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Does Precise Case Information Limit Precautionary Behavior? Evidence from COVID-19 in Singapore

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Abstract

Limiting the spread of contagious diseases can involve both government-managed and voluntary efforts. Governments have a number of policy options beyond direct intervention that can shape individuals' responses to a pandemic and its associated costs. During its first wave of COVID-19 cases, Singapore was among a few countries that attempted to adjust behavior through the public provision of detailed case information. Singapore's Ministry of Health maintained and shared precise, daily information detailing local travel behavior and residences of COVID-19 cases. We use this transparency policy along with device-level cellphone data to quantify how local and national COVID-19 case announcements trigger differential behavioral changes. We find evidence that individuals are three times more responsive to outbreaks in granularly defined locales. Conditional on keeping infection rates at a manageable level, the results suggest economic value in this type of transparency by mitigating precautionary activity reductions.

Keywords: COVID-19, Transparency, Precautionary behavior

JEL Codes: H12, I18, R50

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1 Introduction

In the first wave of Singapore's COVID-19 infections, the country relied on a strategy, near unique among government responses, to mitigate the disease's spread. Rather than implement shelter-in-place orders or enforce business closure, the strategy entailed isolating potential patients, monitoring those they recently contacted, and sharing detailed data on confirmed cases, including their residence and places they visited. The motivation for this final piece was both to encourage those who potentially contacted cases to seek testing and also to induce the potentially affected into more cautious behavior while mitigating the impact on regular activity elsewhere.

In this paper we study the efficacy of Singapore's transparency with case information in achieving the goal of localizing voluntary activity reductions around detected cases. To address this question we take advantage of localized case announcements in combination with device-level cellphone location data for more than 10% of Singapore's population to track several movement responses to positive cases. We study both inflow patterns to and outflow from areas in which positive cases live or visited. We consider both as an individuals' reluctance to leave their residence could capture their fear about transmitting the disease while fear of traveling to infected locations reflects infection risk. As our principal objective is to document the magnitude of these behavioral changes, our reduced form approach is agnostic to how individuals formulate or perceive these risks.

Our results are consistent across different outcomes — including travel distance or the likelihood of staying home — that people are significantly more responsive on the margin to local case, both those near their homes and the places they visit. We find that an additional COVID-19 case in an individual's home census area decreases her daily travel distance on the following day by 89 meters (0.64% compared to the average) on average while a non-local case reduces travel by 28 meters (0.2%).[1] Further, a local case increases the probability of staying home on the following day by 0.14 percentage points (0.54%) while we do not observe changes in response to non-local cases. These adjustments hold across different activity types and are not specific to shopping, commercial, or visiting other residences. Our second set of results show that local cases reduce inflow travel as well. On average, an additional case reduces the probability of entering that area by 0.34 percentage points (5.09%).

[1]The most narrowly defined census area we use is a geography with an average population of 15 thousand residents, as of 2015 estimates.

To provide context to our estimates, we use the results to explore a simple counterfactual in which Singapore does not offer this detailed case information. We emphasize that we are not attempting to link the counterfactual to changes in transmission risk but rather pin down movement responses to the information. Using our estimates we argue a conservative bound in which individuals might aditionally change their travel under this alternative policy. In the best case scenario for local travel, in which individuals underestimate their self-assessed risk of infection, we find daily travel increases on average by less than half a kilometer, 3% compared to a baseline taken at the end of our study period. In the worst case scenario, in which individuals overestimate their risk, daily travel decreases on average by more than 3 kilometers (-20%).

In the microeconomic literature we contribute to papers that analyze behavioral responses to the COVID-19 epidemic and related governmental interventions using cellphone data (Abouk and Heydari, 2020; Allcott et al., 2020; Andersen, 2020; Barrios and Hochberg, 2020; Borg et al., 2020; Brzezinski et al., 2020; Courtemanche et al., 2020; Dave et al., 2020b; Engle et al., 2020; Fan et al., 2020; Farboodi et al., 2020; Gao et al., 2020; Gupta et al., 2020; Nguyen et al., 2020 Painter and Qiu, 2020; Siedner et al., 2020; Tucker and Yu, 2020) in the United States. Besides the location of our study, our research differs in two dimensions. First, our cellphone data are not aggregated on any geographical level. Hence, we can identify individual-level changes in response to case announcements. Second, and key to our question, as Singapore releases residence and local travel records for COVID-19 cases, we can evaluate an individual's localized responses rather than information shared at levels that may have virtually no impact on her risk assessment. Chen et al. (2020), Harris (2020), and Almagro and Orane-Hutchinson (2020) consider COVID-19 infection exposure within cities; each uses zip-code level data of infections in New York City. The authors do not study behavioral responses to the virus but rather evaluate the determinants of the virus's spread. Argente et al. (2020) utilizes an approach closest to ours. The authors study the South Korean case disclosure policy, which is similar to Singapore's. They analyze the flows of individuals across neighborhoods in Seoul using aggregated cellphone data and incorporate their results in an SIR model where virus spread is related to these flows. The authors conclude that the disclosure policy lowered the number of infections. Our approach differs as we do not model the virus spread; instead we shed light on the causal linkage between movement responses and local and non-local cases.

