Does Precise Case Disclosure 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 announcement 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 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 the scope of precautionary activity reductions.

Keywords: COVID-19, Transparency, Precautionary behavior

JEL Codes: H12, I18, R50

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 closures, 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 twofold. First, the policy could encourage those who potentially contacted cases to seek testing. Second, it might induce the potentially infected into more cautious behavior while mitigating the impact on regular activity elsewhere.

In this paper we study the efficacy of disclosing precise case information in limiting the scope of voluntary activity reductions. To address this question, we take advantage of local case announcements in combination with device-level cellphone location data for more than 10% of Singapore's population to track movement responses to positive cases. To tease out the impact of case announcements on precautionary behavior, we make use of spatial variation in these announcements at granular and successively larger geographic areas. Differences in individual responses to cases proximal to their typical routines compared to those more distant provide an opening to estimate the differential effect of more precise information. Although the extent of a contagious disease is a function of travel behavior, we argue and demonstrate that, in our period of study cases, are effectively exogenous because of its then-limited spread.[1]

In our empirical exercises we study both inflow patterns to and outflow from areas in which positive cases live or visited. Our results are consistent across different outcomes — including travel distance or the likelihood of staying home; people are significantly more responsive on the margin to local cases, 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%).[2] 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

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[1] We show that this intuition about exogeneity holds up to empirical scrutiny in our Online Appendix.

[2] The home census area we use is a geography with an average population of 15 thousand residents, as of 2015 estimates. Local is defined by larger census areas with an average area of $10km^2$.

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%) and individuals partially reroute their traffic to locations proximal to their typical destinations. We take these results to mean that precautionary changes in individual travel and activity behavior are more localized with precise case information. Cases more distant from a person's regular activities have a mitigated or null effect.

To provide context to our estimates, we explore a counterfactual using a stylized model. In the counterfactual, Singapore does not disclose 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 additionally 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., 2020c; Engle et al., 2020; Fan et al., 2020; Farboodi et al., 2020; Gao et al., 2020; Glaeser et al., 2020; Gupta et al., 2020; Mangrum and Niekamp, 2020; Nguyen et al., 2020 Painter and Qiu, 2020; Siedner et al., 2020; Tucker and Yu, 2020) in the United States.[3] 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, we can evaluate an individual's response to highly local cases rather than to those at aggregated geographies 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

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[3]Also, see Brodeur et al. (2020) for an extended discussion on the COVID-19 literature evaluating non-pharmaceutical intervention in the United States.

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 like 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 using individual data.

We also add to the academic and public policy discussion on COVID-19-related non-pharmaceutical interventions.[4] 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., 2020b). 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 which the Wisconsin 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

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[4]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.

mandatory measures. Empirical research weighing 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 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 Sections 5 and 6. Finally we discuss the results in Section 7 and conclude with stylized counterfactual results in Section 8.

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 where this patient had visited in Singapore. They continued to provide detailed reports every evening 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).[5]

While the Singaporean government's disclosure was 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 communicating and managing the risk of dengue and zika infections. 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 public locations and global hotspot travel declarations. 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.

3 Data

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

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[5]From the February 5$^{\text{th}}$ 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.

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.[6]

In our analysis we group cases into census-defined geographies that partition Singapore. We use three successively larger census areas to disentangle the impact of highly local and more distant cases on individual travel and activity behavior. The first are "subzones", denoted by light dotted lines in Figure 4; these are used to detect movement from individual's home area as will be described later in this section. The second are 55 planning areas, denoted by dashed lines in Figure 4; we later call these areas "subregions" for clarity. Depending on the context, cases in the same subregion to which a person travels or works are known as "local". Planning areas can be further aggregated into five large regions, delineated by thick solid lines in the same figure. Cases within the same region are "non-local".

Figure 4 illustrates the cumulative number of cases across Singapore's subzones 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.

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."

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[6]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.

