Is no news bad news? The impact of disclosing COVID-19 tracing information on consumer dine out decisions

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Abstract
Food markets around the world have been disrupted by the COVID-19 pandemic via consumer behavior upended by fear of infection. In this article, we examine the impact of disclosing COVID-19 contact tracing information on food markets, using the restaurant industry in China as a case study. By analyzing transaction data at 87 restaurants across 10 cities, we estimate difference-in-difference (DID) models to ascertain the impact of COVID-19 infections and contact information tracing on economic activity as measured by a daily number of transactions. Empirical results show that while the overall number of new COVID-19 infections at the national level caused a dramatic drop in numbers of transactions in all restaurants, restaurants in cities that disclosed contact tracing information of COVID-19 infections experienced a 23%–35% higher number of transactions than the ones in cities that did not disclose such information during the recovery period. Ultimately, we show that in the absence of a shelter-in-place mandate, disclosing contract tracing information to mitigate consumers’ uncertainties about risks of being infected can contribute to a faster recovery of food markets, in addition to reducing COVID-19 infections.

**KEYWORDS**
contact tracing information, COVID-19, food markets, risk uncertainties

**JEL CLASSIFICATION**
D12, D80, D90, I12, L81

1 | INTRODUCTION

The spread of the COVID-19 virus has brought unprecedented challenges to global food markets. Policies aiming to mitigate the spread of the virus, such as social distancing, restaurant shutdowns, and disclosing contact tracing information concerning COVID-19 infections, have had substantial impacts on consumer behavior. Understanding the impacts is essential in the postpandemic era for policymakers who seek to create an effective blueprint for the renormalization of consumer behavior and, ultimately, the recovery of food markets.

In this article, we discuss how disclosing contact tracing information on COVID-19 cases could initiate food market recovery, using China’s restaurant industry as a case study.¹ When the Chinese government adopted the Great Lockdown policy at the beginning phase of the pandemic, on January 23, 2020, most restaurants were shut down for almost 1 week, starting to reopen gradually after Febru-

¹ Contact tracing information disclosure refers to a city government’s choices for providing the public information on the locations and timelines of the spread of COVID-19 infections (see Table A.1 in Appendix A for an example).
ary 3, 2020. At the end of March, when China reported zero new cases of COVID-19, about 80% of them had reopened (China Ministry of Commerce, 2020). In contrast, the recovery of the number of transactions was only 30% compared to the prepandemic period, indicating an unexpected delay of market recovery despite the efforts made by the government and industry. Our research shows that business at restaurants located in cities that disclose contact tracing information about the locations and timelines of COVID-19 infections resumes faster in terms of numbers of transactions.

We estimate the causal effects of disclosing COVID-19 contact tracing information on consumers’ decisions to dine at restaurants by applying difference-in-difference (DID) analysis to a dataset that includes the number of transactions for 87 restaurants in 10 cities across China between December 1, 2019, and March 27, 2020. We merge this data with COVID-19 infection counts, whether or not the restaurants are located in cities that disclose COVID-19 contact tracing information, and other control variables. We find that the spread of the pandemic, measured by the national numbers of new infections and active cases, has a significant and negative effect on the number of restaurant transactions. This result is consistent with the findings of Goolsbee and Syverson (2021) that the overall severity of the pandemic could prevent consumers from going out because of fear of being infected. In contrast, we find that restaurants located in cities that disclose contact tracing information appeared to have a faster rate of sales recovery, amounting to an approximately 24%–35% increase relative to cities without the disclosure. The empirical results are robust to a battery of robustness tests, suggesting a causal relationship between disclosing contact tracing information and market recovery.

To explain the findings, we develop a theoretical framework to elucidate how disclosing contact tracing information could help consumers eliminate their uncertainties about the risk of being infected and then rationally update their consumption decisions. That is, by reading information about the locations and timelines of COVID-19 cases, consumers may feel reassured about dining out in areas not reported on the travel path of the infected cases, which may ultimately lead to faster recovery of the markets.

The restaurant industry provides a good case study to examine the effects of the COVID-19 pandemic and information disclosure on food markets. First, the restaurant industry has played an important role in providing food-away-from-home throughout the world during the pandemic. As the largest disruptions of food markets due to COVID-19 stem mainly from restaurant shutdowns, the transaction recovery for the restaurant industry is critical to the food market’s recovery. Second, by focusing on Chinese restaurants in key cities, we also provide a unique case study relevant to the behavioral responses of urban consumers whereas COVID studies on China food systems tend to focus on impacts on the rural population (Wang et al., 2021). Third, the restaurant data allow us to quantify the value of COVID-19 contact tracing information in a quasi-natural experiment setting.

China’s national government adopted an information disclosure policy similar to that of most countries, including the United States: both national and local governments announce COVID-19 case counts every day, but the decision to provide the public with contact tracing information on infected cases is left to local governments. Consequently, there have been significant variations across restaurant locations in terms of whether or not to provide contact tracing information, allowing us to employ a DID analysis to compare the differences in transaction recoveries between the treatment and control groups of cities.

Our findings are consistent with the literature on the impact of disclosing information on consumer behavior. A couple of COVID-19 studies focus on the role of information by discussing how disclosing information about infections influences the spread of the disease (Argente et al., 2022), and how information about the severity of the pandemic shapes individuals’ expectations concerning macroeconomic performance and their spending plans (Baker et al., 2020; Cavallo, 2020; Coibion et al., 2020). In our case, when facing an uncertain risk of being infected by COVID-19, consumers can use the tracing information to update their prior beliefs on the state of the world and form new beliefs to make better decisions. To our best knowledge, this article is the first attempt to address how contact tracing information promotes economic recovery by showing that greater information transparency contributes to a faster recovery of consumer visits than does keeping consumers in the dark during the COVID-19 period.

