Cost of Zero-Covid: Effects of Anti-contagious Policy on Labor Market Outcomes in China

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Abstract

We study the effect of the anti-contagious policy on labor market outcomes. By exploiting variation in the duration of the zero-Covid policy in China, which is triggered by the outbreak of new cases of COVID-19, we find that a 10% increase (3.7 days in average) in the duration of the zero-Covid policy caused the probability of unemployment to increase by around 0.1. We show that the estimated policy effect is disentangled from the health shock effect. By a back of envelope calculation, we estimate the increase in unemployment probability would decrease by 12 percent if the restriction of lifting the zero-Covid policy was relaxed from a 14-day zero case window to a 5-day restriction. Moreover, the zero-Covid policy decreases the labor income and hours worked for employed individuals, and the policy effect is heterogeneous across demographic groups. We also examined the policy effect during different phases of the pandemic, and the results imply that the stringent containment during the first stage of the pandemic caused the negative impacts on the labor outcomes, while the subsequent precise containment strategy did not generate significant influence on the labor market outcomes.

Keywords: COVID-19, Zero-Covid policy, unemployment, labor market

JEL Codes: I12, J20, J18

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1 Introduction

Most countries around the world have taken various containment measures to limit the spread of COVID-19, including closing public gathering places, limiting transportation services, implementing stay-at-home mandates or lockdowns, and so on. However, consensus regarding the economic impact of the anti-contagious measures is limited. Some critics of anti-contagious policies claim that they slow economic growth and hurt consumer spending, while proponents argue that the economy would still deteriorate without these measures due to the fear of viruses.

In this paper, we examine the effect of anti-contagious policies on labor market outcomes. One of the substantial challenges in evaluating the costs and benefits of different anti-contagious policies is to distinguish between the economic damage caused by the anti-contagious measures and the direct public health shock. In the face of this unprecedented pandemic, most countries are unable to contain the emergence of new cases right after implementing the disease prevention policies, thus leading to a persistent public health shock as well as the impacts of the mitigation policies (Goolsbee and Syverson (2021)).

In contrast, after the outbreak of the pandemic in Wuhan, China quickly adopted the most stringent disease prevention and control policies, which effectively stopped the spread of the virus in most areas (Qiu et al. (2020); Fang et al. (2020); Lai et al. (2020); Hsiang et al. (2020); Sudarmawan et al. (2022)). This zero-Covid policy adopted by the Chinese government requires immediate disease prevention measures after finding new cases, as well as a 14-day observation window before lifting the restrictions. When new Covid cases arise, this approach aims to eliminate the virus as soon as possible. Therefore, the economic fallout is mainly due to the anti-contagious policy in China, rather than the public health shock.

Another challenge to accurately estimating the impact of the zero-Covid policy is the spillover effect. As soon as a city implements stringent anti-contagious measures, such as city lockdowns, human mobility within its region and with other regions will fall dramatically and business will cease. This implies that a city's zero-Covid policy could influence the economic activities in its nearby regions if their economic connections were strong before the outbreak. We control the spillover effect by calculating every city's nearby zero-Covid policy duration, and our results show that the estimated local policy effect is not driven by the spillover effect.

Last challenge to identifying the causal effect of anti-contagious policy is the endogeneity of the policy decision. As the infected population grows and the severity of the mass panic increases, it is more likely to have a more stringent policy intervention while the damage to the economy fundamentals may already have occurred. This endogeneity correlates the economic decline with the anti-contagious policy, and hinders the identification of causal effects. Fortunately, this concern was alleviated in the case of China. Under zero-Covid policy, cities exposed to the Covid received nearly a homogeneous treatment. Since the emergence and temporal distribution of new Covid cases are unpredictable, and the containment actions are taken immediately, the starting date and duration of disease prevention measures are exogenous to variables affecting pandemic policy, such as city-related economic factors and the scale of the outbreak.

Our paper exploits the policy design and employs a generalized Difference-in-Differences (DID) strategy to estimate the causal effect of the duration of zero-Covid policy on labor market outcomes. The estimation result indicates that an 10% increase (in average 3.7 days) in the policy duration causes the individual unemployment (we refer to U-4 unemployment definition here and in the rest of this paper – a worker is "unemployed" if unemployed or discouraged) probability increase by around 0.1. Furthermore, our result disentangles the labor market impact of the anti-contagious policy from the public health shock and the spillover effect from nearby regions. Since the zero-Covid policy puts the health loss prevention to the top priority, we provide the evidence of the necessary economic cost in eliminating the pandemic where disease containment is the first consideration.

This paper relates to several strands of literature. First, it is related to the increasingly large empirical literature on the impact of COVID-19 pandemic on labor market. The pandemic causes a general negative effect on labor outcomes including employment, hours worked and income, with heterogeneous magnitudes across different countries and among different groups of workers. [1,2] Our paper contributes to this literature by providing evidence from the most recent Chinese individual-level microdata to analyze the pandemic impact on labor market outcomes. China insists in the public health policy that aims to minimize the mortality of COVID-19 at any cost, and our work provides a benchmark of the zero-Covid policy in pandemic containment policy comparisons. Our result helps better understand the economics cost of the most restrictive anti-contagious policy at the individual level.

Within the recent studies estimating the impact of the anti-contagious policy on labor markets, our work is most related to several literature. Gupta et al. (2020) applies a DID structure to estimate the causal effect of social distancing policies on labor market in US during the early phase of the pandemic. Their counterfactual estimate shows that social distancing policies explain about 60% of the realized decline in employment, while without the social distancing policies it is likely to endure a more severe public health problem which could in turn deteriorate the labor outcomes. Hoshi et al. (2022) uses a measure of people's mobility with policy instruments and implements a 2SLS estimate on the effect of restricted mobility induced by policy on labor market outcomes. Their use of policy as an instrument helps create the exogenous change in mobility, while our framework enables us to create exogenous length of city lockdown and causally interpret the lockdown impact on labor outcomes. Aum et al. (2021a,b) provide a benchmark for the marginal unemployment rate change in the number of infections where there is no mandated lockdowns in South Korea. In the contrary, our result provides a reference for the marginal change in individual unemployment probability where the region implements anti-contagious measures without a significant scale of the pandemic in China.

[1] Coibion et al. (2020), Mongey et al. (2021), Larrimore et al. (2022), Forsythe et al. (2020), Béland et al. (2020) analyze the pandemic impact on the US labor market and household income; Zimpelmann et al. (2021) investigates the working hour and income change in Netherlands; Alstadsæter et al. (2020) investigates the labor market disruption in Norway; Adams-Prassl et al. (2020) documents immediate impact of the pandemic on the employment status for workers in UK, US and German; Borjas and Cassidy (2020) investigates the shock on the US immigrant employment ; Cajner et al. (2020) and Kurmann et al. (2021) analyzes the pandemic shock on the US labor market from both of supply and demand sides by using the payroll data and real-time establishment-level data; Benzeval et al. (2020) investigates the idiosyncratic impact of the pandemic for different demographic groups in US; Chetty et al. (2020) investigates the heterogeneous impact on the labor market based on a granular level real-time private company data.

[2] A survey on this topic could be found at Brodeur et al. (2021); an overview on the global labor market influence could be found at OECD (2020, 2021).

Our identification design allows us to analyze the disentangled impact of zero-Covid policy on the labor market, rather than the combined impact of both COVID-19 mitigation policies and its public health shock, as the public health shock is limited to the minimum level in China. There is no other research, to our best knowledge, conduct analysis on the impact of anti-contagious policies on labor market without the existence of the public health shock caused by the pandemic. Compared to most countries analyzed in the literature, China experienced very limited pandemic surge in 2020 after the very first outbreak in Wuhan. As Chinese society is not largely influenced by the health threat of the pandemic, the zero-Covid policy contributes the most to the observed labor market disruption. Thus, our result sheds light on the isolated policy effect on the economic activity during the pandemic.

Second, this paper contributes to the research on the economic impact of COVID in China. Recent literature on COVID-19 impact in China (Zhang (2021); He et al. (2020); Qi et al. (2022); Chen et al. (2022)) focuses the influence from city lockdowns, while this paper identifies the zero-Covid policy effect at all intensities. During the early period of China's anti-Covid campaign, many cities implemented lockdowns to block the spread of the virus quickly and efficiently, while more cities which experienced mild outbreak of the epidemic chose less stringent measures to contain this public health crisis. Our estimation results include the impact of zero-Covid policy not limited to the lockdown, but any anti-contagious measures will be counted. Our work unveils the unclear question that how much impact did these non-lockdown measures impose on the labor outcomes.

Finally, this paper is related to the research on human mobility restriction in response to pandemic threats. Many countries implemented measures that limit the human mobility flows to stop the transmission of infectious diseases (Cooper et al. (2006); Bajardi et al. (2011); Wang and Taylor (2016); Charu et al. (2017)). Meanwhile, the evaluation of restrictions on human mobility remains obscure for two major concerns, the negative economic impacts and the effectiveness of such policies in containing the pandemic. It is also hard to disentangle the impact of human mobility from other channels (Ferguson et al. (2006); Hollingsworth et al. (2006)). In this paper, we provide an estimation of the disentangled effect in the labor market of one specific mobility restriction policy, the zero-Covid strategy, which is proved to be effective in delaying and containing the spread of the pandemic (Fang et al. (2020)). Our results contributes to the evaluation of human mobility restriction policy by providing a reference of the potential economic cost of halting the pandemic in perspective of labor outcomes.

This paper is organized as follows. Section 2 introduces China's anti-contagious policies after the outbreak of COVID-19. Section 3 summarizes the individual survey data, Covid data and regional economic data. Section 4 displays our identification strategy and our estimation results are reported in Section 5. Section 6 concludes this paper.

2 Background

China's zero-Covid policy [3] consists of two components, stringent clearance and dynamic clearance. Stringent clearance includes policy responses such as quarantine, lockdown and traffic restriction. However, in regions with mild outbreaks, dynamic clearance policies with fewer restrictions on human mobility are implemented. In the initial outbreak of the pandemic, from January to February 2020, the stringent clearance prevailed in areas with COVID cases. As the government started aiming to resumption of work and production after Feb 17, 2020, zero-Covid policy became a hybrid between stringent clearance and dynamic clearance.

2.1 First Phase: Stringent Clearance

China implemented a series of unprecedented lockdowns and non-pharmacological anti-contagious policy measures in an effort to halt the spread of COVID-19 since January 23, 2020 [4] [5]. Based on Figure A1, by January 25, 30 out of China's 31 provinces had enacted First level emergency response, measures taken including case isolation, suspension of public transportation and public space closure, etc. ((Qiu et al., 2020) and (Tian et al., 2020)). Local governments reacted

[3]The zero-Covid defined by Chen et al. (2022) only refers to the stringent clearance in our paper.

[4]according to Emergency Response Law of the PRC, the emergency events are classified into 4 levels, First as extreme important and Forth as normal. The First level emergency response is coordinated by the central government, Second level is led by province government, third level is led by the prefecture government and the Fourth level is led by county government. There is no specific instruction on how to response to different emergency levels,(i.e, lockdown or travel restriction), this province level indicator is considered as a bellwether for province government's attitude towards COVID.

[5]Ironically, shown in Figure A1, Hubei province, the center of COVID outbreak, only acted the Second level emergency response on Jan 24, and upgraded to the First level on the next day.

with *stringent clearance* policies in response to the unprecedented national emergency. The entire Hubei province implemented lockdown in Jan 24, and its residents could not leave their prefectures. There were also strict anti-contagious policies implemented in other provinces, including a partial shutdown, a ban on traffic leaving and a 14-day self-quarantine period for visitors. According to Qiu et al. (2020), up to 14,000 health checkpoints were set up at ferry and highway service centers. By February 16, more than 250 prefectures rolled out measures such as "closed management of communities", "family outdoor restrictions", "only one person of each family may go out for shopping once every 2 days", "tracing and quarantining close contacts of suspicious cases" and so on[6]. Under such stringent clearance policies, in January and February, economic activities were rigorously suppressed (Fang et al., 2020). In Appendix section 4, we provided two anecdotal stories about the *Stringent Clearance* during January 2020.

There is an important point to note that the 14-day observation window has already been set as epidemiological criteria to define a suspected case since January 18, 2020 (Li Q, 2021) and was publicly mentioned in a National Health Commission guidance on January 22 [7]. Following the central government's guidance, local governments soon implemented this 14-day observation window, which will be an important instrument we use to construct our major treatment variable.