Figure 1: **Total Cases Across Singapore, through 17 March 2020**
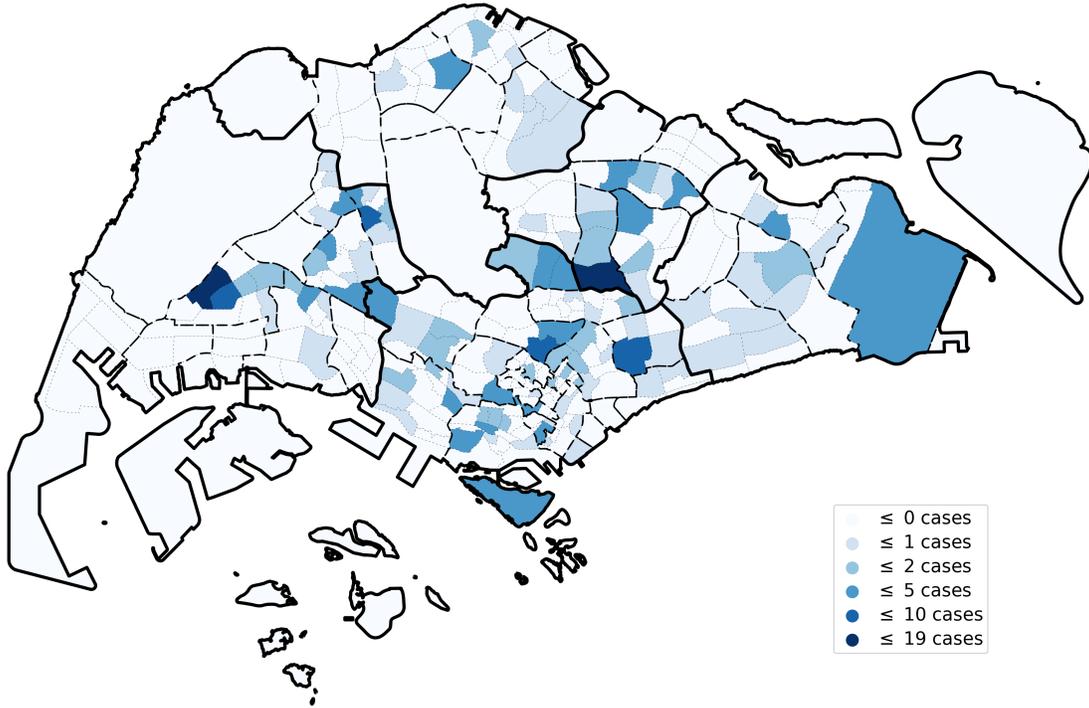
Notes: Solid lines demarcate the five regions of Singapore. Dashed lines denote planning areas, or subregions. The smallest units are subzones.

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.[7] 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.

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[7]Lifesight estimates home locations using their location data. Their method counts device pings during non-working hours. Home locations are identified by where the devices consistently ping over these non-working hours.

A secondary challenge of using cellphone data is the quality of the collection. We implement two types of cleaning filters on the data. We eliminate observations that indicate errors in how the GPS data was collected. 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 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 hundred cases a day.

Figure 2: **Average Distances Traveled and Cases, through 17 March 2020**
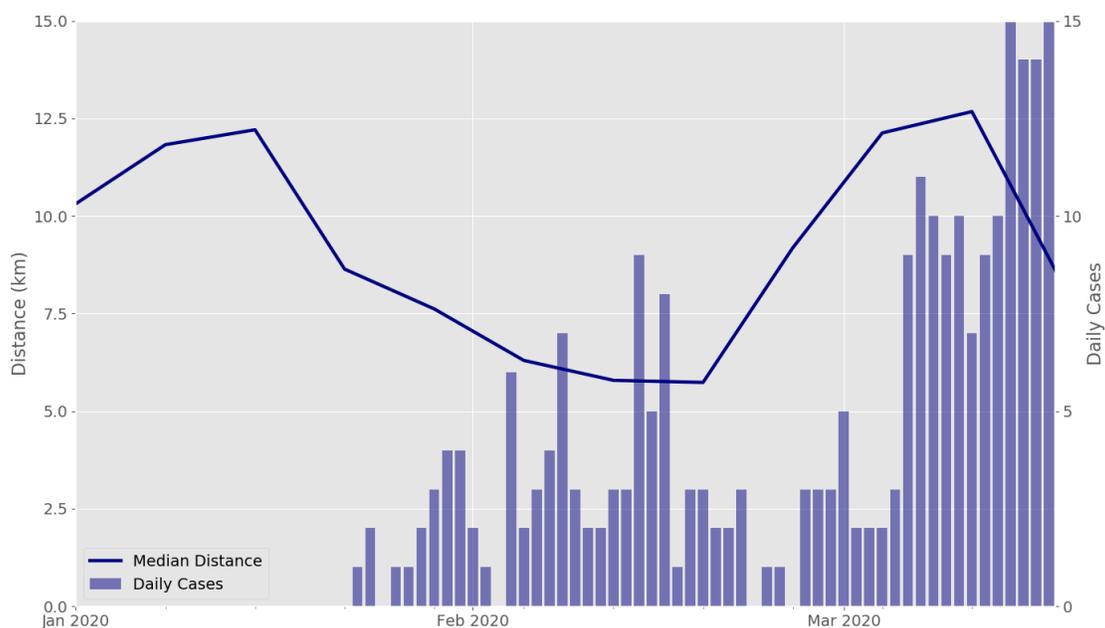
Notes: Traveled distances are calculated as a daily average per individual to remove day-of-week effects. The distribution of travel distances are highly skewed right and so we present the median of this measure. The case dates reported are assigned to the evening on which the government shared detailed 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 17th 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 Subzones 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 17th, 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 classifications across Singapore to project individual location pings to activity types. From Open Street Maps data, we classify pings based on the coordinate's land-use type —

commercial, residential, retail, or industrial.[8] 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 local to residencies affects the outward travel behavior of local individuals differently from those that are farther away. As noted in Section 3 we define a case as local if it occurs in the home subregion of the resident, as depicted in Figure 4.[9] Second, we assess if case announcements local to places individuals visit influence their travel inflow.

