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The COVID-19 pandemic and unemployment: Evidence from mobile phone data from China[☆]

Teng Li[a], Panle Jia Barwick[b,c], Yongheng Deng[b], Xinfei Huang[a,*], Shanjun Li[d,c]

[a] International School of Business and Finance and Institute of Advanced Finance, Sun Yat-sen University, China
[b] University of Wisconsin–Madison, United States
[c] NBER, United States
[d] Cornell University, United States

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ABSTRACT

Based on mobile phone records for 71 million users and location tracking information for one million users over almost three years, this study examines the labor market impacts of the COVID-19 pandemic in China's Guangdong province, whose GDP is larger than that of all but the top 12 countries in the world. Using a standard difference-in-differences framework, our analysis shows dramatic and protracted effects of the pandemic on the labor market: it increased unemployment by 72% and unemployment benefits claims by 57% even after the full reopening in 2020 relative to their levels in the same period in 2019. The impact was also highly heterogeneous, with women, workers older than 40, and migrants being more affected. Cities that rely more on export or that have a higher share of the hospitality industry in GDP but a lower share of the finance and healthcare industries experienced a more pronounced increase in unemployment. The lingering impact likely reflects the global transmission of the pandemic's effects through the supply chain and trade channels.

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. However, in addition to the substantial time lag associated with survey data and their 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 unemployment benefit agencies prior to 2018 and is supplemented by household surveys for the years afterward.[2] Measuring unemployment accurately is particularly challenging in the Chinese context due to the large fraction of the population who do not have local household registrations (*Hukou*) and hence are excluded from the unemployment surveys. In addition, 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 official national unemployment rate has varied within 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 the 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,

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* Corresponding author.
E-mail addresses: liteng27@mail.sysu.edu.cn (T. Li), pbarwick@wisc.edu (P.J. Barwick), yongheng.deng@wisc.edu (Y. Deng), huangxf3@mai.sysu.edu.cn (X. Huang), sl2448@cornell.edu (S. Li).

[1] In addition to the delay, it is technically challenging to measure unemployment accurately from survey data. Their quality also varies considerably over time due to changes in participation rates, modifications to survey methodologies, 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, the report at https://www.marketwatch.com/story/china-unveils-urban-survey-unemployment-rate-2018-04-17-1485030.

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

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with a GDP larger than that of 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 users from January 2018 to September 2020. 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 observations from the year 2020 as the treatment group and those from the year 2019 as the control group. The key identification assumption is that the labor market outcomes would have tracked between the two groups in the absence of the pandemic and hence that the observed differences can be attributed to the pandemic rather than to time-varying unobservables. Results from event studies provide strong support for the assumption of common trends between the two groups prior to the event date.

We leverage two unique features of our data to estimate pandemic labor market impacts: a) the information on the number of individuals who stopped commuting to work for an extended period of time (non-commuters) as a measure of unemployment, and b) the information on the number of unique individuals who contacted unemployment benefit 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 a shift to work-from-home (WFH), a key confounder in interpreting the results based on commuting patterns. We also conduct a host of robustness checks and find that our results are robust to alternative variable definitions, data selection and model specifications.

Several key findings emerge from our analysis. First, the pandemic increased unemployment in Guangdong by 72% and unemployment benefit claims by 57% after the full reopening relative to those during the same period (from May to September) in 2019. The effect does 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 that reported in the government statistics, which registered 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.

Second, the pandemic's impact on unemployment was highly uneven across demographic groups and more pronounced among women, people over 40, and especially migrants. The escalating increase in unemployment among migrants shows no sign of abatement during our sample period. This echoes the massive reduction in the number of migrant workers reported by the National Bureau of Statistics (NBS) and indicates the possibility of large-scale layoffs among this group.[4]

Third, the pandemic's impact was more substantial in cities with a high labor share of the hospitality, real estate, or transportation industries but less severe in cities where employment is concentrated in the finance, health care, or education industries. In addition, the impact was more pronounced in cities that rely heavily on export, reflecting the global nature of the shock in an interconnected world economy. Industry composition accounts for 39% of the heterogeneity in the pandemic's unemployment impact across cities, while trade exposure contributes 29% of the heterogeneity.

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

The key contribution of our study is twofold. First, our study adds to the emerging literature that leverages granular and high-frequency mobile phone data to better measure economic and social activities. Examples include studies that use 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 understand mobility, knowledge spillovers, and racial disparities in voting wait times (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). Our study illustrates how this type of data can also be used to understand the labor market impact of the pandemic in near real time.

Second, our study contributes to the burgeoning literature on the impacts of the pandemic. The literature is now too large to discuss in detail, but our paper is closely related to studies that use mobile phone data to measure mobility and social distancing after the onset of the pandemic (Couture et al., 2020; Gupta et al., 2020) and that focus on the labor market impacts of the pandemic (Adams-Prassl et al., 2020; Ahn and Hamilton, 2020; Alon et al., 2020; Bick and Blandin, 2020; Coibion et al., 2020; Cajner et al., 2020). Compared to the samples in these studies, our mobile phone data benefit from a large sample size and fine resolution on both the temporal and spatial dimensions, and our study is the only one on China.

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. Sections 4 and 5 present the event-study and regression results, and Section 6 concludes.

2. Background and data

2.1. Background and data sources

Exploiting the increasing availability of high-frequency and high-resolution mobile phone data is particularly advantageous in the context of China, as its cellphone penetration rate is high among developing countries. According to the 2018 wave of the 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 data for 2018 from the National Bureau of Statistics. Appendix Figure A1 shows a strong correlation between the number of mobile phone users and the number of residents by city. Cities with a higher GDP per capita (represented by the size of the circles in Figure A1) 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 11% of China's GDP and approximately a quarter of China's foreign trade (China Statistical Yearbook 2020). Its major cities include Shenzhen and Guangzhou, which rank among the wealthiest and economically most advanced cities in China. Within Guangdong province, cities differ substantially in terms of both population and GDP, as Table A1 illustrates.[5] 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 for Guangdong's relevance is that its number of daily confirmed COVID-19 cases—like that of most other provinces in China—has been under a few dozen since the full reopening (Appendix Figure A2). Our measures on the pandemic's consequences could thus apply to other regions as well.

[4] According to the NBS statistics, the number of migrant workers decreased by 3.8 million in September 2020 from that in 2019. Please refer to the NBS article at http://www.stats.gov.cn/tjsj/sjjd/202010/t20201019_1794729.html.

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

Our data come from one dominant cellular service provider in China. We have access to detailed phone usage data (encrypted IDs of the calling party and the receiving party, date of calls, and call duration in seconds) from January 2018 to September 2020 for all of the provider's 71 million users in Guangdong province, who account for 63% of all mobile users in the province. We observe some user demographic information, such as age, gender, and the city where the phone number is registered. In addition, we observe geocoded phone locations at five-minute intervals for one million randomly selected users during the same period.

The cellular service provider delineates Guangdong province into 787 cell tower areas (similar to zip codes in the U.S.) for billing purposes. We use "cell tower areas" and "neighborhoods" interchangeably in this analysis.

Guangdong's lockdown Guangdong's provincial government acted swiftly and adopted vigilant procedures at the onset of the pandemic. Guangdong was one of the first provinces to release detailed information on newly confirmed cases (daily new cases, location, gender, etc.), starting from as early as February 3, 2020. These procedures proved successful and have kept the number of daily confirmed cases under a few dozen since the full reopening. As shown in Figure A2, the number of daily confirmed new cases peaked at 254 on January 31, and quickly declined to under 50 three weeks into the lockdown period. The number of cases has been modest since then and varied 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 return to work after February 24. It is worth noting that the lockdown procedures in Guangdong were not as strict as those implemented in the epicenter Wuhan. On February 24, 2020, Guangdong province entered its Phase I reopening, which lasted 76 days. During the Phase I reopening, people were allowed (and encouraged in certain industries) to go back to work and visit outdoor public places. The 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, which remained closed till mid-July of 2020.

While Guangdong's COVID case numbers are low, this does not imply that the pandemic has had little or only a modest effect on the local economy. On the contrary, the measures implemented to alleviate the public health impact of the pandemic were able to significantly affect the economy. As shown in our analysis in the main text, the pandemic has inflicted sizeable damages on Guangdong's labor market, leading to a 72% increase in the number of unemployed and a 57% increase in unemployment benefit claims after the full reopening during May to September in 2020 relative to those during the same period in 2019. As Guangdong's economy is among the most resilient among the economies of the provinces in China, the aggregate labor market implications of the pandemic could be much more severe than 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 Guangdong province's labor market outcomes. Specifically, individuals' commuting patterns observed over a long period of time help us monitor their employment status. We then use changes in these commuting patterns to construct unemployment measures. In addition, we take advantage of information on calls to the designated unemployment benefits hotline – (12333) – and use the number of people who contacted the hotline (combined with the changes in commuting patterns) to construct measures of unemployment benefit claims.

Work commute

The first feature of the mobile phone data that we leverage for our analysis is the geocoded location information (in longitude and latitude) collected by mobile devices at 5-minute intervals when they are powered on.[6] 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 at which the user spends the most time between 10 pm and 7 am each month.[7] These geocoded locations trace out individuals' spatial trajectories over time and allow us to record the time of arrival and departure at their job locations.

We provide two pieces of evidence that our assignment of home and work locations 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 the commuting distance decays exponentially (Figure A3), 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 6.6 km, close to the average commuting distance of 8.7 km reported in the 2020 travel survey conducted by the Guangzhou Municipal Transportation Bureau (GMTB, 2020). Second, for the city of Guangzhou (the provincial capital), Appendix Figure A4 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 city center gains population, and the suburbs lose population during the daytime relative to that in the nighttime. However, these differences are much more pronounced on weekdays than on weekends, especially in the busiest parts of the city center. The enlarged area in Appendix Figure A4 illustrates this for a famous industrial park in Guangzhou. These spatial and temporal patterns of population density are remarkably consistent with the GMTB reports (GMTB, 2020).