We also add to the academic and public policy discussion on COVID-19-related nonpharmaceutical interventions.[2] Strict governmental policies such as shelter-in-place orders, non-essential business closings, and school closure reduce travel activity and the spread of a virus (Dave et al., 2020c). However, they come with economic costs such as unemployment (Baek et al., 2020; Couch et al., 2020; Kim et al., 2020), educational costs (Doyle, 2020), health costs such as lower preventive and emergency medical care (Lazzerini et al., 2020), and psychological costs (Galea et al., 2020; Hsing et al., 2020). In comparison, voluntary travel reductions, even in the absence of strong governmental intervention, suffices to reduce the spread of COVID-19 while potentially limiting economic effects. Dave et al. (2020a) exploit a natural experiment in Wisconsin where the State Supreme Court lifted a state-wide shelter-in-place order and find no evidence that the repeal of the lockdown impacted social distancing or COVID-19 cases. While our paper does not compare governmental interventions directly, we first show that there are behavioral responses in the absence of strict policies and that it is highly dependent on the nature of information shared.

The macroeconomic literature has contributed several theoretical frameworks to link various infection mitigation policies to aggregate welfare outcomes. Alvarez et al., 2020, Chari et al., 2020, and Acemoglu et al., 2020 evaluate the welfare impact of dynamic lockdown policies and policies targeted toward containing infections or targeting subpopulations of different risk, respectively. Chudik et al., 2020 uses data on infection and recovery rates, along with varied policies implemented at the Chinese provincial level, to assess the economic and epidemic impact of voluntary and mandatory measures. Empirical research weighing these alternative lockdown versus voluntary policies, however, require sufficient understanding of voluntary responses to different information regimes governments could implement. We believe our research is a valuable step in estimating this behavior. On the optimal level of activity reduction, Hall et al., 2020 provides an estimate of the maximal consumption a planner would be willing to give up to reduce infections, while Eichenbaum et al., 2020 explores the decentralization of such a policy. A fundamental problem the latter research identifies is that individuals' voluntary responses will, by nature, not internalize externalities their behavior imposes vis-à-vis infection risk.

The paper is organized as follows. In Section 2 we present background on Singapore's disclosure

[2] There is also a literature on governmental policies in response to pandemic influenza. See, for example, Blendon et al. (2008), Fineberg (2014), or Vaughan and Tinker (2009). However, those pandemics differ in severity, infection symptoms, etc.

policy during the first wave of infections. Section 3 describes our data. In Section 4 we introduce our empirical strategy, and results are presented in Section 5. Finally we discuss the results in Section 6.

2 Institutional Background

A key element of our analysis is the quality of the information Singapore released on COVID-19 cases. The only other country to match this Singapore's disclosure was South Korea, though their first wave of infections was of a much larger scope than Singapore's. Following the global spread of the pandemic, travelers returning to Singapore accelerated new case counts following mid March. After this point Singapore only provided daily aggregate case numbers and eventually introduced a lockdown policy.

Singapore detected its first COVID-19 case on January 23rd. Along with the public announcement, the Ministry of Health (MoH) indicated the travel history of this case — a visitor from Wuhan, China — its intention to start contact tracing, and other cases pending confirmation. Additionally, the report indicated the locations this patient had visited while in Singapore. Following the first cases the government made case announcements late in the evening. They continued to provide detailed reports through the first wave of infections, which we define as ending around March 17th. A sample of the location data provided for an early cluster born from a Chinese tour group follows:

> Besides Yong Thai Hang (24 Cavan Road) and Diamond Industries Jewellery Company (Harbour Drive), the tour group also visited Meeting You Restaurant (14 Hamilton Road), Royal Dragon Restaurant (2 Havelock Road), T Galleria by DFS (25 Scotts Road) and D'Resort @ Downtown East (1 Pasir Ris Close).[3]

While the Singaporean government disclosed the information to encourage people to come forward for tracing and testing, few official recommendations or restrictions limited standard movement from "life as normal". This is a strategy the Singaporean government also uses for commu-

[3] From the February 5th MoH press release. The full text of the press release can be found at https://www.moh.gov.sg/news-highlights/details/four-more-confirmed-cases-of-novel-coronavirus-infection-in-singapore.

nicating and managing the risk of dengue and zika infections[4]. For COVID-19 the first significant policy announcement was moving the Disease Outbreak Response System Condition (DORSCON) to Orange on February 7th following several days of community transmission. Singapore uses this system to coordinate its policies in a health crisis and communicate the severity and possibility of spread within the community. While the announcement led to a brief run on supermarkets, few official movement restrictions immediately followed. The effect of the announcement was to introduce temperature stations at most public locations as well as requiring checks for travel to global hotspots. The first legal movement restrictions were stay-at-home notices issued to any travelers from China on February 17th.

This relatively lax regime persisted through mid March; only on March 13th did the government mandate social distancing measures. We use this policy history to emphasize that most changes in domestic travel behavior through mid-March should be attributed to voluntary activity reductions. While businesses voluntarily started split-team work arrangements no later than February 17th, businesses did not widely implement gathering restrictions for customers.