When estimating individual responses to infection announcements local to an individual's residence, we face a few 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 for case announcements. We tackle this identification challenge by solely using variation on the individual level, controlling for national patterns by using day fixed effects. A second threat is that movement itself affects the transmission rate of the disease and in turn announcements. We argue that while this is theoretically inarguable, the low case count in our time period renders individual past behavior functionally irrelevant to the spread of the disease. Unless there is minimal heterogeneity in the travel behavior for people living in and visiting affected areas, any individual patterns cannot empirically account for specific case announcements. Indeed, in robustness checks we find that inflow and outflow travel behavior through the fortnight prior to a case announcement is not significantly related to that event.[10]

One specific challenge to the argument that travel behavior changes are voluntary is the in-

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[8]Using cell phone or GPS data to accurately assign activities is not straightforward in a dense city like Singapore. More details of our approach are discussed in the Online Appendix.

[9] 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.

[10]Details are in the Online Appendix.

troduction of split-team work initiatives. These arrangements began no later than February 17$^{\text{th}}$. By this point some businesses had introduced mandatory part-time telecommuting. Changes in movement because of workplace decisions are still voluntary but not necessarily a function of individual-level discretion. Nonetheless, in our second analysis focusing on the inflow travel, we may also pick up non-voluntary changes if travel reductions into working locales are due to home office arrangements. Completely separating out the effect of these decisions is not possible, but we believe that date fixed effects should soak up the impact of these policies. It is unlikely businesses are making these decisions based on highly localized cases but rather on the basis of national patterns. In the first analysis, outward travel behavior should not be sensitive to these arrangements as they are dependent on cases in an individual's home location. In the Online Appendix, we provide two robustness checks to deal with the work-from-home issue specifically.[11]

Finally, we intend to identify the response to local as well as Singapore-wide cases separately. As aggregate cases do not vary within Singapore, separately identifying these responses while simultaneously controlling for national trends is not possible. Therefore, we approximate responses to aggregate cases by using case announcements within the five regions of Singapore, depicted in Figure 4, 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 as an aggregate response given the geographic and population size of a region.[12] The advantage is that using regions permit employing a day-level fixed effect structure, which controls for national trends. Meanwhile, this control allows studying the impact of case announcements across regions; these five regions are still much larger than the 55 geographic units we use for local cases.[13] We summarize our empirical strategy in the following regression model:

$$\mathbf{a}_{ijkt} = \beta_1 LocalCases_{jkt-1} + \beta_2 NonLocalCases_{kt-1} + \gamma_i + \rho_t + \varepsilon_{ijkt} \tag{1}$$

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[11]We first evaluate if subregions with more office space feature stronger inflow travel responses to local case announcements. We find some correlation between the number of offices and the strength of our effect. However, independent of the number of offices, there remains a significant linkage between inflow travel and the number of local cases.

In a second robustness check we test for work-from-home arrangements impacting outflow travel. In that exercise, found in the Online Appendix, we develop a crude measure of an individual's workplace and assess whether workplace cases interfere with the major findings presented in Section 5.

[12]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.

[13]In additional robustness checks found in the Online Appendix, we show our results are minimally changed by different fixed effects structures, including regional or subregional fixed effects.

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 a place with an industrial-, commercial-, or retail-use classification ($IndRetCom_{ijkt}$);[14] a dummy which takes the value one if i visits a place with a residential-use classification ($Residential_{ijkt}$) outside the own home. Hence, we estimate a linear probability model for the last outcome variables.[15] $LocalCases_{jkt}$ are the number of announced cases in subregion j in the evening of $t-1$, and $NonLocalCases_{kt-1}$ are the announced cases in region k.[16] γ_i are individual and ρ_t 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 visited before getting tested. 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 j in time t. Our full model specification follows:

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

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 are signs of substitution between areas visited, i.e. if individuals tend to visit subregion j in the event there is

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[14]Specifically we check for whether any of i's ping coordinates intersect with land classified according to these categories. See Appendix 1.4 for details.

[15]In the Online Appendix we verify that a conditional logit version of this model for the relevant outcome variables does not substantially alter our findings when most observation are not dropped.

[16]In the Online Appendix we tested if, rather than local cases, local incidence values drive our estimate. Using an alternative measurement of cases, weighted by population, we show that the number of cases rather than the incidence of cases matter for behavior.

a new case in neighboring subregions.

5   Results

We now turn to analyze responses to local and aggregate case announcements. In this section we present average responses to case disclosures. In the next section we take advantage of our individual-level data to decompose these responses by individual demographics and characteristics, like existing travel behavior.

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)
$NonLocalCases_{jt-1}$	-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 place. $Residential$ is a dummy that takes the value one if an individual enters a residential place except their 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 $t-1$. $NonLocalCases$ are the cases announced in region k. 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.

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 local 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, calculated 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 case is announced. Accordingly, the local response is more than twice the size of the non-local response.