Notably, using contact tracing information to evaluate whether the “state” of the overall environment is safe from COVID-19 distinguishes this study from previous literature on the effect of market information about product quality, such as nutrition information signaling through food labels (Cawley et al., 2020; Gao & Schroeder, 2009; Kariuki & Hoffmann, 2021; Zhu et al., 2016), health insurance quality (Beaulieu, 2002; Dranove & Jin, 2010; Jin & Sorensen, 2006), sending children’s school test scores to

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2 During the pandemic, consumers significantly shifted their food-away-from-home demand to food-at-home demand because food retail outlets such as grocery stores and supermarkets remained operating at full capacity in most countries during the entire COVID-19 period (Goolsbee & Syverson, 2021).
parents (Hastings & Weinstein, 2008), and signals sent by financial markets (Dellavigna & Pollet, 2009). In this respect, this analysis is closer to Kapoor and Magesan’s (2014), which explores the effects of public information provision on pedestrian countdown signals.

2 DATA AND EMPIRICAL STRATEGY

The main data source used in the analysis is a confidential, proprietary dataset from one of the largest restaurant conglomerates in China. In our analysis, we utilize data from 87 restaurants across 10 cities in China for on-site dining transactions from December 1, 2019, to March 27, 2020. The locations of these restaurants reach a potential market of 140 million people. Supplementary datasets provide the date that a city started to disclose contact tracing information, the spread of COVID-19, and income proxy variables. Combining all the data, we obtain a panel dataset with daily transactions aggregated at the restaurant level. We exclude the period from January 23, 2020, to February 2, 2020, because all the restaurants were shut down at this time, which coincided with the 2020 spring festival recess, because of COVID-19.

The dependent variable of interest is the daily number of restaurant-level transactions during the pandemic, as a proxy for economic activities. In support of this, Goolsbee and Syverson (2021) note that the number of consumer visits (approximately equivalent to the number of transactions) is a plausible measure of economic activity. In this study, our primary interest is consumers’ decisions to dine out, which can be aggregated as the number of transactions at the restaurant level. Figure 1 illustrates the daily number of transactions across the 87 restaurants in the sample. Hovering around one million transactions in 2019, before the pandemic, the number of transactions drops significantly in early February 2019, during the spring festival, but quickly rebounds to the previous level. In contrast, in 2020, the number of transactions drop much more during the spring festival, which coincides with the temporary shutdown policy due to COVID-19. Although the transactions did not recover immediately after the spread of COVID-19 was well controlled, they soon reflected an upward trend towards recovery.

The main explanatory variable of interest is whether city governments decide to disclose contact tracing information on COVID-19 casethat is, the treatment variable. In Appendix A, Table A.2 shows that the sampled cities, labeled by letters A, C, D, E, G, H, I, and J, started to disclose during the week of February 3–10, 2020, while local governments in cities B and F decided not to disclose any contact tracing information. In the empirical analysis, we categorize the cities disclosing contact tracing information as the treatment group and the other cities as the control group. Figure 2 illustrates the differences in the average daily transactions between the two groups. Although the number of transactions of restaurants located in the control cities is consistently higher than those in treated cities, the treated cities show a faster increase in the number of transactions after February 3, 2020.

Although Figure 2 is suggestive of the potential effect of contact tracing information disclosure, we need a more rigorous analysis that uses other explanatory variables that control for other drivers of the number of restaurant transactions during the pandemic. We begin by including the number of infections during the pandemic as a primary driver of changes in transactions. To specify, we include the number of new infections both at the national and local levels in our regressions. Figure 3 illustrates the trends in new national and local COVID-19 infections in China from January 23 to March 28, 2020. The trend shows that new infections dropped dramatically 3 weeks after the COVID-19 incidence on January 23. That is, the pandemic came under control before the end of our sample period.

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3The conglomerate owned four different chains with 160 restaurant locations across 10 Chinese cities in 2019, with total sales of around 1 billion RMB (around 150 million U.S. dollars), which accounts for 2.3% of the total sales of China’s restaurant industry. Our data come from the largest of these chains, which covers a wider geographic area than the other chains.
4On average, each restaurant has around 125 seats, with the average size of the dining area being around 3837 square feet. The average score for restaurants in the sample is approximately 4.5 out of 5.0 on Dianping (China’s Yelp), which is high among medium-priced casual restaurants (about 89 CNY or $13 per person).
5Although the first reopening occurred on January 28, 2020, most of the restaurants reopened after February 2, 2020.
6Because the spring festival is scheduled every year using the lunar calendar, the recess periods for the 2019 and 2020 spring festivals are different.
7Other government policies that may affect restaurant visits during COVID include lockdown, cross-city mobility restrictions, and economic stimulus. However, during 2020, these policies were implemented at the national level, not specifically targeting the treatment or control cities, and simultaneously at the national level. Therefore, these policies are unlikely to be confounded with COVID-19 tracing information policies implemented at city levels. For example, during the start of treatment (February 3–10, 2020), 59% of the restaurants in the treatment group were closed versus 46% of restaurants in the control group. If anything, our treatment effects are a lower bound of the true estimates. Likewise, using Harvard Dataverse from China Data Lab (2020) for February 3 to March 15, 2020, there is no difference on mobility rates between treatment and control cities affected by mobility restrictions.
8As a confidential requirement for using the data, instead of disclosing restaurant locations and names we are required to use letters A to J to represent the 10 cities.
9We exclude the number of new infections from international travelers because all city governments in our sample adopted a quarantine policy.
include other control variables to account for factors that may affect restaurant transactions, including an income proxy to partially address the confounding of COVID-19 income effects with dining-out decisions, city and time (day) fixed effects, and restaurant-specific fixed effects to account for unobserved factors.