3 Data

We draw on two principal data sources for our analysis. We obtain coronavirus case information via daily announcements from the Singaporen MoH, and the marketing company Lifesight provided the cellphone location data.

3.1 Coronavirus Data

In Section 2 we discussed Singapore's disclosure of key details for each COVID-19 case. Announcements included a list of the new cases confirmed in the previous day. On or within a day, the announcements would provide additional information about these cases including an approximation of their home area — typically the street block — locations visited, and linkages to previously announced cases.[5]

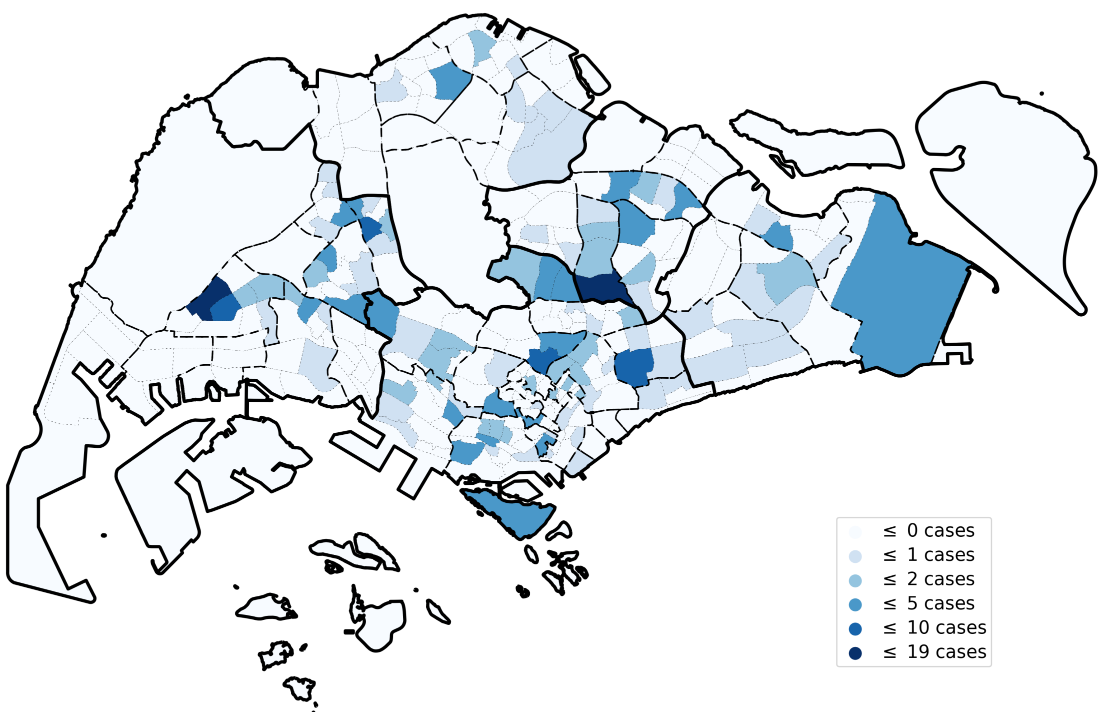
[4] See, for example, near-building-level information provided for dengue fever cases https://www.nea.gov.sg/dengue-zika/dengue/dengue-clusters.

[5] While possible to geocode the data provided by the MoH, we take advantage of a site (https://sgwuhan.xose.net) put together by computer programmer Ottokyu that mapped the reported cases and their linkages.

In our analysis we group cases into census-defined geographies that partition Singapore. Figure 1 illustrates the cumulative number of cases across the smallest of these geographies through March 17th, which again we loosely refer to as the "first wave" of infections. A case is linked to an area if the government announces the home, or hotel for a traveler, falls within that location. While the government shared information on 250 cases during this period, the figure illustrates the geographic dispersion of cases from the commercial and high income south-center of the island to the industrial areas and hinterlands in the east, north, and west.

We use two successively larger census areas to disentangle the impact of highly local and more distant cases on invididividual travel and activity behavior. The first are planning areas, denoted by dashed lines in Figure 1; we later call these areas "subregions" for clarity. There are 55 subregions across Singapore. They can be further aggregated into five large regions, delineated by thick solid lines in the same figure.

Figure 1: **Total Cases Across Singapore, through 17 March 2020**



Notes: Solid lines demarcate the five regions of Singapore. Dashed lines denote planning areas, or subregions. The smallest units are subzones.

For each case we identified three potentially important dates for people to respond to the information in the case briefing: the date of a case's confirmation, of announcement, and when the MoH provided final details on issues like home residence. Given that our research agenda asks how people respond to information, we focused on the announcement and information dates. Our analysis in this paper uses the information dates, though results do not qualitatively change with the alternative measurement; for the rest of the paper we call this date for information disclosure the "announcement of the case."

3.2 Cellphone Data

The marketing company Lifesight provided our principal data on individual behavior. This dataset contains granular location information for individuals over long time periods by tracking pings from specific cellphones. Each observation in this main dataset is an individual ID, unique to the phone; a timestamp; and a longitude-latitude coordinate for that person at that time. Supplemental data provide home location estimates for individuals in the dataset.[6] These data cover the time period January to March of 2020 and parts of 2019.