We use the changes in the number of commuters before and after the lockdown and the changes relative to the number during the same period in 2019 as our measure of pandemic-induced unemployment. Changes in commuting patterns on a continuing basis can provide a valuable barometer of employment status, especially when participation in unemployment benefit programs is low, as is the case in China. To the extent that some of these changes reflect a post-lockdown shift to more flexible work modes, such as 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. Panel A of Table A2 presents descriptive statistics of key variables used in the commuting sample, which consists of one million users randomly extracted from all mobile phone users. In the regression analysis below, we aggregate the noncommuter sample to the neighborhood–fortnight level (34,965 observations).

Calls to unemployment benefit agencies The second data feature that we leverage is the detailed records of calls (with the time and duration of each call) to the designated government hotline (12333) for unemployment benefit agencies. The hotline offers the public a comprehensive one-stop service, provides eligibility information, helps with unemploy-

[6] 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, and 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 have exploited long panels of location data to examine labor market dynamics (Barwick et al., 2019).

[7] The 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.

ment 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 the all-inclusive character of the help from customer services. Figure A5 shows the weekly Baidu index for the keywords “12333” and “unemployment insurance” in Guangdong province from 2019 to 2020.[8] The correlation of the Baidu index of the two keywords during the sample period is 0.83. The comovement of these two indices offers additional support for using the number of calls to the 12333 hotline as a proxy for the number of individuals claiming unemployment benefits.

The number of individuals making calls to 12333 provides an estimate for the level of unemployment benefit claims. During our sample period, 6,208,225 individuals contacted the unemployment benefit agencies via the designated hotline. However, despite the popularity of the hotline, lifetime unemployment benefits in China are capped at 24 months, thus limiting choices for people who have already exhausted their benefits. Therefore, instead of focusing on the level of unemployment calls, our analysis below exploits its changes over time. We show that changes in unemployment calls can provide useful information on unemployment benefit claim rates and short-run labor market dynamics that is otherwise unavailable through official statistics.

As people might contact the hotline multiple times to claim unemployment benefits, we treat multiple calls from the same user as one claim incident and therefore use the number of individuals calling the unemployment hotline, instead of the number of calls to 12333, to construct our unemployment benefit claim 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 terms “number of individuals calling the unemployment hotline” and “number of unemployment calls” are used interchangeably throughout the analysis. Appendix Figure A6 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 available only annually at the city level, is reasonably high at 0.7 for 2019.

Our analysis based on unemployment calls counts only the first time when a user contacts the unemployment benefit hotline. We aggregate the duration of all subsequent calls in calculating “call duration to the hotline”. Our main analysis excludes users under age 18, as they are unlikely to be working due to the Law on Protection of Minors. The results excluding users under age 25 (to eliminate those still in school) are almost identical. Panel B of Table A2 presents descriptive statistics of key variables used in the unemployment-call sample. During our sample period, 6,208,225 individuals contacted the unemployment benefit agencies via the designated hotline. In the regression analysis below, we aggregate the caller data to the neighborhood–day level (489,514 observations).

Some of our analyses separately examine migrants and nonmigrants. It is important to note that migrants working in Guangdong without Guangdong *Hukou* became eligible for unemployment benefits from 2014.[9] The new regulation was designed to attract migrants and help improve labor relations. One data limitation is that we do not observe whether an individual has nonlocal *Hukou* status—the basis of the official definition of migrant status. Instead, we define migrants as individuals who registered their phone numbers outside Guangdong province. This is an imperfect measure of migrant status, as workers from outside Guangdong who bought and registered their mobile phones in Guangdong are treated as nonmigrants in our analysis. Consequently, the actual unemployment gap between migrants and residents might be even larger than our estimates.

3. Empirical framework

Our analysis employs the 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 is January 23, 2020, for the year 2020 and February 3, 2019, for 2019, two days before Chinese New Year in the lunar calendar. We use observations from the year 2020 as the treatment group and those from 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. We delineate the interval from 60 days before the lockdown to 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-the-week, event-day, holiday, and treatment group fixed effects. The identification assumption is that conditional on inclusion of these controls, there would have been no systemic differences in time-varying unobservables between the two groups in the absence of the pandemic. Results from event studies that are discussed below support this assumption of common trends 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:

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

where c denotes a neighborhood (area covered by the nearest cell tower), i denotes the treatment group (an observation from the year 2020) and the control group (an observation from the 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 after the lockdown.

y_{cit} is the outcome variable, such as the log number of noncommuters. 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 the event-study coefficients, capturing differences between the treatment and control groups. The variable d_i is a dummy equal to one for the treatment group, and $\mathbb{1}(.)$ is an indicator variable for each 10-day interval of the sample. We include neighborhood fixed effects γ_c, group fixed effects γ_i, and 312 event-day fixed effects η_t. We also include day-of-the-week fixed effects and holiday fixed effects ξ_{it} that vary by group and time (e.g., the International Labor Day holiday fell on different lunar calendar days in 2019 and 2020). Standard errors are clustered at the event-day level.

Since the key regressors are dummy variables, $\hat{\beta}$ is not a consistent estimator of the percentage change in unemployment, with a larger bias when $\hat{\beta}$ is further from zero. While we report $\hat{\beta}$ in all figures and tables, we calculate the percentage changes using $100 * \Big(\exp[\hat{\beta} - \widehat{var(\beta)}/2] - 1\Big)$, a consistent estimator of the percentage impact, throughout the paper. The second component in the bracket reduces the finite-sample bias.[10]

[8] The Baidu index, which is similar to Google Trends, is a keyword-analysis tool launched by Baidu, the largest search-engine company in China. The index reflects the search frequency of certain keywords on the Baidu website.

[9] See the announcement by Guangdong’s Human Resources and Social Security Department: https://www.gdhrss.gov.cn/sy/20140801/10101.html.

[10] This adjustment method was proposed by Kennedy (1981).

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

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

where $\boldsymbol{Z}$ is a vector of city attributes in 2019 and 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. The variables d_i and $\mathbb{1}(.)$ and fixed effects α_c, γ_i, η_t, and ξ_{it} are the same as in Equation (1). The key coefficient is τ, 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 over all periods and focus on heterogeneity across industries and cities.

4. Event studies

As discussed in Section 2.2, we use the number of individuals who used to commute to work regularly but stopped commuting altogether in a given period as a measure of unemployment. To mitigate potential measurement errors (see a detailed discussion of this below in Section 4.4), we refine our analysis by limiting our sample to individuals who stopped commuting altogether *and* did not use any e-mail/virtual meeting apps when their commuting patterns changed.

Our second measure is related to unemployment benefit claims, where we use the number of people who contacted the designated unemployment benefits hotline 12333 as a proxy for the number of people claiming unemployment benefits. We also refine our analysis by limiting the sample to individuals who both contacted the hotline and stopped commuting altogether, though the results based on this sample are nearly identical.

This section first presents event-study figures for both measures to illustrate their time series patterns, especially the pronounced increases after the onset of COVID-19. Then, we devote an entire subsection to addressing threats to our empirical analysis, especially the issue of flexible work arrangements (such as work-from-home) that also affect commuting patterns and the concern that the 12333 hotline also provides other services, such as consultations on social security or labor dispute issues.

4.1. Noncommuters

We exploit the 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 she visits a work location at least once during that time window. To accommodate the possibility of (partial) WFH during and after the lockdown, we construct three commuter measures 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 she visits a work location at least once in that month. Noncommuters during a certain time window are individuals who used to commute to work but no longer commute during that period.

Fig. 1 shows the event study with noncommuters as the measure of unemployment, with the observations from the same period in 2019 as the control group. Panel (a) depicts the event-study coefficients that measure changes in the number of noncommuters. Panel (b) repeats the analysis but limits the sample to noncommuters who do not use any of the e-mail/virtual meeting apps available to mobile phone users during our sample period (see the discussions in Section 4.4 for more details). This helps exclude people who work from home. For 2020, the event date (or day zero) is the lockdown, January 23, 2020, two days before Chinese New Year. Correspondingly, the event date in 2019 is February 3, 2019, also two days before the 2019 Chinese New Year. The 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. The Phase II reopening, or full reopening, started on May 9, 2020, 108 days after the lockdown, when shopping malls, supermarkets, restaurants were allowed to fully reopen.

There are three salient patterns from both panels. First, for the pre-lockdown period, there was virtually no difference between the 2019 and 2020 groups in the number of noncommuters, lending support to the assumption of parallel trends between the treatment and control groups, the key identification assumption of our analysis. Second, during the lockdown, the number of noncommuters increased severalfold in the 2020 group relative to that in 2019 group. The increase reflects not only changes in unemployment but, more importantly, temporary leaves with/without pay and work from home due to the strict nature of the lockdown. Third, as the economy opened up, the increase in noncommuters gradually came down to approximately 70% by the end of the Phase I reopening and remained stable till the end of September, four months after the Phase II (full) reopening. The increase in panel (b) is slightly smaller, but the pattern stays the same. While these changes are unprecedented, they are milder than those observed in the U.S. during the same period: the unemployment rate in the U.S. increased from approximately 3.6% in the second half of 2019 to 13% and 8.8% in the second and third quarters of 2020, respectively, according to the U.S. Bureau of Labor Statistics.

