Coronavirus Pandemic and Unemployment: Evidence from Mobile Phone Data in China*

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Abstract

Based on mobile phone records for 71 million users and location tracking information for one million users over two years, this study examines the labor market impacts of the COVID-19 pandemic in China's Guangdong province, whose GDP is larger than all but the top 12 countries in the world. Using a standard difference-in-differences framework, our analysis shows dramatic and protracted effects on the labor market: the pandemic increased unemployment by 72% and unemployment benefit claims by 57% in September 2020, nearly five months after the full reopening. The impact is also highly heterogeneous with females, workers older than 40, and migrants being affected more. Cities that rely more on export or have a higher share of GDP in the hospitality industry but a lower share in the finance and healthcare industries experienced a more pronounced increase in unemployment. The lingering impact likely reflect the global nature of the pandemic and the interconnectedness of the world economy.

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1 Introduction

Effective and targeted policies to address the adverse consequences of the COVID-19 pandemic for the economy rely on prompt and accurate measures of the labor market effects across different demographic groups and geographic regions. Traditional measures of labor market outcomes, in particular unemployment rates, are based on surveys. In addition to the substantial time lag and limited availability for small geographic areas, statistics inferred from surveys suffer from considerable uncertainty and are routinely revised.[1]

In China, information on unemployment is derived from the number of individuals who registered with the unemployment benefit agencies prior to 2018 and supplemented by household surveys afterward.[2] Measuring unemployment accurately is particularly challenging due to a large fraction of the population who do not have local household registrations (Hukou) and hence excluded from the unemployment surveys. Besides, reporting and aggregation errors, as well as potential data manipulations, have also been documented (Giles et al., 2005; Liu, 2012; Cai et al., 2013). China's national unemployment rate varies between a tight range of 3.1%-4.3% over the past two decades leading to questions about its reliability (Feng et al., 2017), especially in the face of rapid and unprecedented social and economic changes brought about by the pandemic.[3]

This study leverages high-frequency and high-resolution mobile phone usage data in Guangdong, the most populous province in China, with a GDP larger than all but the top 12 countries in the world. Our primary data source consists of location tracking information for one million randomly selected users and mobile phone records for 71 million

[1] In addition to the delay, it is technically challenging to measure unemployment accurately. Their quality also varies considerably over time due to changes in participation rates, modifications in the survey methodology, inconsistencies and measurement errors in sample responses, or rotation group bias (Poterba and Summers, 1986; Jones and Riddell, 1999; Card, 2011; Feng et al., 2017; Meyer et al., 2015; Krueger et al., 2017; Heffetz and Reeves, 2019).

[2] See for example an report at https://www.marketwatch.com/story/china-unveils-urban-survey-unemployment-rate-2018-04-17-1485030.

[3] Please refer to this Wall Street Journal article at https://www.wsj.com/articles/chinas-jobs-rebound-doesnt-appear-as-robust-as-the-government-claims-11591551390.

users from January 2018 to September 2020.[4] We examine the pandemic's labor market impacts for various demographic groups and across cities with different industrial structures by employing the standard difference-in-differences (DID) framework. We use year 2020 as the treatment group and year 2019 as the control group. The key identification assumption is that the labor market outcomes would track each other between the two groups in the absence of the pandemic, hence the observed differences could be attributed to the pandemic rather than time-varying unobservables. Results from event studies provide a strong support this common trend assumption between the two groups prior to the event date.

We leverage two unique data features to estimate the impact on the labor market: a) the number of individuals who stopped work commute during an extended period of time (non-commuters) as a measure of unemployment, and b) the number of unique individuals who contact the unemployment benefits agencies via the designated hotline (12333) as a measure of unemployment benefit claims. We first validate these two measures and then provide several pieces of evidence to show that our unemployment impact is unlikely to be driven by work-from-home (WFH), a key confounder to interpreting the results based on commuting patterns. We also conduct a host of robustness checks and find that our results are robust to variable definitions, data selection and model specifications.

Several key findings emerge from our analysis. First, the pandemic has increased unemployment in Guangdong by 72% and unemployment benefit claims by 57% in September 2020, nearly five monthly after the full reopening. The effect did not show a diminishing trend within the five-month window before September 2020, the end of our data period. The sharp rise in unemployment is much higher than the government statistics that report an increase of 13.3% in Guangdong province's unemployment rate (from 2.26 percentage points in January-March to 2.56 percentage points in July-September) during the same period.

[4]Researchers in recent studies have used mobile phone data to improve labor market measurements (Toole et al., 2015; Barwick et al., 2019), track human movement in real-time and at a fine spatial scale, and quantify the pandemic's impact on voting behaviors, mobility, and social contacts (González et al., 2008; Ahas et al., 2010; Couronné et al., 2013; Chen et al., 2018; Kreindler and Miyauchi, 2021; Chen et al., 2020; Atkin et al., 2020; Chen and Pope, 2020; Couture et al., 2020; Gupta et al., 2020).

Second, the pandemic's impact on unemployment is highly uneven across demographic groups and more pronounced among females, people over 40, and especially migrants. The escalating increase in unemployment among migrants shows no sign of abatement during our sample period. This echoes a massive reduction in the reported migrant workers by the National Bureau of Statistics (NBS) and indicates the possibility of a large-scale layoff among this group.[5]

Third, the pandemic's impact is more substantial in cities with a high labor share of hospitality, real estate, or transportation industries but less severe in cities where employments are concentrated in finance, health care, or education industries. In addition, the impact is more pronounced in cities that rely heavily on export, reflecting the global nature of the shock in an interconnected world economy. Industry compositions account for 39% of the heterogeneity in the pandemic's unemployment impact across cities, while trade exposure contributes to 27% of the heterogeneity.

Lastly, our results unmask the severity and uneven impact of the pandemic's labor market implications, which speak to the importance of conducting analysis at granular levels. In addition, these results illustrate the rippling effect of the pandemic across cities within a country and countries worldwide through the supply chain and the trade channels. The industry composition of a city (or a country), exposure to trade, and the nature of the supply chain are crucial factors in determining the pandemic's effect on its economy. Our measures help us understand the pandemic's labor market impact at a granular level and inform targeted policies to help the most severely affected groups and regions.

The rest of this paper is organized as follows: Section 2 discusses the context of the study and provides descriptive evidence. Section 3 lays out the empirical framework. Section 4 presents the results and Section 5 concludes.

[5]Please refer to this NBS article at http://www.stats.gov.cn/tjsj/sjjd/202010/t20201019_1794729.html.

2 Background and Data

2.1 Background and Data Sources

Exploiting the increasingly available high-frequency and high-resolution mobile phone data is particularly advantageous for China, as its cellphone penetration rate is high among developing countries. According to the 2018 China Family Panel Studies, a nationally representative longitudinal survey of individuals' social and economic status, 89% of correspondents sixteen years and older reported possessing a cellphone. In addition, each household owns 2.5 cell phones on average according to the National Bureau of Statistics (2018). Appendix Figure A.1 shows a strong correlation between the number of China Mobile users and the number of residents by city. Cities with a higher GDP per capita (represented by the size of the circles in Figure A.1) tend to have higher mobile phone ownership.

The context of our analysis is Guangdong, the most populous province with the largest provincial GDP in China. Guangdong contributes to 11% of China's GDP and around a quarter of China's foreign trade (China Statistical Yearbook 2020). Its major cities include Shenzhen and Guangzhou, among the wealthiest and economically most advanced cities in China. As Table A.2 illustrates, cities in Guangdong differ substantially in terms of both population and GDP in 2019.[6] The economy of Guangdong is widely recognized as the most dynamic and resilient among all provinces in China (World Bank, 2010; Gong et al., 2020). Another reason that makes Guangdong relevant is that the number of daily confirmed COVID cases are under a few handfuls since the full reopening (Appendix Figure A.2), similar to most other provinces in China. Our measures on the pandemic's consequences could apply to other regions as well.

Our data come from China Mobile, the dominant cellular service provider in China. We have access to detailed phone usage (encrypted IDs of the calling party and the receiving

[6] Among the 21 cities, Guangzhou has the largest population (15.31 million), while Yunfu only has a 2.55 million population. The economic scale of the largest city, Shenzhen, at $390 billion in 2019, is almost 30 times as large as that of the least city, Yunfu.

party, date of calls, and call duration in seconds) for all of its 71 million users in Guangdong Province from January 2018 to September 2020, accounting for 63% of all mobile users in the province. We observe some user demographic information, such as age, gender, and the place where the phone number is registered. In addition, we have access to the location records every five-minute interval for one million randomly selected users during the same period.

Guangdong's lockdown Guangdong's provincial government acted swiftly and adopted vigilant procedures since the onset of the pandemic. Guangdong was one of the first provinces to release detailed information (frequency, location, gender, etc.) on the newly confirmed cases, starting from as early as February 3, 2020. These procedures proved successful and have kept the number of daily confirmed cases under a few handfuls since the full reopening (Figure A.2). As shown in Figure A.2, the daily confirmed new cases reached a peak of 254 on January 31 and quickly reduced to under 50 three weeks into the lockdown period. The number of cases has been modest since then and varies between 0 and 34 throughout the Phase I and Phase II reopening.

The lockdown in Guangdong lasted 32 days from January 23 to February 24, 2020. The provincial government issued an order on February 6, 2020, and encouraged workers in some industries to get back to work after February 24. It is worth noting that the lockdown procedures in Guangdong are not as strict as the lockdown procedures implemented in the epicenter Wuhan. On February 24, 2020, Guangdong province entered Phase I reopening, which lasted 76 days. During Phase I reopening, people were allowed (and encouraged in certain industries) to go back to work and visit outdoor public places. Phase II reopening, or full reopening, officially started on May 9, 2020, when all businesses, including shopping malls, supermarkets, and restaurants, were allowed to open fully. The only exception was movie theaters that remained closed till mid-July.

While Guangdong's number of COVID cases is low, it does not imply that the pandemic has a modest or no effect on the local economy: the measures implemented to reduce the health impact of the pandemic could significantly affect the economy. As shown in our analysis in the main text, the pandemic has inflicted sizeable damage to Guangdong's labor market, leading to a 72% increase in the unemployment people and a 57% increase in unemployment benefit claims by in September 2020. As Guangdong's economy is among the most vigilant across provinces in China, the aggregate labor market implications could be much more severe than those suggested by the national statistics.

2.2 Unemployment Measures

We leverage two features of the mobile phone data to understand the impact of the pandemic on unemployment rate and unemployment benefit claims, respectively.

Work commute The first feature of the mobile phone data is the location tracking information (in longitude and latitude) collected by mobile devices during 5-minute intervals except when they are powered off.[7] We randomly select one million mobile users and use their location information at 5-minute intervals from January 2018 to September 2020 to construct their job and home locations. We define the work location as the location where a user spends at least 5 hours a day between 9 am and 6 pm for at least fifteen workdays in a given month. The home location is similarly constructed, except that we use the location with the longest duration between 10 pm and 7 am each month.[8] These geocoded locations trace out individuals' spatial trajectories over time and allow us to record the time of arrival and departure at job locations.

We provide two pieces of evidence that our assignment of home and work locations

[7] Recent developments and the widespread diffusion of geospatial data acquisition technologies have enabled the creation of highly accurate spatial and temporal data. Passive collection of geolocation information – which underlies our data collection procedure – works on all traditional mobile networks (2G, 3G, or 4G). Researchers have used such mobile positioning data to study urban and transportation issues (González et al., 2008; Ahas et al., 2010), though few studies exploited long panels of location data to examine labor market dynamics (Barwick et al., 2019).

[8] Location information from 7 am - 9 am and 6 pm -10 pm is discarded because people are likely on the move during these time intervals.

captures an intuitive spatial distribution of users in our sample. First, we use the coordinates of work and residential locations to compute the commuting distance for users with valid job location information. The distribution of commuting distance decays exponentially (Figure 1), which is consistent with evidence from other studies using both cell phone data and household surveys (Miyauchi et al., 2020; Rao, 2021). Additionally, the average commuting distance in our sample period is around 6.6km, close to the average commuting distance of 6.9km reported in the 2017 travel survey by the Guangzhou Municipal Transportation Bureau (GMTB). Second, for the city of Guangzhou (the provincial capital city), Appendix Figure A.3 plots the log difference between the number of users at 11 am and the number of users at 11 pm, averaged separately for weekdays and weekends in 2019. The figure includes all geographic locations recorded in the data. On both weekdays and weekends, the center of the city gains population, and the suburbs lose population during the daytime relative to the nighttime. But these differences are much more pronounced on weekdays than weekends, especially in the center of the city. For instance, the enlarged area is a famous industrial park in Guangzhou, which clearly shows that the daytime population is much larger than the nighttime population. The differences are amplified on weekdays. The spatial pattern of population density is consistent with those from the report by GMTB.

We use reductions in the number of people working on-site before and after the lockdown and relative to 2019 as our measure of pandemic-induced unemployment. Changes in commuting patterns, especially on a continuing basis, could provide a valuable barometer of changes in unemployment, especially when participation in the unemployment benefit programs is low (as is the case in China). To the extent that some of these changes reflect more flexible work modes post the lockdown, such as work-from-home (WFH), they should be interpreted as an upper bound estimate on pandemic-induced unemployment. However, we provide multiple pieces of evidence below that our measure of unemployment based on commuting patterns over an extended period of time is unlikely to be driven by WFH.

