutional mismanagement continues to stifle its long-term growth. Mismanagement, however, is not the only force working against Ukraine's economic development. A declining population, reliance on foreign energy, and volatility in its political regime all slow the country's economic development. In comparison to Ukraine, neighboring nations like Romania have enjoyed substantial development in the post-communist era. This study presents a framework for improving Ukraine's income per capita by using Romania's economic composition as a model for post-communist economic growth. To this end, I detail Ukraine's recent economic and political history and use development accounting to compare it with Romania. I conclude with policy recommendations for Ukrainian authorities to reach Romanian levels of income per capita.

Economic and Political Histor

Ukraine is a large Eastern European state bordered by Russia, Poland, Romania, Belarus, Moldova, and the Black Sea. A lack of employment prospects in the country contributes to its declining population, currently estimated at 43.9 million people, down from 52 million in 1991 (World Bank 2020). Strong historical ties with and close geographical proximity to Russia have led to a reliance on Russian imports: 15% of Ukrainian imports came from Russia in 2018 (OEC World 2020). This reliance is especially evident in the energy sector, where Ukraine's dependence on energy imports presents risks of manipulation through threats of burdensome unit pricing by Russian firms like Gazprom (Central Intelligence Agency 2020

Mass anti-corruption protests occurred in November 2013 after Ukrainian President Viktor Yanukovych abandoned an association agreement with the European Union in favor of strengthening trade ties with Russia. The protests, catalyzed by Yanukovych's action and fueled by calls to eliminate corruption and Russian influence, came to be known as the Revolution of Dignity and resulted in Yanukovych's ousting. Seizing on Ukraine's moment of weakness, Russia annexed Crimea, began a proxy war in the Donbas region of eastern Ukraine, and initiated an economic sparring match, crippling a Ukrainian economy still weakened by the 2008 financial crisis (Central Intelligence Agency 2020). Today, the proxy war continues between Russian-backed separatists and Ukrainian forces in the Donbas, the area that used to be Ukraine's center of industrial output. Marred by violence, Eastern Ukraine is struggling to regain its productive footing. Bluszcz and Valente (2020) estimate that the region's GDP per capita was 43% lower from 2013 to 2016 than what it would have been if the conflict had not occurred. While firms may have the labor and capital required to be productive, individual productivity has been severely constricted by the conflict.

Lastly, Ukraine's business ownership profile is dominated by oligarchs. These oligarchs wield tremendous power to control output. Corruption is rampant in both the public and private sectors, undermining state institutions. Oligarchy is spurred on by laws like the 2001 moratorium on the sale of agricultural land, where parcels of Soviet collectives were doled out to individuals with the condition that the land must stay under that individual's control or that of an heir (Matuszak and Olszański 2020). As a result, massive agricultural conglomerates either lease land from tens of thousands of individuals or farm without a contract on land left vacant after heirless owners die. This system not only discourages landowners from making improvements to their fields but also creates the opportunity for mass-leasers to make a fortune while exploiting the citizens who own the land. These limits on market access further dampen substantial long-term economic development.

However, international organizations have attempted to incentivize progress. Ukraine received an IMF bailout package in 2014 worth $17 billion as well as two stand-by agreements in 2018 and 2020 worth $3.9 billion and $5 billion, respectively (Ministry of Finance of Ukraine 2020). These agreements require Ukraine to demonstrate progress in strengthening its institutions and combating corruption, among other conditions, to aid in economic growth. Shocks from the 2014 recession and 2020 COVID-19 pandemic, however, have tested Ukraine's economic resolve, making the satisfaction of these loan condition requirements more difficult.

Taking stock of its background, Ukraine is a foreign energy dependent nation with high levels of corruption, severe economic and political instability, a declining population, and outdated laws that dissuade private investment. Unlike other Warsaw Pact nations, Ukraine cannot seem to escape its current cycle of recession and stagnation.

Development Accounting

Precedent exists for the comparison of Ukraine to Poland to propose development policy, but given Ukraine's current state, Poland is not an attainable target. Simply put, Poland's output per capita is too high of a target; Ukraine needs to walk before it can run. A pragmatic choice for development accounting comparison, however, is Romania. Romania is an ideal comparison because its economy, like Ukraine's, was characterized by instability in the 1990s

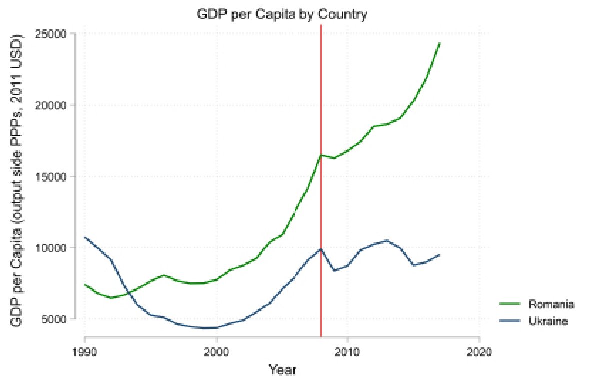

Figure 1. Source: Penn World Tables 9.1

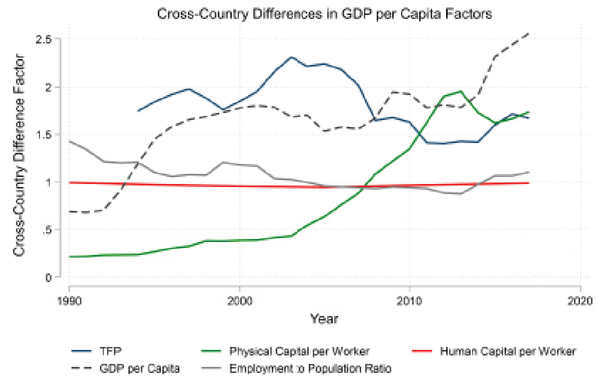

Figure 2. Source: Penn World Tables 9.1

and early 2000s. Romania's economy grew similarly to Ukraine's up until 2007, supported by post-communist privatization and trade with Western Europe. Unlike Ukraine, Romania joined the European Union in 2007, bolstering an economic network that staved off lasting damage from the impending 2008 financial crisis. Romania has since evolved into one of Europe's fastest growing economies, ranking sixth in Europe for 2019 GDP growth (World Bank 2020). A comparison of GDP per capita by country in Figure 1 provides the greatest motivation for comparing the profiles of Ukraine and Romania. After the fall of the Soviet Union, Romania's GDP per capita was lower than Ukraine's but with roughly the same downward trend.

While Ukraine suffered from declining output and increasing inflation in the 1990s, Romania reinvented itself with some success. By 2000, however, Ukraine's GDP per capita growth was on track with Romania, a trend that continued until 2008, identified by the red line in Figure 1. From 2008, Romania's output per capita has grown exponentially, while Ukraine continues to stagnate. Romania is an excellent model for developing economic policy in Ukraine because their growth rates since 2008 vary wildly despite similar trends until that point. In 2017, the last available year in the Penn World Tables (PWT) v9.1 data, Romania's GDP per capita was 2.56 times higher than that of Ukraine (Penn World Tables 9.1 2017).

To examine the causal factors behind this output per capita difference factor of 2.56, I perform development accounting, capitalizing on the work of Hall and Jones (1999) and Caselli and Wilson (2004). Caselli and Wilson present Hall and Jones' adaptation of the neoclassical production function in per-worker terms, given by

$$\frac{Y}{EMP} = Ak^{\alpha}h^{1-\alpha},$$

where Y is output, EMP is the employed population, k is the amount of capital available to each worker, h is the level of human capital, and A represents the residual variation or Total Factor Productivity (TFP). To engage this model, I use the Penn World Tables dataset, version 9.1.