4.2. Calls to unemployment benefit hotline

We next examine the second outcome: the pandemic's impact on unemployment benefit claims based on calls to 12333. Figure 2 depicts the differences in the log daily number of individuals calling the unemployment hotline between 2019 and 2020. Similarly to in Fig. 1, there are no differential trends in the calls to 12333 between the two year groups before the lockdown period, leading credence to the parallel trend assumption.

In contrast to the sharp increase in noncommuters during the lockdown as shown in Fig. 1, the number of people contacting the unemployment benefits hotline dropped significantly during this period. This is likely due to uncertainty about the severity and duration of the pandemic during its initial stage. In addition, the increase in the number of noncommuters during the lockdown was likely driven by changes in work arrangements instead of unemployment.

As the severity of the pandemic unfolded in China, the number of individuals calling the unemployment benefit hotline increased sharply during Phase I. The jump in the number of people calling 12333 began in April 2020, when the mobility restrictions were beginning to ease in Hubei province (the epicenter of the pandemic) and other provinces.[11] The increase stabilized at approximately 50% by the end of the Phase I reopening and remained there till the end of our data period. Both the pattern and magnitude are consistent with those in Fig. 1.[12]

We repeat the event study limiting the sample to individuals who both called the unemployment benefits hotline and stopped commuting altogether. The patterns are nearly identical to those in Fig. 2.

[11] This is consistent with the evidence that migrants were allowed to return to their place of work and that some filed unemployment benefit claims upon job losses. See https://baijiahao.baidu.com/s?id=1663364477907792823&wfr=spider&for=pc and https://baijiahao.baidu.com/s?id=1663996321334096972&wfr=spider&for=pc.

[12] For comparison, initial unemployment benefit claims 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 at the end of September according to the U.S. Bureau of Labor Statistics.

4.3. Migration post-unemployment

While migration is not the focus of our empirical analysis, examining whether people migrate after experiencing negative labor market shocks could provide insights on our understanding of how people adjust and adapt to changes in employment status. Since it is straightforward to identify the date of contact with unemployment benefits agencies, we conduct an event study on whether individuals migrated to other cities after they called 12333.

To do so, we keep the IDs for all individuals who contacted the unemployment benefits hotline and track their residential cities during the period between 150 days before the call and 360 days after. The period [-150, -121] before the call serves as the reference group. We drop observations from the lockdown period from January 23, to February 24, 2020. The dependent variable is a cumulative measure of whether individuals have migrated to a different city by period t. To increase the precision of the residential city measure, all valid observations must have resided in a given city for at least two months. Migrated individuals are those who lived in one city for at least two months and moved to and lived in another city or other cities for at least two months.

Appendix Figure A7 presents the event study. Approximately 10% of workers migrated to other cities two months after contacting the unemployment benefit agencies. The fraction increases modestly over time to 13% one year after the call. Hence, the majority of individuals who experienced negative employment shocks appear to have stayed in the city where they lived, with a small fraction migrating to other cities in search of employment opportunities.

4.4. Threats to the validity of the empirical strategy

There are two major threats to the validity of our empirical strategy. First, flexible work arrangements, such as WFH, might pose a challenge to our analysis using commuting patterns. Second, the 12333 hotline provides other services such as support in applying for social security benefits. This could inflate our measure of the number of people claiming unemployment benefits. We next examine each threat in turn.

Work from home One could argue that the increase in the number of noncommuters post-lockdown may be partially driven by the increase in WFH. We present several pieces of evidence that our definition of noncommuters—those who do not visit their workplace at all for an extended period of time (such as two weeks or a month)—accurately reflects individuals' unemployment status and that changes in the number of noncommuters are unlikely to be primarily driven by WFH.

First, we examine each individual's usage of all virtual meeting apps and e-mail apps available in the Apple App Store and Android Google Play store.[13] Under the two-week window definition of commuters, the share of commuters using these apps at least once during a two-week window is 30.3%, 26.1%, and 24.7% for the lockdown, Phase I, and Phase II reopening periods, respectively. In contrast, the share of noncommuters using any virtual meetings and e-mail apps at least once during a two-week window is 5.2%, 0.6%, and 0.06% for the lockdown, Phase I, and Phase II reopening periods, respectively. The patterns are very similar when we limit our attention to virtual meeting apps or use one week or one month as the relevant time window to define commuters and noncommuters. Furthermore, the sharp contrast in usage patterns between commuters and noncommuters is very similar for the observations from 2019: the share of commuters using these apps at least once during a two-week window is 21.2%, relative to 1.0% among noncommuters. If our measure of noncommuters in 2020 were primarily driven by a significant increase in the fraction of telecommuting workers in 2020 relative to that in 2019, we would expect the virtual meeting app usage patterns to be very different over these two years. We would also anticipate much higher usage of virtual meeting apps among noncommuters in 2020. Neither prediction is supported by our data.

Second, our three commuter (noncommuter) measures are highly correlated (their correlations exceed 0.92). More than 94% of individuals who are noncommuters over a two-week window remain noncommuters over the entire month. These patterns hold in both 2019 and 2020. If noncommuters in 2020 mostly consisted of people who work from home and visit their offices once in a while, we should anticipate the persistence in noncommuting (or telecommuting) patterns to be significantly lower in 2020 when the lockdown restrictions were gradually lifted than in 2019. This is not what we observe. These patterns provide evidence that our commuter measures accommodate flexible work modes (e.g., occasional WFH). When individuals stop visiting their workplace altogether over an extended period, as defined in our analysis, they are most likely not working (unemployed) rather than working from home.

Third, an important indicator of whether normal economic activities have resumed is time spent away from home (including both outdoor and indoor activities). We compare the time spent on nonwork activities away from home in 2020 to that in 2019. We define nonwork activities as those occurring in places other than home and the workplace and lasting for at least half an hour. Fig. 3 presents the event-study plot. The pattern is consistent with that in Figure 1. Before the lockdown period, there was no difference in the time spent on nonwork activities between 2019 and 2020, but there was a sharp drop during the lockdown period in 2020. Time spent on nonwork activities gradually recovered during the Phase I reopening. It fully recovered and even slightly increased during the Phase II reopening. This presents strong evidence that economic activities had returned to their pre-pandemic level and that people were free to spend time away from home, which further lends support to the limited role of WFH by the end of the Phase II reopening.

Purpose of calling 12333 While we do not directly observe individuals' purpose of contacting the unemployment benefits agencies, it helps to examine individuals' commuting patterns before and after they called the hotline. If their phone calls were motivated by fear of (as yet unrealized) unemployment or if people were calling for information on social services other than unemployment benefits, then we should not observe any changes in their commuting patterns.

To examine this point, we extract a sample that includes all individuals who made calls to the unemployment hotline. Then, we examine their commuting patterns, especially any changes in those patterns (whether they ever stopped commuting altogether), from four months before the call to one year after it.[14] Panel (a) of Fig. 4 depicts the cumulative probability of any pause in commuting lasting at least two weeks among these callers with respect to the event of contacting the unemployment benefit agencies. The probability of a pause in commuting is practically zero three months before the call, increases to approximately 20% one month before the call, and quickly jumps to 80% two to three months after the call. In other words, the majority of individuals experience changes in commuting patterns (which we interpret as changes in their employment status) surrounding the time when they contact the 12333 hotline. This provides evidence that people contacted the government unemployment benefits agencies primarily because they either had already lost or were about to lose their jobs rather than because they were collecting information on other social services. We repeat this

[13] To gauge the prevalence of WFH, we obtain an exhaustive list of all 21 virtual meeting apps and 29 e-mail apps in the Apple App Store and Android Google Play store. The virtual meeting apps include Ailiao, Alitong online, Chubao, DingDing, Feige, Feiyin, Laidian, Shangqitong, Shuoba, SKYPE, Tencent Meeting, Tongtong, uu Online, Weihui, Weiwei, Yiliao, Youhuatong, Youliao, Youxin, Zhangshangbao, and Zoom. The e-mail apps include 139 Light Mail, 139 Mail, 189 Mail, 21CN Light Mail, 21CN Mail, 263 Mail, Ali Mail, Baidu Mail, China Mobile Mail, Coremail, Foxmail, Gmail, Hotmail, Ke Space, Live Mail, Mail Master, Mi Mail, Microsoft Outlook, Qixinbao, QQ Mail, Sina Mail, Sohu Mail, Tencent Mail, TOM, Wangyi Mail, Woo Mail, Yahoo Mail, Youqia, and Yun Home.

[14] Extending the event window to six months before the call or earlier makes a difference, as the probability of noncommuting before the call is low.