One identification concern is that the impact of COVID-19 on dine-out decisions is contaminated by differences in demographics, particularly income shocks associated with the pandemic (Swinnen & Vos, 2021).\textsuperscript{10} Due to the lack of availability of contemporaneous data on income variations, we use daily air pollution at the city level as an income proxy variable. The relationship between air pollution and business activities/income has been extensively studied as an environmental Kuznets Curve in the environmental economics literature (see Copeland and Taylor (2004) for a review). As our sample cities are either the largest metropolises or provincial capital cities in China, business activities are closely reflected by traffic flows. Additionally, it has been shown that the changes in air pollution during the COVID-19 pandemic are strongly correlated with reduced vehicle travel (Cicala et al., 2021) and, consequently, business activities. We use the daily air pollution index PM 2.5, as it is the most utilized air pollution index in China. The PM 2.5 data are collected for each restaurant location and day from the China National Environmental Monitoring Center.

Table 1 presents the summary statistics of the variables utilized in the DID model. The total number of observations in the treatment and control groups is almost balanced at 47% and 53%, respectively. The average PM2.5 index is around 45, showing improved air quality during the pandemic due to the reduction of business activities. We also include an indicator variable, isOpen, to account for a restaurant’s closing and reopening status as a control for the supply-side shocks.

We develop a baseline DID model with the following regression form:

\[ y_{ict} = \alpha_{Info} + \beta_{kt}COVID_{kt} + X_{ict}\Lambda + \lambda_c + \theta_t + \mu_i + \epsilon_{ict}, \]  

where \( y_{ict} \) denotes the log of the daily number of transactions in restaurant \( i \) on day \( t \) in city \( c \). Info equals 1 if, on a given day, a restaurant is in a city with contact tracing information disclosure and 0 otherwise. The coefficient \( \alpha \) represents the percent change in the number of restaurant transactions resulting from COVID-19 contact tracing information. \( COVID_{kt} \) denotes the number of COVID-19 cases on day \( t \) reported at the basis (\( k = Nat \) for national level, \( k = Loc \) for cases at the local level) of count \( j \) (\( j = new \) for new cases; \( j = act \) for active cases), and \( X_{ict} \) is a vector of non-COVID control variables. The rest of the variables (\( \lambda_c, \theta_t, \mu_i \)) denote fixed effects for cities, time (day), and restaurant-specific fixed effects, respectively. The term \( \epsilon_{ict} \) is an i.i.d. error term.

In the times of COVID-19, a week could make a significant difference in terms of food consumption. Since the treatment dates started on five different dates from Febru-

\textbf{FIGURE 1} Daily number of daily transactions in the sample restaurants on January–March 2019 and 2020

Note: The figure shows the trend of the average number of transactions at restaurants located in all the cities from January to March in the years 2019 and 2020. The y-axis is the daily number of transactions, and the x-axis is the months. The straight line is the trend in 2019 and the dotted line is the trend for the year of 2020.

\textsuperscript{10} Swinnen and Vos (2021) also point out to supply disruptions as culprits on COVID impacts on global food systems.
FIGURE 2  Log of the daily number of transactions (residuals) in restaurants located in the treatment and control groups
Note: The figure graphs the different trends of transaction recovery for treated (the straight line) and controlled cities (the dotted line). The y-axis is the residual of daily number of transactions at the city level derived by regressing the log daily numbers of transactions on the pre-COVID-19 dummy, time and location fixed effects.

FIGURE 3 National COVID-19 case counts, January–March 2020
Notes: The figure graphs the spread of COVID-19 against time. The y-axis is the case counts. The straight and dotted lines graph the case counts for daily new infections and active cases, respectively. March 25, 2020, was the first day of nationwide reports of zero new infections in China during the pandemic period.

ary 3 to 10, 2020, with a 7-day maximum gap, it seems reasonable to be concerned about whether or not average treatment parameters in the benchmark DID model are appropriate. To this end, we adopt the framework of
**TABLE 1** Summary statistics

| Variable name | Definition | Mean   | Std. dev. | Min  | Max  |
|---------------|------------|--------|-----------|------|------|
| Info          | =0 for pretreatment; =1 for post-treatment | .222   | .415      | 0    | 1    |
| $y_{ict}$     | Log of no. of transactions | 2.896  | 1.678     | 0    | 5.775|
| Lag$y_{ict}$  | The 1st lag of Ln(transactions) | 2.908  | 1.660     | 0    | 5.775|
| lnOnline      | Log of no. of orders from online platforms | 3.319  | 1.170     | 0    | 5.398|
| Pollution_pm25| PM 2.5 Index | 43.649 | 37.006    | 3.5  | 273.083|
| isOpen        | =0 if the rest. is closed; =1 if otherwise | .855   | .353      | 0    | 1    |
| Lockdown      | Dates before and after Wuhan lockdown = 0; dates during Wuhan lockdown | .509   | .500      | 0    | 1    |
| COVID_Natnew  | Daily national new infected cases (divided by 100) | 6.334  | 16.729    | 0    | 141.090|
| COVID_Locnew  | Daily locally new infected cases (divided by 100) | .430   | .060      | 0    | 43   |
| COVID_Natact  | Daily national active cases (divided by 100) | 145.958| 193.368   | .010 | 580.97|
| COVID_Locact  | Daily local active cases (divided by 100) | .695   | .935      | 0    | 3.320|

The identification of the effects of contact tracing information on restaurant transactions crucially depends on the parallel-trend assumption. In other words, restaurant transactions in cities with and without location signals should show similar trends prior to the onset of the pandemic. That is, the differential changes in the number of transactions are not driven by pre-existing differences in the trends of transactions. To empirically test for the parallel trend’s assumption, we perform a parallel trends test by allowing $\alpha$ to vary across the treatment and control groups before the realization of the treatment, using December 1, 2019, as the pretreatment reference date. The specification is

$$y_{ict} = \sum \alpha_t (PrePost_t \times Inf_{oc}) + \beta_{kjt}COVID_{kjt} + X_{ict} \Lambda_{ict} + \mu_{ic} + \theta_t + \varepsilon_{ict},$$  

(3)

where $PrePost_t$ are day dummies equal to 1 for every date before February 3, 2020, and 0 otherwise. $Inf_{oc}$ denotes the cities that received treatment afterward, beginning when the actual treatment effects took place, from February 3 to 20, 2020, in the corresponding cities.