From the PWT data, I calculate the variables from equation (1). I calculate the left-hand side by dividing the real output side GDP at chained PPPs by the reported employed labor force for each country, and I calculate k by dividing capital stock at current PPPs by the number of reported workers. Human capital and TFP levels are indexed by the authors of the PWT and can therefore be directly implemented into the model with the former calculated by applying the work of Barro and Lee (2001). To fully evaluate the output per capita from Figure 1, the model from equation 1 is extended such that

$$y = \frac{Y}{Population} = \frac{EMP}{Population} * \frac{Y}{EMP},$$

requiring the calculation of the ratio of a country's employed persons to its population. Equations (1) and (2) are calculated and then compared between Romania and Ukraine to explain what is driving the output per worker in the former to be greater than the latter by a factor of 2.56. We therefore have

$$\frac{y_{ROM}}{y_{UKR}} = \frac{EMP_{ROM}/POP_{ROM}}{EMP_{UKR}/POP_{UKR}} * \frac{A_{ROM}}{A_{UKR}} * \left(\frac{k_{ROM}}{k_{UKR}}\right)^{\alpha} * \left(\frac{h_{ROM}}{h_{UKR}}\right)^{1-\alpha}$$

which represents the ratios of both output per capita and the determinants of this gap between Romania and Ukraine.

From the 2017 observations for Romania and Ukraine, the ratio of employed persons to population between countries is 1.10, the TFP ratio is 1.667, the capital per worker ratio is 1.735, and the human capital ratio is 0.998. These numbers, along with their historical values in Figure 2, indicate it is neither the amount of individuals Ukraine employs in its labor force nor the amount of human capital available to each worker that causes it to lag behind Romania. Rather, the joint effects of TFP and capital per worker explain why Romania leads Ukraine in GDP per capita by a factor of 2.56. It is also important to note that while the cross-country ratio of capital per worker is higher than the TFP ratio, capital per worker is raised to the power of α, which both Hall and Jones (1999) and Mankiw, Romer, and Weil (1992) evaluate to be approximately equal to 1/3 for most countries. Figure 3 plots these ratios with the physical capital per worker ratio raised to the 1/3 power and human capital per worker raised to the 2/3 power. Economic policy to bring Ukraine to Romania's level of GDP per capita must therefore focus primarily on closing the TFP gap as illustrated in Figure 3, with increasing physical capital stocks as a secondary goal.

Policy Recommendations

To accomplish the task of catching up to Romania in terms of economic growth, I propose three policies for the Ukrainian government to pursue: institutional reform, infrastructure accessibility and development, and research and development (R&D) incentives. First, Ukraine must overhaul its institutions. While IMF and World Bank loan conditions have made an impact on the incidence of corruption, it remains a serious issue for Ukraine's economic development. In 2019, 37% of Ukrainian firms paid bribes to public

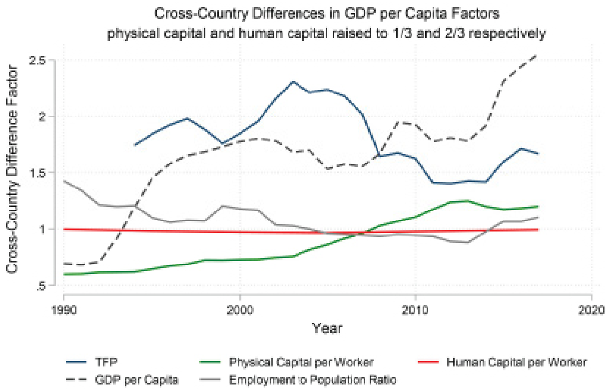

Figure 3. Source: Penn World Tables 9.1

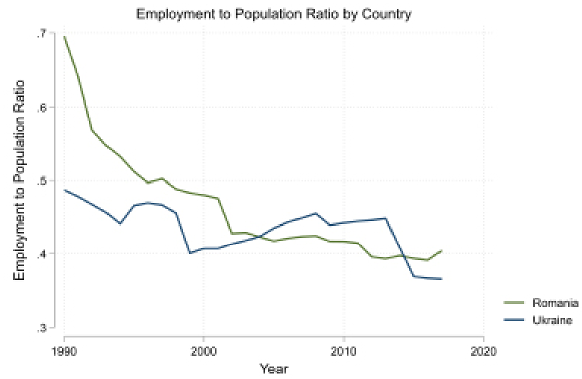

Figure 4. Source: Penn World Tables 9.1

officials and 24% of firms reported informal payments to public officials to cull favor. Additionally, Ukrainian firms spend double the amount of time preparing and paying taxes every year compared to Romanian counterparts (World Bank 2020). The administrative inefficiencies of these tax policies create additional costs for firms, reducing the amount of time and money that could be focused on production. Furthermore, Ukraine has not taken a proper census since 2001. Any economic projections or policy analysis are held back by a near 20-year data vintage. Taking a regular census ensures well-informed economic policies are implemented, relying on the most accurate population and labor statistics possible.

Reforming the governmental and economic institutions of Ukraine would make the business environment more productive by reducing excessive time and money costs while building a robust body to accurately assess and improve the economic health of the state. Ukraine has many avenues for reform in the public and private sectors. Reducing the incidence of corruption, for example, promotes a healthier free market, where competition between unsubsidized and uncorrupted firms is stronger, resulting in greater productivity. These anti-corruption measures have already started in Ukraine with independent agencies like the National Anti-Corruption Bureau and stronger laws against collusion and bribery between the government and private sector. Fisman (2001) quantifies the value of government connections in the private market, finding firms with strong ties to the government are weakened by a lack of innovation due to their assumption of market security. Weak market competition due to government favor of specific firms therefore leads to an absence of innovation and stagnant productivity. This is confirmed by Alder (2009), who finds firms that employ managers based on political connections generate productivity losses of up to 20%. A free market will incubate innovation, thereby boosting TFP in Ukraine.

An example of Ukrainian market reform is the implementation of an agricultural land market, set to start in 2021. The ability to buy and sell land is incredibly important to the productivity of the plot. Farms that own more land benefit from economies of scale and productivity: Adamopoulos and Restuccia (2014) identify how in the US, the difference in value added per worker is 16 times larger for large farms compared to small farms. Foster and Rosenzweig (2011) demonstrate this conclusion also holds for less developed nations like India. Interestingly, the differences in returns based on farm size observed around the world do not apply to Ukraine due to its predominant land-lease megafarm-ownership profile. That is to say, the few oligarchs who run Ukraine's top agriculture producers have less incentive to care for the farmland because they have no opportunity to own it, while the actual owners of the land who have no interest in farming don't care to maintain it either. The rigidity of property rights makes Ukraine less productive.

As discussed by Goldstein and Udry (2008), property rights have a large impact on crop yields due to the incentives they create for farmers to properly care for their fields, such as implementing procedures like crop rotations to enhance the longevity of the land. In Goldstein and Udry's study, Ghanaian farmers who are insecure in their land holdings ignore proper farming techniques, while those who own their land outright are more productive because they can afford to follow optimal farming techniques like a long fallow or crop rotation. Ukraine is an extreme extension of the case of Ghana: landowners have no ability to sell their land to another party, forcing farms into the insecure farmer case of Ghana through lease agreements. The removal of barriers to ownership in the agricultural land market will increase output per worker by incentivizing higher-quality farming techniques, thereby unlocking large-farm economies of scale (Adamopoulos and Restuccia 2014).

Second, while Ukraine reports 100% access to electricity, it takes 267 days on average to have a permanent electricity connection installed in a structure. Romania, in comparison, reports only a 174-day average installation time (World Bank 2020). If Ukraine wishes to increase productivity, particularly through TFP, it is not enough to have a robust power grid. Firms must be able to access the grid in a timely manner so that operations, and therefore output, can expand with new technologies. Firms looking to expand into Eastern Europe will likely choose a nation known for quick and cheap start-up costs. As Ukraine is slower and more expensive than Romania in this regard, it will not be able to attract international firms with advanced production processes, therefore failing to improve its TFP figure. Not only do the proprietary production processes increase TFP through the introduction of new firms, but Greenstone, Hornbeck, and Moretti (2010) also estimate significant spillovers impact neighboring incumbent plants as well. Greenstone, Hornbeck, and Moretti (2010) estimate that there is a 12% increase in TFP for incumbent plants five years after a new plant opens in the US, a trend that can be replicated in Ukraine.