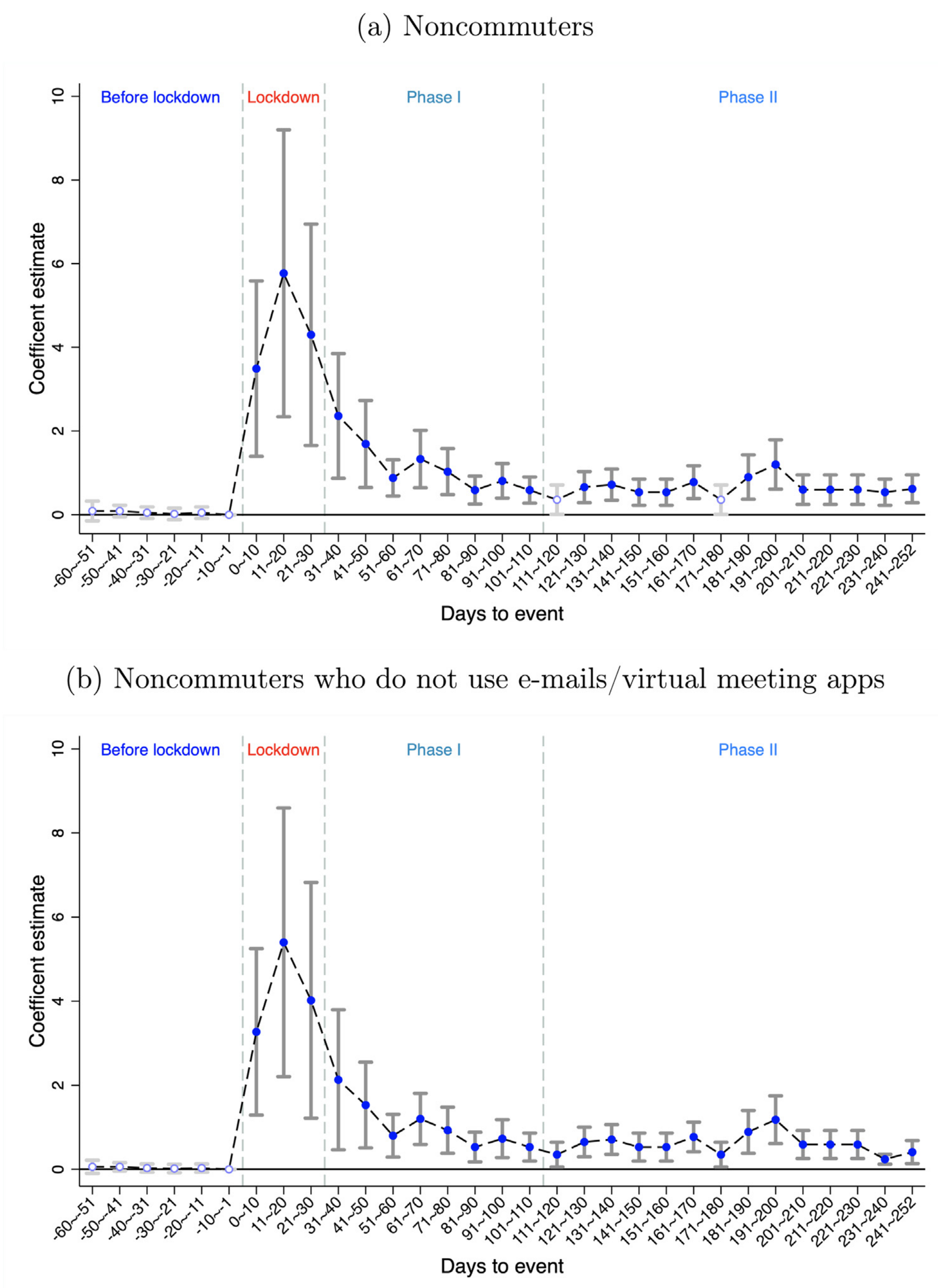


Fig. 1. Event study on differences in noncommuters between 2019 and 2020. Notes: Both panels report the event study coefficients for noncommuters (which we use as a measure of unemployment), with 2019 as the control group. The dependent variable is the number of noncommuters (in log) in panel (a) and the number of noncommuters who stopped using e-mail/virtual meeting apps (in log) in panel (b). Panel (a) depicts changes in the number of noncommuters in 2020 relative to that in 2019, and panel (b) is based on the number of noncommuters who also stopped using e-mail/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.

analysis with a more stringent definition where stopping commuting altogether is defined with the one-month window (not visiting one's workplace for an entire month). The results that emerge are nearly identical (see panel (b) of Fig. 4).

One might be concerned that the pandemic has greatly enhanced people's awareness of government-provided services, including unemployment benefits and the designated hotline 12333. If this were the case, then our measure of changes in unemployment benefits claims in 2020 relative to those in 2019 would be inflated because people would be more likely to call the hotline in 2020 to inquire about potential benefits. To examine whether this conjecture holds, we repeat the analysis in panel (a) of Fig. 4 separately for 2019 and 2020 and plot the patterns in Figure A8, where the red line with diamonds represents 2019 and the blue line represents 2020. If people became more aware of the existence of the hotline in 2020 and were simply reaching out for benefits-related information, then we would expect a much lower probability of noncommuting in 2020 than in 2019. This is not what we see. Rather, the pattern for 2020 is remarkably similar to that in 2019. In fact, the two lines are hardly distinguishable.

The fact that the pattern of calling and stopping commuting appears virtually indistinguishable between 2019 and 2020 provides strong evidence that the character of the calls individuals made to government agencies upon job loss was stable over the years and not affected by the pandemic. This lends credence to our DID empirical strategy of comparing changes in 2020 pre- and post-lockdown to changes in 2019 during the same period. Finally, it also helps address the concern over WFH. If the majority of noncommuters in 2020 were people working from home rather than unemployed workers, we should expect rather different patterns between the two years. The fact that there are no changes in the relationship between calling the unemployment benefit hotline and the commuting patterns from 2019 to 2020 suggests that the increase in unemployment in 2020 relative to the level in 2019 is unlikely to have

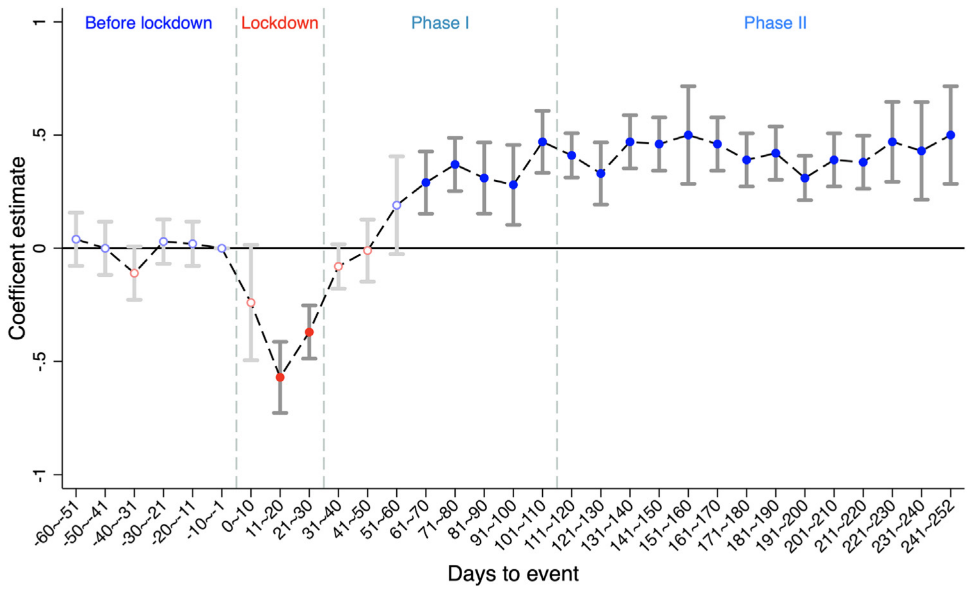


Fig. 2. Event study on differences in calls to the unemployment benefit hotline between 2019 and 2020. Notes: This graph depicts the event study coefficients for the daily number of individuals calling the unemployment hotline 12333 (in thousands) in 2020 relative to that in 2019. The dependent variable is the number of calls to the unemployment benefit hotline (in log). 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.

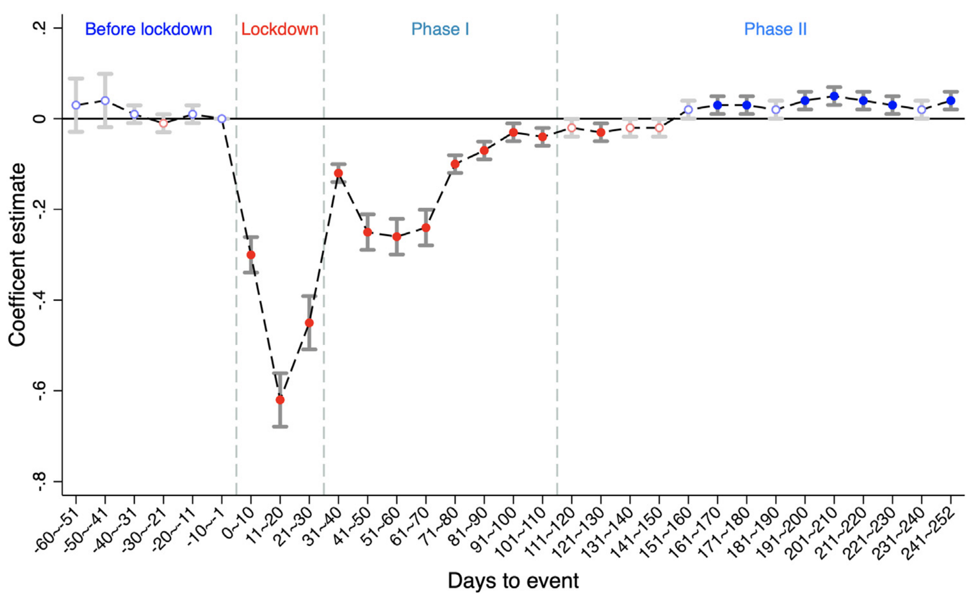


Fig. 3. Event study on hours spent on non-work activities away from home. Notes: This figure depicts the changes in the hours of non-work activities away from home in 2020 relative to that in 2019. See Fig. 1 for the explanation of various event days. Non-work activities as those that happened at places other than home and the workplace and lasted for at least half an hour.

been driven by the shift to work from home, a phenomenon that was much less common in 2019.