Second, restaurant transactions are highly affected by the prevalence of COVID-19, which may confound the impact of contact tracing information. We, therefore, perform a triple differences (DDD) test to quantify the impact by including an additional treatment $G_i$, which is equal to the variable of national new infections, to capture the causal impact of the pandemic. The empirical equation is as follows:

$$y_{ict} = \alpha Inf_{oc} + \gamma G_i + \alpha^{DDD} Inf_{oc} G_i + X_{ict} \Lambda_{ict} + \mu_{ic} + \theta_t + \varepsilon_{ict},$$  

(4)
where all notation is as defined above.

Third, another concern about our DID estimates is that standard error estimates may not exhibit asymptotic properties of large samples due to the relatively small number of clusters. In our case, we have 10 cities with only two control cities. Following Cameron and Miller’s (2015) suggestion, we utilize a wild cluster bootstrap using Webb (2014) weights to overcome the lack of a large sample inference due to a relatively small number of clusters. Using Roodman et al. (2019) procedure, we compute standard errors clustered at the city level using a wild bootstrap with 1000 replications. In addition, we conducted a permutation test with 500 random replications for pseudo treatments to test if the estimated parameters for the treatment were obtained by chance.

3 | EMPIRICAL RESULTS

Columns (1)–(3) in Table 2 report the benchmark results from Equation (1) by adding more control variables when all the errors are clustered at the city level. The results show that the treatment effect is significantly positive at a level of around .259–.291 in the dependent variables, given different model specifications, which corresponds to an approximately 23%–35% increase after a semilog transmission of but-for transactions in the control group postpandemic.

The income proxy variable, as measured by PM 2.5, has a significant but relatively small effect on the number of transactions during the pandemic period. Another interesting feature of the results, in terms of explaining the number of transactions, is the significant degree of inertia of repeated transactions in a restaurant, as the coefficient of the lag of the log number of transactions is around .48. The daily number of new infections and active cases at the national level shows a strong and negative correlation with restaurant transactions, as expected, while local-level case counts are insignificant.

As specified in Equation (2), we employ a model that addresses time-varying treatment effects. The results indicate that the average estimated coefficients were virtually identical, dispelling any biases of our time-invariant treatment effects in Equation (1) (Bertrand et al., 2004; Deshpande & Li, 2019; Callaway and Sant’Anna 2018).11 Columns (1)–(3) in Table 3 report the decomposed estimates of $\alpha^{DD}$ with control variables. The model specifications are the same as in Table 2 but with four different treatment coefficients due to the different treatment time groups. The level of estimates is around .259–.291 with the same model specifications as in Table 2. All estimates are significant at the 1% confidence level. The effects of the other control variables are also similar to those in Table 2.

The results for the parallel trends assumption, as illustrated in Figure 4, showing the estimates of $\alpha_t$ from Equation (3), indicate no systematic differences in pre-trends across treatment and control groups. Thus, the benchmark results in Table 2 do not appear to be driven by systematic pretreatment trends.

Columns (4)–(6) in Table 3 report the results of the DDD estimation of Equation (4). The DDD estimates remain significant at the 1% confidence level, but the overall impact becomes larger, from 25.8% to 29.5%. In addition, the DDD estimates for Info*Covid are not statistically significant for model specifications (4) and (5), and only marginally so (at the 10% level) for model (6). Thus, no additional significant effects are picked up by a DDD model with respect to the effect of contact tracing information disclosure.

We also test for apparently different trends due to contact tracing information by dropping the control group and only comparing the treatment subgroups, as the treatment varies over time in our analysis. Columns (7)–(9) in Table 3 show that the treatment is insignificant, indicating no systematic differences between the different treated subgroups due to different treatment dates. This evidence provides further support to our findings that the differences only appear between the treatment and control groups.

Another concern with DID results is that the changes in consumer restaurant transactions may reflect the impact of unobserved characteristics, such as friendlier industry-recovery policies adopted by different cities and expedited approval of reopen applications. If the differences in restaurant transactions increase because of the recovery-friendly policies instead of location signals, online orders should also show the similar trend. We use the number of online orders as a placebo test, because if the DID estimates of online orders are significant, the impact on restaurant transactions may come from the omitted city-level recovery policy. Columns (10)–(12) in Table 2 report the results from the placebo test. In contrast to the treatment’s signifi-