Calls to unemployment hotline The second feature is detailed call records (time and duration of each call) to the designated government hotline (12333) for unemployment benefits. The hotline offers comprehensive one-stop social insurance public service, provides eligibility information, helps with unemployment registration, and facilitates applications for unemployment benefits. Relative to filing online or visiting local social security bureaus, calling the designated hotline 12333 is the preferred choice for many due to its simplicity and all-inclusive help from customer services. Figure 2 shows the weekly Baidu Index for the keywords – "12333" and "unemployment insurance" – in Guangdong province from 2019 to 2020. The correlation of the Baidu Index of the two keywords is 0.83 during the sample period. The co-movement of the index for the two keywords offers additional support for using the 12333 hotline as a proxy for individuals claiming unemployment benefits.

Despite the popularity of the hotline, the number of individuals making calls to 12333 provides an estimate for the level of unemployment benefit claims. It could also serve as a a lower bound for the effect on unemployment: not all unemployed workers reach out to government agency to claim unemployment benefits, especially those who are optimistic about finding a new job soon. In addition, the lifetime unemployment benefits in China are capped at 24 months, thus limiting choices for people who have exhausted benefits in the past. Therefore, instead of focusing on the level of unemployment calls, our analysis below exploits its changes. We show that the measure of changes in unemployment calls can provide useful information on short-run labor market dynamics otherwise unavailable through official statistics.

As people might reach out to the hotline multiple times to claim their unemployment benefits, we treat the multiple calls from the same number as one incident of claim, and therefore use the number of individuals calling the unemployment hotline, instead of the number of calls to 12333, to construct our first unemployment measure. In addition, calls that failed to go through to the receiving party and calls shorter than 30 seconds are excluded from the analysis. For brevity, the term "the number of individuals calling the unemployment hotline" and "the number of unemployment calls" are used interchangeably throughout the analysis. Appendix Figure A.4 plots the number of individuals calling the unemployment hotline across cities in 2019. The correlation between city-level unemployment calls and the official unemployment rate released by the NBS, which is only available annually for city-level statistics, is reasonably high at 0.7 in 2019.

In our analysis based on unemployment calls, we use the number of individuals making calls to the 12333 hotline, instead of the number of calls to 12333, to address the issue that people might reach out to the hotline multiple times to claim their unemployment benefits. In other words, we only count the first time when a user reaches out to the unemployment benefit hotline. We aggregate the duration of all subsequent calls when we measure call duration to the hotline. Our main analysis excludes users under the age of 18, as the individuals under the age of 18 are unlikely to be working subject to the Law on Protection of Minors. Results excluding users under the age of 25 (to eliminate those still in school) are almost identical (see appendix).

Some of our analyses examine heterogeneous labor market prospects between migrants and non-migrants. It is important to note that migrants who had been working in Guangdong but did not have Guangdong Hukou became eligible for unemployment benefits since 2014.[9] This was largely designed by the Guangdong government to attract migrants and to help improve labor relations. As we do not observe whether an individual has local Hukou status – the official definition of migrants, we define migrants as individuals who registered their phone numbers outside Guangdong province. This is an imperfect measure of local Hukou status. Workers from outside Guangdong can buy and register their mobile phones in Guangdong and be treated as non-migrants in our analysis. Consequently, the actual gap in unemployment between migrants and residents might be even larger than our estimates.

[9]Please refer to the announcement by Human Resources and Social Security Department in Guangdong, https://www.gdhrss.gov.cn/sy/20140801/10101.html.

3 Empirical Framework

Our analysis employs the difference-in-differences (DID) approach by comparing labor market outcomes in 2020 before and after the event date (when Guangdong implemented the lockdown) with those before and after the same (lunar) calendar dates in 2019. As Guangdong's lockdown occurred two days before the 2020 Chinese New Year, we use the lunar calendar instead of the standard almanac calendar to define the event date. Specifically, the event date for 2020 is January 23, 2020, while the event date for 2019 is February 3, 2019, two days before the Chinese New Year in the lunar calendar. We use the year 2020 as the treatment group and the year 2019 as the control group. In other words, our analyses compare changes in labor market outcomes before and after the event date in 2020 with changes in labor market measures before and after the exact event date in 2019.

The lockdown in Guangdong lasted 32 days, from January 23 to February 24, 2020. On February 24, 2020, Guangdong province entered Phase I reopening, which lasted 76 days. During Phase I reopening, people were allowed (and in certain industries, people were encouraged) to go back to work and visit outdoor public places. Phase II reopening, or full reopening, officially started on May 9, 2020. All businesses, including shopping malls, supermarkets, and restaurants, were allowed to reopen fully. The only exception was movie theaters that remained closed till mid-July. We delineate the interval from the 60 days before the lockdown to the 252 days after the lockdown into four periods: before lockdown (60 days), during the lockdown (32 days), Phase I reopening (76 days), and Phase II full reopening (144 days).

To control for potential differences in time-varying unobservables, we include a rich set of fixed effects such as day-of-week, event-day, holiday, and the treatment group fixed effects. The identification assumption is that after including these controls, there are no systemic differences in time-varying unobservables between the two groups in the absence of the pandemic. Results from event studies support this common trend assumption between the two groups prior to the event date. We use the following DID framework and ten-day intervals to trace out the dynamic impact of the pandemic over time:

$$y_{cit} = \sum_{q=-5}^{24} \beta_q \cdot d_i \cdot \mathbb{1}\Big(t \in [q * 10 + 1, (q+1) * 10]\Big) + \alpha_c + \gamma_i + \eta_t + \xi_{it} + \varepsilon_{cit}, \qquad (1)$$

where c denotes a neighborhood (a cell-tower area), i denotes the treatment group (year 2020) or the control group (year 2019), and t denotes the event-day (t=0 stands for January 23 in 2020 and February 3 in 2019). The event window is sixty days before the lockdown and 252 days post the lockdown.

y_{cit} is the outcome variable, such as the log number of non-commuters. We report results based on log(outcome+1) to avoid taking the logarithm over zero. However, results based on the inverse hyperbolic sine function (which is very similar to the log function and can handle zero values) are very similar. β_q are event-study coefficients, capturing differences between the treatment group and the control group. Variable d_i is a dummy, that is equal to one for the treatment group. $\mathbb{1}(.)$ is an indicator variable for each 10-day interval of the sample. We control for neighborhood fixed effects γ_c, group fixed effects γ_i, and 312 event-day fixed effects η_t. We also control for the holiday fixed effect ξ_{it} , that varies by group and time (e.g., the International Labor Day holiday falls on different lunar calendar days in 2019 and 2020) as well as day-of-week fixed effects. Standard errors are clustered at the event-day level.

Due to the nature of the key regressor being a dummy variable, $\hat{\beta}$ itself is not a consistent estimator of the impact on unemployment in percentage term and the bias is larger when $\hat{\beta}$ is farther away from zero in out case. While we report $\hat{\beta}$ in all of our figures and tables, we interpret the percentage impact using $100 * \Big(\exp{[\hat{\beta} - \widehat{var(\beta)}/2]} - 1\Big)$ throughout the paper. This is a consistent estimator of the percentage impact and the second component in the bracket reduces the finite-sample bias.

To further explore the heterogeneity across cities and the importance of industrial composition and trade exposure, we employ the following specification:

$$
\begin{aligned}
y_{cit} \quad = \quad & d_i \cdot \mathbb{1}\Big(t \in [0, 252]\Big) \cdot \boldsymbol{Z}'\tau + d_i \cdot \boldsymbol{Z}'\mu + \mathbb{1}\Big(t \in [0, 252]\Big) \cdot \boldsymbol{Z}'\rho \qquad (2) \\
& + \beta \cdot d_i \cdot \mathbb{1}\Big(t \in [0, 252]\Big) + \alpha_c + \gamma_i + \eta_t + \xi_{it} + \varepsilon_{cit},
\end{aligned}
$$

where $\boldsymbol{Z}$ is a vector of city attributes in 2019. η, μ, and ρ are corresponding coefficients. For example, $\boldsymbol{Z}$ could be a city's labor share in each of the 13 major industries, dummies for the 21 cities, or a city's export-over-GDP ratio. In addition to the interaction between the pandemic treatment and city attributes, we control for all lower-level interactions in the regression. Variables d_i, $\mathbb{1}(.)$ and the fixed effects α_c,γ_i, η_t, ξ_{it} are the same as in Equation (1). The key coefficient is η, which measures the heterogeneous impact by city characteristics $\boldsymbol{Z}$ based on their values in 2019. Unlike in Equation (1), where we estimate the pandemic's impact for each ten-day interval, here we estimate the average effect η over all periods and focus on heterogeneity across industries and cities.

The regression analyses using the number of commuters are analogous, except that the observation is at the neighborhood and time-window level. For example, for the commuter definition that uses two weeks as the relevant time window, the observation is at the neighborhood-fortnight window. We drop the day-of-week fixed effects, replace event-day fixed effects with event-fortnight fixed effects and cluster the standard errors at the fortnight level.

China Mobile delineates Guangdong province into 787 mobile phone cell tower areas (similar to zip codes in the U.S.) for billing purposes. All regressions control for cell-tower-area (or neighborhood) fixed effects. Regressions for unemployment calls aggregate the sample to the neighborhood and day level, with a total of 489,514 observations. Regressions for commuting patterns further aggregate to the neighborhood-week, neighborhood-fortnight, and neighborhood-month level when appropriate.

4 Empirical Results

4.1 Event Studies

We present two sets of even studies with the first being based on location tracking data and the second being based on the calls to designated unemployment hotline.

Non-commuters We exploit variation in commuting patterns based on the location tracking data for one million randomly selected users. We treat an individual as commuting to work for a given time window (e.g., two weeks) if he visits his work location at least once during that time window. To accommodate the possibility of (partial) work-from-home (WFH) during and after the lockdown, we have constructed three commuter definitions using different time windows: a week, two weeks, and a month. For example, under the definition that uses a month as the relevant time window, an individual is classified as a commuter for a given month if he visits a work location at least once in that month. Figure 3 shows the event study of non-commuters as a measure for unemployment with the same period in 2019 as the control group. Panel (a) depicts the changes in the number of non-commuters while panel (b) is based on the number of non-commuters who also stopped using email/virtual meeting apps. For 2020, the event date (or day zero) is the lockdown, January 23, 2020, two days before the Chinese New Year. Correspondingly, the event date in 2019 is February 3, 2019, also two days before the 2019 Chinese New Year. Phase I reopening started on February 24, 2020, 32 days after the lockdown, when people were allowed to go back to work and visit outdoor public places. Phase II reopening, or full reopening, started on May 9, 2020, 108 days after the lockdown. Shopping malls, supermarkets, restaurants were allowed to fully reopen.

There are three salient patterns from both panels. First, before the lockdown period, there was virtually no difference in the number of non-commuters between 2019 and 2020, lending support for the parallel trend between the treatment and control groups, the key identification assumption of our analysis. Second, during the lockdown, the number of non-commuters increased sharply by more than five folds in 2010 relative to that in 2019. The increase could reflect not only the changes in unemployment and but perhaps more importantly temporary leaves with/without pay or work from home due to the strict nature of the lockdown. Third, as the economy opens up, the increase gradually came down to about 70% by the end of Phase I reopening and the change remained stable by the end of September, even four months after Phase II (or full) reopening. Panel (b) focuses on the number of non-commuters who also stopped using emails or virtual meeting apps in order to exclude people who work from home. While the increase is slightly smaller but the pattern stays the same. While the impact on the labor market is dramatic, it is milder relative to that observed in the U.S. during the same period: the unemployment rate in the U.S. increased from about 3.6% in the second half of 2019 to 13% in the second quarter of 2020 and 8.8% to the third quarter according to the U.S. Bureau of Labor Statistics.

Work-from-home One could argue that the increase in non-commuters during Phase II reopening may be at least partly driven by the increase in work-from-home (WFH). We present four pieces of evidence that our non-commuter definition – not visiting the workplace at all during an extended period of time (such as a week, two weeks, or a month) – reflects individuals' unemployment status and that changes in non-commuters are unlikely to be primarily driven by WFH.

First, we examine the usage of all 51 virtual meeting apps and email apps that are available in the Apple store and Andriod gallery among commuters and non-commuters.[10] Using the two-week window to define commuters, the shares of commuters using these apps at

[10] To gauge the prevalence of WFH, we obtain an exhaustive list of all 21 virtual meeting apps and 30 email apps available in the Apple store and Andriod gallery. The virtual meeting apps include: Tencent meeting, DingDing, Zoom, SKYPE, uu online, Alitong online, Ailiao, Chubao, Feige, Feiyin, Laidian, Shangqitong, Shuoba, Tongtong, Weihui, Weiwei, Yiliao, Youhuatong, Youliao, Youxin, Zhangshangbao. The email apps include: 139 mail, 139 light mail, 189 mail, 21CN mail, 21CN light mail, 263 mail, Gmail, Live mail, Microsoft Outlook, QQ mail, TOM, Ali mail, Ke space, Sohu mail, Qixinbao, Tencent mail, Wangyi mail, Woo mail, Mi mail, Sina mail, Yahoo mail, Youqia, Mail master, Yun home, China Mobile mail, Baidu mail, Hotmail, Foxmail, Coremail.

least once during the two-week window were 30.3%, 26.1%, 24.7% during the lockdown, Phase I, and Phase II reopening periods, respectively. In contrast, the shares of non-commuters using any virtual meetings and email apps at least once during the two-week window were 5.2%, 0.6%, and 0.06% during the lockdown, Phase I, and Phase II reopening periods, respectively. The patterns are very similar when we a) limit to virtual meeting apps, and b) use one week or one month as the relevant time window to define commuters and non-commuters. Furthermore, the sharp contrast in usage patterns between commuters and non-commuters is very similar in 2019: the share of commuters using these apps at least once during the two-week window was 21.2%, relative to 1.0% among non-commuters. If our measure of non-commuters in 2020 is primarily driven by a significant increase in the fraction of workers who telecommute in 2020 relative to 2019, we would expect the virtual meeting app usage patterns to be very different over these two years. We would also anticipate a much higher usage of virtual apps among non-commuters in 2020. Neither prediction is supported by data.