5. Regression results

The specification for the regression analyses is analogous to that for the event study except that, instead of coefficients at ten-day intervals, we report coefficients for each of the four periods: 1–30 days before the lockdown, during the lockdown, Phase I, and Phase II. The reference group is 31–60 days before the lockdown. The regressions for unemployment calls are at the neighborhood and day level, with a total of 489,514 observations. The regressions for commuting patterns are aggregated to the neighborhood–week, neighborhood–fortnight, and neighborhood–month levels when appropriate.[15]

5.1. Effect on unemployment

Table 1 reports parameter estimates for the percentage increase in the number of noncommuters measured by the two-week window with the ten-day intervals grouped into four periods.[16] During the lockdown period, the number of noncommuters increased nearly 43-fold, reflecting the draconian nature of the lockdown. The number of noncommuters increased by 163% during the Phase I reopening and by 72% during the Phase II (full) reopening.[17] The effect size is robust to the use of alternative window lengths of one week or one month in defining noncommuters (see Section 5.4). Since economic activities had largely returned to their pre-pandemic level by Phase II and WFH is unlikely to have played a major role during Phase II as shown in Section 4.4, we interpret the 72% increase in the number of noncommuters as the im-

[15] We also adjust the fixed effects accordingly. For example, We drop the day-of-the-week fixed effects and replace the event-day fixed effects with event-fortnight fixed effects and cluster the standard errors at the fortnight level when we measure noncommuters using a two-week window.

[16] The omitted group is 31–60 days before the lockdown.

[17] The coefficients of Phase I and Phase II in Table 1 are 1.03 and 0.59, respectively. The effect sizes reported throughout the main text are calculated by $\left(\exp[\hat{\beta} - \widehat{var(\beta)}/2] - 1\right)$, as discussed in Section 3.

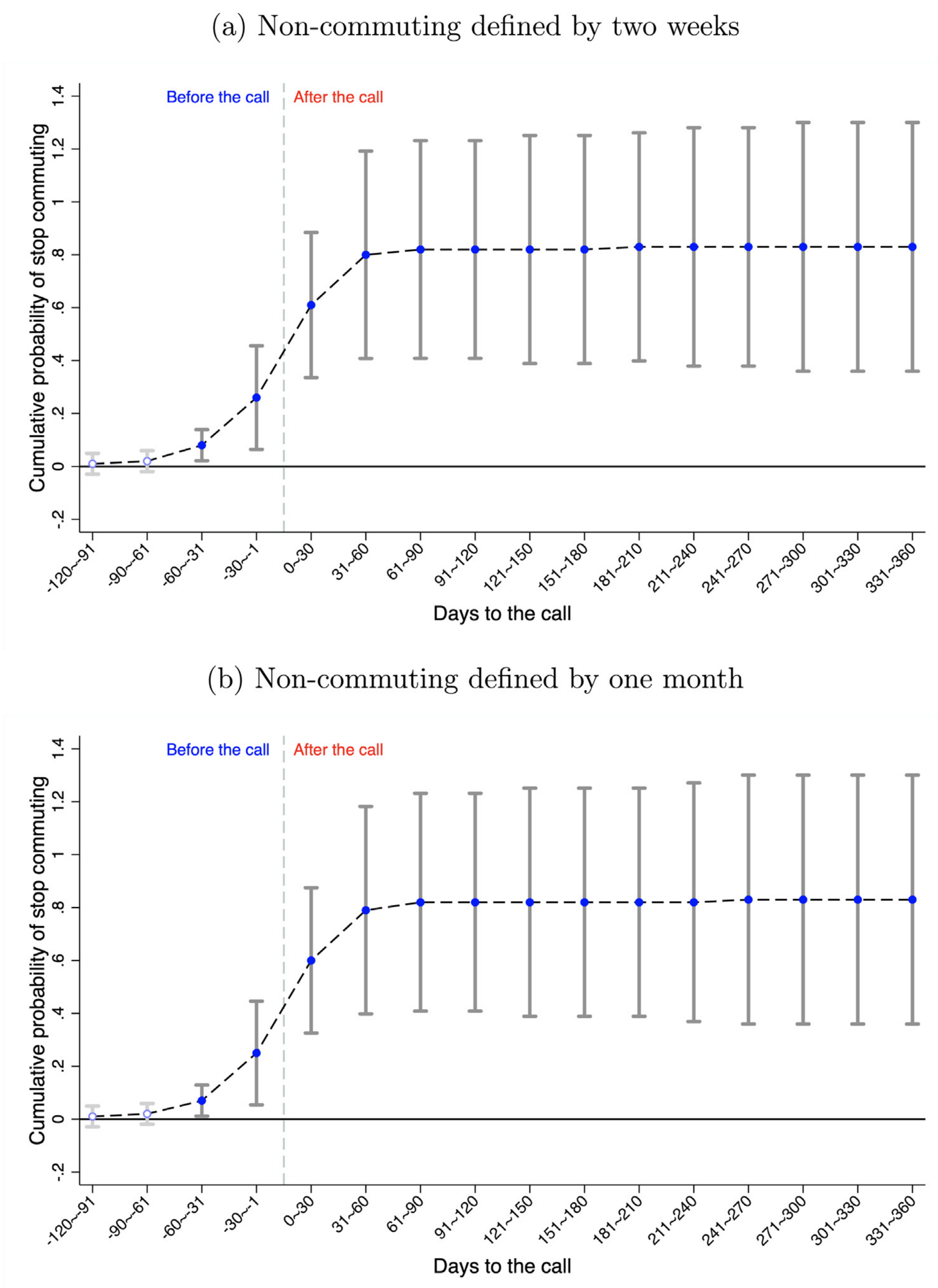


Fig. 4. Cumulative probability of stopping commuting for individuals calling unemployment benefit hotline: different non-commuter definitions. Notes: This figure shows the cumulative probability of stopping commuting for at least two weeks (panel a) and one month (panel b) among individuals reaching out to the unemployment benefit hotline. The vertical line denotes the event day of calling 12333. The x-axis denotes the days to the call. The standard errors are denoted by the length of the vertical bars. About 20% of the callers had already stopped commuting one month prior to the call. The cumulative probability grows to 80% by three months after the call. These cumulative probabilities are remarkably robust to different non-commuter definitions (whether by week or by month).

pact of the pandemic on unemployment. In 2020, the average number of noncommuters was 38,729, or 7.4% of all workers (commuters plus noncommuters). The 72% increase during the Phase II reopening relative to the baseline corresponds to a 5.3-percentage-point increase in the number of noncommuters over the same period in 2019.

The use of commuting patterns to measure unemployment offers a significant advantage over the use of measures derived from applications for unemployment benefits: it is not subject to participation bias (eligible people not participating). It is estimated that 66% of eligible households do not participate in major social programs in the U.S. due to inertia, lack of information, stigma, and time and hassle costs associated with applications (Ribar, 2020). Nonparticipation is much more severe in developing countries due to limited program benefits. In comparison, 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, respectively, but returned to their pre-pandemic level in Phase II. 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 status based on changes in commuting patterns.

5.2. *Effect on unemployment benefit claims*

Table 2 examines the pandemic's impact on the log number of individuals calling the unemployment benefit hotline (column (1)) and the call duration (column (2)). The table presents the coefficient estimates of β_q in Eq. (1) except that the ten-day intervals are grouped into four periods: 1–30 days before the lockdown, during the lockdown, Phase I reopening, and Phase II (full) reopening. Echoing the results in Figure 2,

Table 1
The pandemic's impact on noncommuters and working hours on-site.

Variable	(1) No. of noncommuters (two-week window, in log)	(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 pandemic's effect on the number of noncommuters defined over a two-week window and duration of on-site working hours. It is similar to Equation (1), except that the ten-day intervals are grouped into four periods: before the lockdown, during the lockdown, Phase I reopening, and Phase II full reopening. The observations are at the neighborhood by fortnight level. The dependent variable is the log number of noncommuters in column (1) and log number of average working hours for commuters in column (2), respectively. A non-commuter is an individual who visits his work location at least 15 days in the previous 30 days and stops commuting altogether in the current two-week window. 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. $^{*}p < .1$, $^{**}p < .05$, $^{***}p < .01$.

the number of individuals calling the unemployment benefit hotline decreased by 31% during the lockdown and increased by 28% during the Phase I reopening and nearly 57% during the Phase II full reopening. As discussed above, the level of unemployment calls is a lower bound estimate of the number of unemployed workers, 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 (Fig. 2), the percentage change in unemployment calls in the Phase II reopening period estimated in Table 2 (57%) is likely a reliable measure of the percentage change in unemployment benefit claims as a result of the pandemic.

The call duration displays a similar pattern: the average call time dropped during the lockdown but increased after the reopening. The call duration increased partly because more migrants—who generally need to provide more information than local residents—applied for unemployment benefits after the reopening (as we show below in the heterogeneity analysis).

Finally, Table 3 replicates the analysis in Table 2 but is limited to individuals who both called the unemployment benefit hotline and stopped commuting. These individuals are less likely to be misclassified as unemployed. The results are similar to those reported in Table 2.[18] For example, the effect size on unemployment claims during the Phase II reopening is 0.57 in Table 2 and 0.49 in Table 3. The effect on the call duration is 0.75 for the full sample (all individuals calling the hotline) and 0.78 for the restricted sample (individuals who both reach out to the hotline and stop commuting altogether).[19]

5.3. 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 Fig. 1 by gender, age, and migrant status and plot the coefficient estimates in panels (a)–(c) of Fig. 5. Specifically, the dependent variable is the difference between women and men in the logarithm number of noncommuters in panel (a), between individuals 40 years old and above and those under 40 in panel (b), and between migrants and nonmigrants in panel (c). Women are more affected by the pandemic. The percentage increase in female noncommuters is 10–20 percentage points higher than that in male noncommuters during the lockdown and Phase I reopening. The gap becomes smaller and statistically insignificant for the Phase II reopening.