In our empirical analysis we assume that for each individual data are representative of, if not a complete record of, their movement within a day. We find that, while there is significant variation in observation counts per person in a day, conditional on a single person that variance is limited. We take this feature of the data to support, beyond the standard advantages, the value of using models with individual fixed effects to capture inherent differences in observation frequency.

A secondary challenge of using cellphone data is the quality of the collection. We implement two types of cleaning filters on the data before using them for analysis. We eliminate observations that indicate errors in how the GPS data was collected.[7] In the second category of filters we eliminate observations that imply unrealistic movement behavior, such as moving at improbable speeds. In our Online Appendix we provide more details on cleaning the data as well as a general discussion of its quality.

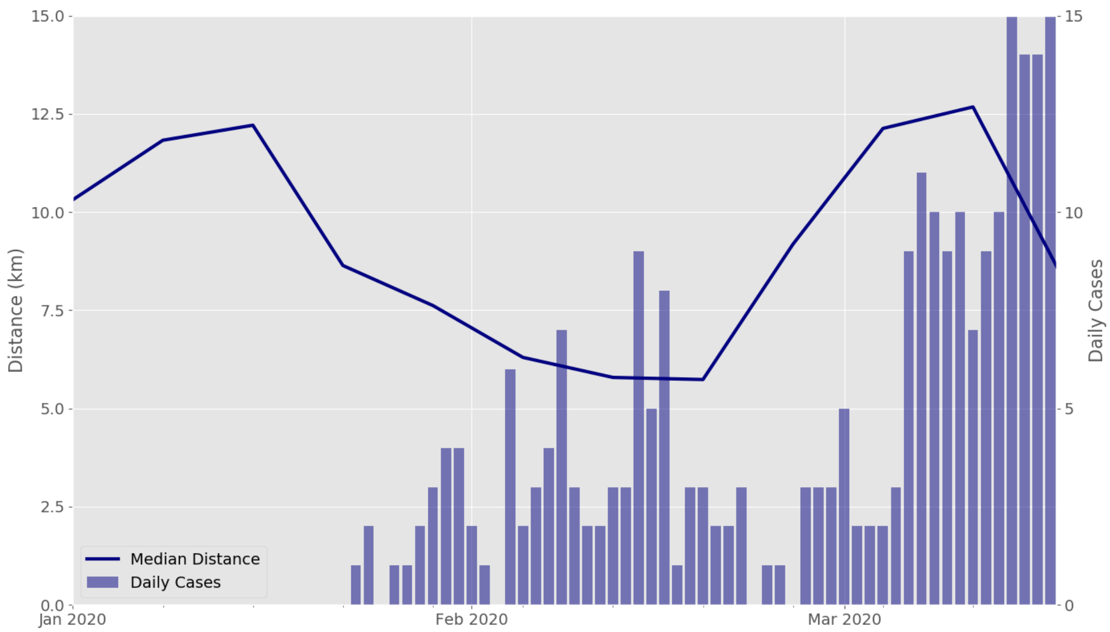
Figure 2 superimposes the timeline of daily COVID-19 cases against the median travel distance

[6] Lifesight estimates home locations using their location data. Their method counts device pings during non-working hours, such as the late night or early morning. Home locations are identified by where the devices consistently ping over these non-working hours.

[7] One particularly irksome example are "IP Resolved" observations. When a company collecting GPS coordinates cannot do so accurately, they assign the phone to one of a handful of pre-determined locations in a process known as IP Resolution.

of individuals in our filtered sample. The qualitative travel pattern is reflective of what our analysis will find. The onset of the first wave of infections reveals an initial drop in travel which slightly recovers as the infection rate appears to slow down. Our sample ends just as individuals begin to respond to the harbinger of the large second wave of infections, eventually cresting at several hundreds cases a day.

Figure 2: **Average Distances Traveled and Cases, through 17 March 2020**



Notes: Traveled distances are calculated as a daily average per individual to remove day-of-week effects. The distribution of travel distances are highly skewed right and so we present the median of this measure. The case dates reported are assigned to the evening on which the government shared detailed location information on positive cases.

Table 1 summarizes the size of our data and various outcome variables. Panel A provides statistics on the subsample of the cell data we use from January to March 17$^{\text{th}}$ 2020. While we have many pings from an individual on any given day, the analysis selected for this paper focuses on outcomes derived at the person-day level. Panel B of Table 1 we include summary statistics for person-day outcomes in our analysis, including whether an individual stayed at home or what distance they traveled. We caution against drawing conclusions from aggregate views of the data. Heterogeneity skews level averages and amplifies the pattern of travel reductions in February followed by a slight recovery in early March.