Second, our three commuters (non-commuters) measures have a high correlation (exceeding 0.92). More than 94% of individuals who are non-commuters over two weeks remain non-commuters over the entire month. These patterns hold in both 2019 and 2020. Suppose non-commuters in 2020 mostly consist of people who work from home and visit offices once in a while, we should anticipate the persistence in non-commuting patterns to be lower in 2020 than that in 2019, in contrast to what we observe. These patterns provide evidence that our commuter measures accommodate flexible work modes (e.g., WFH from time to time). When individuals stop visiting their workplace over an extended period, as defined in our analysis, they are most likely not working rather than WFH.

Third, we restrict the sample to people who have made calls to the unemployment hotline and examine their commuting patterns. Appendix Figure A.5 depicts the cumulative probability of ever stopping commuting (for at least two weeks) among these callers with respect to days to unemployment calls. The two lines represents the pattern separately for 2019 and

2020. The cumulative probability increases from less than 20% thirty days before the calls to almost 90% by the 90 days after the calls. If the increase in non-commuters were driven by WFH, we would have observed different patterns, i.e., a lower probability in 2020 than in 2019. The pattern being very similar between the two years provides further evidence that the increase in unemployment in 2020 relative to 2019 were unlikely to be driven by WFH.

Fourth, we examine the impact of the pandemic on non-work trips by comparing the number of non-work trips in 2020 and that in 2019. Appendix Figure A.6 shows the event study plot. The pattern is consistent with that in Figure 3. Before the lockdown period, there was no difference in the number of non-work trips between 2019 and 2020, but there was a sharp drop during the lockdown period. The number of non-work trips gradually recovered during the Phase I reopening. It fully recovered and even showed a slightly increase during the Phase II reopening. The pattern is in line with the anecdotal evidence that economic activities have largely returned to the pre-pandemic level. This further lends support on the limited role of WFH by the end of Phase II reopening.

Calls to unemployment hotline We then examine the impact on unemployment benefit claims based on calls to 12333. Figure 4 depicts the differences in the daily number of individuals calling the unemployment hotline between 2019 and 2020. Similar to Figure 3, there does not appear to be a differential trends in the calls to 12333 between the two years before the lockdown period, leading credence to the parallel trend assumption. Opposite to the increase in non-commuters shown in Figure 3 during the lockdown, the number of calls dropped significantly. This is likely due to the uncertainty about the severity and duration of the pandemic during the initial stage. In addition, the increase in the number of non-commuters during the lockdown was likely driven by changes in work arrangement than unemployment. However, as the severity of the pandemic unfolded in China, the number of individuals calling the unemployment hotline increased sharply one month after the beginning of Phase I reopening, and the trend continued to the end of our data period.[11] Interestingly, the increase in the number of calls to 12333 stabilized at about 50% by the end of the Phase I reopening and remained till the end of our data period. Both the pattern and magnitude are consistent with those in Figure 3. In contrast, the initial claims of unemployment benefits in the U.S. skyrocketed from 0.2 million in February 2020 to 6.1 million in April 2020, and gradually decreased to 0.8 million in the end of September according to the U.S. Bureau of Labor Statistics.

4.2 Regression Results

Figures 3 and 4 provide compelling evidence that the pandemic has greatly affected the labor market in 2020 and the changes were unlikely caused by other unobservables and WFH. We now use Equation (1) to quantify the pandemic's effect on unemployment. All regressions include event-day, day-of-week, holiday, treatment group, and neighborhood fixed effects. Similar to the discussion on data patterns above, our regression analysis also consists of two parts. The first part leverages changes in commuting patterns and the second part exploits variation in the number of individuals making calls to the unemployment hotline. As our key explanatory regressor being a dummy variable, we interpret the coefficient estimates following this formula $(100 * (\exp[\beta - var(\beta)/2] - 1))$ to be more precise, though we report β in all the figures and tables.

4.2.1 Effect on unemployment

Table 1 reports parameter estimates for the percentage increase in non-commuters following Equation (1), grouping the ten-day intervals into four periods. During the lockdown period, the number of non-commuters increased by nearly 43 folds, reflecting the draconian nature of

[11] Some of the time-series variations in the figure reflect differences between weekdays and weekends and during holidays. For example, few people reached out to the unemployment hotline right before the Phase II reopening, which coincides with International Labor Day, a 5-day public holiday from May 1 to May 5 (event day 100 to event day 104).

the lockdown, rather than the increase in unemployment. The number of non-commuters increased by 163% during the Phase I reopening and by 72% during the Phase II full reopening. In 2019, the average number of individuals who stopped commuting to their work location for at least two-weeks and had no new job location was 38,729, or 7.4 percentage points. The increase of 72% during the Phase II reopening relative to the baseline in 2019 implies an 5.3 percentage point increase. As discussed in the previous section, WFH is unlikely play a large role especially toward the end of the Phase II reopening, we therefore interpret the 72% increase in non-commuters as the impact of the pandemic on unemployment at that time. As discussed in Section 4.4 below, the effect size is robust to alternative window lengths of one-week or one-month in defining non-commuters.

Using commuting patterns to measure unemployment has a significant advantage over measures derived from applications for unemployment benefits: it is not subject to participation bias (eligible people do not participate). Due to the inertia, lack of information, stigma, time, and "hassle" associated with applications, it is estimated that 66% of eligible households do not participate in major social programs in the U.S. (Ribar, 2020). Non-participation is much more severe in developing countries due to the limited program benefits. As a result, commuting patterns observed over an extended period of time provide a real-time and likely more accurate indicator of the underlying labor market dynamics.

Column 2 of Table 1 examines changes in work duration among individuals working on-site. Hours on-site dropped by 19% and 8% during the lockdown and Phase I reopening periods, respectively but returned to the 2019 level in Phase II reopening. The pandemic does not seem to have brought about dramatic changes in the nature of working on-site during the Phase II reopening, lending further support to our strategy of measuring unemployment based on changes in individuals commuting to work.

4.2.2 Effect on unemployment benefit claims

For regressions that examine changes in unemployment calls, we utilize a total of 489,514 neighborhood-day observations. Panel A of Table 3 presents the coefficient estimates of β_q in Equation (1), except that the ten-day intervals are grouped into four periods: 1-30 days before lockdown, during the lockdown, Phase I reopening, and Phase II full reopening. Column (1) shows the pandemic's impact on the number of unemployment calls, while column (2) examines call duration. Echoing results in Figure 8, the number of individuals calling the unemployment hotline decreased by 34% during the lockdown and increased by 28% during Phase I reopening and nearly 57% during Phase II full reopening. The call duration displayed a similar pattern: the average call time dropped during the lockdown but increased after the reopening. Call duration increases partly because more migrants applied for unemployment benefits after the reopening (as we show below in heterogeneity analysis) and that they generally need to provide more information than local residents.

Panel B of Table 3 further groups the three periods during and post the lockdown into one group. The coefficient estimates directly measure the pandemic's average labor market effect. Overall, the pandemic has increased the number of unemployment calls by 27% in aggregation from the lockdown to the end of September 2020. This is very similar to the magnitude discussed above when we compare the raw cumulative number of individuals calling the unemployment hotline between 2020 and 2019, suggesting that the role of confounders (as captured by our rich set of fixed effects and controls) is limited. As discussed above, the level of unemployment calls is a lower bound estimate of the number of unemployed, as not all individuals who have lost jobs file for unemployment benefits. However, in light of the remarkable similarity in unemployment calls between 2020 and 2019 prior to the pandemic, the percentage change in unemployment calls during the pandemic period estimated in Table 3 (27%) is likely a reliable measure of the percentage change in unemployment benefit claims as a result of the pandemic.

4.3 Heterogeneity analysis

Unemployment To examine the pandemic's differential impacts across demographic groups, we repeat the baseline event-study analysis shown in Panel (a) of Figure 3 by gender, age, and migrant status and plot coefficient estimates in panels (a)-(c) of Figure 5. Specifically, the dependent variable is the difference in the logarithm outcome variable (i.e., non-commuters) between females and males (panel a), between individuals 40 years old and above and those under 40 (panel b), and between migrants and non-migrants (panel c). Females are more affected by the pandemic. The number of non-commuters among females was about 10-25 percentage points higher than that among males during lockdown and Phase I reopening. However, the gap became smaller and statistically insignificant during the Phase II reopening.

Older workers fared worse than younger cohorts: the number of non-commuters among workers 40 and above was about 20-60 percentage points higher than those under 40 during the lockdown and Phase I reopening periods. Even during the Phase II reopening, the gap between the two age groups still remained at about 20 percentage points. Lastly, migrants are most severely affected by the pandemic. The number of non-commuters among migrants increased more during lockdown but especially the Phase I reopening relative to that among non-migrants. By the end of the Phase II reopening, the number of non-commuters among migrants was still about 40 percentage points higher than that among non-migrants, highlighting the disproportionate large and lingering burden on migrants. According to the NBS, there were 291 million migrant workers in 2019, constituting 36% of the total workforce nationwide. By September 2020, the number dwindled to 179 million. The escalating number of migrants reaching out to the unemployment benefit office in our sample is consistent with the massive reduction in migrant workers reported by the NBS and suggested a large-scale layoff among migrant workers.

To further examine the heterogeneous impact across demographic groups, we conduct an analysis at the neighborhood level in Guangzhou. Specifically, we regress the percentage change the number of non-commuters between 2019 and 2020 on the quadratic forms of average housing price and the migrant share based on 2018 data in each neighborhood (i.e., cell-tower-area). Figure 10 plots the predicted percentage changes against the average housing price and the migrant share. It shows that the impact on unemployment is stronger in neighborhoods with a lower housing price and a higher share of migrants. These results corroborate the existing literature documenting that the least advantaged social groups, including migrants, are most vulnerable to adverse shocks and risks (Banerjee and Duflo, 2012). Finally, the disproportionately more harsh impacts on females, older workers, and migrants likely reflect the pandemic's heterogeneous shocks across industries. These groups are more likely to work in hospitality industries, including restaurants and hotels, which have been hard hit by the pandemic, and less likely to work in the less affected education and high-tech industries.

There is considerable variation across cities in Guangdong in terms of population and GDP (Appendix Table A.2). Panel (a) of Figure 6 examines the impact of heterogeneity across cities. The figure reports coefficients on the interactions of the pandemic treatment variable (which is one from January 23, 2020, to September 30, 2020) and city dummies, following Equation (3). Heterogeneity across cities is sizeable with the number of non-commuters increasing by 20-150% for all but three cities. The economically more developed cities such as Guangzhou and Zhuhai experienced the largest increase while the less developed cities such as Yangjiang and Shaoguan seemed to be unscathed. At least two factors drive the differential effects across cities. First, cities have different industry compositions. Among the seven cities that experienced the most significant increase in unemployment calls, the average share of the workforce in hotel and catering, real estate, and transportation was 13.9% in 2019, while the average share was less than 3% among the seven least affected cities. To illustrate the heterogeneous impact across industries directly, we run a separate regression following Equation (3), where we interact the pandemic treatment variable with city-level labor shares by the industry for all thirteen major industries.

Panel (b) of Figure 6 reports coefficients for all industries. The hotel and catering, real estate, and leasing and business experienced the largest increase in non-commuters, ceteris paribus. In comparison, the finance, health care, and education sectors witnessed reductions in non-commuters after the lockdown in January, consistent with findings using data from other countries (Adams-Prassl et al., 2020; Alon et al., 2020). To evaluate the importance of industry compositions, we predict the number of non-commuters in logs (the dependent variable) using coefficient estimates and each city's observed labor share across industries and compare the range of predicted values with the observed range of the dependent variable. Variation in industry composition across cities contributes to 38.7% of changes in the unemployment rate.

In addition to differences in industrial composition, cities also have differential trade exposure measured by the total export relative to local GDP in 2019. For the 21 cities in Guangdong, the median export-to-GDP share in 2019 is 14.7%. Shantou city has the least exposure to international trade, whose export-to-GDP ratio is only 2.5%. At the other extreme is Dongguan, whose export-to-GDP ratio is 91.0%. As shown in panel (a) of Figure 6, the pandemic's impact on Shantou's unemployment is much milder relative to that on Dongguan. In Table 2, we interact the pandemic treatment variable with a city's export-to-GDP share. As expected, the interaction coefficient is statistically significant and positive. A one percentage point increase in 2019's export-to-GDP ratio is associated with a 0.37% increase in the number of non-commuters for a given city. Similar to industry compositions, variation in the export-to-GDP ratio is also critical and explains 28.5% of the heterogeneity in the pandemic's unemployment impact across cities.

The sizeable estimates in Table 1 (a 72% increase in the unemployment rate) and the significant heterogeneity across cities and industries as highlighted in Figure 6 speak to the severity of the pandemic's labor market implications, the uneven impact of the pandemic, and the importance of conducting analysis at granular levels. In addition, these results illustrate the rippling effect of the pandemic across cities within a country and countries across the world through the supply chain and the trade channels, where the industry composition of a city (or a country), the nature of the supply chain, and exposure to trade are crucial factors in determining the effect that the pandemic has on its economy (Forsythe, 2020; Goldberg, 2020; von Gaudecker et al., 2020; World Trade Organization, 2020a,b).