Older workers fared worse than younger cohorts: the percentage increase in the number of noncommuters among workers 40 and above was approximately 20–60 percentage points higher than that for workers under 40 during the lockdown and Phase I reopening periods. Even for the Phase II reopening, the gap between the two age groups still remains at approximately 20 percentage points. Lastly, migrants were more severely affected by the pandemic. The number of noncommuters among migrants increased more during the lockdown but especially the Phase I reopening relative to that among nonmigrants. By the end of the Phase II reopening, the percentage increase in the number of migrant noncommuters was still approximately 40 percentage points higher than that among nonmigrants, highlighting the disproportionately large and lingering burden on migrants. According to the NBS, the number of migrant workers in 2019 declined by 3.8 million nationwide by September 2020. The escalating number of migrants who stopped commuting to work in our sample is consistent with the massive reduction in the number of migrant workers reported by the NBS and suggests large-scale layoffs among migrant workers.

To further examine the heterogeneity in the impacts across demographic groups, we conduct an analysis at the neighborhood level in Guangzhou, for which we have data on micro-level social economic variables. Specifically, we regress the percentage change in the number of noncommuters between 2019 and 2020 on the quadratic forms of the 2018 average housing price and migrant share in each neighborhood (i.e., cell tower area). Fig. 6 plots the predicted percentage changes against the average housing price and migrant share. Neighborhoods with a lower housing price and a higher share of migrants experienced a higher percentage increase in noncommuters. Specifically, a one-standard-deviation increase in the migrant share and housing price would induce 22.1-percentage-point increase and 2.7-percentage-point decrease in noncommuters, respectively. 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). Additionally, we test whether the number of migrants can explain the underestimation of the official unemployment rate. First, we compute the differences between the official unemployment rate in 2020 and those we derived based on the commuting sample. Next, we find that the correlation between the differences and the ratio of migrants across cities in 2020 reaches 0.63, indicating that migrants are one major contributor to the underestimation of the official unemployment rate.

In addition, the disproportionately harsh impacts on women, older workers, and migrants likely reflect the heterogeneity in the pandemic shocks across industries, as we show below in Fig. 7. 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 A1). Panel (a) of Figure 7 examines the impact of this heterogeneity across cities. The figure reports coefficients on the interactions of the pandemic treatment variable (which takes one from January 23, 2020, to September 30, 2020) and city dum-

[18] We also replicate Table 3 for the commuting sample, including those who contact the unemployment benefit hotline. The estimates are only slightly smaller than those in Table 3, and the qualitative patterns remain the same.

[19] In Table 2, the coefficient for Phase II is 0.45 for calls to the unemployment benefit hotline and 0.56 for the call duration. In Table 3, the coefficients are 0.4 and 0.58, respectively. The effect sizes reported in the main text are calculated by $\left(\exp[\hat{\beta} - \widehat{var(\beta)}/2] - 1 \right)$, as discussed in Section 3.

Table 2
The pandemic's impact on calls to unemployment benefit hotline and call duration.

Variable	(1) No. of individuals making calls (in log)	(2) Average call duration (in log)
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)
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: The dependent variable is the log number of individuals calling unemployment benefit hotline in column (1) and the log of average call duration in seconds in column (2), respectively. The table examines the pandemic's effect on the number of individuals making unemployment calls and call duration. It is similar to 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 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<0.10; **p<0.05; ***p<0.01.

Table 3
The pandemic's impact on calls to unemployment benefit hotline by noncommuters.

Variable	(1) No. of noncommuters making calls (in log)	(2) Average call duration (in log)
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)
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 results in Table 2, but limits the sample that reaches out to the unemployment benefit hotline to noncommuters. 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.

mies, following Equation (2). The heterogeneity across cities is sizeable: the number of noncommuters increased for all but one city, and the range varies from 10% to as high as 150%.[20] Economically more developed cities such as Guangzhou and Zhuhai experienced the largest increase, while less developed cities such as Yangjiang seem to have remained 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 corresponding average share was less than 3% among the seven least affected cities. To illustrate the heterogeneity in the impact across industries directly, we run a separate regression following Equation (2) where we interact the pandemic treatment variable with city-level labor shares by industry for all thirteen major industries.

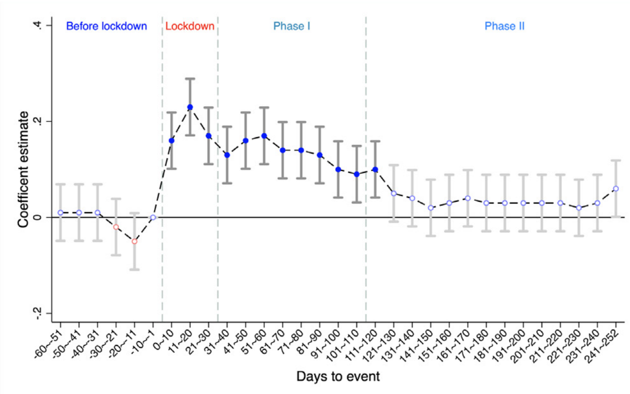
Panel (b) of Figure 7 reports the coefficients for all industries. Hotels and catering, real estate, and leasing and business experienced the largest increase in the number of noncommuters, ceteris paribus. In comparison, the finance, health care, and education sectors witnessed reductions in the number of noncommuters, consistent with findings based on data from other countries (Adams-Prassl et al., 2020; Alon et al., 2020). To evaluate the importance of industry composition, we predict the number of noncommuters in logs (the dependent variable) using the coefficient estimates and each city's observed labor share across industries and compare the range of the predicted values with the observed range of the dependent variable. The variation in industry composition across cities contributes to 38.7% of the changes in the number of noncommuters.

In addition to their differences in industrial composition, cities have differential trade exposure, as measured by 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, with an export-to-GDP ratio of only 2.5%. At the other extreme is Dongguan, whose export-to-GDP ratio is 91.0%. As shown in panel (a) of Figure 7, the pandemic's impact on Shantou's unemployment was much milder than that on Dongguan's. In Table 4, 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 ten-percentage-point increase in the 2019 export-to-GDP ratio is associated with a 4.4% increase in the number of noncommuters for a given city. Like the variation in industry composition, the variation in the export-to-GDP ratio is critical and explains 28.5% of the heterogeneity in the pandemic's unemployment impact across cities.

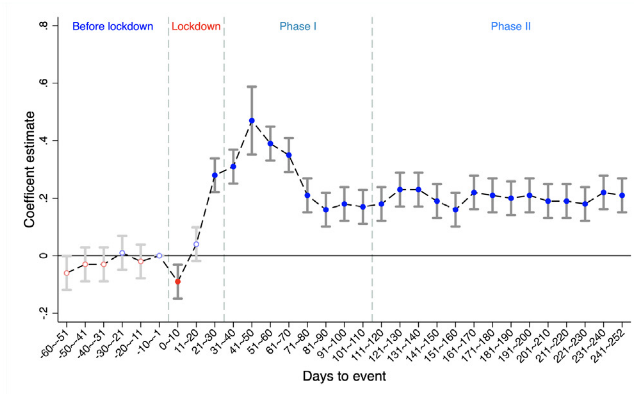
The sizeable estimates in Table 1 (a 72% increase in the unemployment rate) and the significant heterogeneity across cities and indus-

[20] The effect size for Yangjiang is -5% but statistically insignificant at the 10% level.

(a) Female v.s. male



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



(c) Migrants v.s. nonmigrants

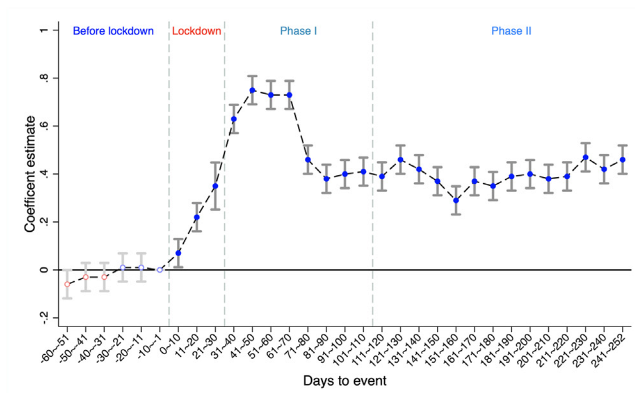


Fig. 5. Heterogeneity in the pandemic's impact on unemployment across demographic groups. Notes: All event-study graphs plot coefficient estimates of β_q from Equation (1). The dependent variable is the difference between log(female noncommuters) and log (male noncommuters) in panel (a), the difference between log noncommuters who are 40 and above and log noncommuters who are below 40-year-old in panel (b), and the difference between log noncommuters who are migrants and log noncommuters who are nonmigrants in panel (c). This analysis uses one week to define noncommuters. All regressions include neighborhood, event-week, and the treatment group fixed effects. The standard errors are clustered at the event-week level. Using two weeks or one month to define noncommuters delivers similar results.

Table 4
Heterogeneity in the pandemic's impact on noncommuters by export-to-GDP ratio.

Variable	(1) No. of noncommuters (two-week window, in log)	(2) Working hours (in log)
Phase II re-opening * (Export/GDP in 2019)	0.37***	–0.02*
	(0.07)	(0.01)
Phase II re-opening (=1)	0.03***	–0.06***
	(0.01)	(0.01)
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 (2), where we interact the phase II re-opening dummy with a city's 2019 export-to-GDP ratio (in percentage). The dependent variable is the log number of noncommuters in column (1) and log of the average working hours for commuters in column (2), 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. $^{*}p < .1$, $^{**}p < .05$, $^{***}p < .01$. Results are similar using one week or one month to define noncommuters.