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In specifications (2) to (4) of Table 2 we consider dummy variables as outcomes. To allow for a 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 of staying home on a forthcoming day by 0.54 percentage points (0.54%). We do not observe a statistically significant response to non-local cases 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 a subregion 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 to that locale by 0.12 percentage points (-0.4%). In comparison, non-local cases lead to a 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 decreases the probability of visiting a residential building by 0.06 percentage points (-0.07%), while a non-local 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 non-local cases in all specifications.

Our second set of results concern the inflow of individuals into subregions 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 visited 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.

In all model specifications an announcement that a local resident has tested for COVID-19 reduces the probability individuals enter the locale. In our favored model specification with subregion-individual and day fixed effects, an additional case decreases the probability of visiting the region

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.

by 0.081 percentage points (-1.2 %). The impact of an announcement claiming a case visited sub-region j has a similar qualitative effect. In specification (3) we find that such an announcement decreases the probability of a visit in the subregion by 0.017 percentage points (0.26%). Finally, we show significant substitution between neighboring subregions. An additional case announcement in neighbors around j increases the probability of an individual visiting j instead by 0.048 percentage points (0.21%).

6   Heterogeneity Analysis

Within this section we deconstruct our main results by exploring heterogeneous travel responses along two critical dimensions. First, we investigate precautionary behavior changes across the distribution of typical travel distances. Using a quantile regression we show that the differences in responses between local and non-local cases are especially high for individuals who travel more. Individuals in high percentiles of the travel distance distribution respond less to non-local cases but reduce their movement more in response to locally announced cases. Second, we analyze the effect of locally announced cases across neighborhoods of varying socioeconomic status. Our principal finding is that individuals with homes in wealthier neighborhoods respond more strongly to local case announcements. Further, we observe a correlation between the usage of public transit and the strength of the travel response.

We start by comparing the impact of case announcements across consumers of different baseline travel behaviors. Specifically, we investigate the effect of local and non-local cases depending on an individual's quantile of typical traveled distance per day. We unpack this relationship (equation 1) using an unconditional quantile regression.[17] Figure 3 reports the unconditional quantile regression coefficients of local and non-local cases on travel distance in meters.

Two major patterns emerge from this analysis. First, the impact of case announcements atten-uates for people with lower baseline travel distances. We observe a limited response to local and non-local cases in the lower 40 percent of the travel distribution. From the 50th to 70th percentiles, individuals decrease their travel to the same extent in response to locally and regionally announced

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[17]Note that the unconditional quantile regression in comparison to the conditional provides the advantage of interpreting the effects as the effects over the distributions of other covariates are marginalized. For an econometric discussion see Firpo et al. (2009). Further, Borah and Basu (2013) provides an empirical comparison.

infections. However, from the 80th percentile, a second pattern emerges; responses to local and non-local cases diverge for the most traveled people. Those individuals respond much less to non-local than local cases. Specifically, the coefficient on non-local cases remains constant for the 70th, 80th, 90th, and 95th percentile and is even increasing and insignificantly different from zero. In comparison, the non-local cases' coefficient largely decreases for the higher percentiles. Overall, the result shows that those individuals in the upper part of the distribution respond to case announcements differently depending on their proximity. One interpretation of the result is that it is costly for individuals who usually travel to decrease their travel. In comparison to individuals in the lower parts of the travel distribution, they do not respond to non-local cases but only to those cases close to their home.