Unemployment benefit claims We repeat the heterogeneity analysis on unemployment benefit claims based on call data to unemployment hotline. Figures 8, 9 and 10 present heterogeneous impacts across demographic groups, cities and industries, as well as household income and migrant shares. The results are qualitatively similar to those that are based on non-commuters: females, workers over 40, and migrant workers had a large increase in unemployment benefit claims since the pandemic. The same is true for areas with a lower income and a high migrant share as shown in Figure 10. There is also a significant amount of heterogeneity across cities and industries, closely mirroring patterns reported in Figure 6. Finally, as shown in Table 4 cities with a larger export-to-GDP ratio experience a larger increase in unemployment benefit claims during the pandemic, consistent with the result from Table 2 based on commuting data.

One might be concerned that increases in the number of individuals calling unemployment hotline are merely driven by a higher awareness of unemployment benefits post the pandemic. However, as shown above, changes in the number of individuals calling the unemployment hotline are highly uneven across industries. There is also a great deal of heterogeneity across cities, and the numbers vary from -9% in Yangjiang to 70% in Guangzhou, which closely mirrors the industry and worker composition across cities. These patterns are unlikely to be purely driven by a significant increase in awareness of the unemployment hotline post the pandemic, as information on these government services is primarily disseminated at the national and provincial level, instead of at the industry (or city) level.

4.4 Robustness Checks

The analysis of commuting patterns above uses the two-week window to define a commuter. We have constructed two alternative measures of commuters using the one-week window and one-month window. It is worth noting that these three commuter measures are very highly correlated: the correlation between the one-week and two-week measure is 0.95; the correlation between the one-week and one-month measure is 0.92; and the correlation between the two-week and one-month measure is 0.97. In addition, more than 94% of individuals who are non-commuters over two weeks remain non-commuters over the entire month. This is evidence that our commuter measures accommodate flexible work modes (e.g., working at home from time to time). When individuals stop visiting their workplace over an extended period as defined in our analysis, they are essentially not working rather than working at home as we discussed in detail in Section 4.1.

In Tables A.3 and A.4 we repeat the entire analysis using a one-week window and one-month window as alternative cutoffs. The estimates of the effect size being 75% and 71% from the alternative windows are almost identical to that of 72% in Table 1 in the main text. These patterns corroborate the evidence above that WFH is unlikely to be driving our results and that our measures of commuting accommodate hybrid work modes.

Our main analysis excludes all users under the age of 18. As some users between the age of 18 and 25 might still be in schools, we exclude users under the age of 25 as a robustness analysis. Results on the percentage changes in non-commuters (Table A.5) and unemployment calls (Table A.6) barely change when we limit to users aged 25 and above.

Table 5 replicates the analysis in Table 3 but is based on the restricted sample of non-commuters. These individuals who both called the unemployment hotline and stopped commuting are less likely to be mis-classified as being unemployed. The results are very similar to those reported in Table 3. For example, the effect size on unemployment claims during the Phase II reopening is 0.57 in Table 3 compared to 0.49 in Table 5. The effect on call duration is 0.75 and 0.78 for the full sample and restricted sample, respectively.

The last two robustness checks are based on weighted regressions by replicating Tables 1 and 3 with the average number of commuters per day in each neighborhood in 2018 as the weight. The results are reported in Tables A.8 and A.9 for non-commuters and unemployment calls, respectively. The results are slightly larger in the weighted regressions but qualitatively the same.

5 Conclusion

Our analysis contributes to the recent studies on the broad labor market impacts of the pandemic (Bick and Blandin, 2020; Coibion et al., 2020; Ahn and Hamilton, 2020; Cajner et al., 2020). Compared to these studies, our mobile phone data benefit from large sample size and fine resolutions in both temporal and spatial dimensions. The pandemic led to a 72% increase in unemployment and 57% increase in unemployment benefit claims in September 2020, nearly five months after the full reopening, relative to the pre-pandemic level (September 2019). From January 23, 2020 (the beginning of the lockdown) to the end of September 2020, the number of unemployment benefit claims increased by 27%, relative to the same period pre-pandemic. While dramatic, these effects are smaller than those in the United States, partly due to the differences in the composition of the economy between these two countries. The service sector, which has been hard hit by the pandemic, employs 79% of the workforce and produces 68% of the GDP in the United States, compared to 47% and 50% in China in 2018. In addition, the draconian measures adopted in China to control the pandemic have reduced the spread of the virus more effectively (Hsiang et al., 2020; Kraemer et al., 2020; Zhang et al., 2020) and likely mitigated the impact on the economy as a result.

Our findings on the uneven labor market impacts across demographic groups and industries are consistent with recent studies in other countries (Adams-Prassl et al., 2020; Alon et al., 2020). However, we conduct the first quantitative analysis on China using large-scale datasets. Our research adds to the literature by showing that the pandemic's adverse impact on the labor market is more severe in areas that rely more heavily on export and hence more exposed to external shocks through global trade channels.

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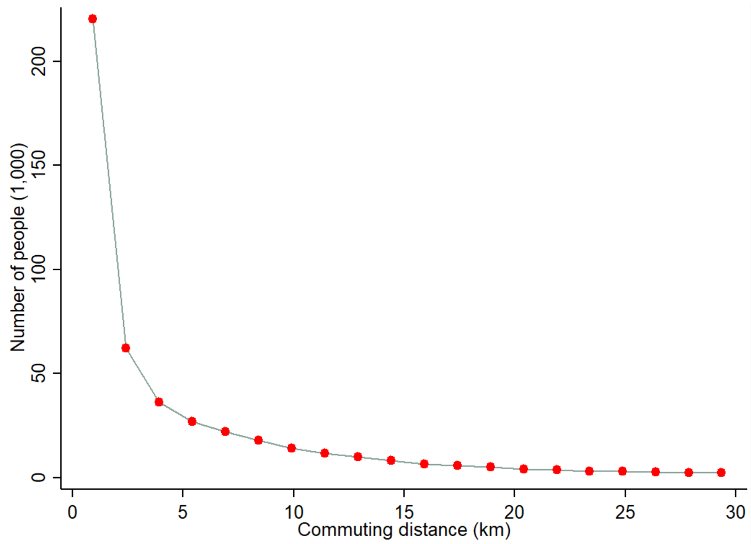
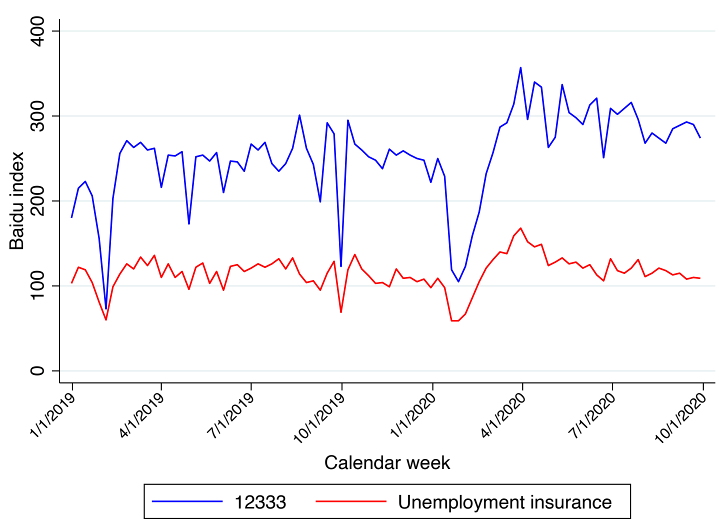
Figure 1: The number of people commuting by commuting distance



Notes: This graph shows the number of people commuting at each distance. We use the coordinates of job and home locations to compute the spherical commuting distance.

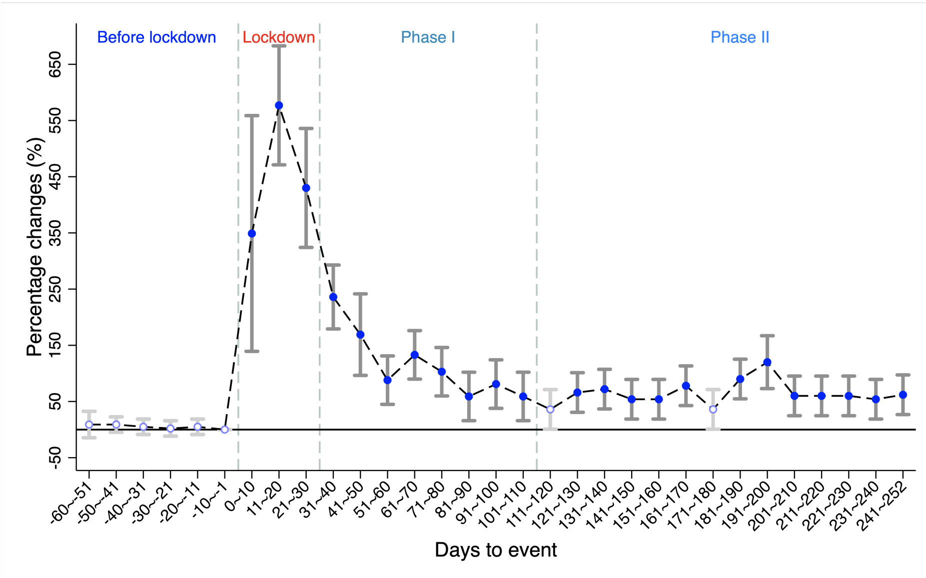
Figure 2: Correlation of Baidu search index using keywords of "12333" and "unemployment insurance" in Guangdong province



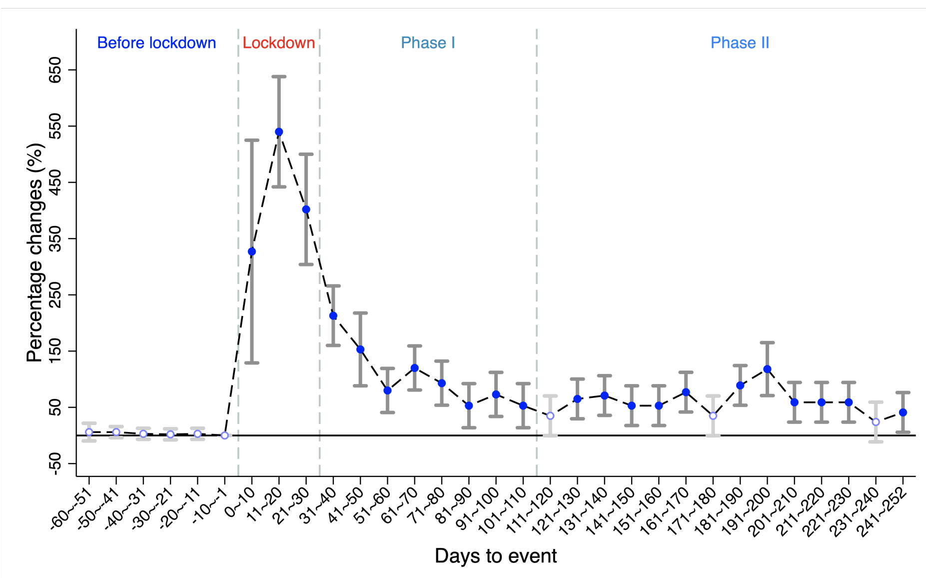
Notes: This graph shows the weekly Baidu Search Index for the keywords – "12333" and "unemployment insurance" – in Guangdong province from 2019 to 2020. Baidu Index, which is similar to Google Trends, is a keyword-analysis tool launched by Baidu, the largest search engine company in China. It reflects the search frequency of certain keywords on the Baidu website. The correlation between the two keywords during the sample period is 0.83.

Figure 3: Event study on non-commuters

(a) Non-commuters

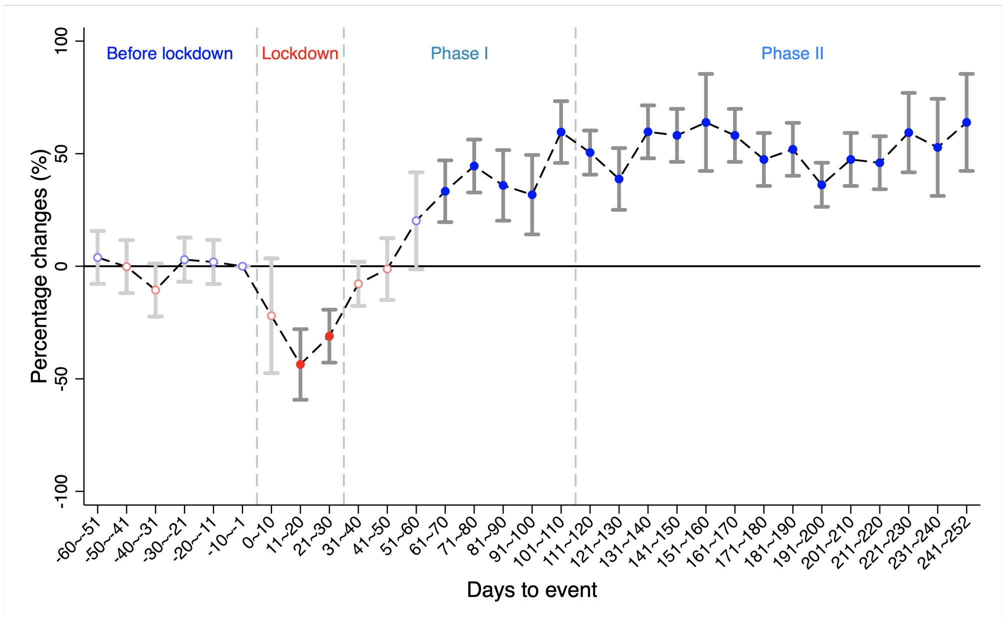


(b) Non-commuters who stopped using emails/virtual meeting apps



Notes: Both panels shows the event study of non-commuters as a measure for unemployment with 2019 as the control group. Panel (a) depicts the changes in the number of non-commuters in 2020 relative to that in 2019, and panel (b) is based on the number of non-commuters who also stopped using email/virtual meeting apps. The event days are based on the lunar calendar. For 2020, the event date (or day zero) is the lockdown, January 23, 2020, two days before the Chinese New Year. Correspondingly, the event date in 2019 is February 3, 2019, also two days before the 2019 Chinese New Year. Phase I reopening started on February 24, 2020, 32 days after the lockdown, when people were allowed to go back to work and visit outdoor public places. Phase II reopening, or full reopening, started on May 9, 2020, 108 days after the lockdown. Shopping malls, supermarkets, restaurants were allowed to fully reopen.