In a similar vein to infrastructure and productivity enhancement lies internet access. As of 2019, Ukraine had only 16 broadband subscriptions per 100 individuals, while there were 27 connections per 100 individuals in Romania (World Bank 2020). Lack of robust internet access impedes the exchange of ideas and information, hurting productivity development in any economy. In their analy-

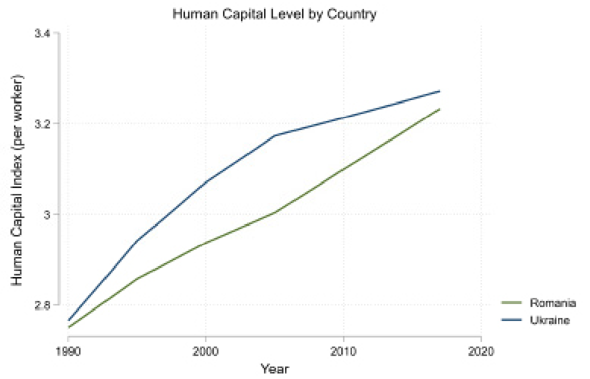

Figure 5. Source: Penn World Tables 9.1

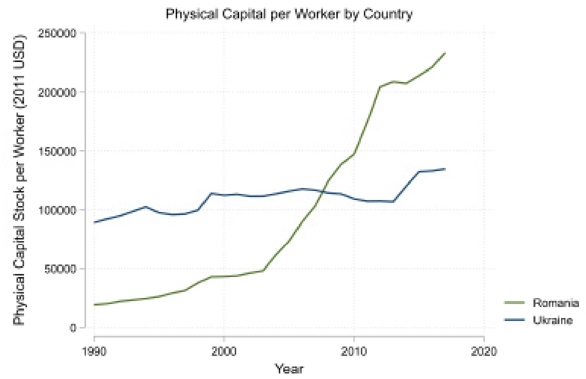

Figure 6. Source: Penn World Tables 9.1

sis of OECD information technology (IT) implementation, Chou, Chuang, and Shao (2014) find IT boosts TFP through positive externalities and innovations. Firms adapt technologies to enhance the productivity of their organization, regardless of if the technology was originally intended for that purpose. IT implementation also attracts new TFP-generating businesses. As described in a McKinsey Global Institute survey, 2.6 jobs are created for every job destroyed through obsolescence (Manyika and Roxburgh 2011).

Third, to close the GDP per capita gap with Romania, Ukraine must promote programs to encourage firm involvement in the production of high-tech goods and the development of advanced production methodologies. While Romania reports that 10% of manufactured exports are high-tech goods, Ukraine only reports 5% (World Bank 2020). Both countries experience low levels of high-tech exportation, but Romania leads due to higher order infrastructure accessibility, as discussed above, and human capital dedicated to research and development.

Comparing Ukraine to Romania, differences in human capital are negligible, as both countries provide education opportunities to their citizens at similar levels. Romania only tops Ukraine when it comes to advanced production and a research-focused labor force. For Ukraine to reach Romania, it must incentivize research and development through grants, subsidies, or tax reductions for firms in order to modernize its economy and boost productivity. This follows from Zachariadis (2004), where aggregate R&D intensity, or the proportion of resources allocated to R&D, in the OECD has a statistically significant impact on productivity and output through TFP. Recent trade deals with the European Union and the UK will boost Ukraine's economy, but more value can be extracted from these agreements if OECD nations see Ukraine as a place where technology blooms.

National R&D participation is also afflicted by Ukraine's continuous population decline. A continuous exodus of individuals seeking economic opportunities abroad hurts the long-term economic outlook of Ukraine with respect to high-tech production and output. By incentivizing R&D for individual firms, Ukraine can create more high-paying opportunities, encouraging citizens to stay in the country. This will increase TFP and therefore GDP per capita levels (Ulku 2007).

While Ukraine has struggled politically and economically since the collapse of the Soviet Union, it has the potential for lasting long-term economic change. As Romania succeeded in the late 2000s, Ukraine can redefine itself by adopting policies focused on boosting TFP and making itself an attractive destination for large international firms. By prioritizing institutional reform, infrastructure access, and research and development, Ukraine can break free from its current cycle of recession and stagnation and improve its position in the global economy.

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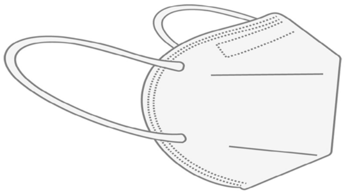

Investigating Altruistic Demand During COVID-19

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Amherst College

INTRODUCTION

Policies in the United States aimed at curbing the spread of COVID-19 have been largely issued and enforced at the local level. As a result of the localized nature of these policies, which include social distancing requirements, mask mandates, and business closures, US states have demonstrated varied levels of enforcement, rooted in varied levels of public support for the regulations (Justia 2021). Understanding people's level of support for these interventions requires that we reframe our thinking about individual decision-making by incorporating the incentives particular to pandemic conditions.

In the presence of a contagious virus, each individual's health status and behavior directly impacts others' health, freedom, and well-being more than it does in non-pandemic circumstances. In Section I, I discuss how an altruistic individual's heightened demand for her own health in a pandemic due to others' reliance on her health may mitigate the moral hazard effects of health insurance on her support for risk-mitigating regulations. I explain how individuals whose in-pandemic preferences do not change due to altruism can be encouraged to adopt pseudo-altruistic preferences and engage in socially beneficial behavior through cost-sharing insurance plans. In Section II, I explore how individuals' indifference curves become interdependent in a pandemic. Drawing on insights from the Grossman health economics model, I show how adjustment of one's own preferences to accommodate others does not necessarily shift welfare loss across individuals. Instead, it can mitigate society's total welfare loss in a pandemic.

SOCIAL LOSS THROUGH MORAL HAZARD

Moral hazard arises when some individuals lack incentive to guard themselves against risk because they are partially protected from its consequences, as in the case of health insurance. Because health insurance decreases the financial cost an individual incurs if she is infected with COVID-19, we would expect that insured individuals are more likely to engage in behaviors that increase their infection risk and less likely to support institutional restrictions on their behavior. global pandemic, *The New York Times* (2020) reported that some US states had no mask mandate and businesses were mostly open.

If moral hazard explains whether states adopt certain regulations in the pandemic, we should find that states with more uninsured people have more business restrictions and mask mandates, and vice versa. However, this is not the case (see Figure 1). As of December 10th, 2020, more than seven months after the World Health Organization declared the COVID-19 outbreak a global pandemic, *The New York Times* (2020) reported that some US states had no mask mandate and businesses were mostly open.

Some of these states lacking restrictions have large uninsured populations, according to Becker's Hospital Review (2020), and had high infection rates at the time, according to the Centers for Disease Control and Prevention. States with no restrictions, high uninsured populations, and high infections rates included Tennessee, Wyoming, and South Dakota. Michigan, on the other hand, had a fairly low infection rate in mid-December and a lower percentage of uninsured, but businesses were mostly closed, and a mask mandate was in place.

Why do we not see the moral hazard effects of insurance on risky behavior? It may be that individuals' demand for health, especially the demand of more altruistic individuals, changes in a pandemic wherein one's own health affects the health of those around her. Altruistic demand may mitigate the effects of moral hazard, and therefore the differences in mandate statuses that we see are explained by other factors, such as belief in the severity of the virus, belief in mandate efficacy, and trust in the government to

Figure 1

State*	% Population Uninsured**	Business Closures***	Mask Mandate	Cases/100K Last 7 Days****
Florida	25	open	no	43.5
Oklahoma	24	open	no	N/A
Georgia	23	open	no	47.7
Tennessee	19	open	no	72.3
Wyoming	17	open	no	91
South Dakota	16	open	no	97.7
Illinois	13	closed	yes	78.4
Indiana	13	open	yes	103.1
Michigan	12	closed	yes	67.9
Ohio	11	open	yes	77.3
Wisconsin	10	open	yes	77.8
Iowa	9	open	yes	71.7
Minnesota	8	closed	yes	95.2

*These particular 13 states are included to highlight the lack of correlation between insurance status and mandate.
**Source: Becker's Hospital Review
***Source: *The New York Times*
****Source: Centers for Disease Control and Prevention

Figure 1:
Uninsured Population Rates, State Business Closures, Mask Mandate Prevalence, and Covid-19 Cases

Figure 2

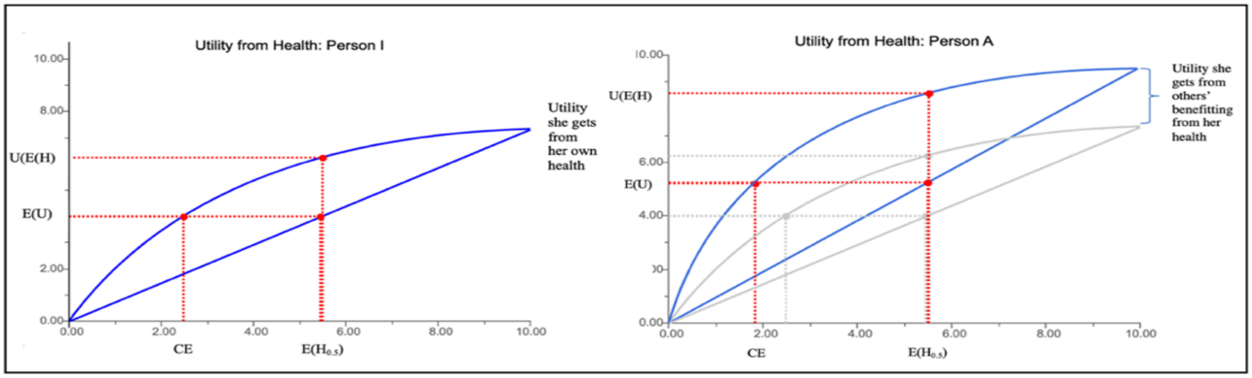

Figure 2:
The Utility Person I and Person A Derive From Their Own Health

act in the people's interest.