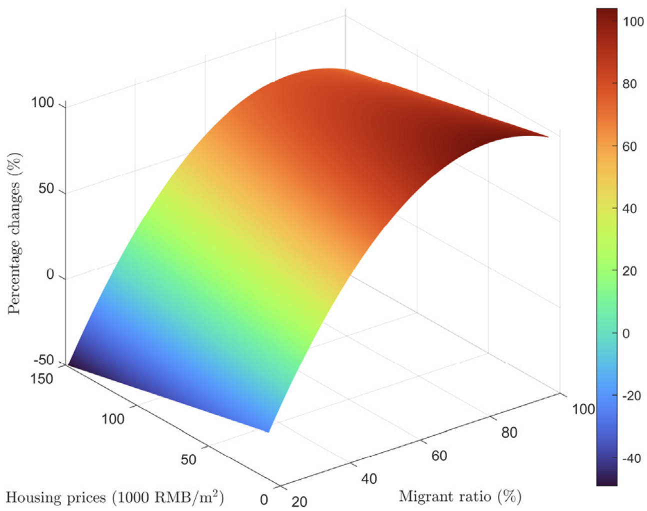


Fig. 6. Changes in unemployment by income and migrant share. Notes: This graph depicts the percentage changes in the number of noncommuters between 2019 and 2020 at the neighborhood level (i.e., cell-tower-area) based on a regression that includes quadratic forms of the average housing price and migrant share for each neighborhood in Guangzhou. The housing prices come from Soufang.com. Migrant shares are based on our phone data in 2018.

tries as highlighted in Figure 7 speak to the severity and unevenness of the pandemic's labor market impacts and the importance of conducting analysis at granular levels. In addition, these results illustrate the ripple effect of the pandemic across cities within a country and across countries around the globe through the supply chain and trade channels, where a city's (or country's) industry composition, its exposure to trade, and the nature of the supply chain are crucial determinants of the effects of the pandemic on its economy (Forsythe, 2020; Goldberg, 2020; von Gaudecker et al., 2020; World Trade Organization, 2020a; 2020b).

Unemployment benefit claims

We repeat the heterogeneity analysis for unemployment benefit claims based on our data on calls made to the unemployment benefit hotline. Figures 8, 9 and A9 present the heterogeneity in the impact across demographic groups, cities and industries, as well as by household income and migrant share. The results are qualitatively similar to those based on noncommuters: women, workers over 40, and migrant workers experienced a large increase in unemployment benefit claims with the onset of the pandemic. The same is true for areas with low income and a high migrant share, as shown in Figure A9. There is also a significant amount of heterogeneity across cities and industries, closely mirroring the patterns reported in Fig. 7. Finally, as shown in Table 5, a ten-percentage-point increase in the export-to-GDP ratio is associated with a 2.7% increase in the number of unemployment benefit claims for

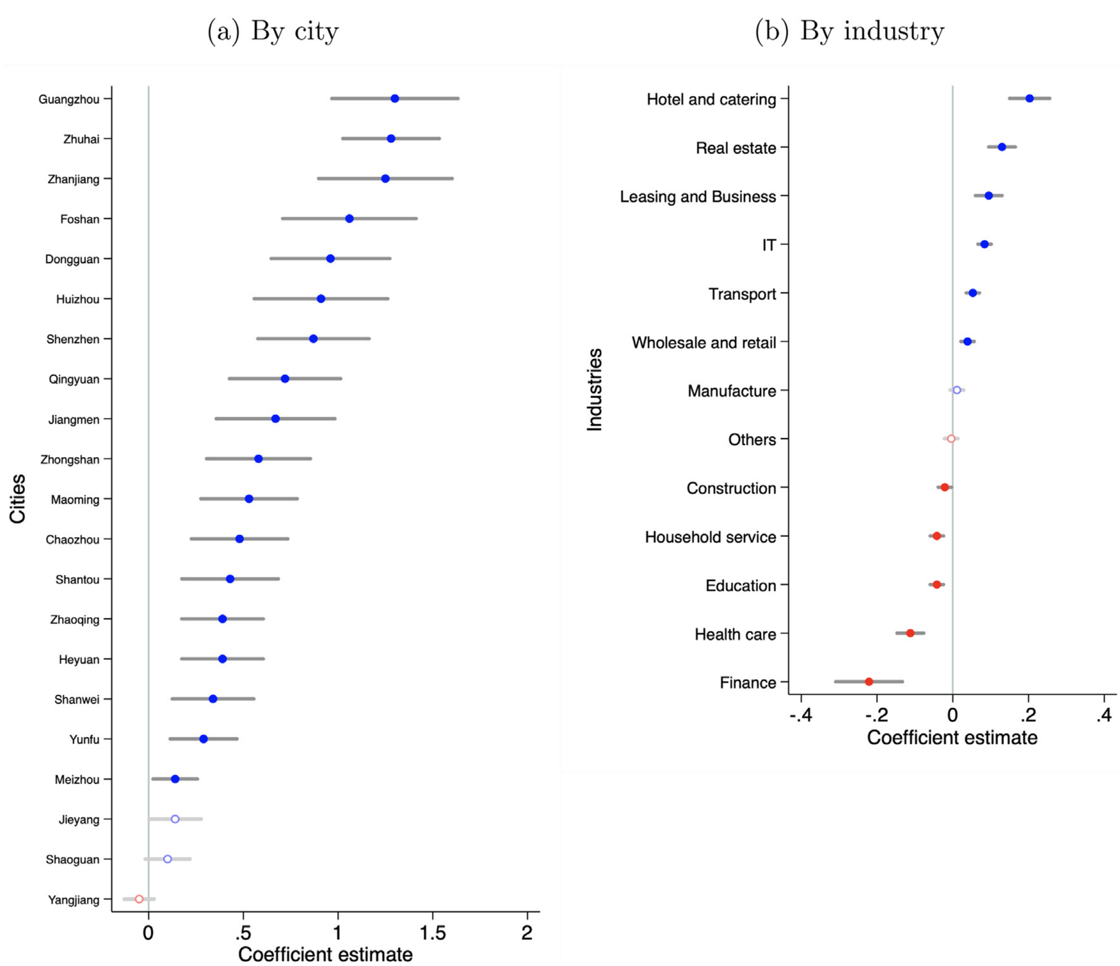


Fig. 7. Heterogeneity in the pandemic's impact on unemployment across industries and cities. Notes: This figure illustrates heterogeneity in unemployment (i.e., noncommuters) across cities (panel (a)) and industries (panel (b)) following Equation (2). In panel (a), we add interactions between the after-lockdown 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 noncommuters relative to 2019. This analysis uses one week to define noncommuters. 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 when using two weeks or one month to define noncommuters.

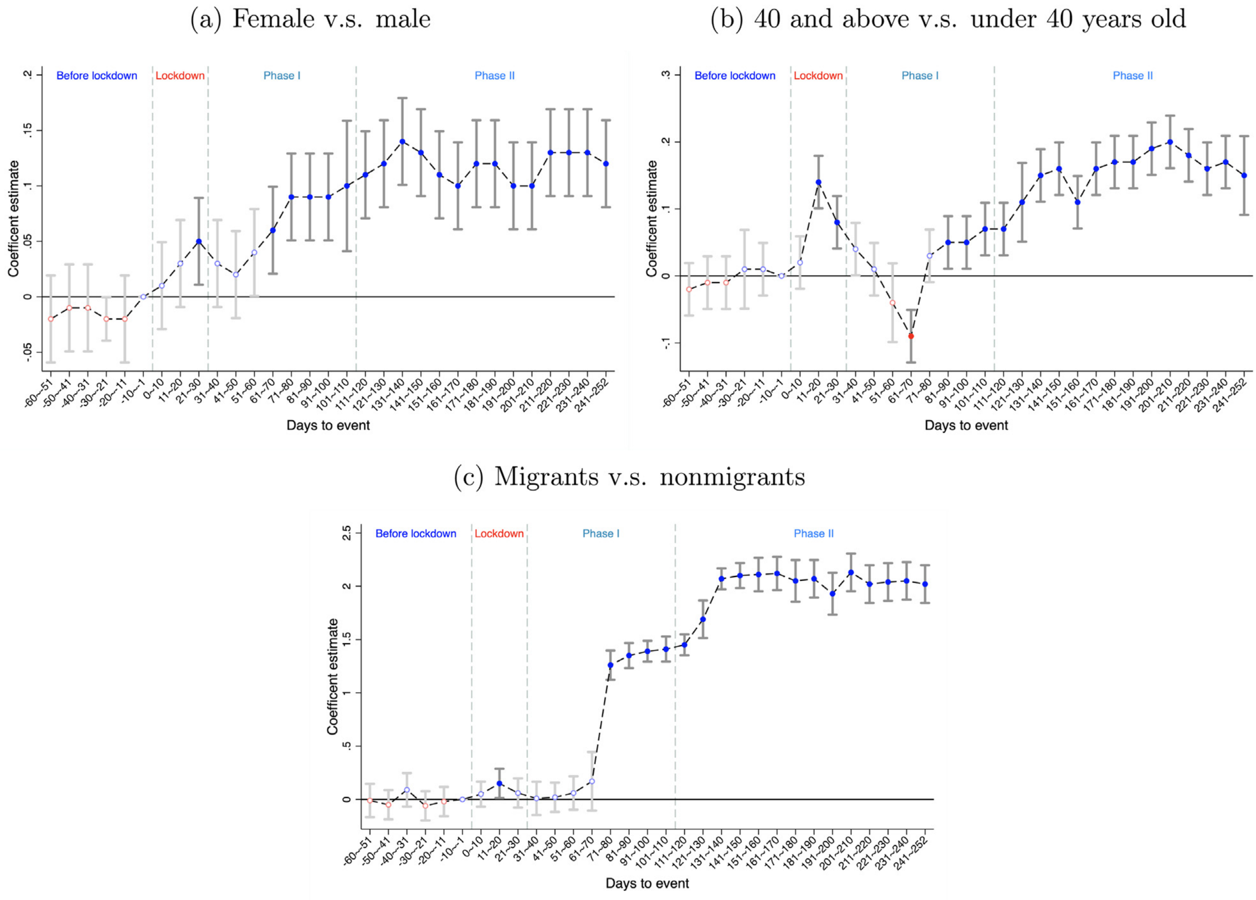


Fig. 8. Heterogeneity in unemployment benefit claims across demographic groups. Notes: All event-study graphs plot coefficient estimates of β_q from Equation (1). The dependent variable is the difference between log number of female calling the hotline and log number of male calling the hotline in panel (a), the difference between log number of individuals who call the hotline and are 40 and above and log number of those who call the hotline and are under 40 in panel (b), and the difference between log number of migrants calling the hotline and log number of nonmigrants calling the hotline in panel (c). 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.