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11 Figure B.2, in Appendix B, shows the results from the decomposition DID estimate without any control variables. The graph shows how the two-way fixed effect estimate is calculated: by taking an average of the 16 y-axis values weighted by their x-axis values. The distribution of the standard 2x2 standard DID estimates ranges from −1 to around 1.5 and gives an estimate of $\alpha^{DD}$ at the level of 0.439. We estimated a decomposed DID parameter $\alpha^{DD}$ as a weighted average of 25 distinct 2x2 standard DID estimates: five comparisons between each treated subgroup, and 20 (given by the combinatorial number $C(5, 2) \times 2 = 20$) comparisons for two treated subgroups between an earlier treated subgroup and a later non-treated subgroup, and between a later treated subgroup and an earlier non-treated group. The empirical equations are listed in Appendix B.
### TABLE 2  Benchmark DID estimation results

| Variables | (1)         | (2)         | (3)         |
|-----------|-------------|-------------|-------------|
| Info      | .2586***    | .2689***    | .2908***    |
|           | (6.81)      | (6.54)      | (5.76)      |
| $Lag_{yt}$| .4811***    | .4806***    | .4796***    |
|           | (12.62)     | (12.60)     | (12.38)     |
| Other controls: |           |             |             |
| isOpen    | 1.1060***   | 1.1058***   | 1.1065***   |
|           | (10.60)     | (10.58)     | (10.49)     |
| Pollution_{pm25} | .0003  | .0003       |             |
|           | (1.97)      | (1.67)      |             |
| Lockdown  | −1.3470***  | −1.1447***  | −1.3695***  |
|           | (−13.02)    | (−16.23)    | (−12.145)   |
| COVID_{Natnew} | −.5778*** | −.1320**    |             |
|           | (−7.88)     | (−2.69)     |             |
| COVID_{Locnew} | .1335 | −.0108      |             |
|           | (.34)       | (−.03)      |             |
| COVID_{Natact} | −.0010** |            |             |
|           | (−2.87)     |             |             |
| COVID_{Locact} |          | .0403 |            |
|           |             | (1.37)      |             |
| Constant  | 1.5824***   | 1.5784***   | 1.5816***   |
|           | (18.36)     | (18.49)     | (18.01)     |
| Restaurant FE | Yes | Yes | Yes |
| City FE   | Yes         | Yes         | Yes         |
| Day FE    | Yes         | Yes         | Yes         |
| Number of chains | 87 | 87 | 87 |
| $R$-squared | .946 | .946 | .946 |

Notes: This table reports results from Equation (1). The explanatory variable is the log daily number of transactions. The DID explanatory variable is variable “Info.” The values of $t$-statistics are reported in the parentheses. All standard errors are clustered at the city level using wild bootstrap with 1000 replications with weight type Webb. ***, **, and * represent significance at 1%, 5%, and 10% level, respectively.

### FIGURE 4  Parallel trend test

Notes: The figure shows the coefficient estimates for Equation (3). The shaded areas are the 95% confidence intervals for the estimates. All the estimates before the date of contact tracing information disclosure are insignificant.
### Table 3: Robustness tests

| Variables     | Decomposed DID | DDD estimates |
|---------------|----------------|---------------|
|               | (1) (2) (3)    | (4) (5) (6)   |
| Info          | .2586*** (.540)| .2908*** (5.08)| .2899*** (5.45) |
| Info*COVID_Natnew | −.4820*** (−2.54) | −.6352*** (−3.28) |
| Other control variables | Yes Yes Yes | Yes Yes Yes |
| Restaurant FE | Yes Yes Yes | Yes Yes Yes |
| City FE       | Yes Yes Yes | Yes Yes Yes |
| Day FE        | Yes Yes Yes | Yes Yes Yes |
| Month FE      | Yes Yes Yes | Yes Yes Yes |
| Variables     | Dropping control groups | Placebo tests |
|               | (7) (8) (9)    | (10) (11) (12) |
| Info          | .1421 (.58)    | .0602 (1.25)  |
| Info*COVID_Natnew |   | .0427 (.38)  |
| Other control variables | Yes Yes Yes | Yes Yes Yes |
| Restaurant FE | Yes Yes Yes | Yes Yes Yes |
| City FE       | Yes Yes Yes | Yes Yes Yes |
| Day FE        | Yes Yes Yes | Yes Yes Yes |
| Month FE      | Yes Yes Yes | Yes Yes Yes |

Notes: This table collects results from robustness checks discussed in Section 3. The values of t-statistics are reported in the parentheses. All standard errors are clustered at the city level using wild bootstrap with 1000 replications with weight type Webb. ***, **, and * represent significance at 1%, 5%, and 10% level, respectively.

A representative consumer’s utility to choose \( a \) at state \( s \) is \( u(s, a) + \epsilon_a \), where \( u(s, a) \) is a state-dependent utility including all the systematic determinations of the utility conditioned on state \( s \) and choice \( a \), and \( \epsilon_a \) is a random utility shock following an i.i.d. Gumbel distribution and is state-independent. When uncertainty occurs, the consumer has a prior belief about the likelihood of each state but does not know the real state. The prior belief is a probability distribution \( h(\cdot) \) on \( S \), where \( h(S) = [0, 1] \) with \( h(H) + h(L) = 1 \). The consumer maximizes their expected utility against the prior following optimization problem:

\[
\max_{a \in A} E_{h(\cdot)} u(s, a) + \epsilon_a.
\]

**4 | POTENTIAL MECHANISMS**

Our main empirical finding is that cities that provided information on locations and timelines for infected cases experienced a higher number of consumer restaurant visits during the COVID-19 pandemic. In this section, we discuss the conceptual explanations for the empirical finding.

Consider a population of consumers deciding whether to dine at a restaurant or to eat at home. We denote their choice alternatives by \( a \in A = \{0, 1\} \), where \( a = 1 \) denotes going out to dine; otherwise \( a = 0 \). The uncertainties about being infected by COVID-19 when consumers visit a neighborhood restaurant are measured by a binary state variable \( s \in S = \{H, L\} \), where \( s = H \) denotes that the degree of risk of being infected by coronavirus is “high” when visiting a neighborhood restaurant, and \( s = L \) for “low” risk of being infected.