Figure 3: **Unconditional Quantile Regression Results**
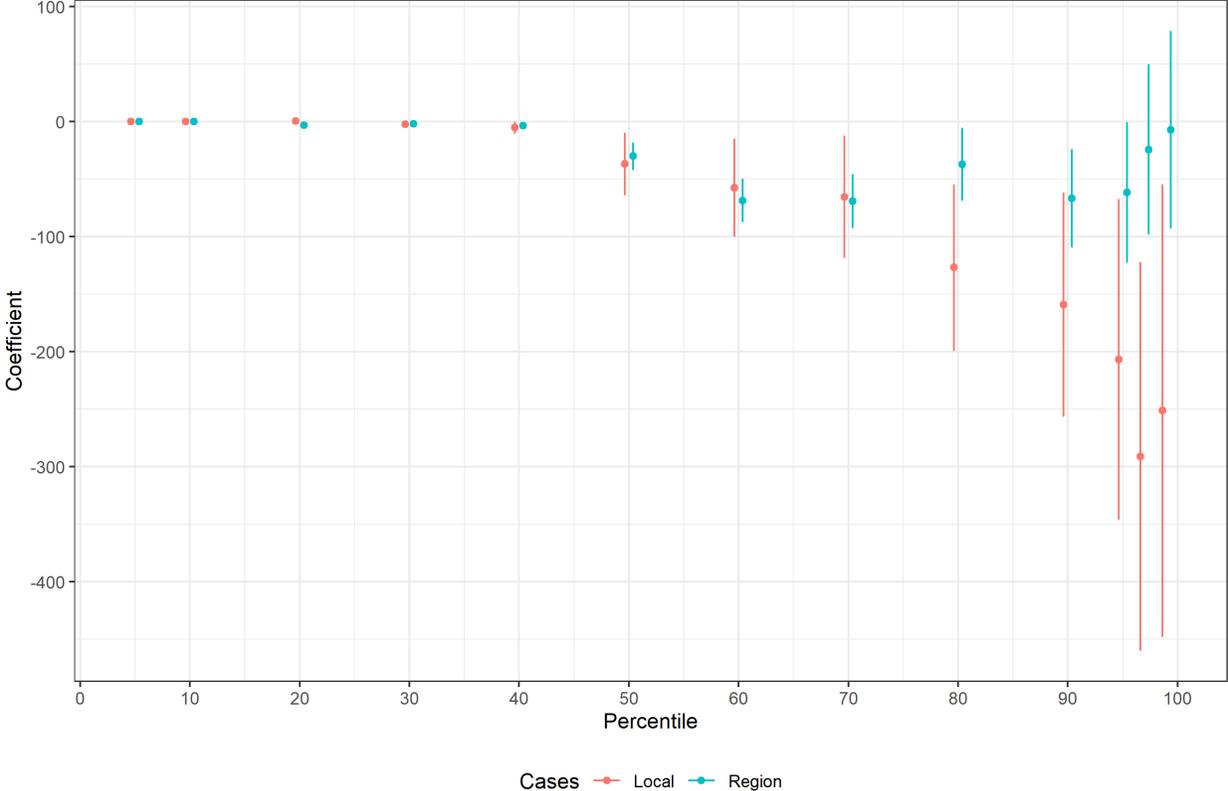
Notes: The figure displays coefficients from an unconditional quantile regression of equation 1 For each percentile ($\{5, 10, 20, 30, 40, 50, 60, 70, 80, 90, 95, 99\}$) we show the coefficient in response to local (β_1) and non-local (β_2) cases. The regression includes individual and date fixed effects. The error bars correspond to the 95% confidence interval.

We additionally focus on response heterogeneity based on home neighborhood demographics and characteristics.[18] To show evidence for the correlation we evaluate a simple extension from our main model in equation 1:

$$TravelDist_{ijkt} = \beta_1 LocalCases_{jkt-1} + \beta_2 NonLocalCases_{kt-1} + \tag{3}$$
$$\beta_3 LocalCases_{jkt-1} \cdot log(\mathbf{C}_{jk}) + \beta_4 NonLocalCases_{kt-1} \cdot log(\mathbf{C}_{jk}) +$$
$$\gamma_i + \rho_t + \varepsilon_{ijkt}$$

The additional feature interacts local as well as regional, non-local cases with a characteristic of the local area j in region k, collected in a vector of local characteristics $\mathbf{C}_{jk}$: population density, average household income, share of high education (at least post-secondary education), the share of population older than 65 years old, the share of population living in a private condominium or landed property, the share of the population using public transport to their workplace, and the average time transport takes to the workplace. For each of these characteristics, we present evidence from a separate regression. Hence some of these factors may proxy for similar relevant demographics, e.g. household income and education. We take the logarithm of each statistic of a local area to compare effects to each other. For example, a 10% higher average income in a local area is correlated to an average additional travel distance change of $\beta_3/10$ in response to the announcement of an additional local case. Note that individual fixed effects γ_i soak up any non-interacted characteristic terms. Our key interest is if some characteristics are correlated with a greater travel distance response.

Figure 4 shows the coefficients $\hat{\beta}_3$ for each individual regression, i.e. the correlation between local characteristics and the average travel distance response to local cases. Note that each coefficient should only be interpreted as a correlation; we are not discerning any causal mechanism at play. We first observe a negative but insignificant correlation with density, that is more dense areas show a higher reduction in travel distance. For high income and highly educated individuals, we observe a strong and negative correlation. We also find that areas with more residents over 65 in which a higher percentage of households live in private condominiums and landed property, both connected to higher social-economic status in Singapore, show negative correlations to travel

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[18]These characteristics are based on 2015 Singaporean household surveys (Statistics Singapore, 2016).

responses. Finally, we evaluate the correlation with two variables that provide information about the population's work commute. We specifically find that a higher share of individuals who use public transport is correlated with a higher reduction in travel distance after the announcement of local cases. Further, the longer the commute time, the higher the reduction.

7   Discussion

We separate our discussion of these results by inflow and outflow behavior around locales with newly announced cases. Our focus is on the relative impact of local and non-local case types on the travel behavior of individuals traveling to or from the affected areas.

Beginning with the outflow analysis, summarized in Table 2, recall local cases refer to those in the subregion of an individual's residence. Non-local cases refer to those in areas outside the home subregion but in the same region; therefore, these cases might nonetheless be "local" to areas the individual visits away from home.[19]

Nonetheless, the results support that local case announcements have a stronger marginal impact on travel outcomes than non-local cases. We find that 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. In contrast non-local cases yield no effect. A case local to home, does not marginally increase the 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

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[19]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.