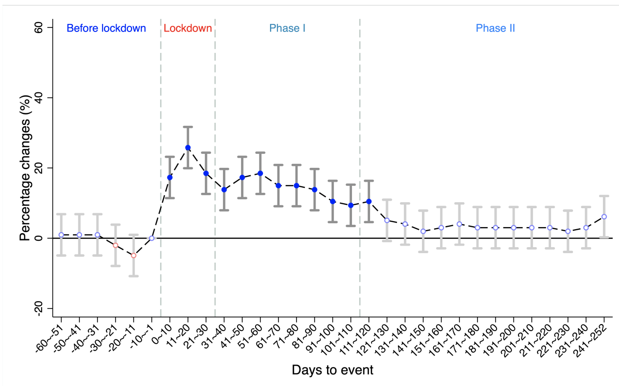
Figure 4: Differences in unemployment calls between 2019 and 2020



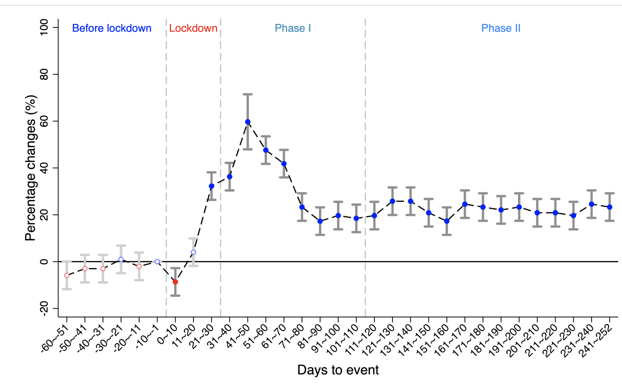
Notes: This graph depicts the event study on the daily number of individuals calling the unemployment hotline 12333 (in thousands) in 2020 relative to that in 2019. The event days are based on the lunar calendar. For 2020, the event date (or day zero) is the lockdown, January 23, 2020, two days before the Chinese New Year. Correspondingly, the event date in 2019 is February 3, 2019, also two days before the 2019 Chinese New Year. Phase I reopening started on February 24, 2020, 32 days after the lockdown, when people were allowed to go back to work and visit outdoor public places. Phase II reopening, or full reopening, started on May 9, 2020, 108 days after the lockdown. Shopping malls, supermarkets, restaurants were allowed to fully reopen.

Figure 5: Heterogeneity in unemployment impact across demographic groups

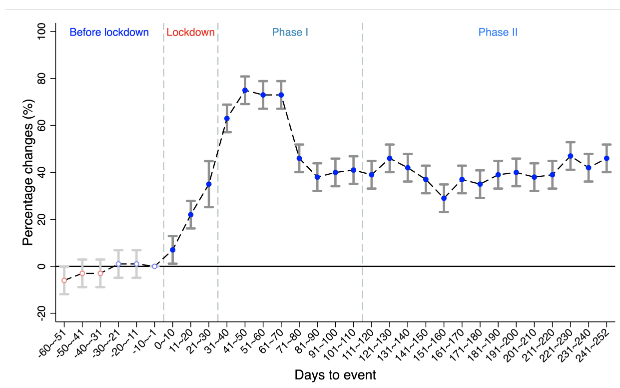
(a) Female v.s. male



(b) 40 and above v.s. under 40 years old



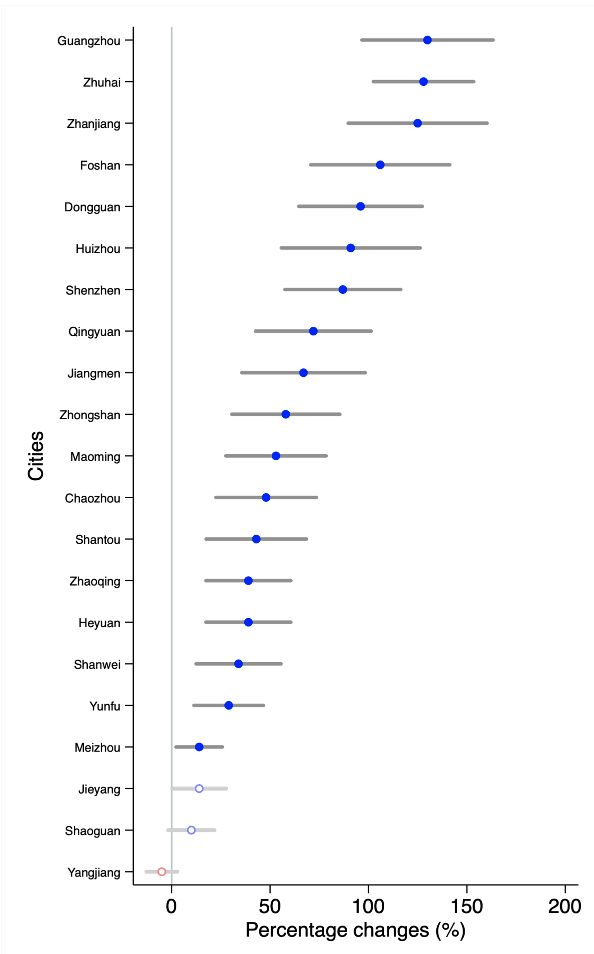
(c) Migrants v.s. non-migrants



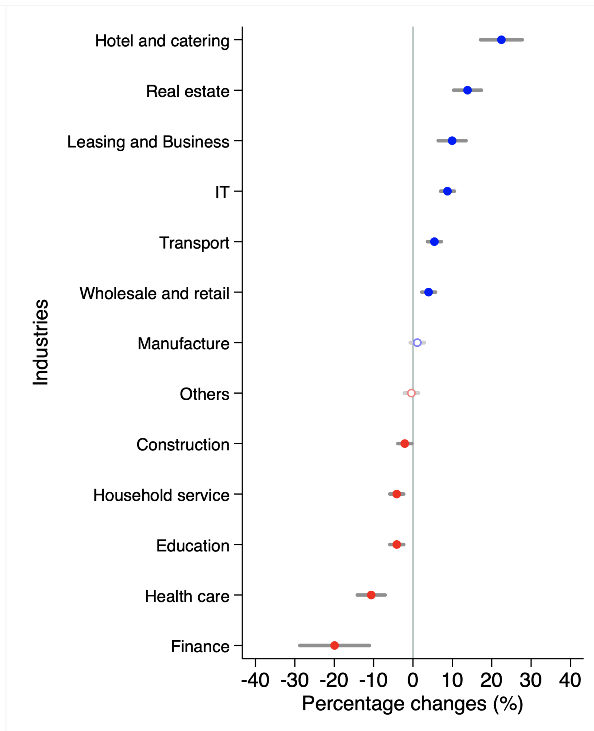
Notes: All event-study graphs plot coefficient estimates of β_q from Equation (1), which are the percentage changes in non-commuters in 2020 relative to 2019. The dependent variable is the difference between female and male non-commuters (in logarithm) in panel (a), the difference between individuals age 40 and above and those below 40-year-old non-commuters (in logarithm) in panel (b), and the difference between migrants and non-migrants (in logarithm) in panel (c). This analysis uses one week to define non-commuters. All regressions include neighborhood, event-week, and the treatment group fixed effects. The standard errors are clustered at the event-week level. Results are qualitatively similar, using two weeks or one month to define non-commuters.

Figure 6: Heterogeneity in unemployment impact across industries and cities

(a) By city

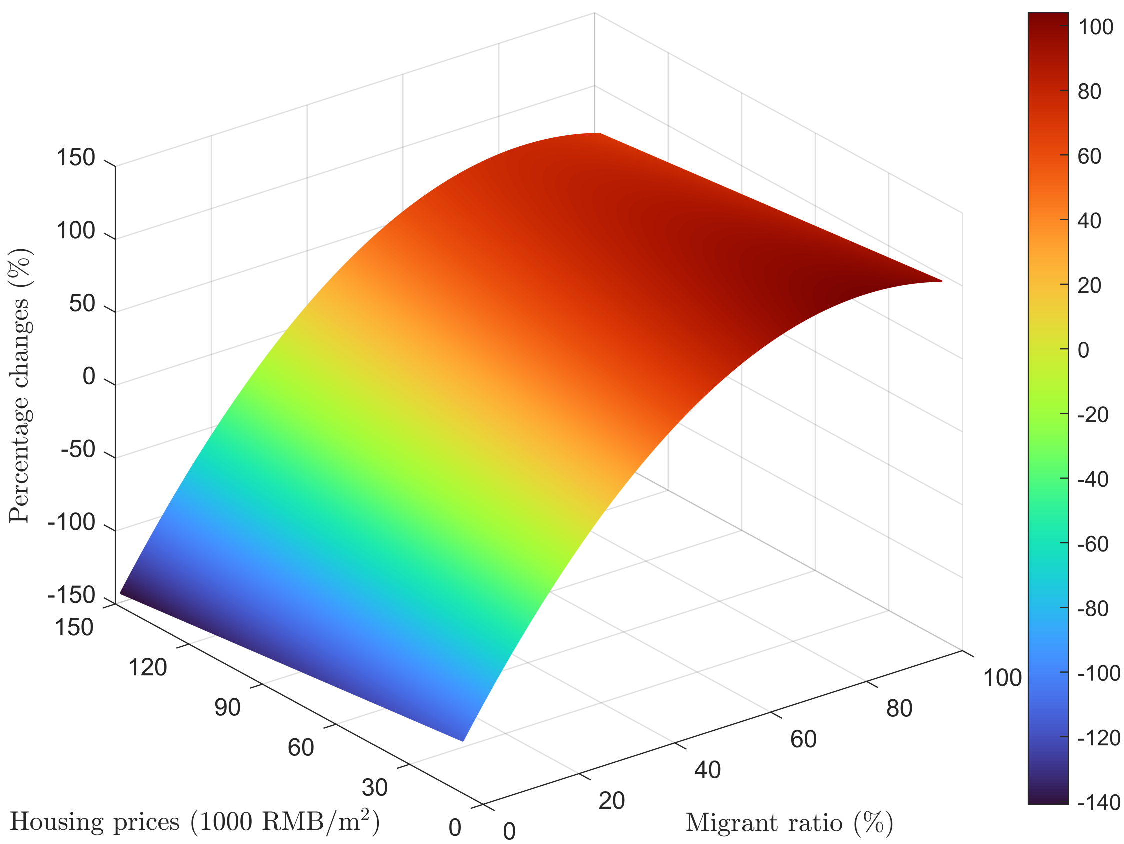


(b) By industry



Notes: This figure illustrates heterogeneity in unemployment (i.e., non-commuters) across cities (panel (a)) and industries (Panel (b)) following Equation (3). In panel (a), we add interactions between the after-lockdown dummy and city fixed effects. In panel (b), we add interactions between the after-lockdown dummy and a city's share of employment in each of the 13 industries. A positive change indicates an increase in non-commuters relative to 2019. This analysis uses one week to define non-commuters. Both regressions include neighborhood, event-week, and the treatment group fixed effects. The standard errors are clustered at the event-week level. Results are similar using two weeks or one month to define non-commuters.

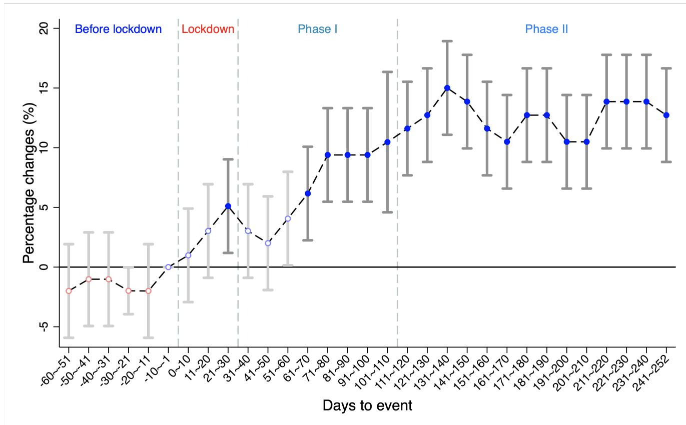
Figure 7: Changes in unemployment by income and migrant share



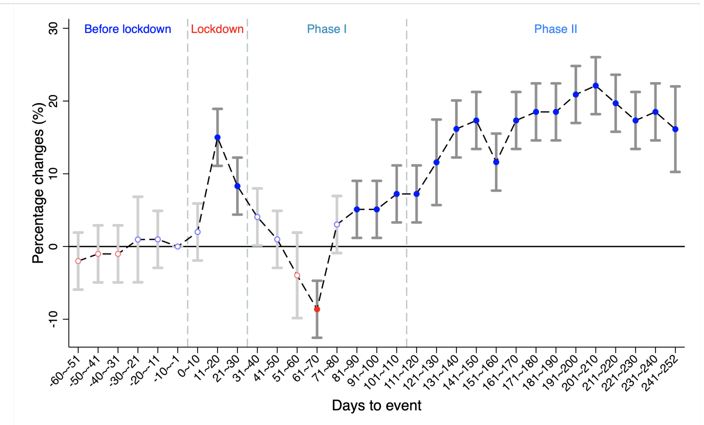
Notes: This graph depicts the percentage changes in the number of non-commuters between 2019 and 2020 at the neighborhood (i.e., cell-tower-area) level based on a regression including quadratic forms of the average housing price and migrant share for each neighborhood in Guangzhou. The housing prices from Soufang.com and the migrant shares are based on our phone data in 2018.