For the permutation test, we randomly selected a set of cities as pseudo cities that disclose tracing information, estimated the regression model using pseudo control and treatment groups. We repeated this process 500 times to build a \( p \)-value for the pseudo treatment effects. On average, the pseudo treatment should have no effect on consumer behaviors because they are simply randomly chosen. The \( p \)-value of the permutation test is approximately 0 with a 95% confidence interval from 0 to .007, which indicates that the real estimate is at the extreme tail of the distribution of pseudo effects, suggesting that it is unlikely that the real effect was observed due to chance.
The probability of consumer $i$ choosing $a$ is

$$\text{Prob}[a] = \frac{e^{U(a)}}{\sum b \in A \Delta U(b)},$$

under a logit model specification, where $U(a) \equiv E_{\mu(s)} u(s, a)$.

Suppose, now, that the city government discloses the location and timeline information on visits by infected individuals. This information is modeled as a random signal $Z$, along with its conditional distribution $F_{Z|S}(\cdot|s)$, $s \in S$. The realization of $Z$ is $z \in \{0, 1\}$, where $z = 1$ represents that the neighborhood of the restaurant intersects with locations visited by infected individuals, and otherwise $z = 0$. On observing signal realization $z$, the consumer updates their prior belief to formulate a subsequent belief $f(\cdot|z)$ and maximizes the expected utility against this new belief:

$$\max_{a \in A} E_{f(\cdot|z)} u(s, a) + \varepsilon_a,$$

and the choice probability for $a$ is calculated as

$$\text{Prob}[a|z] = \frac{e^{U(az)}}{\sum b \in A \Delta U(bz)}$$

with $U(a|z) \equiv E_{f(\cdot|z)} u(s, a)$.

In the binary-state setting, the expected utility against the consumer’s prior $U(a)$ is equal to $h(s')u(s, a) - u(s', a) + u(s', a)$, and the expected utility against the subsequent utility $U(a|z)$ is equal to $f(s|z)(u(s, a) - u(s', a)) + u(s', a)$, where $s$ and $s'$ are the two mutually exclusive states in the doubleton $S$. These results generate an explanation that exactly matches our empirical finding. That is, the representative consumer receives signal $Z$ and observes that its realization $z = 0$ will increase the probability of choosing $a = 1$. Meanwhile, the number of transactions for the whole population grows when the probability of dining out for representative consumers increases. The detailed derivations are in Appendix C.

The mechanism shows that when the pandemic comes under control, even though consumers are well aware that the overall state of environment poses less risk of being infected, the disclosure of contact tracing information about the exact timelines and locations of exposure to the virus can be helpful in further mitigating uncertainties by updating consumers’ beliefs about being infected and thereby lead to a faster recovery of transactions by increasing the incidence of dining out in the locations that were not visited by COVID-19 cases.

The validity of the mechanism for explaining the empirical findings is based on the assumption that locations of restaurants are not cross-connected with the locations visited by individuals with COVID-19 infections. If our sample restaurants are close to the travel path of COVID-19 infections, the observed increases in transactions cannot be attributed to the mitigated uncertainties about the risks of being infected. To test this assumption, we calculate the distance between the restaurant locations and all the locations mentioned along the travel paths of COVID-19 cases. The results show that the average probability that a restaurant is located diametrically within 500 m of the locations visited by people with COVID-19 infections is less than 18%. The average distance between a restaurant and locations where COVID-19 cases traveled is 18.52 km. The kernel density of the distances is shown by Figure C.1 in Appendix C. This fact further confirms that the proposed mechanism is a plausible hypothesis to explain the empirical findings.

## 5 Concluding Remarks

Is no news bad news? When it comes to contact tracing information disclosure during the COVID-19 pandemic, our results indicate that the answer is a solid YES. From our analysis of the impact of disclosing the trajectory and timelines of COVID-19 infections on consumer restaurant transactions in a sample of 87 restaurants in China, lack of information does not appear to do much to alleviate the fears that drive consumer decisions to dine out. The coefficient estimates of the variable indicating the provision of contact tracing information (the treatment variable) in a series of DID models indicate that although restaurant traffic is primarily driven down by the number of COVID-19 infections, the provision of contact tracing information can increase the number of transactions between 23% and 35% and smooth the path to economic recovery. A back-of-the-envelope calculation projected to the national level for the Chinese restaurant industry, which had sales of 4.8 trillion RMB (approximately $680 billion USD) in 2019, underscores the substantial potential value of this type of information.

The policy implications of this analysis are significant because restaurants are one of the main components of the food system that are negatively impacted by the pandemic. The dramatic declines in restaurant transactions due to the pandemic have seriously disrupted food supply chains, with severe consequences for farmers, processors, and the economy, and these cannot be overcome without the restaurant industry’s recovery. Simply put, the resumption of restaurant transactions is a necessary condition for faster recovery of the food market from the COVID-19 pandemic. Thus, the results of this research support transparency and the provision of contact tracing information...
to speed up the recovery of the restaurant industry from the COVID-19 pandemic.

Our results also confirm the findings of Goolsbee and Syverson (2021) and Chetty et al. (2020) that it is not lockdown policies per se but rather fear of infection that drives down economic activity. The provision of contact tracing information and timelines by city governments helps consumers update their beliefs about COVID-19 risks and, consequently, make decisions about dining out, leading them to intentionally avoid exposure to hotspot areas while reassuring them that they can safely dine out during the pandemic in restaurants not reported to be in an area of confirmed cases. We argue that being more transparent can reduce consumer uncertainties to a certain extent. Although some governments chose not to disclose contact tracing information about the pandemic, adhering instead to the rationale that consumer behavior and economic activity would resume faster if the information was withheld, our empirical results show just the opposite: greater transparency of contact tracing information can induce faster resumption of consumer dining-out behavior by identifying restaurants and areas where the risk of infection is lower.