Table 1: **Data Summary**

	Jan 2020	Feb 2020	Mar 2020
Panel A: Cell Phone Data			
Person-Day Count	4,140,000	4,762,227	2,404,511
Unique People	546,178	569,803	330,805
Avg Obs Per Person-Day	69.35	69.18	100.66
	(129.27)	(148.88)	(147.88)
Panel B: Travel Statistics			
Avg KM Traveled Per Day	18.54	12.95	16.28
	(25.00)	(21.66)	(24.38)
Avg % Staying Home	22.87	27.80	26.42
	(0.18)	(0.15)	(0.10)
Avg Areas Visited Per Day	2.78	1.99	2.75
	(2.80)	(1.85)	(2.72)
Panel C: Activity Statistics (Percent Visiting Daily)			
Industrial	10.33	9.27	11.45
Commercial	24.50	16.41	24.44
Retail	2.72	1.49	2.62
Ind., Com., or Ret.	31.92	23.94	32.65
Recreation	31.17	19.43	29.99
Residential (Not Home)	80.10	73.59	84.92

Note 1: Data for March 2020 only covers through the 17$^{\text{th}}$, the end of our period of study. The standard deviation for select averages are presented in parentheses.
Note 2: Panel C uses data for a subsample of the dataset with estimates of an individual's residence as it is required to generate the statistics. Panels A and B use the full sample. Versions based on the subsample with home estimates available is in the Online Appendix.

We complement this cellphone data with location information from Open Street Maps. We combine land use and building classifications across Singapore and link individual location pings to these areas. This link is used to qualify what types of activities the individual when the ping was sent. We use high-level classifications for the analysis in this paper, levels at the description of commercial, residential, retail, or industrial. Panel C of Table 1 summarizes tendencies to visit each of these location types in any given day averaged over all individuals in the sample. These statistics all reflect the general pattern of reduction and recovery seen in the other aggregate views of our data.

4 Empirical Strategy

Our empirical analysis contains two components. First, we investigate if the announcement of cases close to residencies affects the outward travel behavior of local individuals differently from those that are farther away. For this analysis we define a case as "close" if it occurs in the home subregion of the resident, as depicted in Figure 1.[8] Second, we assess if case announcements within subregions influence the travel inflow of individuals.

When estimating individual responses to infection announcements close to an individual's residence, we face a few major identification challenges. First, case announcements must arrive as exogenous shocks to individuals and occur with temporospatial heterogeneity. National trends in local travel behavior that may correlate with announcement dates violate the exogeneity assumption of case announcements. We tackle this identification challenge by solely using variation on the individual level, controlling for national trends by using day fixed effects. We argue that after controlling for day-specific mobility trends, case announcements that follow positive COVID-19 tests are exogeneous. One specific challenge may be the introduction of split-team work intiatives. These arrangements began no later than February 17$^{\text{th}}$. By this point some businesses had introduced mandatory part-time telecommunicating. Changes in movement as a result of workplace decisions are still voluntary but not necessarily a function of individual-level discretion. Explicitly separating out the effect of these decisions is not possible, but we believe these types of changes should also be soaked up in our analysis by date fixed effects. It is unlikely businesses are making these decisions on the basis of highly localized cases but rather on the basis of national patterns.

Finally, we intend to identify the response to local as well as Singapore-wide cases separately. As aggregate cases do not vary within Singapore at a given point of time, separate identification of response types while simultaneously controlling for national trends is not possible. Therefore, we approximate responses to aggregate cases by responses to case announcements within the larger regions of Singapore. In detail, we consider the five regions depicted in Figure 1, which are the highest geographical division of Singapore (Urban Redevelopment Authority, 2020). Controlling for the subregion effect, the response of individuals to cases within a region is non-local and proxies

[8] Our results are based on residence estimates. Because these estimates are not available for all individuals, we use a subsample of the data for fitting the regression model. In the Online Appendix we conduct a robustness check by redefining cases close to an individual independent of home location. The alternative definition defines close cases as those in any subregion the individual has visited within the last five days. We find similar results.

as an aggregate response given the geographic and population size of a region.[9] The advantage is that using regions permit employing a day fixed effect structure, controlling for national trends and using the variation of announcement across regions while simultaneously investigating highly localized responses.

We summarize our empirical strategy in the following regression model:

$$\mathbf{a}_{ijkt} = \beta_1 LocalCases_{jkt-1} + \beta_2 RegionCases_{kt-1} + \gamma_i + \rho_t + \varepsilon_{ijkt} \quad (1)$$

Consider individual i with a home located in a subregion j, which itself is a subset of the region k. We consider each individual's travel behavior for each day t. $\mathbf{a}_{ijkt}$ is a vector of outcome variables measuring travel. In detail, we consider four outcome variables in our main analysis: travel distance in meters ($TravelDist_{ijkt}$); a dummy which takes the value one if i stays within the subzone of their home ($StayHome_{ijkt}$); a dummy which takes the value one if i visits an area with an industrial-, commercial-, or retail-use classification ($IndRetCom_{ijkt}$); a dummy which takes the value one if i visits an with a residential-use classification ($Residential_{ijkt}$) outside the own home. $LocalCases_{jkt}$ are the number of announced local cases in subregion j in the evening of $t-1$, and $RegionCases_{kt-1}$ are the announced cases in region k. γ_i are individual and ρ_t date fixed effects. We estimate the effect of local and general case announcements on travel behavior by using the variation of announced cases across subregions and regions, controlling simultaneously for individual as well as date fixed effects.