2.2 Second Phase: Stringent Clearance and Dynamic Clearance

Nearly one month after enforcing its stringent clearance policies [8], the central government attempted to re-boost the economy and partially relax its public health interventions. On February 17, the State Council and National Health Commission of China issued *Prevention Guidance for Novel Coronavirus Pneumonia (version 5)* which required local governments to classify different risk levels for different regions. Low risk areas, which are usually defined as prefectures with no COVID cases, should restrict travel from middle and high risk areas, while mobility within the prefecture and across other low risk areas were permitted. It is noteworthy that there could be *dynamic clearance* policies implemented at low risk areas, such as school closings, cancellation of

[6] no new prefectures adopted such measures between February 20 to June 30, 2020. Qiu et al. (2020)

[7] http://www.nhc.gov.cn/jkj/s3577/202001/c67cfe29ecf1470e8c7fc47d3b751e88.shtml

[8] "In) all Chinese cities, the Spring Festival holiday was extended, and people were advised to stay at home when possible, enforce social distancing and maintain good hygiene." (He et al., 2020)

public events and restaurant closures. The middle risk areas were defined as prefectures without an *outbreak* [9]. On average, the high risk areas were defined as those with more than 10 cases reported within 14 days[10]. The middle and high risk regions were both subject to stringent clearance strategies, including traffic restriction, Fangcang hospital (mobile cabin hospital), community isolation and forced stay-at-home orders [11]. Although this state-issued *Guidance* left local governments with the freedom to manipulate the boundaries between high and middle risk levels, the middle and high risk areas could only become low risk after 14 consecutive days of no case increase. this is considered to be a clear distinction between low risk level and the other two levels.

Local governments immediately followed the central government's guidance. By the end of February, half of China's provinces were out of the First level reaction. There might be high or middle risk areas (prefectures) within a Third level province, but the rest part of the province was more likely to adopt *dynamic clearance* policies or only keep travel restrictions for high risk areas. As of April 30, the national daily cases were already smaller than 50. Beijing and its neighboring provinces were lowered to the second level. Three days later, Hubei was lowered to the Second emergency response level and no provinces remained in the First response level.

3 Data

3.1 CFPS Data

The individual data are from the China Family Panel Studies (CFPS), which is a nationally representative survey conducted by Peking University's Institute of Social Science Survey. This longitudinal survey covers 25 provincial-level regions in China (excluding Hong Kong, Macao and Taiwan, Xinjiang, Qinghai, Inner Mongolia, Ningxia and Hainan), which accounted for 95% of China's total population.

We collect four waves of CFPS data, surveyed in 2014, 2016, 2018 and 2020, giving us a

[9] outbreak are defined as 2 to 5 cases increase in 14 days

[10] the threshold between the middle risk and high risk were set quite differently across local governments

[11] Again, there is no general distinction between the clearance strategies for the mid and the high risk regions. In some cases, residents of high and middle risk regions were strictly required to stay at home, with security patrols checking on violators. Food and medicine could only be ordered through delivery

sample of 139,983 observations. To arrive at the sample used for estimation, we first exclude observations who (i) were surveyed by proxy mode which lacks information on labor outcome (16,696 observations); or (ii) were full-time student (10,617 observations), leaving us with a sample of 112,670 observations. We further restrict attention to individuals who were between age 16 and 64, which reduces the sample to 93,357 observations.

To keep consistency across main results and dynamic effect results, we drop respondents who were not interviewed in CFPS 2018, i.e., 17,141 observations. We drop 8,654 observations whose county is not included in the county list provided by Peking University's Institute of Social Science Survey in 2010. Finally, we drop 811 observations that migrated to another county and 3,408 observations that appear only once in our sample and end up with an estimated sample of 63,343 observations (20,006 individuals). Among the 63,343 observations in our sample, 25.6 percent were surveyed in 2014, who belong to 124 cities. The observation in 2016, 2018 and 2020 is 27.6%, 29.0% and 17.8% respectively[12].

Our main outcome variable concerns individual unemployment status. There are several questions related to employment status in the CFPS questionnaire. Specifically, interviewees (excluding full-time students) are asked for the following questions: (1)"Including agricultural work, waged job, self-employment and private business (housework and unpaid help do not count), have you worked for at least one hour last week?" (2)"Do you have a job, but you are currently on temporary vacation, sick leave or other vacation, or on-the-job training?" (3) "Will you return to the original job position in a certain period or within six months?" (4) "Are you running your own business which is currently in an off-season, but will resume after a while?" (5) "Is your agricultural work (including cropping, managing orchard, collecting agricultural and forestry products, fish farming, fishing, raising livestock, selling agricultural products in market, etc.) in an off-season?" If all answers of an interviewee are "NO", the interviewee is on unemployment, otherwise, the interviewee is on employment [13].

Moreover, there are a question for employed people rather than self-employed people and business owners, "Including salary, bonus, cash benefit, material benefit, and excluding tax, insurances,

[12]Table A1 shows the distributions of sample by survey years.

[13]The definition of unemployment is similar to U-4 unemployment, which includes total unemployment and discouraged workers.

and public housing, how much in total did you make from this job for the last 12 months?" An outcome variable called *Income* is constructed accordingly. Finally, CFPS asked respondents a question, "How many hours per week on average did you work for this job in the past year?" An outcome variable called *Hours Worked* is constructed accordingly. Panel A of Table 1 present summary statistics for labor outcomes in our sample. Average unemployment is 0.173. Among the employed workers, average labor income is 20,992 RMB and average hours worked is 46.3 per week. To capture the responses of hours worked along the intensive and extensive margins. we also include unemployed workers and replace the missing value with zero [14]. Last but not least, in the 2020 survey, 1866 individuals answered the question " Did you loss your job because of COVID?". This binary variable, *COVID Unemployed*, takes value 1 if "Yes" and 0 otherwise.

To investigate heterogeneous effects of COVID-19, we use a series of basic demographics information from CFPS 2018. Specifically, we consider the following dimensions: (1) Gender: a dummy variable taking value 1 if the interviewee is female and 0 otherwise; (2) Age: age of the interviewee; (3) Education: a dummy variable taking value 1 if the highest level of education the interviewee has obtained is middle school or below and 0 otherwise; (4) Age of the youngest child: age of the youngest child of the interviewee. Panel B of Table 1 provides statistic summary for these demographic characteristics.

3.2 zero-Covid Policy *Duration*

The *Duration* of zero-Covid policy implemented in each prefecture is our primary treatment variable. To document the days that a prefecture be labeled as a middle or high risk region, thus potentially the zero-Covid measures were implemented in the prefecture, we rely on the time-series data of the daily new COVID cases from Jan 23 [15] to June 30[16]. Based on the national guidance for COVID-19 containment, a region will remain in middle or high risk level until a consecutive 14-day without new confirmed COVID case, then the risk level will degrade to low. We locate each prefecture's middle or high risk period by excluding the dates that have no COVID positive

[14] A similar measure are used by Gupta et al. (2020)

[15] Jan 23 was the time point when Wuhan lockdown and provinces enacted First level emergency response.

[16] CFPS 2020 survey was collected during the second half of 2020. We would like to ensure the surveyed individual is exposed to the influence of the zero-Covid policy and the pandemic before taking the survey.

cases and are not within a 14-day window of new COVID case, i.e., low-risk period. Panel C of Table 1 summarizes the statistics for zero-Covid policy and COVID-19 at prefecture level. Average duration of policy duration is 37.128 days. Average number of confirmed cases and death is 451.697 and 31.432, respectively. 34.9 percent of these prefectures implemented lockdown policy. Finally, for regression estimation, we use the log of the policy duration plus 1 as the major treatment variable.

Our measure of zero-Covid policy duration is instrumented by the guidance rule enforced by government. To test the validity of the treatment, we need to obtain prefecture level zero-Covid measures between January to June, 2020. However, there is no accurate measure of timing and duration during this period. He et al. (2020) and Zhang (2021) collect information of starting dates for lockdowns without ending dates, and thereby cannot provide the measure for the duration of lockdowns. Hale et al. (2022) generates a stringent index for China's COVID responses, however, the policy are measured at the province level. To our best knowledge, Chen et al. (2022) is the only research that provides the timing and duration of lockdown policies, by using web scraping and manual collection. Although Chen et al. (2022)'s research collects data between April 2020 and January 2022, where only 2 months are considered in our data sample, we could test the validity of our treatment by using *Duration* between April 2020 to January 2022 and compare it with lockdown duration provided by Chen et al. (2022).

We report the correlation between *Duration* or *lnDuration* and lockdown duration in Table A3. Columns (1) to (4) show positive and significant correlations between our treatment and the number of days under lockdown or the lockdown indicator. A 10-day increase in *Duration* is associated with 0.5 more days under lockdown. It is important to recognize that our measure also captures other zero-Covid interventions, which are less stringent than lockdown policy [17]. As we argued in Section 2, for prefectures with mild increase of COVID cases, less stringent policies are more likely to be implemented as they are able to mitigate the spread of virus. In Figure A2, we plot the confirmed COVID-19 cases versus zero-Covid policy duration for each prefecture, while categorized by whether prefectures experienced lockdown or not. The lockdown here is constructed by He et al.

[17] Lockdown are classified as most stringent policy by Anania et al. (2022)

(2020), defined differently from previous data source[18]. We could observe that prefectures with same level COVID cases and zero-Covid duration could vary in their lockdown decisions, which implies that a dummy variable for lockdown could not fully capture the spectrum of zero-COVID policies that a prefecture implemented.

Another way to validate the treatment is to check the survey question "unemployed due to COVID" from CFPS 2020, *COVID Unemployed*. In column (6) and (5) of Figure A3, we observe a positive correlation between *lnDuration* (*Duration*) and *COVID Unemployed* which verifies the validity of our treatment variable.

One potential concern about the construction of our treatment variable is that the intensity and the coverage of the anti-contagious measures during the early stage of the virus outbreak were more stringent compared to the later period when dynamic clearance were recommended by the central government. To cope with this issue, we further construct two duration variables corresponding to different time periods: one for the period between Jan 23 and Feb 17; another for the period after Feb 17 till the starting date of survey collection (June 30) [19]. In this way, we are able to capture the effects of zero-Covid policies on labor market outcomes in different phases of the pandemic.

3.3 Prefecture-Level Data

In addition to COVID-19 cases data, our empirical analysis relies on other prefecture-level data that come from the 2018 China City Statistical Yearbook. These variables include (1)Population; (2) Gross Domestic Product (GDP) [20]; (3) Share of Service Sector in GDP; (4) Highway freight volume, Highway passenger traffic volume, Water freight volume, and Waterway passenger traffic volume. Panel D of Table 1 summarizes statistics for prefecture characteristics in 2018. Average population is 5.586 million and average GDP is 396.489 billion RMB.

[18] They defined a city (prefecture) implements lockdown "when the following three measures were all enforced: (1) prohibition of unnecessary commercial activities for people's daily lives, (2) prohibition of any type of gathering by residents, (3)restrictions on private (vehicles) and public transportation."

[19] On Feb 17, State Council issued official document that regions should be classify into three different risk levels, as a plan to boost the economy

[20] The minimum GDP is 15937.7 thousand RMB

4 Identification

4.1 Baseline Model

We begin by examining whether the zero-Covid policy in China induces individual-level unemployment by estimating the generalized Difference in Differences model:

$$Y_{ipt} = \beta(lnDuration_p \times Post_t) + \sum_{t\in\{1,2,4\}} (X_p \times Year_t)\lambda_t + \theta_i + \delta_{r,t} + \epsilon_{ipt} \tag{1}$$

where Y_{ipt} represents the outcomes of interests (e.g. unemployment and hours of worked) of individual i, in prefecture p surveyed in year t. $lnDuration_p$ is constructed by the method mentioned in Section 3.2, which measures the duration of the zero-Covid policy at prefecture p in 2020 in log form. $Post_t$ is an indicator function that assigns one if the observation is from treated year 2020 and zero otherwise.

The parameter of interest β captures the marginal effects of exposure to zero-Covid policies on the labor outcomes. In contrast with binary treatment DD, the continuous treatment captures more variation in the data, the marginal effect provides more policy implication in real world and allows for comparative discussion with evidence from other countries (Gupta et al. (2020), Aum et al. (2021a) and Aum et al. (2021b)). According to recent discussion (Callaway et al., 2021), a potential selection bias problem [21] arise from the continues treatment DD setting and thereby we also report estimates from the binary treatment DD strategy for robustness purpose — we generate a binary treatment variable that assigns one if the $lnDuration_p$ is larger than the 50th percentile cut off. We will discuss more about the continuous treatment setting and potential challenges in Section 5.4.3 and Appendix 1.

To allow time invariant individual characteristics to influence unemployment or hours worked, we include individual fixed effects, θ_i . To absorbs trends differing across provinces, we include

[21] "Unlike classic selection bias which is the differences in Y(0) for two groups of people, the bias of a continuous treatment difference-in-differences comes from the heterogeneity in gains from the treatment. In other words, if groups of units have heterogeneous gains at some dosage, then the continuous treatment DD is contaminated by differences in different dosage groups own expected returns." (Cunningham, 2021)

province by year fixed effects, $\delta_{r,t}$. $Year_t$ is a series of binary indicators for year 2014, 2016, 2020 and the dummy for year 2018 (t=3) is omitted in the equation. X_p is a set of proxies for prefecture economic status, (i.e. population, GDP and share of service industry in 2018), we include $\sum_{t\in\{1,2,4\}}(X_p \times Year_t)\lambda_t$ to let their effects differ across year [22].