Figure 4: **Heterogeneity of Local Areas, Regression Results**
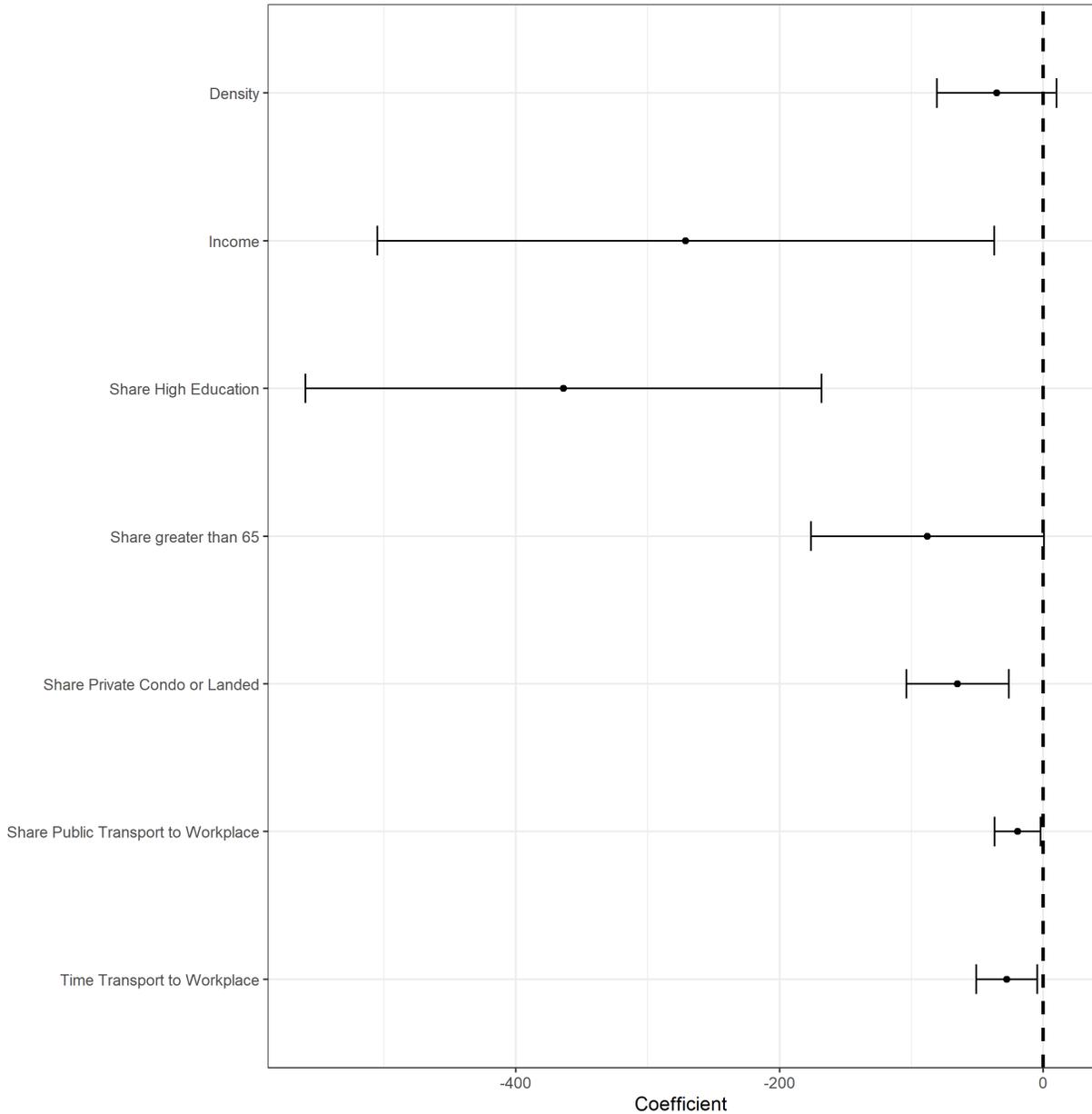
Notes: The figure displays coefficients $\hat{\beta}_3$ of regression 3. Each coefficient corresponds to one average characteristic of a local area. By increasing the value of the characteristic in a local area by 10% we observe on average a change of travel distance by $\beta_3/10$ in response to the announcement of an additional local case. Each regression includes individual and date fixed effects. The error bars correspond to the 95% confidence interval.

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 that individuals reduce their likelihood of traveling to locations

either home to or visited by recently announced cases. We do not, however, find consistent results about which of the two is more influential on this decision. While 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 in neighboring areas increases one's 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.