While our first approach shows whether individuals change their travel behavior in response to living close to infected individuals, our second measures if individuals actively avoid areas where confirmed cases live or areas that those infected people have visited before getting tested. As in the first part of our empirical analysis, our identifying assumption is that local case announcements are exogenous shocks and unexpected by individuals. Considering individual i in time period t, our outcome variable is $Visit_{ijt}$, a dummy variable which takes the value one if i has visited subregion

[9] The size of the five region ranges between 4,267 and 8,873 km^2 and each has a population between 573,000 and 923,000.

j in time t. Our full model specification follows:

$$\begin{aligned} Visit_{ijt} =& \beta_1 LocalCases_{jt-1} + \beta_2 InfectionVisit_{jt-1} (+NeighbourhoodCases_{jt-1}) + \\ & \gamma_i \times \xi_j + \rho_t + \varepsilon_{ijt}, \end{aligned} \tag{2}$$

Where $LocalCases_{jt-1}$ are the number of case announcements for subregion j in the evening of $t-1$ and $InfectionVisit_{jt-1}$ are the number of positive cases who visited subregion j. We control for individual-subregion $\gamma_i \times \xi_j$ and time fixed effects ρ_t. Therefore, we evaluate if individual i changes behavior visiting a specific subregion when cases within those subregions are announced. In a final model specification, we further add $NeighbourhoodCases_{jt-1}$, which indicates the number of case announcements in subregions neighboring j. The final model evaluates if there is are signs of substitution between regions visited, i.e. if individuals tend to visit subregion j in case that there is a risk of infection in neighboring subregions.

5 Results

We now turn to the analysis of responses to local and aggregate case announcements. In Table 2 we present results of model 1. In particular, we present the result for each of the four outcome variables, using individual and date fixed effects.

The first outcome is the travel distance on day t of an individual i living in subregion j. The regression result shows that the announcement of a single case for subregion j in the evening of $t-1$ decreases the travel distance of i on a forthcoming day by 61.32 meters. The mean effect, calculate as the percentage difference from the outcome averaged over individuals, is -0.44%. Simultaneously, individuals decrease their travel distance by 28.26 meters (-0.2%) when a non-local, regional cases outside subregion j is announced. Accordingly, the local response is more than twice the size of the non-local response.

In specifications (2) to (4) of Table 2 we consider dummy variables as outcomes. To allow for more convenient interpretation, we inflate the dummy outcome variables by 100, so the coefficients should be interpreted as percentage point changes. Specification (2) considers if individual i stays within their residence's subzone in period t. An additional local case increases the probability

Table 2: **Estimation of Local and General Response**

	TravelDist	StayHome	IndComRet	Residential
	(1)	(2)	(3)	(4)
$LocalCases_{jt-1}$	-61.433^{***}	0.140^{***}	-0.117^{***}	-0.055^{**}
	(14.429)	(0.034)	(0.035)	(0.026)
$RegionCases_{jt-1}$	-28.045^{***}	0.006	-0.083^{***}	-0.029^{**}
	(6.776)	(0.015)	(0.016)	(0.013)
Individual FE	Yes	Yes	Yes	Yes
Date FE	Yes	Yes	Yes	Yes
Mean Local Effect in Percent	-0.44	0.54	-0.4	-0.07
Mean Aggregate Effect in Percent	-0.2	0.02	-0.29	-0.04
N	9,482,376	9,482,376	9,482,376	9,482,376

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: The table presents results of regression model (1). One observation corresponds to an individual on a specific date. Each model specification corresponds to a different outcome variable. $TravelDist$ is the travel distance in meters, $StayHome$ is a dummy variable that takes the value one if an individual remains at their home subzone for an entire day. $IndComRet$ is a dummy that takes the value one if an individual enters at least one industrial, commercial or retail area. *Residential* is a dummy that takes the value one if an individual enters an a residential area except the own residence. Note, that we multiply outcome variables $StayHome$, $IndComRet$ and *Residential* by 100 such that the coefficients are interpreted in percentage points. *LocalCases* are the number of local cases in a subregion announced in the evening of $t-1$. *RegionCases* are the cases of the region announced. For all models we include individual and date FE. Additional models are reported in the Online Appendix. We calculate the mean local effect and mean aggregate effect as percentage difference from the average outcome. Standard errors are reported in parentheses and clustered on the individual level.

of staying home on a forthcoming day by 0.54 percentage points (0.54%). We do not observe a statistically significant response to cases within a region and conclude that people tend to stay at home or in the immediate neighborhood only as a response to local rather than non-local cases. Model specification (3) considers the outcome if an individual enters an area on day t with an industrial-, commercial-, or retail-use classification. From the regression results, we observe that in response to an additional local case, individuals reduce visits to industrial, retail, or commercial areas by 0.12 percentage points (-0.4%). In comparison, region-wide cases lead to an slight decrease of 0.083 percentage points (-0.29%). Finally, specification (4) considers if an individual enters a residential area outside the own residence. A local case decrease the probability of visiting a residential building by 0.06 percentage points (-0.07%), while an aggregate case is associated with 0.029 percentage points (-0.04%) more visits to a residential area. Thus, we observe a higher response to local cases compared to region-wide cases in all specifications.