We cluster standard errors at prefecture level. In addition to the baseline setting, we use alternative clustering choices (province level, prefecture- year two-way clustering, province-year two-way clustering) as robustness checks.

4.2 Dynamic Model

Similar with Gupta et al. (2020), our generalized DD design rely on an assumption that after adjusting for controls and fixed effects, the patterns in outcome variables would follow a common path in the absence of zero-Covid policy. We employ a dynamic model to examine this assumption.

$$Y_{ipt} = \sum_{t\in\{1,2,4\}} \beta_t(lnDuration_p \times Year_t) + \sum_{t\in\{1,2,4\}} (X_p \times Year_t)\lambda_t + \theta_i + \delta_{r,t} + \epsilon_{ipt} \tag{2}$$

In this model, the parameter of interest β_4 represents the relative effect of duration of zero-Covid policy. β_1, β_2 provide the estimates of the relative impact on labor market outcomes up to six years prior to actual treatment. If the common path assumption holds, we should not observe relative impact from the "placebo" treatments on the pre-treated outcomes.

Same with previous section, we also use a binary treatment DD setting as robustness check. The underlying assumption is the common trends in pre-intervention outcomes between treated and control groups.

4.3 Threats to Identification

There are several threats to the identification assumption underlying our generalized DD design. First, The potential disproportionately distributed spillover effects from neighbor units would bias

[22] one possible solution is to control time variant characteristics, which mean to include post-treatment variables into regression. However, It will result in"Bad Control" problem. Moreover, the data of prefecture level controls in 2020 are not available yet

our estimation in either direction. Second, anticipation of zero-Covid policy shock could impact on post-treated outcomes through channels such as labor mobility or job opening. Third, the particular selection bias problem arise from continuous treatment DD setting — heterogeneous gains across different treatment doses, given a same counterfactual treatment dose ((Callaway et al., 2021) and (Cunningham, 2021)). We present evidence in Section 5.4 to support our identification assumption.

5 Results

5.1 Baseline Result

We first present our estimated zero-COVID policy effect on labor market outcomes using the baseline DD specifications. In Table 2 Panel A, we provide estimates for unemployment. The interaction of log form of the zero-Covid policy duration with a indicator for post-treatment is our DD estimator. In column (1), we control individual fixed effect and year fixed effect, and the result suggests that a longer duration of the anti-contagious policy has a causal impact on the increased chance of unemployment. An 10% increase in the duration of the zero-Covid policy increases the individual unemployment probability by 0.08, which is statistically significant at the 5% level. Rather than year fixed effect, we control for the province by year fixed effect in column (2), the interaction term of prefecture characteristics[23] with year fixed effect in column (3) and (4). Our estimation of the average policy effects remains stable and statistically significant in all these specifications.

As our result estimates the impact of the policy duration on the unemployment status, we are able to compare the negative effect of policies with different restrictions. We construct the duration of zero-Covid policy in the counterfactual scenario where the required zero-Covid window reduces from 14 days to 5 days. Then we predict the policy effect on the labor market using the constructed data. We find that, compared to an increase of 0.0371 in the unemployment probability caused by the current policy, the less restricted 5-day zero-Covid policy would only increase the

[23] log population and log GDP and share of service sector together

unemployment probability by 0.0324 in the average city, which is about a 12% decrease in the marginal policy effect.

Furthermore, Figure A4 displays how many years interviewees' cash or deposit could afford their expenditure if they become unemployed and have no other income. We calculate length of subsistence as the ratio between cash or deposit and family's yearly expenditure. For families located at the bottom 20% income distribution who are extremely vulnerable to unemployment, their saving could only maintain their basic family expenditure for around 6 months.

In Table 2 panel B column (1) - (4), we report the estimated effects of zero-COVID policy on log hours worked. It is noteworthy that we restrict our sample on the individuals who reported positive hours worked in year 2020 and thereby estimates are intensive margin responses. The results remain consistent with different controls. We find that the zero-COVID policy has a significant negative effect on the hours worked, as a 10% increase in policy duration would decrease the hours worked for employed individuals by around 0.2%, averagely 10 hours per week, depending on the regression specification.

As mentioned in section 4.3, the continuous treatment DD setting has a concern that there might exist systematic heterogeneous effects for prefectures with different lengths of the zero-Covid policy. For example, the 9-day duration counterfactual outcome for a prefecture with 10-day duration, would be different with the outcome of a prefecture with 9-day policy in the reality. To cope with this potential bias from the continuous treatment DD setting, we also estimate a binary treatment DD specification: First categorize prefectures into high and low treatment groups, using the median value of the policy duration as the threshold. Then estimate the coefficient of the interaction term of the dummy variable for high treatment groups and the time indicator for post-treatment, using all specifications considered in the baseline model.

The result for this binary DD estimation is reported in Table 2 column (5). It indicates that in average, the probability of unemployment for individuals in the high treatment group is 0.028 higher compared to their peers in the low treatment group, at the significance level of 5%. The estimated effect on hours worked stays negative, but noisy. The insignificant effect could imply that the working schedule has a rigidity in response to the pandemic shock such that the employed workers' working hours do not change much in the short term when comparing between highly and low policy-affected groups. It is also possible that increases in hours for workers who are quarantined in factories, and decreases for those who work from home. Another plausible explanation is that policy effect on hours worked estimated in the continuous setting is majorly driven by the observations that are at the tails of the overall distribution, while a large mass of sample is distributed around the median of the policy duration which causes the average sample in the high and low treatment group do not have a significant difference in the hours worked. Given all those above, the binary DD estimation provides additional evidence for policy effect on unemployment status and uncovers a more complicated patterns in hours worked.

In Table A4, we also report the estimated effect on hours worked for the entire population, including those who reported a zero working hour. Naturally, in the continuous DD settings, we could observe the magnitude of the policy effect increases compared to the intensive margin responses. Furthermore, in the binary DD setting, the policy effect is associated with lower hours worked, significant at the 5% level, which reflects the decreased hours worked from the unemployed groups.

In Table A5, we report the estimated effect of zero-COVID policy on log labor income for individuals who reported a positive earning. The results indicate that a 10% longer policy duration could result in the income decrease by around 2%, after we add full controls into model. The magnitude of the negative policy effect decreases to around 1% when we only control for individual fixed effect and becomes statistically insignificant, which implies the policy effect on labor income is correlated with the regional factors. In column (5), the coefficient for the binary treatment is statistically insignificant, which could be explained similarly by the reasoning for the hours worked result.

5.2 Dynamic Effects

The underlying assumption for the DD estimator is that cities with different policy duration would have parallel trends in the employment situation before the policy is implemented. The observed increase in unemployment probability could be driven only by the pandemic containment measures, but not the unobserved city characteristics that are associated with the pandemic outbreak. We provide the test for pre-trends that might violate parallel trend assumptions of the DD framework by estimating the effect on unemployment of the interaction terms for the policy duration and the dummy variable for each survey year.

Figure 1 reports the estimated dynamic effect result. We could observe that before the pandemic shock in 2020, cities that are associated with a longer policy duration display no trend in unemployment situation. The estimated coefficients for year 2014 and 2016 are not statistically different from zero and year 2018 is the base year. Only the coefficient for year 2020 is positive and significant, which implies the parallel trend assumption is not violated in our model. In Figure 2, we consider the dynamic effect for the binary treatment variable, which gives us similar results as in the continuous setting. We report the dynamic effect estimation for the hours worked in Figure A5, which provides us a consistent pattern for parallel trends before the pandemic and a negative effect for year 2020, although the significance disappears. As we explained in the previous section, it could be a result of the rigidity in the working schedule for employed workers.

5.3 Disentangled Effect

5.3.1 Disentangled from Health Effect

As we mentioned in section 4.3, our estimation presents the isolated effect of the anti-contagious policy on the labor market outcomes, without the influence of the public health shock. Our reasoning is that the pandemic was put under control very quickly after implementing the stringent disease preventive measures, thus there were few prefectures that experienced a considerable outbreak. By June 30 2020, the total confirmed number in China was 83,534, around 50,000 cases were detected in Wuhan and another 18,000 cases were detected in Hubei province. Given the large population base, the health effect were negligible in most parts of China. As the number of confirmed cases is negligible compared to the prefecture population, the infection probability is close to zero and the consumers should have no behavioral change during the period.

However, the outcomes of interests could still be affected through psychological channel — at the beginning of the pandemic, people had limited knowledge to the virus and might choose to stay at home voluntarily for safety concerns. The first few confirmed, or death cases emerged in the region could still generate a psychological shock to the people and disturb the local market.

To ensure that such psychological shock has no significant impact on the labor market and disentangle the policy effect from the public health shock, we exploit the variation between zero-Covid policy duration and Covid severity measures: confirmed cases and death cases. In equation (3), $lnCases_p$ is the prefecture level total confirmed cases in log form. $lnDeaths_p$ is the prefecture level confirmed death cases in log form. Both variables are counted between Jan 23 to June 30, 2020. ω_1 and ω_2 capture the health effect and leave β as the isolated policy effect. The interpretations for other parameters are similar to previous models.

$$\begin{aligned} Y_{ipt} &= \beta(lnDuration_p \times Post_t) + \omega_1(lnCases_p \times Post_t) + \omega_2(lnDeaths_p \times Post_t) \\ &\quad + \sum_{t\in\{1,2,4\}} (X_p \times Year_t)\lambda_t + \theta_i + \delta_{r,t} + \epsilon_{ipt} \end{aligned} \tag{3}$$

We estimate the DD treatment effect of the number of confirmed cases and dead cases and report the results in Table 4 Panel A. In column (1) (2) and (3), besides the DD treatment for the policy duration and other standard fixed effects, we further include the DD treatment for the public health shock, which is the interaction term between the dummy variable for year 2020 and number of confirmed cases, number of death cases and both in the regression, respectively. The results show that none of these estimated coefficients is positive or statistically significant, while the coefficient for policy duration does not change much. This implies that the potential public health shock does not influence the local employment status as the extremely restricted containment policy eliminates the public health concern efficiently. In other words, the results verify that our estimated policy effect is truly isolated from the public health shock and reflects the sole impact on the labor market from the zero-Covid policy.

5.3.2 Disentangled from Lockdown Effect

As we discussed in the Background Section, although economic activities were entirely allowed in the low risk areas, the policies implemented in the mid and high areas were not clearly defined by the central government. Local governments with incentives to recover the economy might implement flexible anti-contagious policies in the mid risk area to maintain economic activities. On the contrary, local governments with incentives to control pandemics, might implement extremely strict policies to contain the virus in the mid risk areas.

To confirm that the policy effect is not majorly driven by these stringent measures, e.g. prefecture-level lockdowns, implemented by local governments during the early stage of Covid, and disentangle the effect of policy intensity and policy duration, we included indicator variables for whether the prefectures have ever locked down during our sample period. Defined by He et al. (2020), the lockdown variable are categorized as prefecture level and community levels. The former is defined as inter-city travel restriction, and the latter is defined as intra-city mobility restrictions. It is noteworthy that our treatments additionally capture the low intensity containment measures neglected by the lockdown variable. For example, for a prefecture that never issued within or between cities mobility restriction, there's still some chance that the governor issued stay-at-home order to a specific district or area that is potentially exposed with COVID-19 cases. In the following model, parameter π_1 and π_2 absorb the lockdown effect and isolate β as the effect generated from the duration of the general disease preventive policy.

$$\begin{aligned} Y_{ipt} = {} & \beta(Duration_p \times Post_t) + \pi_1(Lockdown\_city_p \times Post_t) + \pi_2(Lockdown\_comm_p \times Post_t) \\ & + \sum_{t \in \{1,2,4\}} (X_p \times Year_t)\lambda_t + \theta_i + \delta_{r,t} + \epsilon_{ipt} \end{aligned} \tag{4}$$

In Table 4 Panel B columns (1)(2) and (5)(6), we estimate the DD treatment effect of the zero-Covid policy on individual unemployment status and hours worked controlling for the lockdown variables. We include the interaction term of the dummy variables for lockdown and the dummy variable for year 2020 in the baseline regression to test whether lockdown is the major driven factor of the labor market disruption. In these regression specifications, the estimators of the policy duration remain statistically significant and the magnitude of the coefficients are similar to the baseline results. In columns (3)(4) and (7)(8), we only estimate the effect of the DD treatment for lockdown variables solely on the labor outcomes and the coefficients are all statistically insignificant. These results imply that whether a city implemented lockdown could not explain the negative pattern observed in the labor market. We could confirm that the zero-Covid policy, disentangled from the city lockdown, madea a causal impact on the labor outcomes.

5.4 Threats to Baseline Findings

5.4.1 Spillover Effects

Our baseline estimation relies on the assumption that the prefectures in our sample were not affected by the anti-contagious policies of neighboring prefectures. Potentially, the labor market not only be affected by local anti-contagious policy, but also be influenced by spillover effects from nearby regions. The inter-region traffic and human mobility could be strictly controlled, and therefore decreases local working opportunities. If the zero-COVID spillover effect disproportionately drove up the unemployment probability between sample prefectures, our estimation could be biased.