WHAT HAPPENS WHEN ALTRUISTIC DEMAND REDUCES MORAL HAZARD FROM INSURANCE?

An altruistic individual cares about others' health *per se* — that is, independently from her own. We can adapt an individual's utility curve to incorporate her valuation of others' health in addition to her own, which is particularly relevant when a contagious virus directly links one's own health with that of those around her. This adaptation would indicate that one's utility of health is heightened during a pandemic, if she is altruistic. While this heightening of utility of health is counterintuitive, it of course does not capture the full effect of a pandemic on individuals' well-being, which is undoubtedly a net negative effect. Nevertheless, we can imagine how an individual's health might hold more value to her in a pandemic, which is useful in understanding an individual's public health policy preferences.

Figure 2 shows the utility Person I and Person A derive from their own health. Person A represents an altruistic individual in a pandemic state. Person I represents Person A in a non-pandemic state, wherein her health does not tend to determine others' health, or an individual in a pandemic who does not derive additional utility from the fact that her good health now contributes to others'. If they both believe COVID-19 exposes them to a 50% chance of death — or that they can expect to lose 50% of their health if they are infected — they are each willing to sacrifice some amount of health to avoid the risk of infection.

We can think of this health they would sacrifice as physical and emotional comfort and the mental well-being that comes from free interaction with friends, family, and the public. In each person's case, the health they are willing to sacrifice is her expected health minus the certainty equivalent of that health level, or $E(H_{0.5})$ - CE. We can see this "risk premium" is larger for Person A than for Person I; Person A is willing to sacrifice more to avoid infection.

Person A has a higher marginal utility of health than Person I, and Person A's demand for health is therefore less responsive to price. That is, she is willing to give up more (e.g. sacrificing time spent working an in-person job or paying for delivery rather than going to the grocery store) to acquire an additional "unit" of health. Figure 3 shows the social loss that arises from moral hazard in each person's case, if we assume both of them have health insurance (Bhattacharya, Hyde, and Tu 2014, 207). Person A's decreased price sensitivity due to the increased value she places on her health tilts her demand curve vertically. The social loss from her increased risky behavior is therefore lessened.

In this way, it may be that altruistic demand mitigates the effects of moral hazard, partially explaining why states with higher insured populations are not more likely to lack mandates. If altruistic demand truly has this mitigating effect, it must be true that individuals in certain geographic regions are more altruistic than in other regions — or are at least more likely to believe or care that their health impacts others' health. While well-intentioned people can surely be found anywhere, cost-sharing policies may encourage pseudo-altruistic preferences in certain regions more than others. Pseudo-altruistic preferences could cause an individual's utility curve to look identical to that of an altruistic individual,

Figure 3

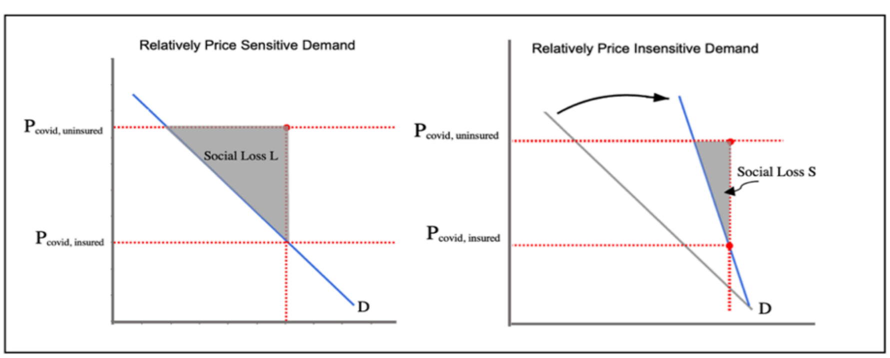

Figure 3:
Social Loss From Price Sensitive vs. Price Insensitive Demand for Health

despite different underlying motivations. Risk pooling by employer-sponsored insurance ensures that people in the plan care about each other's protection, as they will incur a financial cost if their co-workers are in poor health. According to the Kaiser Family Foundation (2020), states with higher insurance rates do tend to have, as expected, higher percentages of people in employer group insurance plans (see Figure 4). Figure 4 also shows that the percentage of people with employer group insurance correlates with the presence of government mandates to curb COVID-19 spread. Approached from this perspective, it seems support for behavioral regulations may be related to people's financial, contractual responsibility to others.

While an insured individual incurs a lesser cost if she becomes sick than if she did not have insurance, an outbreak among co-workers could be very costly to her if she has employer group insurance. Rather than insurance leading to moral hazard and decreased support for public health regulations, employer group insurance could then incentivize people to support mask mandates and business closures that keep themselves, the public, and therefore their co-workers healthy. Conversely, people's lack of financial responsibility to others' health could drive opposition to public health interventions.

Altruistic preferences lead to behavior that attenuates social loss, but pseudo-altruistic preferences that arise from financial obligation to others may have the same effect. This means that while altruism itself is effective in causing behavioral adjustments, policy is not impotent in encouraging these same adjustments.

Figure 4

State	% Population with Employer-Group Insurance*	Business Closures	Mask Mandate
Florida	40.3	open	no
Oklahoma	45.5	open	no
Georgia	48.9	open	no
Tennessee	47.8	open	no
Wyoming	51.1	open	no
South Dakota	51.5	open	no
Illinois	54.6	closed	yes
Indiana	53.3	open	yes
Michigan	50.9	closed	yes
Ohio	52.6	open	yes
Wisconsin	56.5	open	yes
Iowa	54.4	open	yes
Minnesota	57.8	closed	yes

*Source: Kaiser Family Foundation

Figure 4: Uninsured Population Rates, Percentage of Residents With Group Insurance, State Business Closures, and Mask Mandates

SOCIAL LOSS THROUGH UTILITY MAXIMIZATION ON LOWER BUDGET CONSTRAINTS

To an even greater extent in a pandemic, our behaviors affect the conditions in which others make decisions about allocating time. We can capture this interdependency of preferences and behaviors in a pandemic by creating a framework in which indifference curves are responsive to others'.

Figure 5

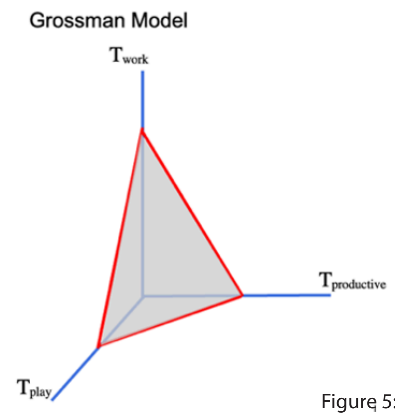

Figure 5: Time Constraints in the Original Grossman Model

We can understand this in the framework of the Grossman Model (see Figure 5), which shows us how people make tradeoffs between work, play, and productive time (time spent investing in health). In the model, health (meaning physical fitness, mental well-being, and lack of illness) is conceptualized as an input to utility, both direct and indirect, as well as an investment good used to generate future health, where the better-educated are assumed to be more efficient producers of health. According to the Grossman Model, if Person Y is a college-graduate, white-collar worker, she is a more efficient producer than a less educated worker in an essential service industry, Person Z (Bhattacharya, Hyde, and Tu 2014, 28–42). We can adapt the Grossman Model to inform our understanding of in-pandemic time tradeoffs by thinking of time spent at home during COVID-19 as time dedicated to the production of health. The Grossman Model operates on the assumption that relevant distinctions exist between our time spent working, playing, and producing health. In a pandemic, however, time spent at home, whether working or playing, is "productive time" — time wherein we are protecting our health, (and by the Grossman Model, producing future health) because we are limiting our exposure to the virus. Time spent in public, whether working or playing, is time when one's health may be at risk, and so it is not productive time, but "public time." The tradeoff is then between public time and productive time, and we can use the Grossman Model on two axes.