Our second set of results concern the inflow of individuals into areas affected by case announcements. Table 3 shows the results of regression model 2 across four different specifications. Specification (1) solely includes subregion fixed effects, (2) adds time fixed effects, and (3) and (4) introduce subregion-individual as well as date-specific fixed effects. In model specification (1) to (3), we consider the effect that the announcement of cases who reside in or visitied subregion j have on the probability of visiting j. In comparison, model (4) investigates these effects as well as the impact of announced cases in subregions neighboring j.

Recall that for the second set of regressions we construct a day-individual-subregion panel. The large sample size makes a regression analysis of the whole sample prohibitive. Therefore, results in Table 3 are based on a bootstrapping procedure in which we draw 10% of the individuals in the full sample and repeat after replacement 100 times.[10]

In all model specifications an announcement that a positive COVID-19 case resides in subregion j reduces the probability for individuals to enter j. In our favored model specification with subregion-individual as well as day fixed effects, an additional case decreases the probability of visiting the region by 0.081 percentage points (-1.2 %). The impact the announcement a case visited subregion j has a similar qualitative effect. In specification (3) we find that such an announcement

[10] We exclude observations from the sample that do not provide variation: (1) subregions that an individual has never visited and (2) home subregions of individuals.

Table 3: **Regression, Visiting Affected Areas**

	Visit	Visit	Visit	Visit
	(1)	(2)	(3)	(4)
$LocalCases_{jt-1}$	-0.31^{***}	-0.099^{***}	-0.081^{***}	-0.344^{***}
	(0.003)	(0.003)	(0.002)	(0.008)
$InfectionVisit_{jt-1}$	-0.276^{***}	-0.149^{***}	-0.017^{***}	-0.014^{***}
	(0.002)	(0.002)	(0.001)	(0.001)
$NeighbourhoodCases_{jt-1}$				0.048^{***}
				(0.001)
Subregion FE	Yes	Yes	No	No
Date FE	No	Yes	Yes	Yes
Subregion×Individual FE	No	No	Yes	Yes
Mean Local Effect in Percent	-4.59	-1.47	-1.2	-5.09
Mean Infection Visit Effect in Percent	-4.08	-2.2	-0.26	-0.21
N	477,903,426	477,903,426	477,903,426	477,903,426

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: The table presents results of regression model (2). One observation corresponds to a combination an individual, subregion, and specific date. We exclude observations from the sample that do not provide variation: (1) subregions that an individual has never visited and (2) home subregions of individuals. Each model specification corresponds to the outcome variable $Visit$, a dummy that takes the value one if the individual visits the subregion in t. Note, that we multiply outcome variable by 100 such that the coefficients are interpreted in percentage points. $LocalCases$ are the number of local cases in a subregion announced in the evening of $t-1$. $InfectionVisit$ are the number of newly announced cases that visited subregion j. Finally $NeighbourhoodCases$ are announced in the immediate neighborhood subregions of j announced in $t-1$. Model specification (1) includes subregion fixed effects, specification (2) adds date fixed effects, and specifications (3) and (4) include date and subregion× individual fixed effects. Results are based on a bootstrapping procedure in which we draw 10% of the individuals in the full sample and repeat after replacement. We calculate the mean local effect and mean infection visit effect as percentage change from the average outcome. Standard errors are reported in parentheses and clustered on the individual level.

decreases the probability of a visit in the area by 0.017 percentage points (0.26%). Finally, we show significant substitution between neighboring subregions. An additional case announcement in regions neighboring j increases the probability of an individual visiting j instead by 0.048 percentage points (0.21%).

6 Discussion

6.1 Framing Responses to Case Information

We view the results of Section 5 as a function of how individuals change their behavior in response to their perception of both the risk of catching and acting as a vector for the virus. In this paper we do not intend to formalize how individuals update these perceptions or whether ambiguity aversion or heterogeity in risk tolerance drive patterns differently across subpopulations.[11] While we leave these open to future research, here we relay how risk perception acts as mechanism to link case transparency to changes in individual precautionary behavior. Note that though precautionary behavior can take many forms, such as voluntary social distancing, we observe movement or activity adjustments, which presumably carry a higher cost both to the individual and to the economy. We treat the alternative to the Singapore government's transparency with case information as a policy regime in which COVID-19 case counts are reported for the city at large, which is what was released after the first wave and more similar to many locales in the United States.

The standard SIR epidemiological model would propose that an objective measurement of infection risk is a function of the population's existing stock of infected people and an individual's "risky" behavior. Assuming people adjust their travel behavior based on this measurement, the introduction of more precise information can have one of two effects. If a case is local to an individual's typical destinations, then the precise information raises that individual's perceived risk of being a vector for the disease or catching the disease. If the case is not local, then the precise information has the opposite effect, that is lowers that risk. All else equal, an individual who does not cross paths with the known travel or residence of an announced case should have no increase in her measured risk. While the qualitative impact the information has on individual's movement through this risk mechanism is theoretically unambiguous, the aggregate impact depends on the

[11] See, for example, Allcott et al., 2020 for a discussion of the latter.

number and distribution of cases and the differential response of individuals to these different levels of measured risk.