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SUPPLEMENTARY INFORMATION
Additional supporting information can be found online in the Supporting Information section at the end of this article.

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APPENDIX A

TABLE A.1 Example of contact tracing information disclosure for a COVID-19-infected case

| Timeline       | Location                                                                 |
|----------------|--------------------------------------------------------------------------|
| Jan 30 to Feb 7| Shopped at GM farmer’s market for vegetables                              |
| Feb 3 17:00    | Went to a maternal store located at LF shopping mall                      |
| Feb 5 14:00    | Shopped at the supermarket located on the second floor of WD shopping square |
| Feb 8 11:00    | Bought a cake at GYX Bakery in LT building                               |
| Feb 9 11:00    | Shopped at LF supermarket                                                 |
| Feb 9 17:00    | Shopped at TT supermarket and picked up a cake at GYX Bakery              |
| Feb 10 11:00   | Shopped at GM farmer’s market for vegetables                              |
| Feb 11 18:00   | Went to GZC clinic with his daughter, received COVID-19 test              |
| Feb 12 16:00   | Went to GZC clinic again and then to HC hospital with his wife. Received COVID-19 test results indicating positive COVID-19 infection |
| Feb 13         | Sent to the centralized quarantine location of WH town                    |
| Feb 14 12:00   | Went to the RM city hospital by ambulance                                 |
| Feb 14 18:00   | Went back to the centralized quarantine location of WH town               |
| Feb 15 13:00   | Sent to the RM city hospital for quarantine and treatment                |

TABLE A.2 Date consumers started receiving location signals

| Groups   | Cities | Treatment date |
|----------|--------|----------------|
| Treated group | D | 02/03/2020       |
|           | E     | 02/03/2020       |
|           | H     | 02/03/2020       |
|           | G     | 02/03/2020       |
|           | A     | 02/06/2020       |
|           | J     | 02/09/2020       |
|           | C     | 02/10/2020       |
|           | I     | 02/10/2020       |
| Control group | B | Do not report   |
|           | F     | Do not report   |
APPENDIX B

DID estimates for time-variable treatments

The parameter $\alpha^{DD}$ in Equation (2) denotes parameters for time-varying treatments. Thus, it follows that decomposing this parameter into various time-varying treatments is key to providing additional information on the effects of the treatment start dates. The decomposition process is as follows. For simplification, assume we only have two varying treatment dates, an early and a late date. The two dates of receiving treatment split the data into three groups: a control group that is never treated, an early treated group that receives the location signals at time $t^*_k$, and a late treated group that receives the signal at time $t^*_l$. The timeline thus breaks into three subperiods: the pre-period; a middle period, when the early treatment group is treated but the late one is not; and a post-period after the late group is treated. The DID coefficient $\alpha^{DD}$ is a treated average for the following four standard $2 \times 2$ DID estimates. The first compares the early treated group and the control group for pre-$t^*_k$ and post-$t^*_l$ periods. The second compares the late treated group and the control group for pre-$t^*_k$ and post-$t^*_l$ periods. The third compares the early treated and the late treated group for pre-$t^*_k$ and the middle period between $t^*_k$ and $t^*_l$. The fourth compares the early treated and the late treated for the middle period between $t^*_k$ and $t^*_l$ and post-$t^*_l$. The fixed-effect DID estimate $\alpha^{DD}$ is then calculated as a weighted average of the four DID estimates. For a graphical illustration and more detailed explanation, please refer to Goodman-Bacon (2021).

Formally, our data are a balanced panel with $T$ periods ($t$) and $IC$ cross-sectional chains that are composed of an untreated group $U$ and group $M = \{1, 2, ..., k, ..., l, ... K\}$, which is ordered by the time at which the treatment occurs. $k$ stands for an earlier treatment city that receives the location signals at time $t^*_k$. $l$ stands for a later treatment city that receives the signal at time $t^*_l$. $U$ represents the control group of cities that never receive the signals. The OLS estimate, $\hat{\alpha}^{DD}$, in Equation (2) is a weighted average for all possible $2 \times 2$ DID estimators:

$$\hat{\alpha}^{DD} = \sum_{k \neq U} s_k \hat{\alpha}^{2\times 2}_{kU} + \sum_{k \neq U} \sum_{l \neq k} \left[ \mu_{kl} \hat{\alpha}^{2\times 2}_{kl} + (1 - \mu_{kl}) \hat{\alpha}^{2\times 2}_{kl} \right]$$

(B1)

where the $2 \times 2$ DID estimators are:

$$\hat{\alpha}_{kU}^{2 \times 2} = \left( \ln(\text{visits}_{i|k|})_{\text{Post}(k)} - \ln(\text{visits}_{i|k|})_{\text{Pre}(k)} \right)$$
$$\hat{\alpha}_{kl}^{2 \times 2} = \left( \ln(\text{visits}_{i|k|})_{\text{Mid}(k,l)} - \ln(\text{visits}_{i|k|})_{\text{Pre}(k)} \right)$$
$$\hat{\alpha}_{kl}^{2 \times 2} = \left( \ln(\text{visits}_{i|k|})_{\text{Mid}(k,l)} - \ln(\text{visits}_{i|l|})_{\text{Pre}(l)} \right)$$
$$\hat{\alpha}_{kU}^{2 \times 2} = \left( \ln(\text{visits}_{i|l|})_{\text{Post}(l)} - \ln(\text{visits}_{i|l|})_{\text{Mid}(k,l)} \right)$$