Figure 8: Heterogeneity in unemployment benefit claims across demographic groups

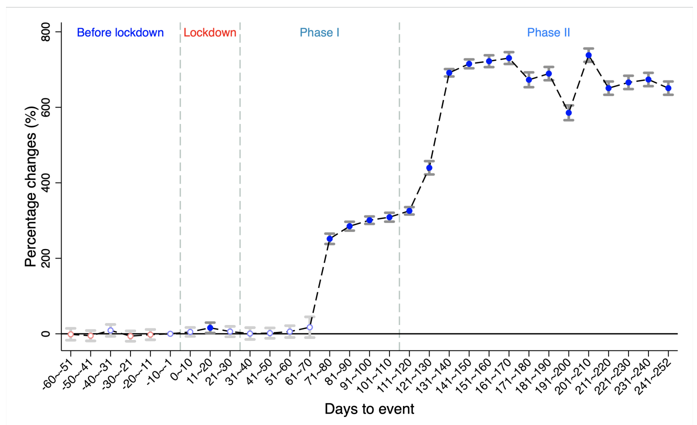
(a) Female v.s. male



(b) 40 and above v.s. under 40 years old



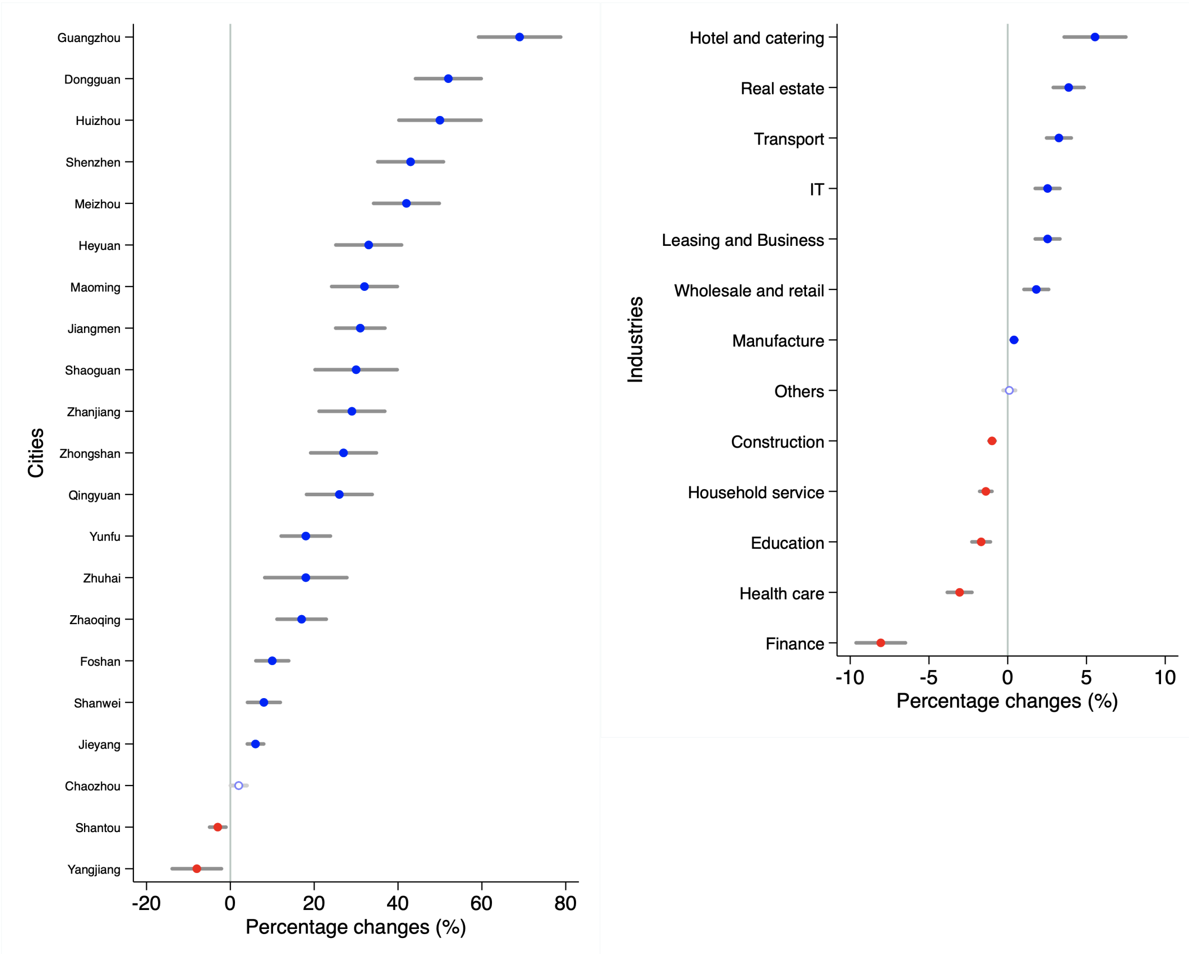
(c) Migrants v.s. non-migrants



Notes: All event-study graphs plot coefficient estimates of β_q from Equation (1), which are the percentage changes in individuals making unemployment calls in 2020 relative to 2019. The dependent variable is the difference between female and male calling the hotline (in logarithm) in panel (b), the difference between individuals age 40 and above and those below 40-year-old making unemployment calls (in logarithm) in panel (c), and the difference between non-migrants and migrants making unemployment calls (in logarithm) in panel (d). All regressions include neighborhood, day-of-week, event-day, holiday, and the treatment group fixed effects. The standard errors are clustered at the event-day level.

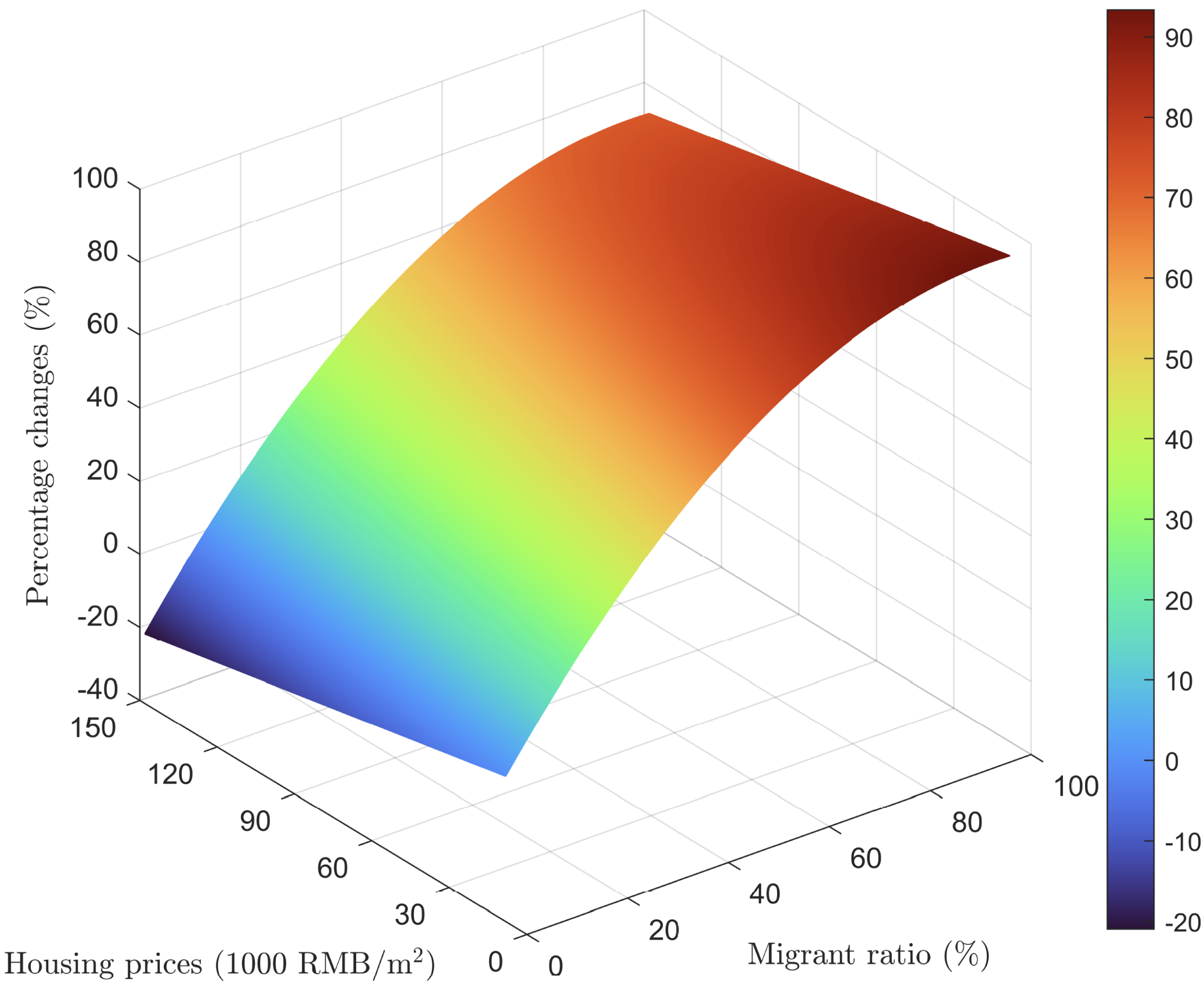
Figure 9: Heterogeneity in unemployment benefit claims across industries and cities

(a) By city　　　(b) By industry



Notes: This figure illustrates heterogeneity across cities (panel (a)) and industries (Panel (b)) following Equation (3). In panel (a), we add interactions between the after-lockdown dummy and city fixed effects. In panel (b), we add interactions between the after-lockdown dummy and a city's share of employment in each of the 13 industries. A positive change indicates an increase in unemployment relative to 2019. Both regressions include neighborhood, day-of-week, event-day, holiday, and the treatment group fixed effects. The standard errors are clustered at the event-day level.

Figure 10: Changes in unemployment benefit claims by income and migrant share



Notes: This graph depicts the percentage changes in the number of individuals calling the unemployment hotline between 2019 and 2020 at the neighborhood (i.e., cell-tower-area) level based on a regression including quadratic forms of the average housing price and migrant share for each neighborhood in Guangzhou. The housing prices from Soufang.com and the migrant shares are based on our phone data in 2018.

Table 1: Effects on commuters and working hours on-site

Variable	(1) No. of non-commuters (in log) Two-week window	(2) Working hours (in log)
1-30 days before lockdown	0.07	0.01
	(0.05)	(0.01)
Lockdown period	4.51***	-0.21***
	(1.26)	(0.02)
Phase I re-opening	1.03***	-0.08***
	(0.36)	(0.01)
Phase II re-opening	0.59**	-0.02
	(0.30)	(0.02)
Observations	34,965	34,965
R-squared	0.92	0.95
Neighborhood FE	Yes	Yes
Event-fortnight FE	Yes	Yes
Treatment group FE	Yes	Yes

Notes: This table examines the percentage change in the number of non-commuters and duration of on-site working hours as a result of the pandemic, following Equation (1), except that the ten-day intervals are grouped into four periods: before lockdown, during the lockdown, Phase I reopening, and Phase II full reopening. The observations are at the neighborhood by fortnight level. The dependent variables in columns (1)-(2) are the number of non-commuters (in logarithm) and average working hours for commuters (in logarithm), respectively. A non-commuter is someone who visits his work location at least 15 days in last 30 days and stops commuting in next two weeks. Both columns include neighborhood, event-fortnight, holiday, and the treatment group fixed effects. Standard errors are reported in parentheses and clustered at the event-fortnight level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 2: Heterogeneity by export-to-GDP ratio using commuting patterns

Variable	(1) No. of non-commuters (in log) Two-week window	(2) Working hours (in log)
Phase II re-opening * Export/GDP in 2019 (%)	0.0037***	-0.0002*
	(0.0007)	(0.0001)
Phase II re-opening (=1)	0.0329***	-0.0623***
	(0.0092)	(0.0065)
Observations	34,965	34,965
R-squared	0.93	0.81
Neighborhood FE	Yes	Yes
Event-fortnight FE	Yes	Yes
Treatment group FE	Yes	Yes

Notes: This table examines whether the pandemic's effect differs across cities with varying exposure to international trade following Equation (3), where we interact the phase II re-opening dummy with a city's 2019 export-to-GDP ratio. The dependent variables in columns (1)-(2) are the number of non-commuters (in logarithm) and average working hours for commuters (in logarithm), respectively. A non-commuter is someone who visits his work location at least 15 days in last 30 days and stops commuting in next two weeks. Both columns include neighborhood, event-fortnight, and the treatment group fixed effects. Standard errors are reported in parentheses and clustered at event-fortnight. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. Results are similar using one week or one month to define non-commuters.