For example, if there exist stronger spillover effects in prefectures with relatively longer zero-Covid policy duration, and weaker spillover effect in prefectures with relatively shorter duration, the coefficient of local policy effect will be overestimated. Alternatively, if the shorter zero-Covid prefectures experienced severe spillover from neighbors and longer zero-Covid prefectures experienced negligible spillover effects, the true policy impacts will be underestimated. In this section, we empirically assess the *Stable Unit Treatment Values Assumption* (SUTVA) by controlling the zero-Covid policy in nearby prefecture. If we observe a negative (positive) correlation between local labor outcomes and zero-COVID policy duration of nearby prefectures, it implies the estimates of local policy effect in the baseline model is overstated (understated) in magnitude.

To measure the duration of zero-Covid policy in nearby prefectures, we firstly collect the zero-Covid policy duration for all neighboring prefectures to in sample the prefectures. Then, we define the $Duration\_Nearby_p$ as the average neighbors' policy duration for a given prefecture p.

$$Duration\_Nearby_p = \frac{\sum_q Duration_q * I(q,p)}{\sum_q I(q,p)}$$

where $I(q,p)$ is the indicator function for whether prefecture p and prefecture q are nearby.

Our estimation controlling for spillover effects as following:

$$\begin{aligned} Y_{ipt} &= \beta(lnDuration_p \times Post_t) + \alpha(lnDuration\_Nearby_p \times Post_t) \\ &+ \sum_{t\in\{1,2,4\}} (X_p \times Year_t)\lambda_t + \theta_i + \delta_{r,t} + \epsilon_{ipt} \end{aligned} \tag{5}$$

In Table 5, we estimate the effect of both local policy duration and nearby policy duration on labor market outcomes. In column (1), we report the estimation of policy impact controlling for spillover effect on individual unemployment probability. The estimated local policy effect remains positive and statistically significant, while the spillover effect has an negative coefficient which is not significant. The coefficient for the local policy duration is also close to the estimates of policy effect in our baseline specification as shown in Table 2. These results imply that the spillovers are unlikely to be present as the nearby policy duration did not contribute to the increase, if not a decrease, in the individual unemployment probability.

In column (2), we report the estimation of policy impact controlling for spillover effect on log hours worked for employed workers. While the spillover effect is still not significant, the magnitude of the local policy effect on the decrease of log hours worked increases from 0.0239 to 0.0424, compared to our baseline estimation. The increase in the local policy impact and the positive coefficients for nearby policy duration suggest that our baseline model might underestimate the size of the negative impact of local policy on the hours worked. This result implies that, in the regions where the duration of local zero-Covid policy is relatively low, the decrease in working hours for the employed workers are partially due to the zero-Covid policy in the nearby regions.

5.4.2 Anticipation

Another challenge to our identification strategy is that patterns of labor outcomes could change in anticipation of zero-Covid policy shock. However, as COVID initially outbroke at China [24], the time interval between outbreak and the roll out of unprecedented policies is too narrow for labor

[24] Chinese government did not admit that the coronavirus has human-to-human transmissibility until Jan 20, 2020. Three days later, Wuhan implemented the city lockdown.

market to anticipate, which address the concern of pre-anticipation bias.

5.4.3 Selection Bias

There are two sources of selection bias in the continuous DD treatment setting — classic selection bias and differences in treatment effects across different treatment doses. In Section 5.2 and 5.4.2, we've already resolved uncertainty on common trends (also referring as classic selection bias (Cunningham, 2021)). In this section, we are going to discuss the later question.

To identify causality with our continuous treatment DD setting, we need a stronger parallel trends assumption that "for all doses, the average change in outcomes over time across all units if they had been assigned that amount of does is the same as the average change in outcomes over time for all units that experienced that dose" (Callaway et al., 2021). If this assumption does not hold, the estimates will be biased. For example, two prefectures with *lnDuration* d_j and d_{j-1}, it is difficult to assume that they don't have heterogeneous policy effects at the same treatment level d_{j-1}, which will result in a selection bias. When we calculate the marginal policy effect, the selection bias is represented by the second term on the right hand side of Equation (6), cited from Callaway et al. (2021). To be specific, our estimation include Average Causal Responses and differences in ATT across prefectures with differing *lnDuration* at the a given treatment level.

$$\frac{\partial \mathbb{E}[\Delta Y_t | D = d]}{\partial d} = \underbrace{ACRT(d|d)}_{\text{average causal responses}} + \underbrace{\left.\frac{\partial ATT(d|l)}{\partial l}\right|_{l=d'}}_{\text{selection bias}} \tag{6}$$

Although there is no compelling method to assess the stronger parallel assumption mentioned above, we do not think the selection bias problem will seriously threat to our identification — the constructed semi-exogenous treatment could alleviate the "select into different treatment dose" concern. Given the number of days without 0 increase (absorbed by fixed effect), it is not easy for local governments to manipulate how many days with 0 cases increase in a 14 days window, as the time point of detecting an increase case is quite random. Shown in Figure A3, given a number of days without 0 increase (X-axis), we can observe large variation in *Duration* (Y-axis), which is driven by the random factors instead of prefecture characteristics. Given the exogeneity of our treatment variable, the marginal treatment effect is less likely be biased by the selection problem.

5.4.4 Placebo Test

To further show that our identification design already eliminates the potential non-treatment influences, we employ the placebo test method suggested by Huntington-Klein (2021). We choose year 2016 or 2018 as fake treatment periods, drop the data in 2020 (the period where the treatment actually happened) and re-estimate our baseline models. We report our estimation results in Table A6. Since we cannot find significant policy effects at the fake treatment periods, it suggests that the common trends assumption holds and our baseline estimations are not contaminated by non-treatment influences.

5.5 Robustness Checks

5.5.1 Balanced Panel

As shown in Table A1, some of individuals did not answer the questionnaire for all four waves in our sample, and individuals' dropout condition might be influenced by some unobservable characteristics that are correlated to labor outcomes. Based on this unbalanced panel, our estimation might be biased due to selection on the omitted variables. To ensure that our estimation is not dramatically influenced by the individuals' dropouts, we estimate the baseline regression specification for individuals who stay in the survey for all four waves, i.e., based on a balanced panel data.

In Table A7, we only keep the individuals who stay in every wave from 2014 to 2020 and estimate the effect of zero-Covid policy on individual unemployment, hours worked and income with the remaining balanced panel data. Compared to the baseline result, the balanced panel estimations have a relatively larger magnitude in the coefficients with at least 10% significance level. This implies that the baseline estimation might underestimate the policy effect for labor outcomes, while the argument that there exists a causal impact of the zero-Covid policy on labor outcomes is not systematically challenged.

5.5.2 Cluster Robust

We want to confirm that our baseline statistical inference is not affected by alternative choices of clustering. In Table A8 columns (1) and (4), we re-estimate the baseline regression specification and implement the two-way clustering by prefecture and by year, allowing errors to be correlated across individuals within same prefecture and same year. In columns (2) and (5), we calculate the standard errors clustered at province level; in columns (3) and (6), we clustered the standard errors by province and by year. Although the standard errors become larger compared to our baseline specification, the statistical inferences on the policy effect are robust to different clustering methods.

5.5.3 Outlier Regions

We next investigate whether our baseline findings are influenced by outliers regions that experienced severe lockdown or extremely long zero-Covid duration. Wuhan and Hubei province went through the initial COVID-19 outbreak and implemented stringent lockdown policies for the first two months of the pandemic. Big cities, including Beijing, Shanghai, Guangzhou, Chongqing and Tianjin, frequently detected new COVID positive cases, resulting in very high zero-Covid policy duration. The treatment effect for individuals who live in these regions might be different from people living elsewhere, affecting the average treatment effect for the whole population. In Table A9, we report our estimation results excluding individuals who live in outlier regions. In columns (1) and (2), we drop individuals in Wuhan; in columns (3) and (4), we drop individuals in Hubei province, and in columns (5) and (6), we further drop individuals in big cities. The estimated policy effect on labor outcomes remain consistent and robust to the exclusions of these outlier regions.

5.5.4 Lag Effects

The 2020 CFPS survey took several months to collect the questionnaires across different regions in China. While the majority of the survey was collected during July and August 2020, a small share of the survey was collected later through the period from July to December 2020. The time variation in the data collection could potentially help us investigate whether the persistent policy impact on the local labor outcomes is varying in its lagging time.

We make the estimation for the subsample from each survey group whose questionnaires were collected in each month from July to December. In Figure A6, we report the coefficient and the standard error of the policy effect on unemployment estimated from the subsamples collected in each month from July to December. We could observe that the policy effect becomes insignificant as time goes, without clear trend of increasing or decreasing. Although this result is partially due to the sample size is smaller in the later month groups, it also implies that the impact of the zero-Covid policy on the unemployment has no significant lag effect which is not captured by our major estimation. The survey data we use for our estimation result are still valid in analyzing the policy effect on labor outcomes.

5.6 Heterogeneous Effects

5.6.1 Separate Phase: Stringent containment and Precise containment

As discussed in Section 2, the policy intensity during January and February is much stronger than the policy intensity after February. *Stringent clearance*, such as lockdown and stay at home order, are more likely to be rolled out between January and late February for COVID containment purpose. After February 17, *dynamic clearance*, such as restaurants closing and travel restriction between risk areas, are dominated zero-COVID policies. Although we cannot measure such intensity with available data, we use different phases, Jan to Feb 17 and Feb 18 to June, as proxies of *stringent clearance* and *dynamic clearance*.

In Equation (5), we use Feb 17 as cutoff for these two phases: Jan 23 to Feb 17, represented by $lnDuration\_JanFeb_p$ and Feb 17 to June 30, represented by $lnDuration\_FebJun_p$ [25]. Between Jan 23 to Feb 17, each prefecture were under the province's First level emergency response, with a smaller standard deviation in the treatment (shown in Figure A1). This estimation separates the policy effect for different phases of the pandemic, which policy implication will be discussed with more details later when we interpret the estimation results.

[25] Again, Feb 17 is the time point of central government guidance for *precise containment*

$$
\begin{aligned}
Y_{ipt} = {} & \beta(lnDuration\_JanFeb_p \times Post_t) + \eta(lnDuration\_FebJun_p \times Post_t) \\
& + \sum_{t\in\{1,2,4\}} (X_p \times Year_t)\lambda_t + \theta_i + \delta_{r,t} + \epsilon_{ipt}
\end{aligned}
\tag{7}
$$

As mentioned in the section 2 and section 4.4, the zero-Covid policy in China experienced a shift around late Feb 2020. The central government issued a guidance to require the local governments to identify the areas exposed to the virus more precisely and limit the influence of the anti-contagious measures only in risky regions. While our estimation results indicate that the local policy duration cause a significant impact on labor outcomes, we are unsure that whether the intensity of the policy treatment is evenly distributed over the whole period from Jan 2020 to June 2020. Potentially, after the issue of the guidance in Feb, the intensity and extent of the zero-Covid policy are much restricted and the policy treatment effect is weakened compared to the early phase of the Covid pandemic.

To examine the policy effect during different time periods, we estimate the coefficients of the DD treatment for policy duration before Feb 17, the policy duration after Feb 17, and both of them, respectively. The results are reported in Table 6. Column (1) shows that the policy duration before Feb 17 is significantly related to the labor outcome change, while column (2) show that the policy duration after Feb 17 is not. The resuColumn (4) displays a similar result that the correlation between the labor outcomes and policy duration after Feb 17 is not statistically significant, which implies that the magnitude of the policy effect after Feb 17 is less obvious compared to the early phase. The precise containment indeed limited the negative impact of the zero-Covid policy to an acceptable level.

5.6.2 Across-group

We estimate the heterogeneous impacts of policy duration on different sub-populations and the estimation results are shown in Table 7 and Table 8. We estimate the policy effect for different groups categorized by gender, age, education, income distribution rank and having a young child. The parameter of interest is the coefficient of the interaction term between the *lnDuration* $\times$ *Post* and sub-population indicators. We find that for groups such as female workers, workers above age 65, workers with education level less than middle school, the bottom 50 percent population in the income distribution, and parents whose children are less than 6 years old, they are more vulnerable to the zero-Covid policy impact on the unemployment status, while whether they are employed by a private sector firm has no impact. Regarding the policy effect on employed workers' hours worked, none of these individual characteristics has an impact, potentially due to the fact that the rigidity in the working schedule limits the difference across different groups.

There could be also a potential labor outcome difference for workers in the agricultural sector versus non-agricultural workers. We re-estimate our baseline models for each group of works and report our results in Table A10. We could find the non-agricultural workers experienced a stronger policy effect on their employment status than the agricultural workers, while the impacts on their hours worked are similar.