We can assume Person Y and Z's indifference curves are derived from identical utility functions; that is, they place the same value on their own health. Both individuals' work and play in public is restricted in the pandemic. When Person Y's public time is restricted, this limitation of her freedom of choice (inward tilt of her budget constraint) moves her optimal bundle to a lower indifference curve (see Figure 6). However, if the pandemic causes Person Y's preferences to change (perhaps she values her health more now than in non-pandemic conditions since she is altruistic and others' health is now affected by her own) the result will be a different optimal bundle.

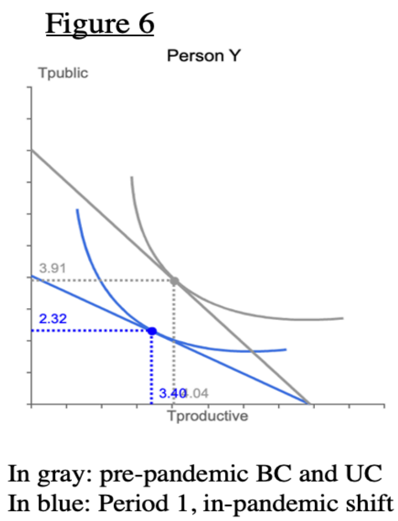

Figure 6:
A Non-Essential Worker's Utility Curve and Budget Constraint Shift due to Government Regulation

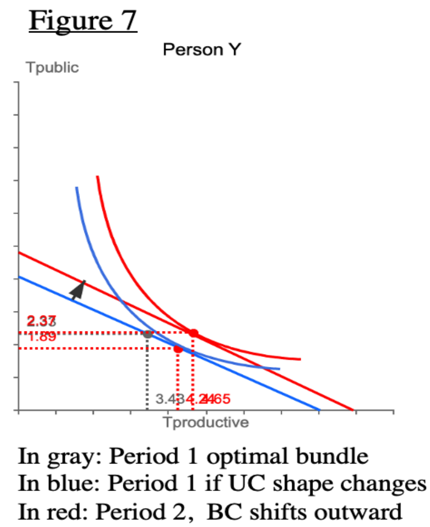

Figure 7:
A Non-Essential Worker's Vertically-Tilting Utility Curve and Shifting Budget Constraint in Pandemic Conditions

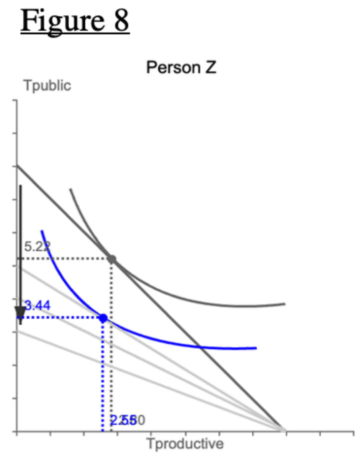

Figure 8:
An Essential Worker's Budget Constraint Responding to the Non-Essential Worker's Optimal Bundle

If her preferences do change in this altruistic way, her indifference curve will become more vertical; she values productive time even more than she did before since productive time improves/protects her health. In Figure 7, her optimal bundle without a vertical indifference curve tilt is shown in gray. We can imagine the indifference curve that would result in this optimal bundle as slightly more horizontal than the curve shown in blue. The blue optimal bundle shows that she will prefer more productive time and less public time if her indifference curve does tilt slightly vertically.

It is possible that this increase in productive time leads to an outward shift in one's budget constraint because, according to the Grossman Model, productive time leads to better health, and healthier individuals can spend more time at work and at play (Bhattacharya, Hyde, and Tu 2014, 39). This is shown by the red budget constraint, indifference curve, and optimal bundle in Figure 7. This outward shift of her budget constraint reflects that the constraints on her choice from forced reduction of public time can cause her budget constraint to slightly shift back out in the next period due to improved health. If altruism increases the amount by which she prefers productive time over public time, her budget constraint will shift out more in the next period. In this way, altruism mitigates an individual's welfare loss. If many individuals also change their preferences in this way, altruism helps mitigate social loss.

Whatever decision Person Y makes contributes to the conditions in which everyone else, including Person Z, makes decisions. The Grossman Model tells us that Person Z's indifference curve is, at all times, more horizontal than Person Y's because she is a less efficient producer of health and therefore prefers to spend less time producing health. The more productive time Person Y spends — that is, out of the public — the less Person Y contributes to the spread of the virus. The greater the number of individuals like Person Y preferring to spend time out of the public, the less Person Z feels she needs to limit her shifts in her essential work, and the less her budget constraint must pivot down, making her worse off (see Figure 8). In this way, the more Person Y's indifference curve changes, the less Person Z's budget constraint pivots downward (so long as there are many people like Person Y). Therefore, the shifting preferences of Person Y is essential to mitigating society's welfare loss. Because this shift in Person Y's indifference curve could be related to her altruistic demand, altruism may play a role in elevating society's welfare.

This implies that if an individual changes her preferences in a pandemic, she creates conditions in which others can trade off ways to spend their time along a higher budget constraint, decreasing their welfare loss without additional injury to herself. This additional injury would be harm beyond the welfare loss already imposed upon her by government behavioral regulations. Person Y cannot avoid the downward pivot of her budget constraint in Figure 6, which is caused by the government's restrictions of the ways she can spend public time. In preferring more health-productive time than she would in a non-pandemic state, thus changing the shape of her indifference curve to the blue curve in Figure 7, she remains on the same budget constraint. The extent to which she shifts her preferences in this altruistic way then determines how far down Person's Z's budget constraint pivots.

CONCLUSION

We have seen that altruistic preferences can shift a person's utility of health upward, decreasing the social loss caused by moral hazard from health insurance. Further, if she prefers even more productive time than she would on the same budget constraint in non-pandemic times, she creates more favorable conditions wherein others can make decisions. In this way, behavior that is reminiscent of altruism, if not attributable to altruistic intentions, does not necessarily shift welfare loss from some individuals unto others. Instead, it could mitigate society's total welfare loss in a pandemic state.

While it is possible that altruistic behavior does not harm the altruistic individual, this is not guaranteed. In fact, there are levels at which productive time depletes one's health. Studies have shown that the effects of isolation on mental health and one's likelihood to abuse substances are severe (Panchal, Kamal, and Cox 2020). If an additional hour of productive time reduces one's health — the loss of mental health outweighing the gain in physical health — then the altruistic person's preference shift toward more productive time in Period 1 (shown by the blue curve in Figure 7) will not lead to an outward shift in one's budget constraint in Period 2 due to improved health. Rather, it will lead to an inward shift. While an individual's preference to spend time in quarantine improves the conditions in which others make decisions, it could leave her worse off.

Though the models and their adaptations face limitations, they show that altruism is a relevant consideration both to explain behavior (how does moral hazard operate differently when our health directly impacts others' health?) and to inform policy aimed at changing behavior (can government encourage the same behavior that results from altruism?). The next question to ask is how we can strengthen our tattered commitment to public health to realize these societal gains.

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Universities and Economic Growth in the United States

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Abstract

As universities become increasingly large players in regional economies, it becomes more crucial to examine their economic impact. As a university student studying economics, my investigation is partially motivated by a personal interest in the relationship between higher education and the economy. This study examines whether or not a causal relationship exists between the establishment of a university and economic growth in a given region. I conducted a statistical analysis of the relationship between economic growth and the establishment of universities at the statewide level. Although I was unable to determine the existence of a causal relationship between universities and economic growth, the outcome of my investigation suggests that the two may be positively correlated. While I was not able to achieve as robust a result as I had hoped, my results prompt further investigation and hint at a relationship which requires more rigorous analysis to be considered conclusive.