Table 3: Effects on unemployment calls and call duration

Variable	(1) No. of individuals making calls (in log)	(2) Duration per call
Panel A:		
1-30 days before lockdown	0.03	0.03
	(0.03)	(0.04)
Lockdown period	-0.37***	-0.36***
	(0.06)	(0.08)
Phase I re-opening	0.25***	0.24***
	(0.03)	(0.05)
Phase II re-opening	0.45***	0.56***
	(0.02)	(0.04)
Panel B:		
Pandemic period	0.27***	0.33***
(Lockdown + Phases I + II)	(0.03)	(0.03)
Observations	489,514	489,514
R-squared	0.81	0.57
Neighborhood FE	Yes	Yes
Event-day FE	Yes	Yes
Day-of-week FE	Yes	Yes
Holidays FE	Yes	Yes
Treatment group FE	Yes	Yes

Notes: Panel A in this table examines the percentage change in the number of individuals making unemployment calls and call duration as a result of the pandemic following Equation (1), except that the ten-day intervals are grouped into four periods: before lockdown, during the lockdown, Phase I reopening, and Phase II full reopening. In panel B, we combine the three periods during the lockdown, Phase I reopening, and Phase II full reopening into one dummy variable, named the 'Pandemic period,' to examine the average impact of the pandemic. The observations are at the neighborhood by day level. The dependent variables in columns (1)-(2) are the number of individuals making unemployment calls (in logarithm) and the average duration of unemployment calls (in seconds, in logarithm), respectively. Both columns include neighborhood, day-of-week, event-day, holiday, and the treatment group fixed effects. Standard errors are reported in parentheses and clustered at the event day: *p<0.10; **p<0.05; ***p<0.01.

Table 4: Heterogeneity by export-to-GDP ratio using unemployment calls

Variable	(1) No. of individuals making calls (in log)	(2) Duration per call (in log)
Pandemic period * Export/GDP in 2019 (%)	0.0020***	0.0022***
	(0.0004)	(0.0004)
Pandemic period (=1)	0.2137***	0.2548***
	(0.0221)	(0.0368)
Observations	489,514	489,514
R-squared	0.79	0.56
Neighborhood FE	Yes	Yes
Event-day FE	Yes	Yes
Day-of-week FE	Yes	Yes
Holidays FE	Yes	Yes
Treatment group FE	Yes	Yes

Notes: This table examines whether the pandemic's effect differs across cities with varying exposure to international trade following Equation (3), where we interact the pandemic treatment with a city's 2019 export-to-GDP ratio (%). The dependent variables in columns (1)-(2) are the number of individuals who made unemployment calls and stopped commuting for at least fortnight in the current month and average duration of unemployment calls in seconds (in logarithm), respectively. The observations are at the neighborhood and day level. Both columns include neighborhood, day-of-week, event-day, holiday, and the treatment group fixed effects. Standard errors are reported in parentheses and clustered at the event-day. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5: Effects on unemployment calls and stopping commuting

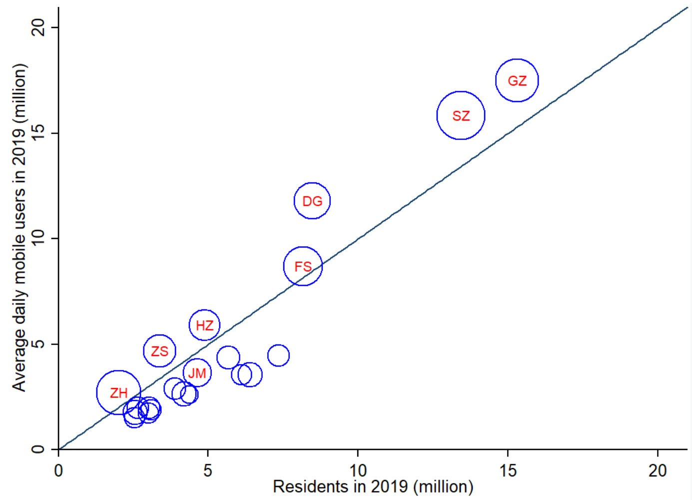
Variable	(1) No. of individuals making calls (in log)	(2) Duration per call (in log)
Panel A:		
1-30 days before lockdown	0.02	0.03
	(0.04)	(0.04)
Lockdown period	-0.42***	-0.40***
	(0.07)	(0.07)
Phase I re-opening	0.23***	0.26***
	(0.03)	(0.04)
Phase II re-opening	0.40***	0.58***
	(0.03)	(0.05)
Panel B:		
Pandemic period	0.24***	0.37***
(Lockdown + Phases I + II)	(0.03)	(0.03)
Observations	489,514	489,514
R-squared	0.81	0.57
Neighborhood FE	Yes	Yes
Event-day FE	Yes	Yes
Day-of-week FE	Yes	Yes
Holidays FE	Yes	Yes
Treatment group FE	Yes	Yes

Notes: Panel A in this table examines the percentage change in the number of individuals who made unemployment calls and stopped commuting for at least two-weeks in the current month and call duration as a result of the pandemic following equation (1), except that the ten-day intervals are grouped into four periods: before lockdown, during the lockdown, Phase I reopening, and Phase II full reopening. In panel B, we combine the three periods during the lockdown, Phase I reopening, and Phase II full reopening into one dummy variable, named the 'Pandemic period,' to examine the average impact of the pandemic. The observations are at the neighborhood by day level. The dependent variables in columns (1)-(2) are the number of individuals making unemployment calls and stopped commuting for at least two-weeks in the current month (in logarithm) and the average duration of unemployment calls (in seconds, in logarithm), respectively. Both columns include neighborhood, day-of-week, event-day, holiday, and the treatment group fixed effects. Standard errors are reported in parentheses and clustered at the event day. *p<0.10; **p<0.05; ***p<0.01.

Appendices. For Online Publication Only

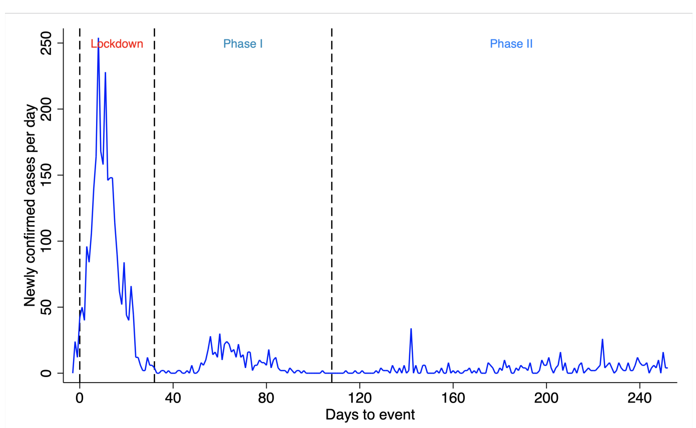
Appendix A: Figures and Tables

Figure A.1: Mobile users vs. residents in 2019



Notes: This graph presents the relationship between average daily mobile users and residents for cities in Guangdong in 2019. The solid line is a 45-degree line. The size of each marker denotes GDP per capita in 2019. We label the city names for those whose GDP per capita in 2019 is above 60,000 RMB (around $8,500). The abbreviated city names are listed in Table A.2.

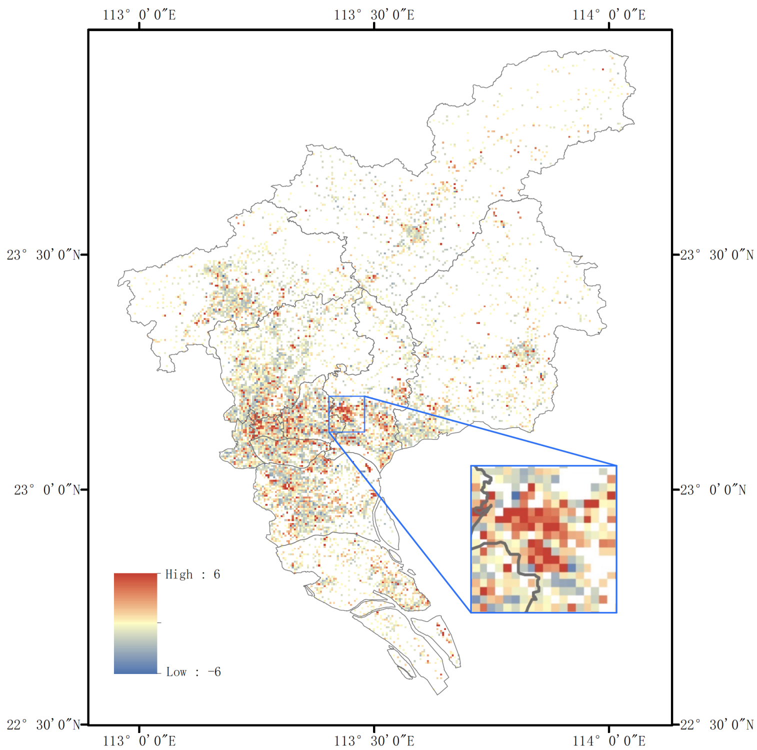
Figure A.2: Newly confirmed COVID-19 cases in Guangdong province



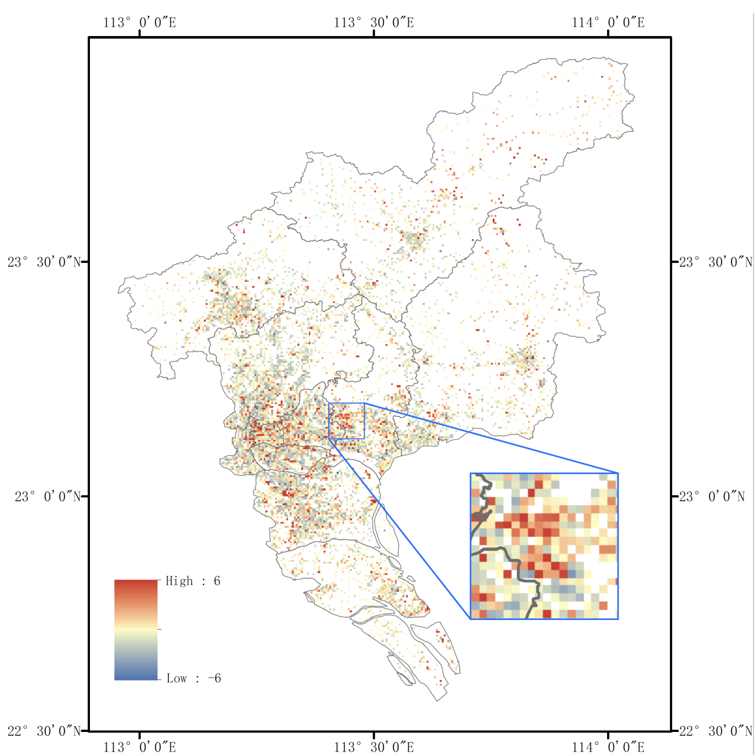
Notes: This graph shows the number of daily newly confirmed COVID-19 cases in Guangdong province. The provincial government started to report the number of COVID-19 cases on January 10, 2020, 13 days before the lockdown in Guangdong.

Figure A.3: Differences in (log) population during daytime and nighttime in Guangzhou

(a) Weekdays

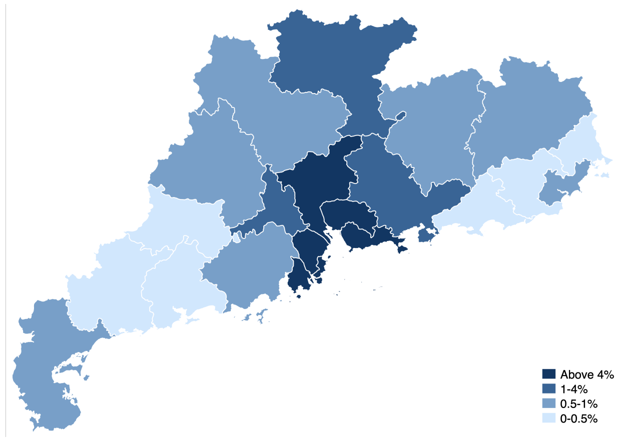


(b) Weekends



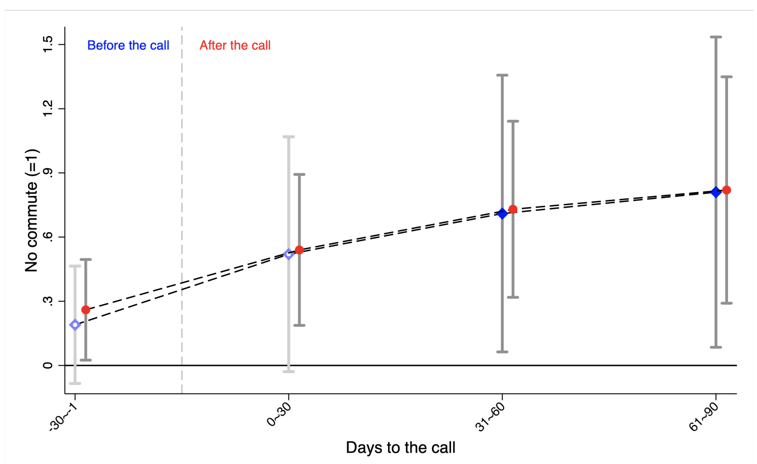
Notes: Panel (a) displays the average daily log difference of users during daytime (11 am) and nighttime (11 pm) at the centroid of every phone tower on the weekdays of 2019. In contrast, panel (b) shows the same measure on the weekends of 2019.