5.6.3 Across-prefecture

We also examine the heterogeneity of the policy effect at the prefecture level, specifically considering the trade and traffic volume by channel as an indicator for each city's regional economic integration level. While a city's labor market could be deeply disrupted by the pandemic containment policy during the outbreak, with higher trade volume, i.e., more closely connected to the nearby regions, it has better capacity to recover from the temporary fall down in local economy.

In Table A11, we report the estimation result of the policy effect on unemployment in subsamples grouped by the traffic volume and freight volume through highway and waterway, where the high volume and low volume groups are separated by the median value. In column (5) – (8), we find that for cities in the high freight volume group, for both highway and waterway channels, the policy effect on the unemployment is not statistically significant while the negative impact remains significant in the low freight volume group.

However, this heterogeneity does not exist for the dimension of traffic volume, as shown in column (1) – (4). It is worth noting that the coefficients are not statistically significant for both high and low traffic volume groups. The potential reason for this significance reversal is that the traffic volume is highly correlated to the policy duration as a city in the central position of the transportation network has larger chance to find a positive Covid case than a city less connected to the rest of the region (as shown in Figure A7). When the positive correlation between traffic volume and policy duration is diluted in each of two separated groups, the policy effect is also diminished within each group.

Finally, the trade and traffic estimation results imply that while the zero-Covid policy restricts the mobility of human beings, freight transportation might not be highly constrained during the pandemic, thus cities with higher trade volume are able to resist the negative policy effect on the unemployment.

6 Conclusions

During the COVID-19 pandemic, countries across the globe adopted drastically differing strategies for mitigating the unprecedented public health crisis. While China was the first country to implement harsh anti-contagious interventions nationally, its effect on the economy remained obscure until very recently. Based on a DID design, we find that when a city's Zero-Covid policy lasts for 10%(3.7 days) longer, the individual unemployment probability increases by around 0.1, and employed workers lose 0.2% and 2% of their income, respectively. Our estimation disentangles zero-Covid policy and the public health shock, where the latter has no significant impact on labor market outcomes. Lockdown policy is widely discussed as the major non-pharmacological intervention in recent literature. However, our paper examines the effect of the general zero-Covid policy, which includes not only lockdowns, but also possible anti-contagious interventions that have been difficult to observe due to data limitations. We also control for spillover effects from nearby cities, which do not contribute to the negative labor market impact. Additionally, our research suggests that only the stringent anti-contagious policy implemented during the early stage of the pandemic negatively impacted labor outcomes, while there was little evidence that the more precise containment policy implemented in the later phase contributed to the labor market disruptions.

COVID-19 has caused millions of deaths and a global humanitarian crisis as many countries were unable to control the spread of the virus after the outbreak of the pandemic. Partially contributing to this catastrophic outcome is the fact that policymakers fearing the potential economic impact of restricting human mobility resisted taking serious disease preventive measures immediately after the outbreak of the virus, and ultimately resorted to herd immunity. We provide a systematic evaluation of the labor market disruption caused by the most stringent containment policy and estimate the economic cost of non-pharmacological interventions to stop the pandemic. It is noteworthy that the data used in this paper were collected during the period when the zero-Covid policy was very effective and the pandemic was controlled extremely well in China. It is reasonable to doubt that our estimation results are not valid under the circumstances where the spread of viruses is more difficult to put under control and the zero-Covid policy has to last longer.[26] The economic cost of the anti-contagious policy would not grow linearly as the duration of the policy increases, but exponentially. However, our work can still serve as a benchmark under such a scenario: the pandemic's scope was constrained soon after its outbreak, and millions of lives were saved. How much would it cost economically? After all, we hope our work will be a useful reference for future policymakers dealing with similar situations, where they will have to choose between health and economic well-being.

[26]This is indeed what happened to many Chinese cities after the emergence of Omicron in China. More stringent measures and city lockdowns are implemented from March 2022 till Mid May, as we are finishing this version of the paper.

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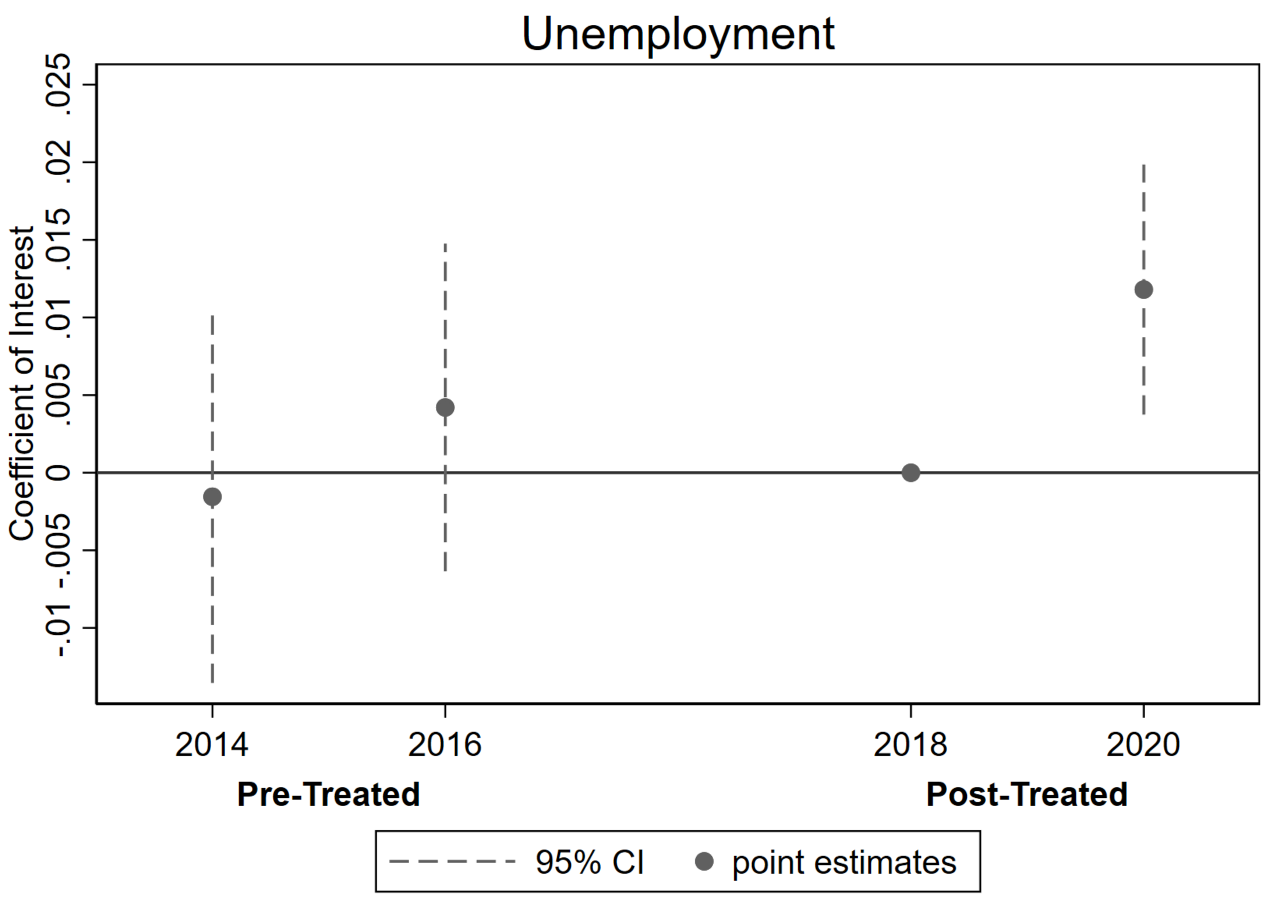
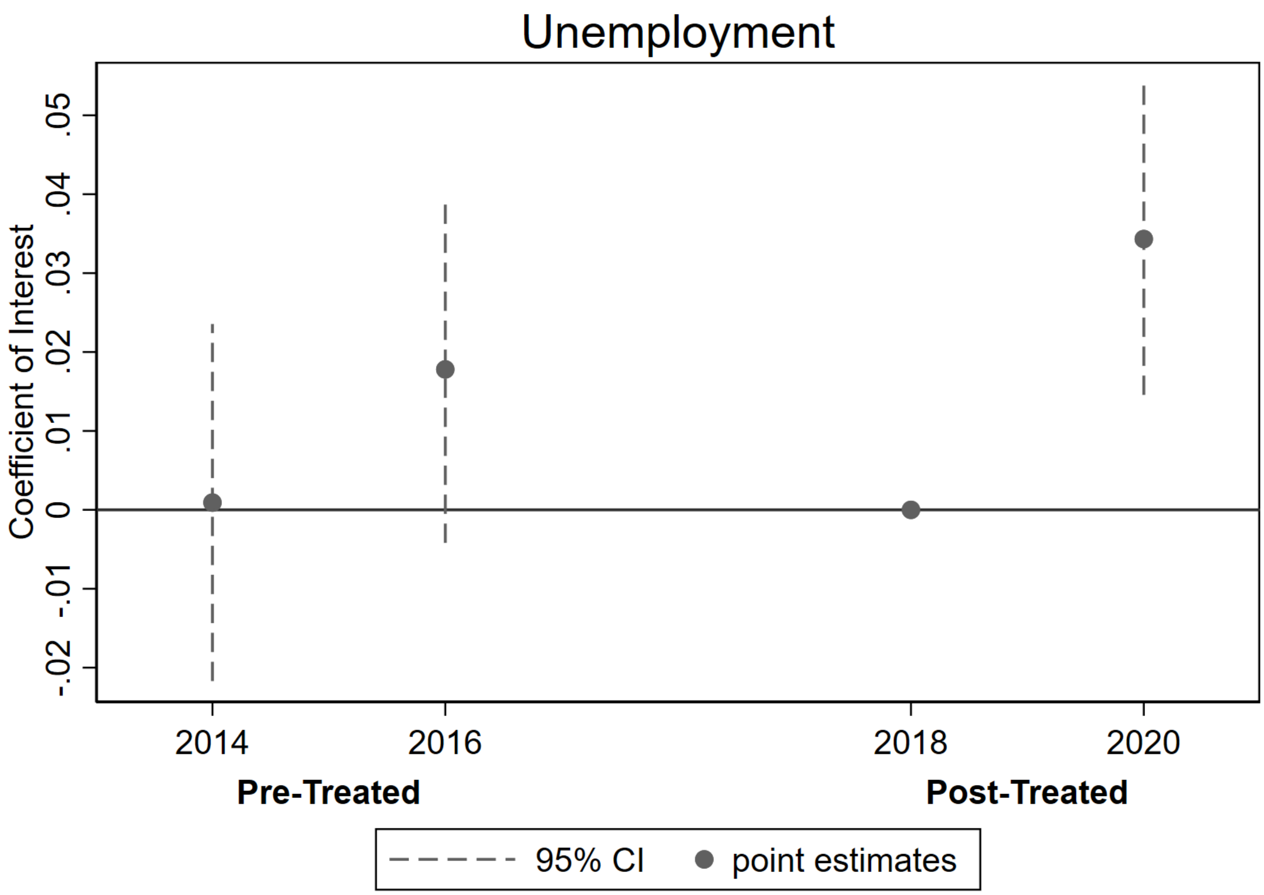
7 Figures and Tables

7.1 Figures



Notes: The figure shows coefficients and 95% confidence intervals from estimating the leads and lags regression in equation (2), where the dependent variable is unemployment dummy. All effects are relative to 2018.

Figure 1: Dynamic Effects Unemployment (continuous treatment)



Notes: The figure shows coefficients and 95% confidence intervals from estimating the leads and lags regression in equation (2), where the dependent variable is unemployment dummy. All effects are relative to 2018.

Figure 2: Dynamic Effects Unemployment (dummy treatment)

7.2 Tables

Table 1: Statistic Summary

	Obs	Mean	Std.Dev	Min	Max
Panel A: Individual D.V.					
Unemployed	63343	0.173	0.378	0.0	1
Hours Worked	37914	46.538	21.880	0.1	133
Hours Worked (Overall)	48601	36.003	27.247	0.0	133
Income	28445	20992.159	25262.527	0.0	100000
COVID Unemployed	1866	0.026	0.160	0.0	1
Panel B: Individual Characteristics					
Gender	63343	0.517	0.500	0.0	1
Age	63343	45.808	11.870	11.0	69
Education (middle school or below)	63343	0.732	0.443	0.0	1
Agricultural Worker	60215	0.432	0.495	0.0	1
Private Sector Worker	19669	0.841	0.366	0.0	1
Youngest Child Age	57690	19.120	11.394	0.0	47
Panel C: Prefecture Treatments					
ZC Duration	126	37.128	21.411	0.0	158
ZC Duration Feb Jun	126	18.349	19.158	0.0	135
ZC Duration Jan Feb	126	18.779	5.095	0.0	24
Covid Case Duration	126	13.921	13.054	0.0	102
Confirmed Cases	126	451.691	4481.225	0.0	50340
Confirmed Deaths	126	31.432	344.629	0.0	3869
Lockdown (City Level)	126	0.349	0.479	0.0	1
Lockdown (Community Level)	126	0.183	0.388	0.0	1
Panel D: Prefecture Controls					
Population 2018 (Thousand)	126	5586.448	4662.472	430.0	34040
GDP 2018 (Billion)	126	396.489	557.217	0.0	3268
Share of Service Sector in GDP	126	48.090	8.518	31.1	81

Notes: Panel A reports individual outcomes variables of interest. Panel B reports descriptive individual characteristics. Panel C report prefecture-level treatment variables. Panel D reports prefecture-level characteristics in 2018.