Introduction

There has been considerable research performed on the economic impact of universities. Much of this research concentrates on the function of universities to increase a region's human capital, surmising that there is the potential for greater levels of productivity and technological development that contribute to economic growth (Bouzekri 2015). Studies have shown that a rise in the number of universities is also associated with higher levels of economic growth (Valero and Van Reenen 2019). The relationship between universities and economic growth is complex, with a number of variables that potentially skew results. The sheer number of variables that influence economic growth make a causal relationship difficult to define. In response to the research that has already been done on the subject, I study whether the presence of a university in a given region directly causes economic growth in that region.

I will begin by examining the existing research that has been done on the relationship between universities and economic growth. Next, I will describe the methods of my research, which include a survey analysis that relies on data from the Federal Reserve Bank of St. Louis (FRED). I will then note the results of my analysis and the limitations of my findings, and I will conclude with some suggestions for future work in the field.

Literature Review

It is widely accepted that higher education and the expansion of human capital has had a positive effect on economic growth. Although the relationship between human capital and economic growth has been studied extensively, the body of literature dedicated to the relationship that exists between universities and economic growth is relatively small. However, nearly all of the literature that is available on this subject supports a causal relationship between universities and economic growth.

A study estimating the contribution of Higher Education Institutions (HEIs) to economic growth in the European Union from 2000 to 2015 showed that colleges and universities contributed significantly to the EU's economic growth by directly increasing human capital endowments and the productive capacities of the population. Furthermore, the study showed that HEIs generate scientific and technological knowledge, which in turn increases technological capital in the economy and leads to direct increases in labor quality and economic growth as a whole (Pastor et al. 2018).

The results of Valero and Van Reenen (2019) reflect the positive relationship between university presence and economic growth that was shown to exist in the European Union by Pastor et al. (2018). The 2019 study implied that a 10% increase in a region's number of universities per capita is associated with 0.4% higher future GDP per capita in that region. Furthermore, the study showed that the effect of universities on growth is not simply driven by direct expenditures of the university, its staff, and its students, but through an increased supply of human capital and greater innovation. Both studies suggest that the existence of universities may cause economic growth. Furthermore, they both indicate that the economic growth prompted by universities is not merely a factor of direct spending by the university and its constituents but rather a factor of the expansion of human capital and technological knowledge.

Valero and Van Reenen (2019) demonstrates the positive effect of universities on growth at the global level, while Pastor et al. (2018) demonstrate this effect at the continental level. Both studies support a positive relationship. There is also evidence for an association at the regional level, as demonstrated by a series of case studies which I describe in the section entitled "Case Studies Supporting Causality" later in the paper to supplement my findings. Valero and Van Reenen (2019) and Pastor et al. (2018) also both argue that human capital and technological innovation, not just capital investment, are the drivers of university-related economic growth. There are many mechanisms through which a university may affect economic growth, including but not limited to human capital creation, technological innovation, and capital investment. The question of whether universities cause economic growth has been asked many times in the previous literature, but generally, the direct economic impacts of universities have been evaluated at the national, continental, or global level.

In contrast to previous research, my investigation concentrates on the relationship between universities and economic growth at the state and city-wide level. I hope to contribute a new perspective by attempting to replicate a causal relationship within multiple US cities. I cover elements that have not been examined in previous studies, including the impacts of non-traditional universities and universities established after 1970.

Hypothesis Development

My primary research question is whether universities cause economic growth in the regions where they are located. Based on the current body of literature, my hypothesis is that there is a positive causal relationship between the presence of a university and subsequent economic growth in a given region. My hypothesis does not argue that any specific aspect of a university causes economic growth, only that there is a causal relationship between a university's presence (which may include factors like human capital expansion or physical capital investment) and economic growth in the surrounding region.

Before introducing my research methods, I lay out two key definitions. First, I define a university as any institution of higher education. I do not differentiate between institutions that provide specified degrees. I also do not differentiate two-year colleges from four-year colleges. For my definition, there are two requirements for an institution to qualify as a "university." First, it must be accredited. This is to ensure that the institutions I use in my analysis are legitimate and to ensure that the university is capable of providing tangible evidence that its graduates have acquired skills — a representation of the expansion of their human capital, which may contribute to economic growth in the surrounding region. Second, it must only offer degrees to students who have completed a secondary education. I do not include high schools or primary schools in my analysis because they do not have levels of capital, human capital, or technological investment comparable to those of tertiary learning institutions. Second, I will define what I mean by "economic growth." In my statistical analysis, I define economic growth as an increase in the variable referred to as "per capita personal income" (PCPI) in the Federal Reserve Bank of St. Louis (FRED) dataset. In my analysis, I surmise that if the PCPI of a given city is higher than the statewide average PCPI, that city has experienced higher-than-average levels of economic growth.

Alternative Explanations for Causality

There are several alternative explanations for economic growth in university towns that affect the implications of my analysis. First, it is impossible to hold constant all factors which might cause economic growth in a given region, which means that any economic growth I discover cannot definitively be said to be a result of the establishment of a university. To illustrate this alternative, we can refer to a hypothetical scenario. Suppose that Southampton and Westhampton are two non-identical towns with the same GDP per capita. Assume an absence of spillover effects. In 2005, a university is founded in Southampton. GDP per capita is measured in Southampton and Westhampton in 2005 and 2010, and in 2010 it is revealed that Southampton has a higher GDP per capita. In this example, where all non-university economic influences cannot be held constant, the fact that Southampton has a higher GDP per capita does not necessarily mean that the university was the cause of that economic growth. We cannot say that there is a causal relationship between the university and a region's economic growth because there are a number of other factors that are almost guaranteed to be influencing GDP per capita at the same time. For example, in 2006, huge oil deposits may be discovered in Southampton that cause an increase in capital investment and jobs in the local drilling industry and subsequently, an increase in GDP per capita.

Due to the sheer number of confounding variables (e.g. employment, recent investments, human capital expansion, and industry growth) affecting economic growth in any given region that I do not have data for, it is difficult to prove that a causal relationship exists between universities and economic growth because I cannot hold all else constant. Although the existing body of research provides substantial evidence to suggest that a causal relationship does exist between universities and economic growth, there is also some evidence that opposes direct causality. As proposed by Valero and Van Reenen in 2019, universities may affect growth in a more "mechanical way," where confounding variables are involved. In an article on the various factors affecting economic development and growth, Jim Woodruff emphasizes investment in physical capital as a driver of economic growth as opposed to the presence of universities (Woodruff 2019). While physical capital including factories, infrastructure, and machinery may come with the development of universities, universities are not the sole contributor to the economic growth in a given region. Therefore, causality is difficult to establish.

Another potential alternative is the possibility that a reverse causal relationship exists between the presence of universities and economic growth rather than the positive causal relationship I hypothesize. Referring to the hypothetical Southampton/Westhampton example (where the two towns are not identical), it is possible that Southampton was only able to fund and construct a university because it was already in a phase of economic growth. In other words, universities may tend to appear in regions whose economies are already growing, and that economic growth is a precursor for — rather than a byproduct of — the establishment of universities. In the following section, I acknowledge both of these alternative explanations and the ways in which they may have impacted my results.

Statistical Analysis

Methods

Since the majority of US universities were founded before 1950, I searched for data that was as close to 1950 as possible as well as data that displayed the evolution of an indicator of economic growth over time. This indicator ended up being per capita personal income growth (PCPI). PCPI data is available through FRED St. Louis from 1970 onwards and covers cities as well as states, allowing me to gain even more specific insight than I had previously anticipated. Through FRED, I was able to find comprehensive data on the evolution of per capita personal income from 1970 onwards. Originally, I chose to focus on Pennsylvania because it has a balance of rural, urban, high-income, and low-income areas as well as a wide range of different types of universities (i.e. two-year versus four-year). Although I was not able to find enough data on Pennsylvania by itself, I was able to identify universities around the country such that my data points reflected a balance between rural, urban, high-income, and low-income areas as well as a variety of university types.

The first step in my process is to develop a list of universities to observe. Using the Wikimedia Commons, I list the universities founded in the United States between 1970 and 1980. I choose to restrict my analysis to this ten-year period not only for efficiency and focus, but also because the number of universities founded in the United States declines sharply after 1980. As a result, there are very few data points available after this time period. I generate a list of 66 universities which fit my criteria.