Table 5
Heterogeneity in the pandemic's impact on calls to unemployment benefit hotline by export-to-GDP ratio.

Variable	(1) No. of individuals making calls (in log)	(2) Average call duration (in log)
Phase II re-opening * (Export/GDP in 2019)	0.24***	0.25***
	(0.05)	(0.06)
Phase II re-opening (=1)	0.43***	0.47***
	(0.06)	(0.09)
Observations	489,514	489,514
R-squared	0.76	0.54
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 (2), where we interact the phase II re-opening dummy with a city's 2019 export-to-GDP ratio (in percentage). 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. $^{*}p < .1$, $^{**}p < .05$, $^{***}p < .01$.

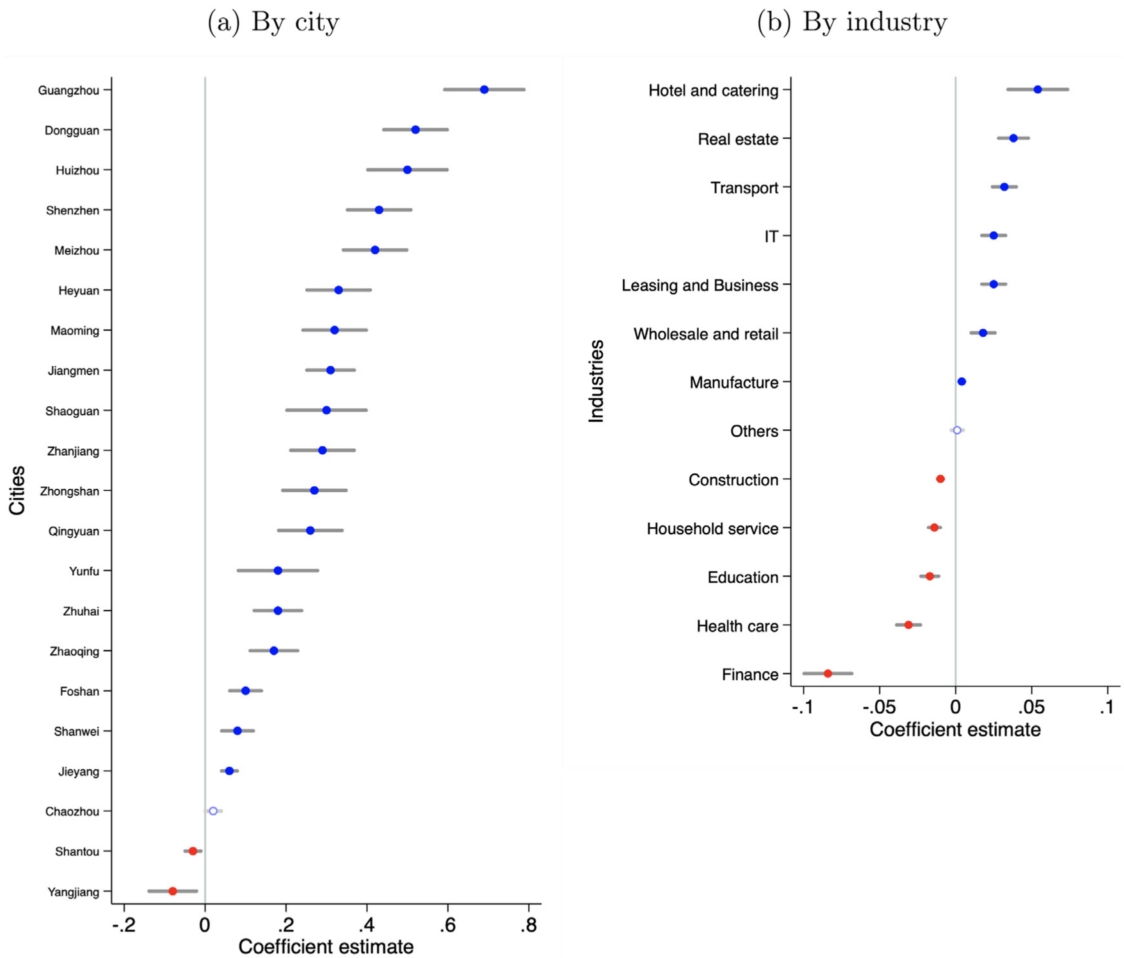


Fig. 9. Heterogeneity in unemployment benefit claims across industries and cities. Notes: This figure illustrates heterogeneity across cities (panel (a)) and industries (panel (b)) following Equation (2). In panel (a), we add interactions between the after-lockdown dummy and city fixed effects. In panel (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 the number of individuals reaching out to the unemployment benefit hotline 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.

a given city, consistent with the result from Table 4 based on our commuting data. At 2.7%, the magnitude is slightly lower than that of the increase in unemployment, which is expected due to the limited participation in unemployment benefit programs.

One might be concerned that increases in the number of individuals calling the unemployment benefit hotline are driven merely by a higher awareness of unemployment benefits after the onset of the pandemic. However, as shown above, women, workers above 40, and especially migrants show a significantly higher increase in hotline calls than other groups of workers. They are precisely the subpopulations that were worst hit in terms of employment during the pandemic. In addition, the changes in the number of hotline calls are highly uneven across industries. There is also a great deal of heterogeneity across cities. The number varies from -8% in Yangjiang to 99% in Guangzhou, closely mirroring the industry and worker composition across cities. These patterns are unlikely to be driven purely by a significant increase in the awareness of the unemployment hotline during the pandemic, as information on these government services is primarily disseminated at the national and provincial levels rather than at the subpopulation, industry, or city level.

5.4. Robustness checks

The analysis of commuting patterns discussed above uses the two-week window to define a commuter. We construct two alternative measures of commuters using the one-week and one-month windows. Reassuringly, these three variables are highly correlated: the correlation is 0.95 between the one-week and two-week measures, 0.92 between the one-week and one-month measures, and 0.97 between the two-week and one-month measures. In addition, more than 94% of individuals who are noncommuters over a two-week window remain noncommuters over the entire month. These patterns suggest that our commuter measures accommodate flexible work modes (e.g., occasional WFH). When individuals stop visiting their workplace altogether over an extended period as defined in our analysis, they are essentially not working rather than working at home, as we discuss in detail in Section 4.4.

In panels (a) and (b) of Table A3, we repeat the analysis using noncommuters defined over the one-week and one-month windows. The estimated effect size is 75% and 71% under these alternative measures, similar to the baseline estimate of 72% in Table 1. Table A4 replicates Table 1 but limits the sample to noncommuters who do not use any e-mail or virtual meeting apps when they stop commuting. This is a demanding set of criteria. The coefficients and the implied effect sizes remain robust at approximately 70%. These patterns corroborate the evidence above that WFH is unlikely to drive our results and that our measures of commuting can successfully discriminate between hybrid work modes and unemployment.

Our main analysis includes users aged 18 years or older. As some users between the ages of 18 and 25 might still be in school, we exclude users under age 25 as a robustness analysis. The results on noncommuters (Table A5) and calls to the unemployment benefit hotline (Table A6) barely change when we limit our sample to users aged 25 and above.

The last two robustness checks replicate the baseline analysis but weight each observation with the average number of noncommuters per day in each neighborhood in 2018. The results are reported in Tables A7 and A8 for noncommuters and unemployment calls, respectively. The results are slightly larger in the weighted regressions but qualitatively the same.

6. Conclusion

Based on granular and high-frequency mobile phone data from China's most populous province, our analysis has found that the pandemic led to a 72% increase in unemployment and a 57% increase in unemployment benefit claims even after the full reopening relative to their counterparts during the same period from May to September in 2019.

While dramatic, these effects are smaller than those in the United States. This is 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, employed 79% of the workforce and produced 68% of GDP in the United States in 2018, compared to 47% and 50%, respectively, in China. 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 during our data period.

Our analysis shows uneven labor market impacts across demographic groups and industries. This heterogeneous impact is consistent with findings from recent studies on other countries (Adams-Prassl et al., 2020; Alon et al., 2020). 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 are more exposed to external shocks through global trade channels. Future research can use our approach to study the longer-term impacts and the impacts of the most recent lockdowns across many cities in China.

CRediT authorship contribution statement

Teng Li: Conceptualization, Methodology, Data curation, Writing – original draft, Writing – review & editing. **Panle Jia Barwick:** Conceptualization, Methodology, Data curation, Writing – original draft, Writing – review & editing. **Yongheng Deng:** Conceptualization, Methodology, Data curation, Writing – original draft, Writing – review & editing. **Xinfei Huang:** Conceptualization, Methodology, Data curation, Writing – original draft, Writing – review & editing. **Shanjun Li:** Conceptualization, Methodology, Data curation, Writing – original draft, Writing – review & editing.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jue.2023.103543.

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