Finally, our result on heterogeneous responses in travel outcomes have an additional purpose in evaluating the impact of localized information. On the one hand, the analysis reveals that the policy could have a differential impact across the average travel distance distribution. Especially for those who travel a lot, we observe a diverging response between local and regional responses. In the case a policy intends to target those individuals, localized information may be a suitable policy tool. On the other hand, the analysis reveals that high-income households respond strongly to local case information. The finding is in line with previous observations in the literature (Almagro and Orane-Hutchinson, 2020, Desmet and Wacziarg, 2020) which shows that economically disadvantaged households have less opportunity in precautionary behavior. Additionally, we find a correlation between travel along vectors carrying greater transmission risk, e.g. on public transport, and the adjustment in precautionary behavior.

8   A Stylized Counterfactual

In this section we develop a simple model to provide light framing for our results and to describe how risk perceptions might impact our estimated travel behavior. We do not explicitly model the role that new information, like case disclosure, plays in updating these perceptions. Rather we use the model to rationalize how imprecise beliefs can generate our findings. We close the section by connecting our empirical results to a back-of-the-envelope counterfactual exercise estimating how people may have behaved without Singapore's local information disclosure.

8.1 Set Up

We start with the setting of a standard SIR model; there is a city's population in which each individual i in day t is in one of four states $\sigma_t \in \{S, K, R, D\}$, susceptible, infected, recovered, and deceased. As we focus on the beginning of a pandemic, we assume for simplicity that only susceptible and infected persons exist. The share in the population that are susceptible or infected are s_t and k_t respectively. Provided the information k_t is available, individuals may distinguish between *local*, e.g. close to their residence or perhaps locations they frequent, and *non-local* cases. Denote local cases $k^l_{it} = \alpha_i k_t$ for some $\alpha_i \in [0, 1]$, which may be based on the individual's perception. Summarize the relevant information for an individual in Θ_{it}.

Every day individuals must choose how much to travel c_{it}. Individuals earn utility from traveling and engaging in daily activity while infection with COVID-19 reduces utility, $u(c_i, \sigma_i)$, so $u'_c > 0$ and $u(c; S, \Theta_i) > u(c; K, \Theta_i)$ for all c. The individual discounts future utility by δ and maximizes expected lifetime utility $\sum_{\tau=t}^{\infty} \delta^\tau u(c_{i\tau}; \sigma_{i\tau}, \Theta_{it})$. As in our time period there is little feedback between infection rates and individual behavior, we assume people do not worry about developing expectations over the city's infection status. Rather they behavior in t as if Θ_{it} will hold for the near future.

One cost of travel is the chance to become infected by COVID-19. The "true" probability an individual gets infected in t is $g(c_{it} \beta k_t) \in [0, 1)$ where $g' > 0$ and β is some known infection factor for the infected population.[20] Hence travel and the rate of infection both result in higher infection rates. Individuals, however, are motivated by their *perceived* infection risk. This perceived infection risk is $\hat{g}_i \equiv g(c_{it} \beta \hat{k}_{it})$, where $\hat{k}_{it} \equiv f_i(k_t)$; any perception errors are based on uncertainty around the number of people infected.

The second cost is based on concerns that an infection, even if undiagnosed, could unwittingly spread the disease. Before a positive diagnosis an individual i is in the susceptible state and is uncertain if she is infected or not; denote the perceived probability of her own infection by $\hat{p}_i \equiv p\left(\hat{k}^l_{it}\right) \in [0, 1]$. Note that this uncertainty is not necessarily based on symptoms but based on concern about one's exposure to local infected people. Let $a(c_{it})$ be a negative externality imposed on others by an infected person. We assume $a' > 0$. Hence, for person i the internalized component

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[20] A classic assumption on g is that it is a logistic function. At this point in the pandemic, the city should be in the convex portion of this function.

of the expected negative externality is $\hat{p}_i a(c_{it})$.

8.2 Individual Decisions

We can summarize the maximization problem of susceptible people in the following Bellman equation.

$$V_i^s(\Theta_i) = \max_c \left\{ u(c; S, \Theta_i) - p\left(\hat{k}_i^l\right) a(c) + \delta \left[g(c\beta\hat{k}_i)V_i^k(\Theta_i) + \left(1 - g(c\beta\hat{k}_i)\right) V_i^s(\Theta_i) \right] \right\} \quad (4)$$

Solving the first order condition yields

$$u'(c; S, \Theta_i) = \underbrace{g'(c\beta\hat{k}_i)\beta\hat{k}_i\delta \left(V_i^s(\Theta_i) - V_i^k(\Theta_i)\right)}_{\text{infection risk cost}} + \underbrace{p\left(\hat{k}_i^l\right) a'(c)}_{\text{externality cost}} \quad (5)$$

Hence the marginal benefit of more travel is weighed against two marginal costs, roughly corresponding to the costs discussed previously. The first factor is the perceived marginal increment of becoming infected. Note that belief crucially depends on the private perception of the risk in the population $\hat{k}_i$. The second effect is the marginal impact on the expected externality. In this model local cases have two channels to impact behavior as compared to non-local cases. Only local cases impose costs through both cost channels identified above. This result is meant to line up with our empirical findings summarized in Table 2, specifically the relative strong impact of local and non-local (regional) cases on travel behavior.