Our main empirical analysis departs slightly from the framing above. We separate our analysis into changes to inflow and outflow behavior based on the proximity of cases to the locales the individual is entering or leaving, respectively. To start with our outflow analysis, summarized in Table 2, local cases refer to those in the vicinity of an individual's residence. Non-local cases refer to those in areas outside the individual's home but in the same region; therefore, these cases might nonetheless be "local" to areas the individual visits away from home.[12]

Nonetheless, the results support that local case announcements have a stronger marginal impact on travel outcomes than non-local cases. We find the reduction in travel behavior and increase in the likelihood of staying home are reflected across multiple channels of adjustment including how often individuals visit shopping areas (specification (3)) and even other non-home residential areas (specification (4)). We take these changes to mean that individuals reduce their travel behavior as they increasingly perceive themselves as a more likely virus vector. Indeed this is most stark when looking at the impact local cases have on people simply staying home while aggregate cases yield no effect. A case local to home, does not marginally increase risk or the range of risk for contracting the disease in locations away from home yet we observe these behavior changes. That non-local cases have a smaller impact suggests that people perceive the risk change from these additional cases to be smaller when occurring away from home.

The inflow analysis, the results of which are summarized in Table 3, affirm our findings of the impact of local case information on travel behavior. For this analysis, local cases are those in the same subregion where the individual might visit. We control for non-local cases through day fixed effects in specifications (3) and (4) as well as cases from neighboring subregions in specification (4). Across specifications we see individuals reduce their likelihood of traveling to locations either home to or visited by recently announced cases, though we do not find consistent results about which of the two is more influential on this decision. While, as before, the risk of disease contraction and becoming a vector for it are inextricably linked, we take these results as stronger evidence of avoiding contraction. Specification (4) particularly supports this finding. We find that new cases

[12] In an alternative definition, the results for which are available in the Online Appendix, we redefine local cases based on subregions the individual has visited within the last five days. Non-local cases are cases in areas where the individual has not been in the same time period. We find similar results to those presented here.

in neighboring areas increases my likelihood to travel to the unaffected subregion. Hence, people are not simply cutting their travel outright but making marginal adjustments in their destinations. The finding suggests that individuals will use precise information to update their risk assessment at levels of granularity the information shared allow.

6.2 The Impact of Singapore's Transparency

We now use these results to speak to the question of how Singapore's transparency policy affected individuals voluntary precautionary behavior. As the basis of comparison we consider the alternative policy regime Singapore adopted after its first wave of infections, that is simply releasing daily aggregate infection numbers for the whole country. The thought experiment we explore is one in which we hold the transmission and distribution of the disease constant under this alternative policy. There is ample evidence of the impact behavior has on transmission rates — see Chudik et al., 2020 for a related context — but we leave it to future researchers to explore how these travel movements link to transmission explicitly.

Singapore's policy effectively sorts aggregate case numbers into cases that are local to an individual's typical travel and residence and those that are non-local. The effect of this sorting on travel behavior and by extension in-person economic activity is an empirical question that depends on case distributions and the responsiveness of individuals to cases of these two types and their responsiveness to aggregate case information. In the ideal counterfactual one could simulate how individuals would adjust their travel behavior to new aggregate case numbers absent the possibility of local information.

While our exercise does not permit this ideal, we instead construct two extreme counterfactuals based on our estimates for the impact on outward travel behavior. In the "local extreme", individuals react to every case as though reacting to a case that appeared local to their residence. In the "aggregate extreme", individuals react to every case as though reacting to cases not close to their residence. The former captures individuals overreacting, using their own behavior as a baseline, to actual risk. These bounds we construct are conservative on both ends, and we would expect reality to rest somewhere in the middle if all else were equal except the information regime.[13] With these

[13] For this simplification we assume that the marginal effect of a case is cumulative over the entire period. Because we do observe some travel reversion days after case announcements, we again emphasize that the bounds are conservative.

two definitions in mind, in the local extreme we find on average individuals would reduce daily travel by an additional 3 kilometers by the end of the first wave. Compared to the average daily distance traveled for the last week in our sample, this would amount to a 20% reduction. In the aggregate extreme travel distance would increase by 350m, or 3% compared to the average. To oversimplify and consider travel distance as proportionally correlated with economic activity, the local extreme carries a larger downside than the potential aggregate extreme upside. We emphasize that we are not attempting to link this to changes in transmission risk. If from a simple epidemiological stand point less travel is good, then the local extreme's downside would be mitigated.

This back of the envelope calculation only considers outward travel outcomes, while there is a separate impact of precise confirmation that we see in specification (4) of Table 3. Specifically, precise information gives individuals the opportunity to minimally adjust their travel decisions by shifting to proximal areas unaffected by a recent case. Modeling the specific location choices of individuals is beyond the scope of this paper and so a quantitative prediction on the impact of shifting to an aggregate information regime is a significant extrapolation. It is safe to suggest, though, informed switching is not an available risk-adjustment tool to individuals in a counterfactual regime with only aggregate case information.

Assessing whether the policy is ultimately effective or not requires understanding the Singapore government's specific objectives. Presumably, two of these objectives, particularly during the first wave of infections, included minimizing transmission of coronavirus while also minimizing the impact on local economic activity. While we intentionally do not touch on this first objective, we are able to present proxies for the latter through changes in individual travel and activity behavior and find robust evidence of individuals responding to granular information with more precise movement adjustments.

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