We use $\ln(\text{visits}_{i|b|})_{\text{a'}}$ to denote the sample mean of $\ln(\text{visits}_{i|c|})_{\text{a'}}$ for chains $i$ in city $b$ starting treatment at time $t^*_b$, during city $a$’s (starting treatment at time $t^*_a$) post period, that is, $[t^*_a, T], (\ln(\text{visits}_{i|c|})_{\text{a'}})$ and $(\ln(\text{visits}_{i|b|})_{\text{a'}})$ are defined similarly. The weights $s$ are

$$s_{kU} = \frac{n_k n_U (1 - \bar{f}_{o_k})}{\bar{v}_{\bar{f}} (\bar{f}_{o_k})}$$
$$s_{kl} = \frac{n_k n_l (\bar{f}_{o_k} - \bar{f}_{o_l}) (1 - \bar{f}_{o_k} - \bar{f}_{o_l})}{\bar{v}_{\bar{f}} (\bar{f}_{o_l})}$$
$$U_{kl} = \frac{1 - \bar{f}_{o_k}}{1 - (\bar{f}_{o_k} + \bar{f}_{o_l})}$$

where $n_{U}$, $n_k$, and $n_l$ represent the sample shares of the control group, the earlier treated cities $k$, and later treated cities $l$, respectively. $\bar{f}_{o_k}$ and $\bar{f}_{o_l}$ are the share of time spent under treatment $f_{o_{ct}}$ is defined as $f_{o_{ct}} = \frac{1}{ct} \sum_c \sum_t f_{o_{ct}}$ and

$$\sum_{k \neq U} \sum_{l \neq k} s_{kl} = 1$$

TABLE B.1 Shares of treatment and control groups

| Treatment date | Number of restaurants | Share of restaurants | Treatment share |
|----------------|-----------------------|----------------------|-----------------|
| Not treated    | 46                    | .53                  | -               |
| 02/03          | 12                    | .14                  | .500            |
| 02/06          | 1                     | .01                  | .472            |
| 02/07          | 22                    | .25                  | .463            |
| 02/09          | 1                     | .01                  | .443            |
| 02/10          | 5                     | .06                  | .435            |

Note: This table shows the number and share of restaurants that are treated on different dates. We split our data into six groups, with five treatment subgroups and one control group. Forty-six restaurants, approximately 55% of the total number of restaurants studied, are not treated. The largest portion receiving treatment reopened on February 3, 2020, after the first date for starting treatment. The last column lists the share of days for each subgroup being treated throughout the sample days.
APPENDIX C
THEORETICAL DERIVATIONS
In a two-state setting, it is straightforward that

\[ U(a) = h(s) (u(s, a) - u(s', a)) + u(s', a) \]

and

\[ U(a|z) = f(s|z) (u(s, a) - u(s', a)) + u(s', a), \]

where \( s \) and \( s' \) are the two mutually exclusive states in the doubleton \( S \). These are used to derive the following, Proposition 1, on how an observed signal realization affects the consumer’s choice probability:

**Proposition 1.** If \( (s|z) > h(s), u(s, a) > u(st, a), \) and \( u(s, b) \leq u(st, b) \), where \( a, b \in A \) with \( a \neq b \), and \( s, s' \in S \) with \( s \neq s' \), then receiving location signal \( Z \) and observing its realization \( z \) will lead their probability of choosing \( a \) increase.

**Proof.** Given \( f(s|z) > h(s) \), since \( u(s, a) > u(s', a) \), we have \( U(a|z) > U(a) \); since \( u(s, b) \leq u(s', b) \), we have \( U(b|z) \leq U(b) \). These equalities give rise to the fact that \( U(b|z) - U(a|z) < U(b) - U(a) \). From the utility functions in the two-state settings above, we know that \( \text{Prob}[a] = \frac{1}{1 + \exp(U(b) - U(a))} \) and \( \text{Prob}[a|z] = \frac{1}{1 + \exp(U(b|z) - U(a|z))} \), leading to the inequality \( \text{Prob}[a] < \text{Prob}[a|z] \). Q.E.D.
The interpretation of Proposition 1 is intuitive. If a consumer observes a signal realization $z$ leading to their belief that the more likely state is $s$, and choice $a$ is more desirable in state $s$ than in the other state $s'$, while the opposite choice $a'$ is (weakly) not as desirable in state $s$ as in state $s'$, the probability of choosing the more desired alternative $a$ will increase. In our context of consumers’ dining-out decisions during the pandemic, it is reasonable to make the following assumption to specify the comparative relations involving $f(s|z)$, $h(s)$, and $u(s,a)$ in which we are particularly interested:

**Assumption 1.** $f(L|0) > h(L)$, $u(L,1) > u(H,1)$, and $u(L,0) = u(H,0)$.

The assumption $f(L|0) > h(L)$ simply states that, after observing no infected cases in neighborhood restaurants, that is, $z = 0$, the consumer will believe that it is more likely that the risk of being infected by the virus in that neighborhood is low. The new belief leads to $u(L,1) > u(H,1)$, showing that dining out is more desirable for the consumer than staying at home. The consumer’s utility is not influenced if they stay at home because $u(L,0) = u(H,0)$. Under Assumption 1, Proposition 1 translates into the following corollary:

**Corollary 1.** Given Assumption 1, the probability of choosing $a = 1$ increases if the consumer receives signal $Z$ and observes its realization $z = 0$.

Corollary 1 shows that if the consumer observes from the location signals that there is no intersection between the neighborhood of the restaurants and locations visited by infected individuals, they are more likely to choose dining out than staying at home. We can aggregate the individual choice probability from Corollary 1 over the population of consumers to derive the testable hypothesis that location signals increase restaurant visits, with a moderate assumption to account for consumers’ similarity. That is, we assume that Assumption 1 applies to all consumers, regardless of heterogeneity.