Next, I create data tables which show the name of each university, the year it was founded, and the city and state in which it was founded. To facilitate my quantitative analysis, each observation in the table includes the PCPI in a given city the year a university was founded (measured in 2020 USD), the PCPI in a given city five years after the university was founded (measured in 2020 USD), and the rate at which PCPI in a given city and state had increased in the 5 years since the university was established. The following section includes a list of the variables I used in my analysis restated in more detail as well as a summary of the steps I used in my analysis. (Note: "economic growth" refers to an increase in PCPI, and PCPI is taken on January 1st of the corresponding year from the FRED database. PCPI growth rates i.e. PCPI_state_5yr and 5-year GR were calculated using FRED data and the formula below.)

VARIABLES

NAME = The name of the university

EST = The year the university was established

CITY = The city in which the university was established

STATE = The state in which the state was established

PCPI_est = The per capita personal income in a given city the year the college, university, or other higher learning institution was founded (measured in 2020 USD)

PCPI_est_5y = The per capita personal income in a given city five years after the college, university, or other higher learning institution was founded (measured in 2020 USD)

5-year GR = The rate at which per capita personal income in a given city has increased in the five years since the university was established, or: [(PCPI_est_5y - PCPI_est) / PCPI_est] + 1. Rounded to two decimal places.

PCPI_state_5yr = The rate at which per capita personal income in the corresponding state has increased in the five years since the university was established. For example, if we are looking at Binghamton University (est. 1970 in NY), PCPI_state_5yr = the NY average PCPI in 1975, minus the NY average PCPI in 1970, divided by the NY average PCPI in 1970, plus one. Rounded to two decimal places.

If 5-year GR > PCPI_state_5yr, then a given city's PCPI is greater with the establishment of a university than the statewide average, which may indicate that the university has contributed to the economic growth in the city where the university is located.

STEPS OF ANALYSIS

After recording the names of the universities and PCPI for each corresponding city, I calculated growth rates for each observation and corresponding state. Thereafter, I compared the baseline rate (that of the state) to the citywide growth rate to see if the city's PCPI grew faster than the statewide average five years after the university was founded.

Note: charts displaying results are present on the following two pages.

RESULTS

NAME	EST	CITY	STATE	PCPI_est	PCPI_est_5y	5-year GR	PCPI_state_5yr	5-year GR > PCPI_state_5yr?
Bingampton University School of M	1970	Binghampton	NY	4077	5890	1.31	1.43	N
A. Gary Anderson Graduate School	1970	Riverside	CA	4294	6739	1.56	1.47	Y
Bainbridge State College	1970	Bainbridge	GA	2349	4021	1.71	1.51	Y
Francis Marion University	1970	Florence	SC	2878	4587	1.59	1.54	Y
Georgetown University School of C	1970	Washington, DC	MD	5806	8725	1.5	1.49	Y
Germanna Community College	1970	Fredericksburg	VA	4162	6030	1.44	1.54	N
Indiana University – Purdue Univer	1970	Columbus	IN	4184	6258	1.49	Missing	N/A

NAME	EST	CITY	STATE	PCPI_est	PCPI_est_5y	5-year GR	PCPI_state_5yr	5-year GR > PCPI_state_5yr?
Amberton University	1971	Garland	TX	4670	7607	1.63	1.57	Y
Belmont College	1971	St. Clairsville	OH	4378	6619	1.51	1.48	Y

NAME	EST	CITY	STATE	PCPI_est	PCPI_est_5y	5-year GR	PCPI_state_5yr	5-year GR > PCPI_state_5yr?
Aiken Technical College	1972	Graniteville	SC	4243	6444	1.52	1.55	N
Asnuntuck Community College	1972	Enfield	CT	5503	8409	1.53	1.51	Y
Baltimore International College	1972	Baltimore	MD	4681	7009	1.49	1.49	No difference
Black River Technical College	1972	Pocahontas	AK	3504	5736	1.63	1.64	N
Burlington College	1972	Burlington	VT	4294	6339	1.48	1.48	No difference
Coggin College of Business	1972	Jacksonville	FL	4935	7338	1.49	1.48	Y
Hamline University School of Law	1972	St. Paul	MN	4714	7612	1.61	1.61	No difference
Five Towns College	1972	Suffolk	NY	5186	7557	1.46	1.46	No difference
University of Florida College of De	1972	Gainesville	FL	4650	7123	1.53	1.48	Y
Gateway Technical College	1972	Kenosha	WI	5088	7597	1.49	1.61	N

NAME	EST	CITY	STATE	PCPI_est	PCPI_est_5y	5-year GR	PCPI_state_5yr	5-year GR > PCPI_state_5yr?
California Coast University	1973	Santa Ana	CA	6234	10488	1.68	1.58	Y
Cerro Coso Community College	1973	Ridgecrest	CA	5860	9451	1.61	1.58	Y
Bunker Hill Community College	1973	Boston	MA	5867	8842	1.51	1.51	No difference
Empire College School of Law	1973	Santa Rosa	CA	5760	9467	1.64	1.58	Y

NAME	EST	CITY	STATE	PCPI_est	PCPI_est_5y	5-year GR	PCPI_state_5yr	5-year GR > PCPI_state_5yr?
Atlanta Metropolitan State College	1974	Atlanta	GA	5373	8282	1.54	1.54	No difference
Berkeley City College	1974	Berkeley	CA	7710	12470	1.62	1.59	Y
Boricua College	1974	New York	NY	6533	9831	1.5	1.5	No difference
Briarcliffe College	1974	Lynbrook	NY	8406	13179	1.57	1.5	Y
Cambridge School of Culinary Arts	1974	Cambridge	MA	9852	6352	1.57	1.55	Y
Frontier Community College	1974	Fairfield	IL	6891	10647	1.54	1.56	N
University of Houston Downtown	1974	Houston	TX	6183	10800	1.75	1.68	Y

NAME	EST	CITY	STATE	PCPI_est	PCPI_est_5y	5-year GR	PCPI_state_5yr	5-year GR > PCPI_state_5yr?
Christian Academy of Louisville	1975	English Station	KY	5877	9526	1.62	1.69	N
Hallmark Institute of Photography	1975	Turners Falls	MA	5539	9067	1.64	1.63	Y
Antioch University Seattle	1975	Seattle	WA	7190	12267	1.71	1.63	Y
Heritage College of Osteopathic Me	1975	Athens	OH	4457	6883	1.54	1.62	N

NAME	EST	CITY	STATE	PCPI_est	PCPI_est_5y	5-year GR	PCPI_state_5yr	5-year GR > PCPI_state_5yr?
Boston Baptist College	1976	Boston	MA	7345	12421	1.69	1.68	Y
Boston University School of Public	1976	Boston	MA	7345	12421	1.69	1.68	Y
Benjamin N. Cardozo School of La	1976	New York	NY	7510	12277	1.63	1.63	No difference
California Pacific University	1976	Pinole	CA	8998	15276	1.7	1.65	Y
Calvary Baptist Theological Semin	1976	Lansdale	PA	6770	10901	1.62	1.61	Y
Carrington College	1976	Sacramento	CA	7780	12077	1.55	1.65	N
Concordia University Irvine	1976	Irvine	CA	8199	14714	1.79	1.65	Y
Martin Community College	1976	Williamston	NC	5414	9049	1.67	1.62	Y
Magnolia Bible College	1976	Kosciusko	MI	6546	9829	1.5	1.64	N
Levine School of Music	1976	Washington, DC	MD	9445	15001	1.59	1.61	N

NAME	EST	CITY	STATE	PCPI_est	PCPI_est_5y	5-year GR	PCPI_state_5yr	5-year GR > PCPI_state_5yr?
Antioch University Santa Barbara	1977	Santa Barbara	CA	8896	14856	1.67	1.59	Y
Broadview University	1977	Layton	UT	6412	9669	1.51	1.51	No difference
California Culinary Academy	1977	San Francisco	CA	10072	16524	1.64	1.59	Y
California Miramar University	1977	San Diego	CA	8412	13573	1.61	1.59	Y
Christendom College	1977	Front Royal	VA	6274	10534	1.68	1.65	Y
Columbia Gorge Community Colle	1977	The Dalles	OR	7637	11558	1.51	1.46	Y
Fisher School of Accounting	1977	Gainesville	FL	6543	11054	1.69	1.66	Y
Georgia Medical Institute	1977	Atlanta	GA	7435	11848	1.59	1.6	N
Martin University	1977	Indianapolis	IN	7749	11633	1.5	Missing	N/A
Lexington College	1977	Chicago	IL	8825	13613	1.54	1.54	No difference
Lake Dow Christian Academy	1977	McDonough	GA	4484	7055	1.57	1.6	N
Keiser University	1977	Fort Lauderdale	FL	7633	11997	1.57	1.66	N