Table 2: Baseline Results: Unemployment and Hours Worked

	(1) Outcomes	(2) Outcomes	(3) Outcomes	(4) Outcomes	(5) Outcomes
Panel A: Unemployment					
lnDuration × Post	0.00831**	0.0109**	0.0125***	0.0109**	
	(0.00385)	(0.00428)	(0.00443)	(0.00456)	
lnDuration_Dummy × Post					0.0284***
					(0.00994)
R-squared	0.469	0.470	0.470	0.470	0.470
Observations	63343	63343	63343	63343	63343
Mean of Unemployment	0.173	0.173	0.173	0.173	0.173
Panel B: log Hours Worked					
lnDuration × Post	-0.0165**	-0.0183***	-0.0215***	-0.0239***	
	(0.00789)	(0.00642)	(0.00743)	(0.00764)	
lnDuration_Dummy × Post					-0.00793
					(0.0271)
R-squared	0.312	0.315	0.315	0.315	0.315
Observations	37914	37914	37914	37914	37914
Mean of Hours Worked	46.54	46.54	46.54	46.54	46.54
Individual FE	✓	✓	✓	✓	✓
Year FE	✓	✗	✗	✗	✗
Province-Year FE	✗	✓	✓	✓	✓
lnPop × Year FE	✗	✗	✓	✓	✓
lnGDP × Year FE	✗	✗	✗	✓	✓
ServiceShare × Year FE	✗	✗	✗	✓	✓

Notes: Unit of observation is an individual. The dependent variable is an indicator for unemployment (Panel A) or the natural log of hours worked(Panel B). lnDuration is the natural log of the duration of the zero-Covid policy. lnDuration_Dummy is a dummy that is 1 if the duration of the zero-Covid policy is above median. Robust standard errors in parentheses, clustered at the prefecture level. ***, **, and * indicate significance at the 1%, 5% and 10% levels.

Table 3: Disentangled Effect: zero-Covid Policy and Public Health Shock

	(1) Unemployment	(2) Unemployment	(3) Unemployment	(4) log Hours Worked	(5) log Hours Worked	(6) log Hours Worked
lnDuration × Post	0.0149**	0.0125**	0.0141*	-0.0357**	-0.0222**	-0.0399**
	(0.00726)	(0.00486)	(0.00754)	(0.0160)	(0.00892)	(0.0181)
lnCases × Post	-0.00363		-0.00182	0.0112		0.0204
	(0.00552)		(0.00736)	(0.0151)		(0.0230)
lnDeaths × Post		-0.00479	-0.00364		-0.00552	-0.0184
		(0.00614)	(0.00837)		(0.0156)	(0.0245)
R-squared	0.470	0.470	0.470	0.315	0.315	0.315
Observations	63343	63343	63343	37914	37914	37914
Mean of Outcome	0.173	0.173	0.173	46.54	46.54	46.54
Individual FE	✓	✓	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓	✓	✓
lnPop × Year FE	✓	✓	✓	✓	✓	✓
lnGDP × Year FE	✓	✓	✓	✓	✓	✓
ServiceShare × Year FE	✓	✓	✓	✓	✓	✓

Notes: Unit of observation is an individual. The dependent variable is an indicator for unemployment (columns 1-3) or the natural log of hours worked(columns 4-6). lnDuration is the natural log of the duration of the zero-Covid policy. lnCases is the natural log of the number of confirmed cases. lnDeaths is the natural log of the number of death. Robust standard errors in parentheses, clustered at the prefecture level. ***, **, and * indicate significance at the 1%, 5% and 10% levels.

Table 4: Disentangled Effect: zero-Covid Policy and Lockdown

	(1) Unemployment	(2) Unemployment	(3) Unemployment	(4) Unemployment	(5) log Hours Worked	(6) log Hours Worked	(7) log Hours Worked	(8) log Hours Worked
Panel A: lnDuration								
lnDuration × Post	0.0110**	0.0105**			-0.0234***	-0.0256***		
	(0.00454)	(0.00475)			(0.00774)	(0.00833)		
Lockdown_city × Post	-0.00264		-0.00157		-0.0165		-0.0188	
	(0.0135)		(0.0135)		(0.0317)		(0.0316)	
Lockdown_comm × Post		0.00885		0.0117		0.0364		0.0296
		(0.0192)		(0.0193)		(0.0367)		(0.0378)
R-squared	0.470	0.470	0.470	0.470	0.315	0.315	0.315	0.315
Observations	63343	63343	63343	63343	37914	37914	37914	37914
Panel B: lnDuration_Dummy								
lnDuration_Dummy × Post	0.0285***	0.0279***			-0.00955	-0.00955		
	(0.00968)	(0.0103)			(0.0271)	(0.0263)		
Lockdown_city × Post	0.000548		-0.00157		-0.0197		-0.0188	
	(0.0130)		(0.0135)		(0.0320)		(0.0316)	
Lockdown_comm × Post		0.00884		0.0117		0.0305		0.0296
		(0.0178)		(0.0193)		(0.0375)		(0.0378)
R-squared	0.470	0.470	0.470	0.470	0.315	0.315	0.315	0.315
Observations	63343	63343	63343	63343	37914	37914	37914	37914
Mean of Outcome	0.173	0.173	0.173	0.173	46.54	46.54	46.54	46.54
Individual FE	✓	✓	✓	✓	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓	✓	✓	✓	✓
lnPop × Year FE	✓	✓	✓	✓	✓	✓	✓	✓
lnGDP × Year FE	✓	✓	✓	✓	✓	✓	✓	✓
ServiceShare × Year FE	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Unit of observation is an individual. The dependent variable is an indicator for unemployment (columns 1-4) or the natural log of hours worked(columns 5-8). lnDuration is the natural log of the duration of the zero-Covid policy. lnDuration_Dummy is a dummy that is 1 if the duration of the zero-Covid policy is above median. Lockdown_city is an indicator equal to 1 if the prefectures have ever locked down. Lockdown_comm is an indicator equal to 1 if the community in the prefecture have ever locked down. Robust standard errors in parentheses, clustered at the prefecture level. ***, **, and * indicate significance at the 1%, 5% and 10% levels.

Table 5: Spillover Effect

	(1) Unemployment	(2) log Hours Worked
lnDuration × Post	0.0116***	-0.0253***
	(0.00437)	(0.00815)
lnDuration_nearby × Post	-0.0168	0.0327
	(0.0202)	(0.0332)
R-squared	0.470	0.315
Observations	63343	37914
Mean of Outcome	0.173	46.54
Individual FE	✓	✓
Province-Year FE	✓	✓
lnPop × Year FE	✓	✓
lnGDP × Year FE	✓	✓
ServiceShare × Year FE	✓	✓

Notes: Unit of observation is an individual. The dependent variable is an indicator for unemployment (columns 1) or the natural log of hours worked(columns 2). lnDuration is the natural log of the duration of the zero-Covid policy. lnDuration_nearby is the natural log of the average neighbors' policy duration for a given prefecture. Robust standard errors in parentheses, clustered at the prefecture level. ***, **, and * indicate significance at the 1%, 5% and 10% levels.

Table 6: Heterogeneity: Separate Phase

	(1) Unemployment	(2) Unemployment	(3) Unemployment	(4) log Hours Worked	(5) log Hours Worked	(6) log Hours Worked
lnDuration_JanFeb × Post	0.0146**		0.0130*	-0.0304***		-0.0267*
	(0.00579)		(0.00699)	(0.00823)		(0.0143)
lnDuration_FebJun × Post		0.00570	0.00203		-0.0125	-0.00473
		(0.00419)	(0.00471)		(0.0106)	(0.0131)
R-squared	0.470	0.470	0.470	0.315	0.315	0.315
Observations	63343	63343	63343	37914	37914	37914
Mean of Outcome	0.173	0.173	0.173	46.54	46.54	46.54
Individual FE				✓	✓	✓
Province-Year FE				✓	✓	✓
lnPop × Year FE				✓	✓	✓
lnGDP × Year FE				✓	✓	✓
ServiceShare × Year FE				✓	✓	✓

Notes: Unit of observation is an individual. The dependent variable is an indicator for unemployment (columns 1-3) or the natural log of hours worked(columns 4-6). lnDuration_JanFeb is the natural log of the duration of the zero-Covid policy between Jan 23 and Feb 17. lnDuration_FebJun is the natural log of the duration of the zero-Covid policy after Feb 17 until the starting date of survey collection (Jun 30). Robust standard errors in parentheses, clustered at the prefecture level. ***, **, and * indicate significance at the 1%, 5% and 10% levels.

Table 7: Heterogeneity: Unemployment by individual characteristics

	(1) Gender	(2) Elder	(3) Middle School	(4) Private Sector	(5) Bottome Income Distribution	(6) Young Child
lnDuration × Post	0.00528	0.00489	0.000878	0.0147	-0.00428	0.0135**
	(0.00499)	(0.00497)	(0.00703)	(0.0180)	(0.00819)	(0.00614)
lnDuration × Post × Female	0.0106*					
	(0.00545)					
lnDuration × Post × Old		0.0122*				
		(0.00648)				
lnDuration × Post × edu_middle			0.0132*			
			(0.00789)			
lnDuration × Post × Private				0.00163		
				(0.0207)		
lnDuration × Post × income_bottom					0.0382***	
					(0.0114)	
lnDuration × Post × child_6						-0.0166*
						(0.00979)
lnDuration × Post × child_18						-0.0000539
						(0.00796)
R-squared	0.470	0.471	0.470	0.309	0.543	0.471
Observations	63343	63343	63343	19669	27651	63343
Mean of Unemployment	0.173	0.173	0.173	0.154	0.260	0.173
Individual FE	✓	✓	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓	✓	✓
lnPop × Year FE	✓	✓	✓	✓	✓	✓
lnGDP × Year FE	✓	✓	✓	✓	✓	✓
ServiceShare × Year FE	✓	✓	✓	✓	✓	✓

Notes: Unit of observation is an individual. The dependent variable is an indicator for unemployment. lnDuratio is the natural log of the duration of the zero-Covid policy. Female, Old, Edu_middle, Private, Income_bottom, Chind_6, and Child_18 is a dummy that is 1 if the interviewee is female, above age 65, workers with education level less than middle school, work in private firm, the bottom 50 percent population in the income distribution, have any child that is less than 6 years old, and have any child that is more than 18 years old, respectively. Robust standard errors in parentheses, clustered at the prefecture level. ***, **, and * indicate significance at the 1%, 5% and 10% levels.

Table 8: Heterogeneity: Hours Worked by individual characteristics

	(1) Gender	(2) Elder	(3) Middle School	(4) Private Sector	(5) Bottome Income Distribution	(6) Young Child
lnDuration × Post	-0.0219**	-0.0230**	-0.0385***	-0.0313	-0.0176	-0.0275**
	(0.00998)	(0.0107)	(0.0134)	(0.0294)	(0.0127)	(0.0115)
lnDuration × Post × Female	-0.00378					
	(0.0182)					
lnDuration × Post × Old		-0.00274				
		(0.0142)				
lnDuration × Post × edu_middle			0.0186			
			(0.0145)			
lnDuration × Post × Private				0.0164		
				(0.0282)		
lnDuration × Post × income_bottom					0.0231	
					(0.0288)	
lnDuration × Post × child_6						-0.000175
						(0.0208)
lnDuration × Post × child_18						0.0144
						(0.0242)
R-squared	0.315	0.316	0.315	0.213	0.231	0.316
Observations	37914	37914	37914	13536	16552	37914
Mean of Hours Worked	46.54	46.54	46.54	52.10	50.18	46.54
Individual FE	✓	✓	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓	✓	✓
lnPop × Year FE	✓	✓	✓	✓	✓	✓
lnGDP × Year FE	✓	✓	✓	✓	✓	✓
ServiceShare × Year FE	✓	✓	✓	✓	✓	✓

Notes: Unit of observation is an individual. The dependent variable is the natural log of hours worked. lnDuration is the natural log of the duration of the zero-Covid policy. Female, Old, Edu_middle, Private, Income_bottom, Chind_6, and Child_18 is a dummy that is 1 if the interviewee is female, above age 65, workers with education level less than middle school, work in private firm, the bottom 50 percent population in the income distribution, have any child that is less than 6 years old, and have any child that is more than 18 years old, respectively. Robust standard errors in parentheses, clustered at the prefecture level. ***, **, and * indicate significance at the 1%, 5% and 10% levels.