8.3 Impact of Information

Finally, we turn to discussing the potential role Singapore's local case disclosure may have played in changing an individual's behavior. In the context of the model, government case disclosures impact an individual's risk evaluation through perceived population infection rates $\hat{k}_i$. The structure of the disclosure may have a different impact on $\hat{k}_i$ and $\hat{k}_i^l$.

As previously noted the goal of the paper is not to estimate people's risk perceptions. We do emphasize, however, how they might respond differently to local case data versus non-local case data. In our back-of-the-envelope counterfactual exercises we will offer two extremes for how individuals might respond to cases. In a "local extreme" people take local case information as

indicative of the infection rates throughout the city. If government information were perfectly accurate, this appears as an over-estimation of the infection probability. In another extreme people respond to all cases as if non-local; in this case the reaction would appear as an under-estimation.

To illustrate these extremes we start with the baseline of Singapore's disclosure regime and assume that individuals take government disclosures as the true infection rates. In this case the government provides sufficient information for everyone to accurately set $\hat{k}_{it} = k_t$ and $\hat{k}^l_{it} = k^l_t$, in contrast to a guess of α_i. In the counterfactual disclosure environment in which the government *does not* provide local information, only k_t is known. To construct $\hat{k}^l_{it}$, the individual must now guess what fraction α_i of cases are local. In the local extreme (LE) counterfactual, $\alpha^{LE}_i = 1$. In the non-local extreme (NE) $\alpha^{NE}_i = 0$. Generally, we can think of the former as a weak over-estimation of the case counts in one's local areas while the latter is an under-estimation.

These counterfactual extremes can predict a wide range of potential impacts on travel behavior. Let k° stand in for the baseline information. In both extremes outlined above $k^\circ = k^{LE} = k^{NE}$ while $0 = k^{l,NE} \leq k^{l,\circ} \leq k^{l,LE}$. It is obvious from the FOC in Equation 5 that optimal travel among the three cases follows $c^{LE} \leq c^\circ \leq c^{NE}$. After presenting the back-of-the-envelope calculations, we discuss more on which extreme we find more plausible with the evidence available.

In the calculation we make two simplifying assumptions, both consistent with the stylized model in this section. The first is to hold the transmission and distribution of the disease constant under alternative policies. 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. Second, we assume that the marginal effects presented in Table 2 apply for the cumulative local and non-local cases for the entire period.[21] Hence, as we use the results from estimating model 1, local cases are those near an individual's residence. In the local extreme we find individuals would, on average, 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 non-local extreme daily travel distance would increase by 350m, or 3% compared to the average.

To oversimplify and consider travel distance as proportionally correlated with economic ac-

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[21]Consistent with the explanation provided above, in the non-local extreme individuals react to every case as though regional. In the local extreme individuals react as if all cases over the time period are local.

tivity, the local extreme carries a larger downside than the potential non-local extreme upside. We emphasize that we are not attempting to link this to changes in transmission risk. If from a simple epidemiological standpoint less travel is good, then the local extreme's downside would be mitigated.

Our stylized counterfactual is agnostic about the exact position of the real counterfactual. However, we present two arguments that a true counterfactual would be closer to the local extreme. That is, without local information we would have observed a larger downside in economic activity. First, there is survey evidence that individuals over-estimated the risk of getting infected and dying from COVID-19 in the beginning of the pandemic (Akesson et al., 2020). Second, the data the government discloses is a lower bound, ignoring many false positive tests, on the true number of infections in the city. It would be reasonable that perceived local infections are higher than government-reported infections, i.e. perceived infections $\hat{k}^l \in [k^{l,\circ}, k^{l,LE}]$.[22]

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. All else equal this should strengthen the case for providing precise information; it is safe to suggest this type of switching is not an available risk-adjustment tool to individuals in a counterfactual regime with only aggregate case information. Modeling the specific location choices of individuals, however, is beyond the scope of this paper.

9   Conclusion

In this paper we contribute to the large literature that developed around the study of COVID-19 by focusing on the impact and efficacy of precise case information disclosure. For severe diseases insufficiently endemic to support extensive lockdown measures, as COVID appeared in its nascency, governments must carefully balance disease management and social or economic concerns. Our paper sheds light on the efficacy and impact of case disclosures as one possible tool during this stage. Singapore's initial decision to provide detailed information about the location and movement of positive cases was a near ideal setting for assessing the efficacy of this policy tool. Because the

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[22]In fact, in the baseline case with this rationale $\hat{k}^l > k^{l,LE}$ as well.

policy was implemented before mandatory movement restrictions, we are able to observe largely voluntary changes in individual behavior.

Assessing whether the policy is ultimately effective requires understanding the government's, here Singapore'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. We find robust evidence of individuals responding to granular information with more precise movement adjustments. Our results showing that individuals carefully adjust their routines to areas proximal to new cases provides the most optimistic case for detailed case disclosure. Hence, we believe it should merit further investigation. Future research could evaluate the trade-off of transmission of COVID-19, or future diseases, and local economic activity, both influenced by localized case information.

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