NAME	EST	CITY	STATE	PCPI_est	PCPI_est_5y	5-year GR	PCPI_state_5yr	5-year GR > PCPI_state_5yr?
American College of Healthcare Sc	1978	Portland	OR	9840	13550	1.38	1.36	Y
Bastyr University	1978	Kenmore	WA	10627	16262	1.53	1.49	Y
California College San Diego	1978	San Diego	CA	10272	14320	1.39	1.36	Y
Collins College	1978	Phoenix	AZ	4693	8313	1.56	1.54	Y
Cuyamaca College	1978	San Diego	CA	10272	14320	1.39	1.36	Y
Independence University	1978	Salt Lake City	UT	7906	11334	1.43	1.42	Y

NAME	EST	CITY	STATE	PCPI_est	PCPI_est_5y	5-year GR	PCPI_state_5yr	5-year GR > PCPI_state_5yr?
Las Vegas College	1979	Henderson	NV	9590	13377	1.39	1.38	Y

NAME	EST	CITY	STATE	PCPI_est	PCPI_est_5y	5-year GR	PCPI_state_5yr	5-year GR > PCPI_state_5yr?
Crossroads Bible College	1980	Indianapolis	IN	9435	13664	1.45	Missing	N/A
Briarcliffe College Patchogue	1980	Patchogue	NY	14830	23138	1.56	1.53	Y
Pacific International University	1980	Springfield	MO	12770	8740	1.46	1.49	N

EXPLANATION AND INTERPRETATION OF RESULTS

My results are displayed in the tables above. The main question of this analysis was whether or not a city achieved a rate of economic growth that was higher than the statewide average over the five years since the university was established. In the five years since a university was established, 38 out of 66 cities surveyed experienced a rate of economic growth that was higher than the statewide average, 15 out of 66 cities experienced a growth rate that was lower than the statewide average, 10 out of 66 cities experienced a growth rate that was equal to the statewide average, and data was not available for 3 out of 66 cities. Overall, 57% of cities with universities experienced economic growth that was higher than the statewide average PCPI increase (using PCPI as a proxy for economic growth). Although this heightened growth rate occurred in cities with universities, there is not enough evidence to support the hypothesis that the economic growth was directly related to their establishment. Furthermore, due to my limited sample size, variables, and time horizon, I cannot make any definitive conclusions about the relationship between universities and economic growth.

Although my results cannot conclusively prove or disprove causality, they show a positive correlation between economic growth and university presence in the cities observed, which loosely supports the consensus in the literature that a positive causal relationship exists. Over half of the cities I surveyed experienced above-average growth rates five years after a university was founded within their boundaries. This tells us that after the establishment of a university, a given city's PCPI grew faster than the PCPI in the rest of its state more often than not. While this does not indicate a causal relationship between university presence and a region's economic growth, it contributes to a broader conversation and prompts further investigations into a potential causal relationship.

DISCUSSION OF METHODS — LIMITATIONS, CONFOUNDING VARIABLES, INCONCLUSIVITY

During the analysis process, I ran into a number of limitations and problems which prevented me from concluding causality. The first limitation I will address is my time horizon. Although the majority of universities were founded before 1950 in the United States, the only economic data that I could find was PCPI from 1970 onward. Certain counties only had data from 2010 onward, and others had no data at all. This restricted my time horizon significantly. As a result of my data restrictions, I also had to restrict the number of universities I observed to those that were founded in 1970 and after, which reduced my sample size. Upon looking at the data, I realized that very few universities were founded after 1980 and subsequently had to restrict my time horizon to the 1970-1980 decade.

The data limitations I faced meant that I had to significantly reduce the scope of my analysis, so I cannot definitively claim that my conclusions are reflective of the broader national population. The only claim I can make based on my results is that there was a positive correlation (not causation) between universities and economic growth in 38 out of 66 cities. Based on previous research, I would theorize that this growth is caused by a combination of human capital expansion and capital investment. While this does not achieve my goal of proving a causal relationship between universities and economic growth, it supports the consensus that universities and economic growth may be positively correlated and suggests that this may also be true of smaller, newer, and local universities. Further analysis might compare the effects of local "hometown" colleges versus larger, more established institutions. I have not examined this in my analysis, but in a potential future study, I would hypothesize that hometown colleges have a greater impact on economic growth due to the higher likelihood of its graduates seeking employment in their hometowns and contributing to local human capital expansion. With a larger sample size, time horizon, and data on variables like employment, human capital expansion, local industry developments, and local human capital index, I may be able to attain a more general understanding of the university-growth relationship and confirm whether the positive correlation I have identified exists on a larger scale.

Another flaw that prevents me from establishing causality is the sheer number of factors that influence PCPI — population size, regional growth trends, local industry trends, and employment. These factors are likely to affect economic growth while being unrelated to university presence. For example, an influx of corporate investment may occur in a given city and result in economic growth regardless of whether that city hosts a university. One of the key reasons I was unable to replicate previous research results and demonstrate causality is because I did not have data on a number of variables (employment, human capital expansion, local capital investment, etc.) that previous studies have been able to take into account. Although my results do not support my hypothesis of a causal relationship between universities and economic growth, they do support a positive association in the cities that I surveyed. I hypothesize that if this simple study were repeated with a larger sample size and with data on factors like employment, consumption, and investment, economic growth would be higher than the statewide average in cities with universities more often than not although there are additional validity concerns associated with my comparison of statewide and citywide growth rates.

Although my research supports a positive correlation between economic growth and universities, there is reason to believe that this correlation is not particularly significant. The comparison between citywide and statewide growth rates presents a problem. There are many plausible explanations for a city's PCPI increasing faster than the statewide average that have nothing to do with the presence of a university. For example, cities may experience faster economic growth rates than states at baseline due to the fact that cities serve as hubs of investment, employment, and consumption. For this reason, they may plausibly tend to grow faster than states, whose average economic growth may be slowed by low-growth, low-population regions. Since cities may already experience higher growth rates than states, comparing a citywide growth rate to a statewide growth rate may not be a valid way to support the claim that economic growth in a city is above average. It is possible that the city would have been growing faster regardless of whether a university was present. Overall, variation in the existing growth tendencies of cities is a factor that may have skewed my results.

Furthermore, while I do not have evidence to show that cities consistently grow faster than states, it is plausible that some cities are simply faster-growing than others, regardless of whether a university is present. Some cities are faster-growing than others for reasons outside the scope of this paper. It is entirely possible that a high-income, high-growth city like San Diego was already growing faster than the California statewide average without the establishment of a university. On the other hand, it is equally possible that a low-income, low-growth city like St. Clairsville, Ohio would experience lower growth than the state average even with the establishment of a university. In both of these cases, the determining factor of economic growth could plausibly be something outside the scope of my analysis, which is one of the reasons I cannot claim causality.

CONCLUSION

Based on my results, the establishment of a university cannot be said to have a direct causal relationship with economic growth. However, my results hint at a positive relationship between university establishment and economic growth that various impact studies have suggested. Although I was unable to conclusively replicate these results, I introduced questions that may not have been addressed in previous research such as the impact of universities in fast-growing cities versus slow-growing cities, the role of two-year or other non-traditional universities, the impacts of universities in citywide economies, and the impacts of universities founded after 1970. As the economic role of universities continues to expand, these questions will become increasingly relevant and warrant further research. Further investigations could investigate the roles of two-year versus four-year colleges by using more thorough data and taking variables like employment and human capital index into account. Future analyses should endeavor to control for any confounding variables, e.g. university spending and regional population. By using more rigorous methods like regional input-output modeling and more comprehensive data, it may be possible to suggest a causal relationship between universities and economic growth for newer universities as well as established ones.

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