8 Appendix

8.1 Anecdotal Evidence: Stringent Containment between Jan and Feb

Confronting a unprecedented public emergency case, Chinese local governments rolled out the most stringent containment policies during January to February, 2020. Although there is little detailed written instruction on how to conduct such containment policies, there were numerous news, coverage and videos on social media revealed local governments reaction by that time [27].

One suggestive example happened in Henan province. Although the daily increased cases is less than 50 and the rural regions were considered as the least affected areas, many villages blocked the entrance and do not allowed any form of visitors. In some cases, during Spring Festival, migrant workers who returned from work places were not allowed to enter the village. In one video on social media [28], village's Communist Party Secretary was using broadcast condemning a villager of hanging out, "are you even a human being? You are so fucked up", one of the public insults from the Secretary. Similar prefecture level or village level lockdown and traffic restrictions also launched in other parts of China (e.g. Heilongjiang, Zhejiang, Jiangxi, etc[29].), among the consequences, a truck driver's experience became a most ridiculous and black humorous story.

Mr.Xiao, a truck driver from Hubei, set off for Sichuan province since Januray 7. However, when he prepared the return trip on Januray 24, Hubei locked down. Mr Xiao had to drove away with no destination. The service areas refused him from stopping, the option of getting off the highway also became impossible, since all the cities rolled out travel restriction on people from Hubei. "People see my license plate, that I come from Hubei, and get scared". After seven days driving, he was found fall asleep in his truck on the emergency lane in Shaanxi province, thousands miles away from his home, "my greatest hope is that I can find a place to stop, get some good sleep and eat something.". Fortunately, police officers got him a hotel room in a service area, Mr Xiao returned back home on Mar 16, 68 days after his adventure [30].

[27] e.g. https://www.bilibili.com/video/BV1a7411k7NB?from=searchseid=5191564554814052769spm$_i d_f rom =$ 333.337.0.0; https://www.bilibili.com/video/BV1H7411g75d?from=searchseid=5191564554814052769spm$_i d_f rom =$ 333.337.0.0; https://www.bilibili.com/video/BV1n7411W7uH?from=searchseid=5191564554814052769spm$_i d_f rom =$ 333.337.0.0; https://www.tuliu.com/read-121860.html;

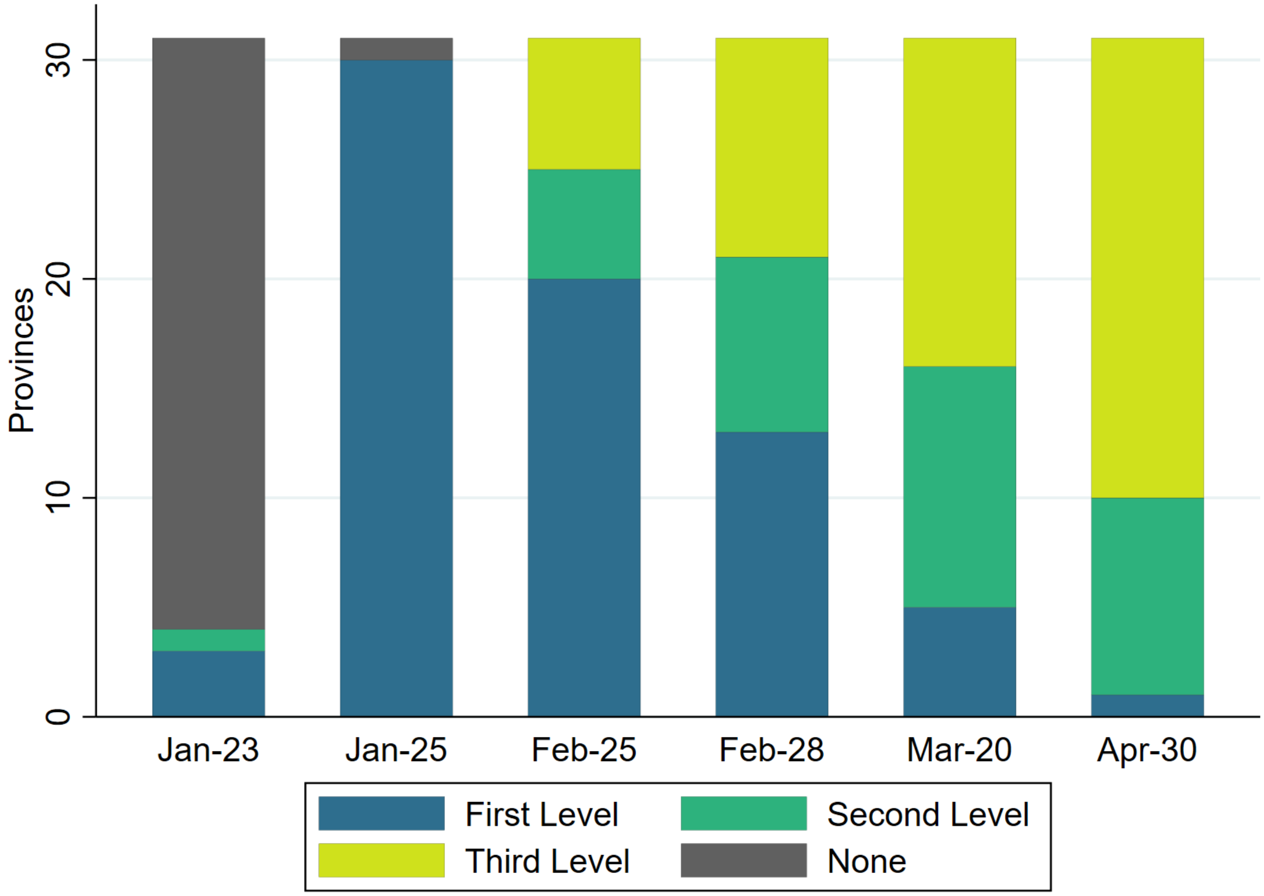
[28] Source: https://www.bilibili.com/video/BV1Y741167Yp/?spm$_i d_f rom = autoNext$.

[29] Source: http://www.moa.gov.cn/xw/qg/202002/t20200224$_6$337603$.htm$.

[30] Source: https://news.cgtn.com/news/2020-02-10/The-road-back-to-Hubei-Truck-driver-says-long-journey-

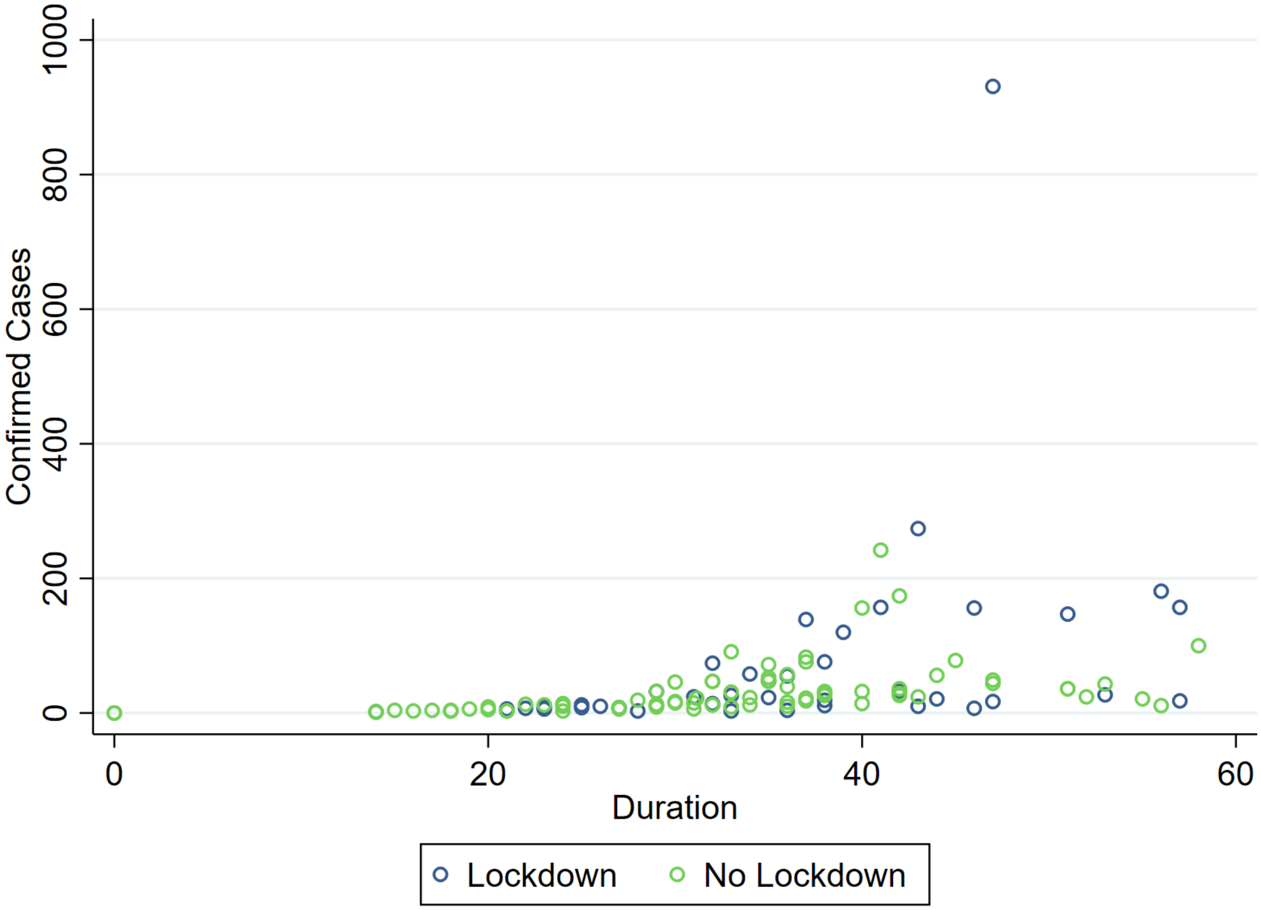
-still-not-over-NY4ba2qOaY/index.html; https://www.wsj.com/video/truck-driver-stuck-on-highway-since-chinas-coronavirus-lockdown/F7097DE9-FCEB-4E13-BCAB-9D715AF84D0B.html; https://www.sohu.com/a/371766675_617717.

8.2 Appendix Figures



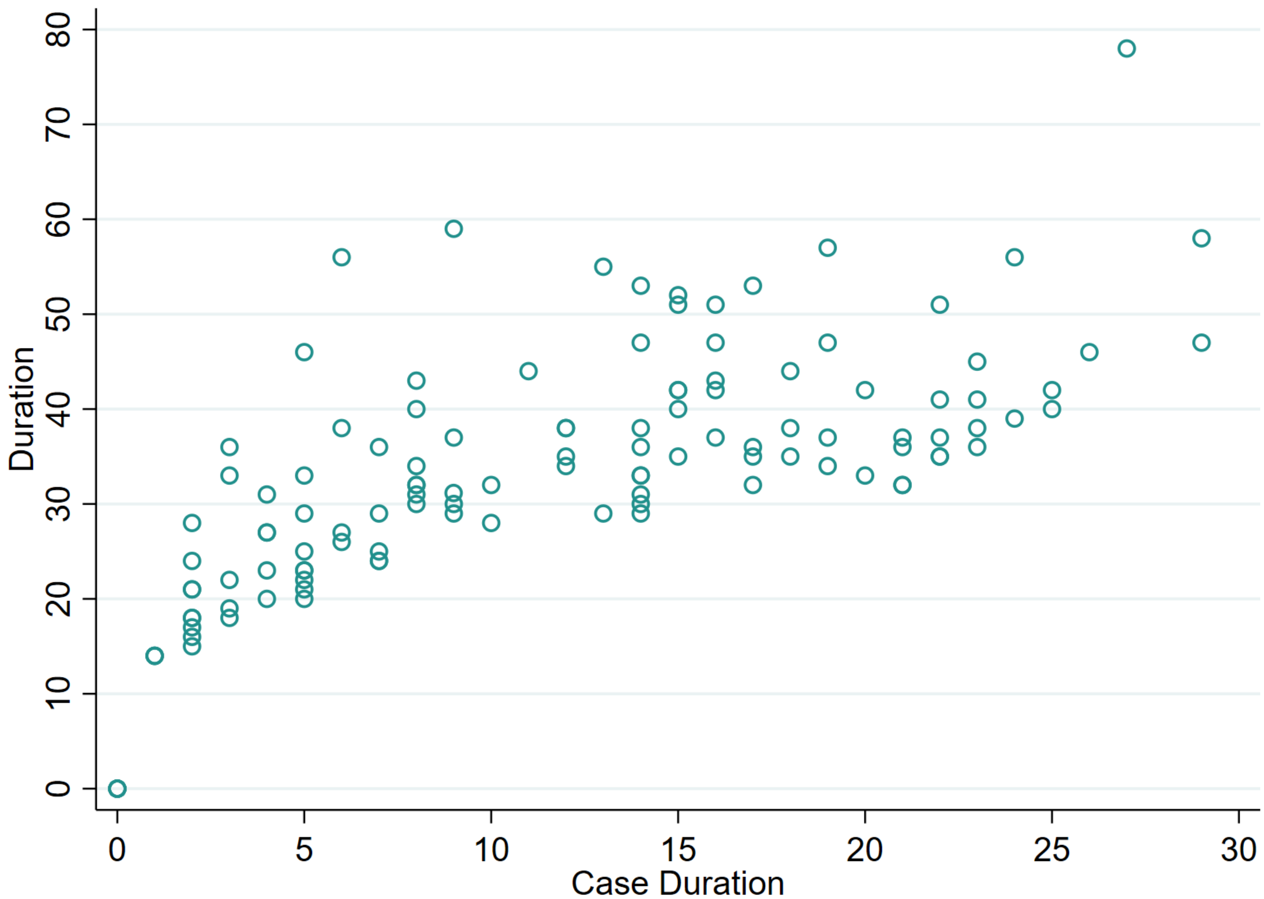
Notes: The figure shows the number of province with different emergency response level.