Figure A.4: Unemployment rate by city in 2019 based on the number of individuals making unemployment calls



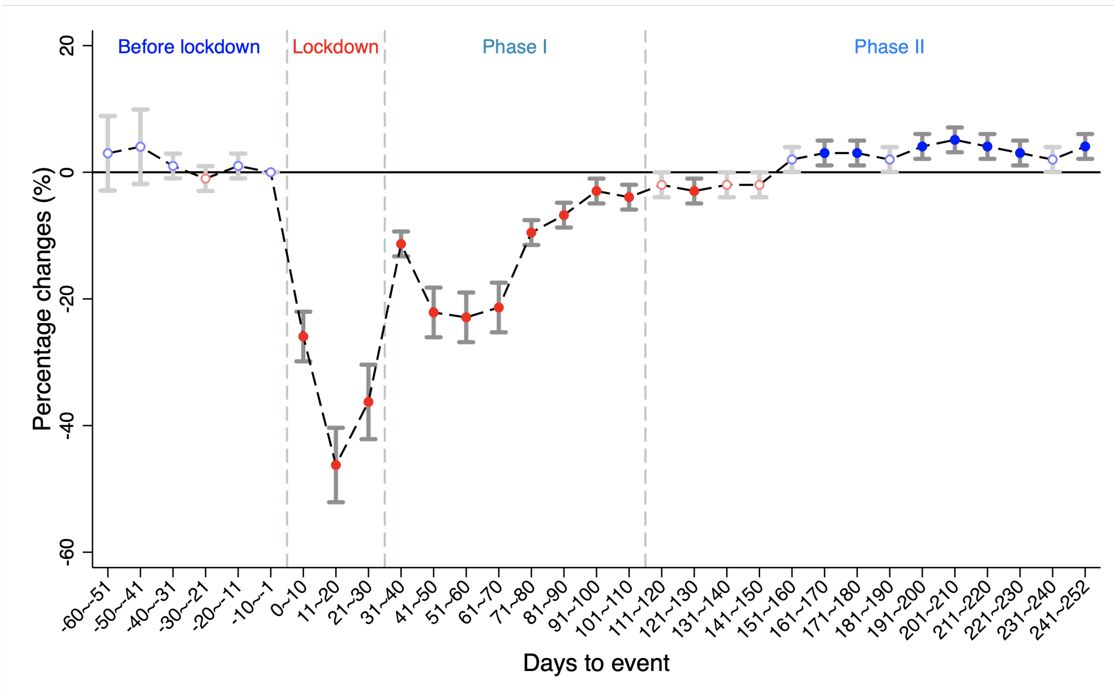
Notes: This graph shows our estimated unemployment rate, which is the ratio of individuals making unemployment calls over the size of the labor force by city in 2019. The correlation between our unemployment measure and the official unemployment measure at the city level is 0.7 in 2019.

Figure A.5: Non-commuters and work from home



Notes: This figure shows the cumulative probability of ever stopping commuting (for at least two weeks) among the eventual callers to the unemployment hotline. The x-axis denotes the days to the unemployment calls. The line with the red dot represents 2019 and that with the blue diamond represents 2020. Since there where less people calling the unemployment hotline in 2019 than in 2020, the standard errors are larger for 2019. 30 days before the unemployment call, about 10% of the callers had already stopped commuting. The cumulative probability grows to close to 90% by three months after the call. The pattern is very similar between 2019 and 2020, suggesting the increase in unemployment in 2020 relative to 2019 were unlikely to be driven by work from home.

Figure A.6: Event study on time of non-work activities



Notes: This figure depicts the changes in the hours of non-work activities in 2020 relative to that in 2019. The event days are based on the lunar calendar. For 2020, the event date (or day zero) is the lockdown, January 23, 2020, two days before the Chinese New Year. Correspondingly, the event date in 2019 is February 3, 2019, also two days before the 2019 Chinese New Year. Phase I reopening started on February 24, 2020, 32 days after the lockdown, when people were allowed to go back to work and visit outdoor public places. Phase II reopening, or full reopening, started on May 9, 2020, 108 days after the lockdown. Shopping malls, supermarkets, restaurants were allowed to fully reopen.

Table A.1: Summary statistics

Variable	N	Mean	Std. Dev.
Panel A: Commuting sample			
No. of non-commuters per two weeks	34,965	37,624	14,327
Working hours	34,965	8.7	1.0
Female (=1)	1,000,000	0.4	0.5
Age	1,000,000	33.6	8.4
Migrant (=1)	1,000,000	0.4	0.4
Panel B: Unemployment-call sample			
No. of individuals making calls per day	489,514	14.6	58.8
Duration per call (seconds)	489,514	169.5	141.7
Female (=1)	6,208,225	0.5	0.5
Age	6,208,225	36.5	9.7
Migrant (=1)	6,208,225	0.6	0.5

Notes: Panel (A) presents the summary statistics for the commuting sample, which consists of 1 million users that are randomly extracted from the full sample. In the analysis, we aggregate data at the neighborhood-fortnight level (34,965 observations). Panel (B) shows the summary statistics for the unemployment-call sample. During our sample period, there are 6,208,225 individuals who have ever contacted the unemployment benefits agencies via the designated hotline. In the analysis, we aggregate data at the neighborhood-day level (489,514 observations).

Table A.2: Cities in Guangdong province

Cities	Population in 2019 (million)	GDP in 2019 ($ billion)
Shenzhen (SZ)	13.44	390.25
Guangzhou (GZ)	15.31	342.44
Foshan (FS)	8.16	155.81
Dongguan (DG)	8.46	137.43
Huizhou (HZ)	4.88	60.54
Zhuhai (ZH)	2.02	49.80
Maoming (MM)	6.41	47.13
Jiangmen (JM)	4.63	45.60
Zhongshan (ZS)	3.38	44.94
Zhanjiang (ZJ)	7.36	44.42
Shantou (ST)	5.66	39.04
Zhaoqing (ZQ)	4.19	32.59
Jieyang (JY)	6.11	30.46
Qingyuan (QY)	3.89	24.61
Shaoguan (SG)	3.03	19.11
Yangjiang (YJ)	2.57	18.73
Meizhou (MZ)	4.38	17.20
Chaozhou (CZ)	2.66	15.67
Shanwei (SW)	3.02	15.66
Heyuan (HY)	3.11	15.65
Yunfu (YF)	2.55	13.36

Notes: This table shows the 2019 population and GDP for each city in Guangdong province. Data source: The Bureau of Statistics in Guangdong.

Table A.3: Effect on non-commuters and working hours (one-week window)

Variable	(1) No. of non-commuters (in log) One-week window	(2) Working hours (in log)
1-30 days before lockdown	0.06	-0.00
	(0.05)	(0.01)
Lockdown period	4.95***	-0.20***
	(1.23)	(0.02)
Phase I re-opening	1.09***	-0.08***
	(0.37)	(0.02)
Phase II re-opening	0.61**	-0.02
	(0.31)	(0.02)
Observations	69,930	69,930
R-squared	0.95	0.94
Neighborhood FE	Yes	Yes
Event-week FE	Yes	Yes
Treatment group FE	Yes	Yes

Notes: This table replicates the analysis in Table 1, except that the data are aggregated at the neighborhood-week level. The dependent variables in columns (1)-(2) are the number of non-commuters (in logarithm) and average working hours for commuters (in logarithm), respectively. A non-commuter is someone who visits his work location at least 15 days in last 30 days and stops commuting in next week. Both columns include neighborhood, event-week, and the treatment group fixed effects. Standard errors are reported in parentheses and clustered at event-week. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.4: Effect on non-commuters and working hours (one-month window)

Variable	(1) No. of non-commuters (in log) One-month window	(2) Working hours (in log)
1-30 days before lockdown	0.02	0.01
	(0.03)	(0.01)
Lockdown period	4.32***	-0.24***
	(1.05)	(0.02)
Phase I re-opening	1.01***	-0.08***
	(0.38)	(0.01)
Phase II re-opening	0.58**	-0.01
	(0.29)	(0.02)
Observations	16,650	16,650
R-squared	0.96	0.95
Neighborhood FE	Yes	Yes
Event-month FE	Yes	Yes
Treatment group FE	Yes	Yes

Notes: This table replicates the analysis in Table 1 but uses a one-month window instead of a two-week window to define commuters. The dependent variables in columns (1)-(2) are the number of non-commuters (in logarithm) and average working hours for commuters (in logarithm), respectively. A non-commuter is someone who visits his work location at least 15 days in last 30 days and stops commuting in next month. Both columns include neighborhood, event-month, and the treatment group fixed effects. Standard errors are reported in parentheses and clustered at event-month. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.5: Regression results on commuters and working hours (excluding people under 25 years old)

Variable	(1) No. of non-commuters (in log) Two-week window	(2) Duration per call (in log)
1-30 days before lockdown	0.06	0.01
	(0.05)	(0.01)
Lockdown period	4.63***	-0.21***
	(1.29)	(0.02)
Phase I re-opening	1.10***	-0.09***
	(0.39)	(0.02)
Phase II re-opening	0.60**	-0.02
	(0.31)	(0.02)
Observations	34,965	34,965
R-squared	0.93	0.88
Neighborhood FE	Yes	Yes
Event-fortnight FE	Yes	Yes
Treatment group FE	Yes	Yes

Notes: This table replicates the analysis in Table 1, except that individuals under 25 years old are excluded from the analysis. The dependent variables in columns (1)-(2) are the number of non-commuters (in logarithm) and average working hours for commuters (in logarithm), respectively. A non-commuter is someone who visits his work location at least 15 days in last 30 days and stops commuting in next two weeks. Both columns include neighborhood, event-fortnight, and the treatment group fixed effects. Standard errors are reported in parentheses and clustered at event-fortnight. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. Results are similar using one week or one month to define commuters.

Table A.6: Effects on unemployment calls and stopping commuting and call duration (excluding people under 25 years old)

Variable	(1) No. of individuals making calls (in log)	(2) Duration per call (in log)
1-30 days before lockdown	0.03	0.03
	(0.04)	(0.05)
Lockdown period	-0.41***	-0.39***
	(0.07)	(0.06)
Phase I re-opening	0.25***	0.27***
	(0.04)	(0.04)
Phase II re-opening	0.41***	0.58***
	(0.04)	(0.05)
Panel B:		
Pandemic period	0.26***	0.36***
(Lockdown + Phases I + II)	(0.04)	(0.04)
Observations	489,514	489,514
R-squared	0.81	0.57
Neighborhood FE	Yes	Yes
Event-day FE	Yes	Yes
Day-of-week FE	Yes	Yes
Holidays FE	Yes	Yes
Treatment group FE	Yes	Yes

Notes: This table replicates the analysis in Table 5, except that individuals under 25 years old are excluded from the analysis. The dependent variables in columns (1)-(2) are the number of individuals making unemployment calls and stopped commuting for at least fortnight in the current month (in logarithm) and the average duration of unemployment calls (in seconds, in logarithm), respectively. Both columns include neighborhood, day-of-week, event-day, holiday, and the treatment group fixed effects. Standard errors are reported in parentheses and clustered at the event-day. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.7: Effects on non-commuters who stopped using email or virtual meeting Apps

Variable	(1) No. of non-commuters (in log)
1-30 days before lockdown	0.06
	(0.04)
Lockdown period	4.19***
	(1.17)
Phase I re-opening	0.98***
	(0.30)
Phase II re-opening	0.57**
	(0.27)
Observations	34,965
R-squared	0.91
Neighborhood FE	Yes
Event-fortnight FE	Yes
Treatment group FE	Yes

Notes: This table replicates column (1) in Table 1, except that a non-commuter is defined as who visits his work location at least 15 days in last 30 days and stops commuting and do not using email or virtual meeting Apps in next two weeks. We control neighborhood, event-fortnight, holiday, and the treatment group fixed effects. Standard errors are reported in parentheses and clustered at the event-fortnight level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.8: Weighted regressions on non-commuters

Variable	(1) No. of non-commuters (in log) Two-week window	(2) Duration per call (in log)
1-30 days before lockdown	0.08	-0.02
	(0.07)	(0.02)
Lockdown period	6.92***	-0.29***
	(2.22)	(0.02)
Phase I re-opening	1.76***	-0.12***
	(0.53)	(0.02)
Phase II re-opening	0.68**	-0.03
	(0.32)	(0.03)
Observations	34,965	34,965
R-squared	0.92	0.95
Neighborhood FE	Yes	Yes
Event-fortnight FE	Yes	Yes
Treatment group FE	Yes	Yes

Notes: This table replicates the analysis in Table 1, except estimated by regression weighted by the average number of commuters per day in 2019 in each neighborhood. Both columns include neighborhood, event-fortnight, holiday, and the treatment group fixed effects. Standard errors are reported in parentheses and clustered at the event-fortnight level. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$. Results are similar using one week or one month to define non-commuters.

Table A.9: Weighted regressions on unemployment calls

Variable	No. of individuals making calls (in log)	Duration per call (in log)
1-30 days before lockdown	0.02	0.05
	(0.06)	(0.06)
Lockdown period	-0.47***	-0.43***
	(0.08)	(0.08)
Phase I re-opening	0.29***	0.29***
	(0.05)	(0.05)
Phase II re-opening	0.50***	0.60***
	(0.05)	(0.05)
Panel B:		
Pandemic period	0.27***	0.42***
(Lockdown + Phases I + II)	(0.04)	(0.04)
Observations	489,514	489,514
R-squared	0.81	0.57
Neighborhood FE	Yes	Yes
Event-day FE	Yes	Yes
Day-of-week FE	Yes	Yes
Holidays FE	Yes	Yes
Treatment group FE	Yes	Yes

Notes: This table replicates the analysis in Table 3, except estimated by regression weighted by the average number of commuters per day in 2019 in each neighborhood. Both columns include neighborhood, day-of-week, event-day, holiday, and the treatment group fixed effects. Standard errors are reported in parentheses and clustered at the event-day. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.