Figure A1: Province Emergency Reaction Level Time Line



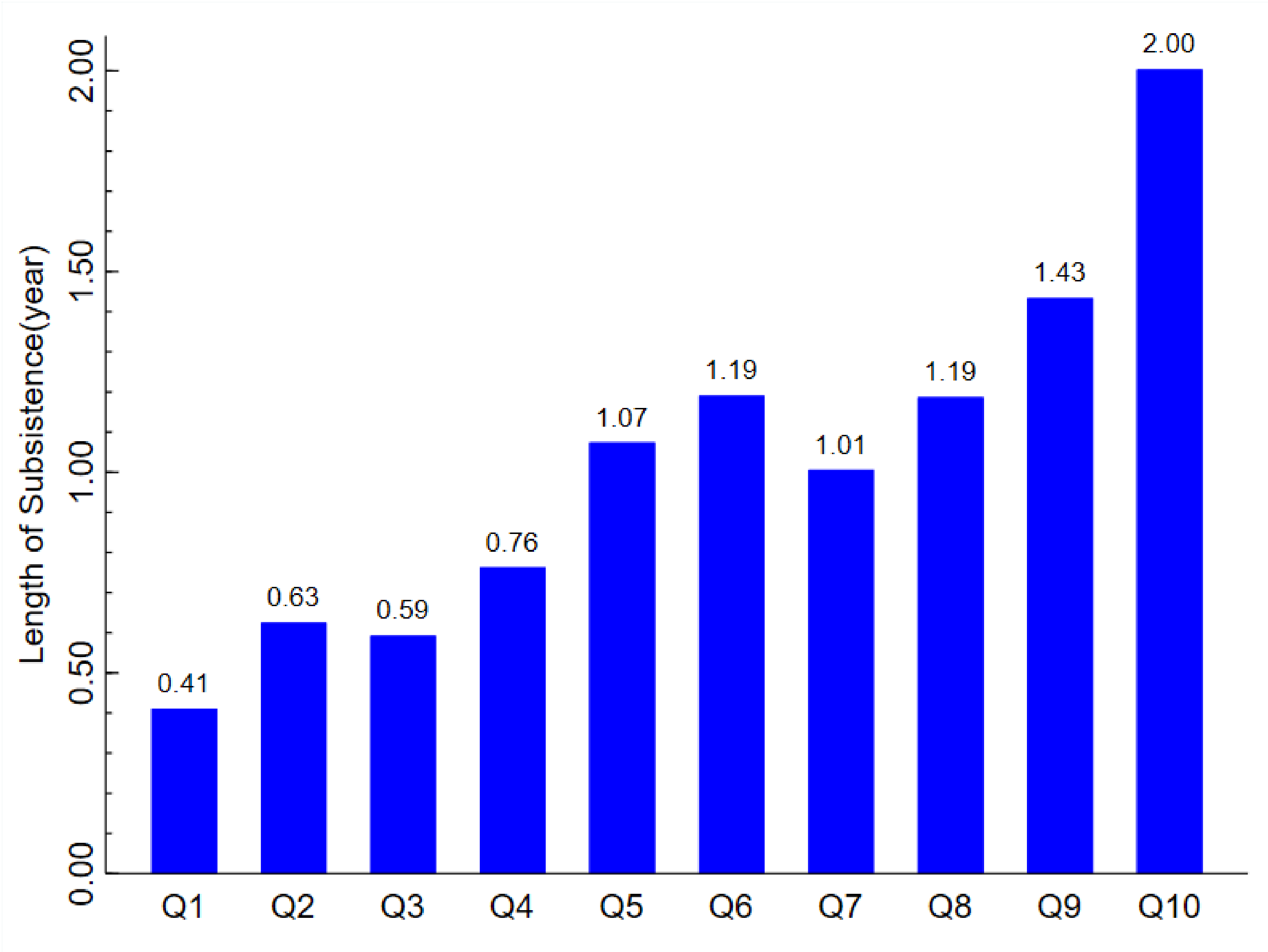
Notes: The figure shows the number of confirmed cases and the duration for prefectures with lockdown (blue circles) or without lockdown (green circles). Duration outliers (95 percentile) are dropped from graph.

Figure A2: Duration and Confirmed Cases by Lockdown



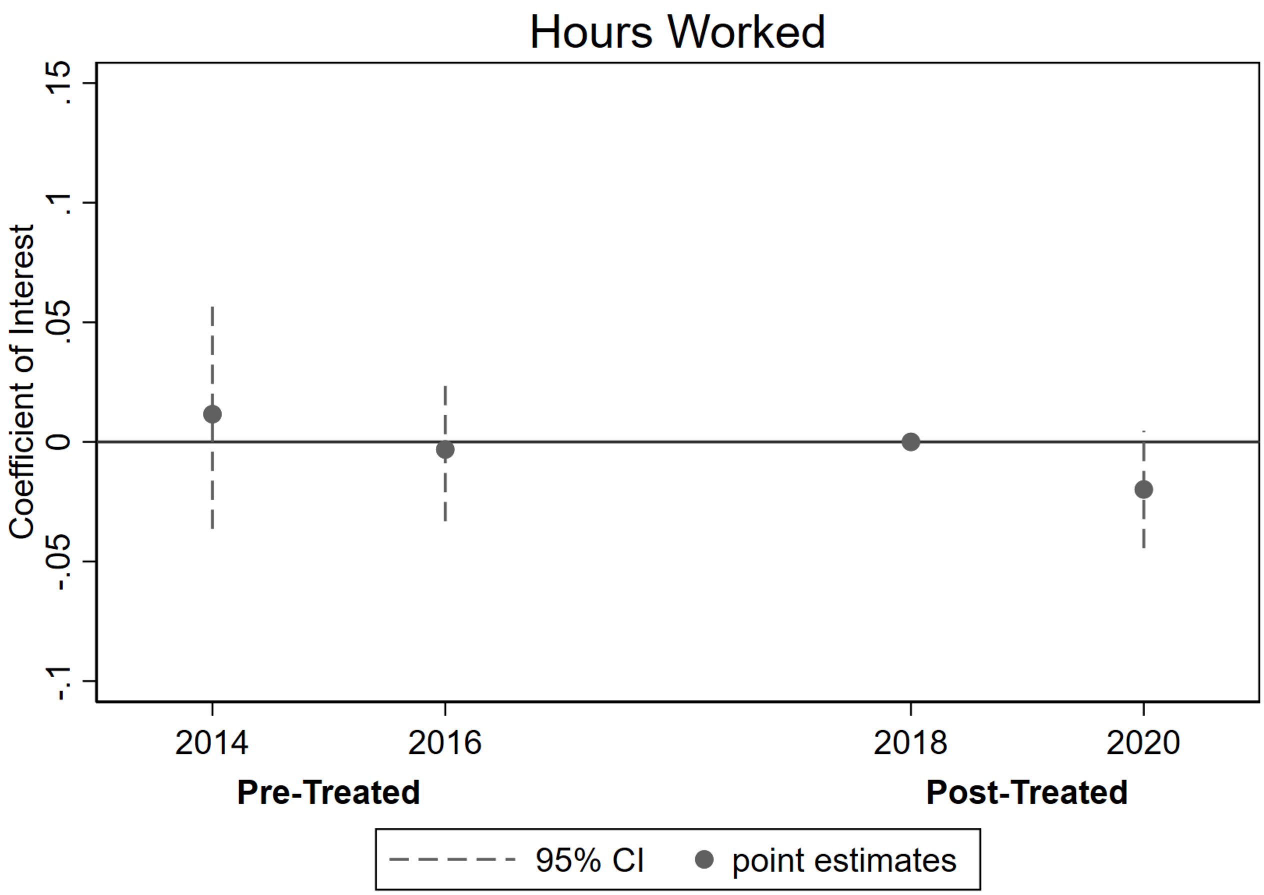
Notes: The figure shows the number of no confirmed cases (case duration) and the duration for prefectures. Case Duration outliers (95 percentile) are dropped from graph.

Figure A3: Pseudo-Random Treatment



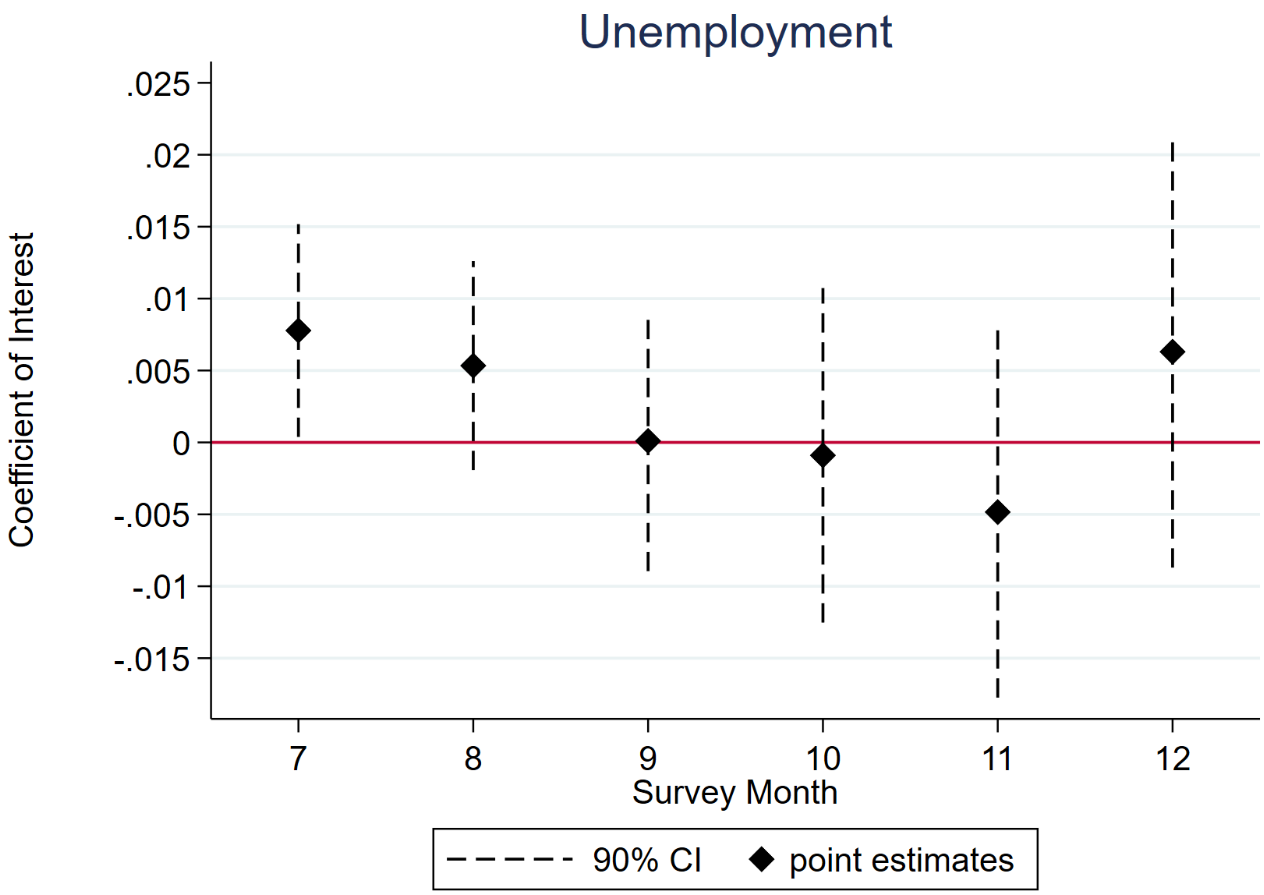
Notes: The figure shows how many years interviewees' cash or deposit could subsist their expenditure if they become unemployed. Y-axis represents length of subsistence = cash or deposit/ family's expenditure. X-axis represents deciles at income distribution.

Figure A4: Subsistence Years After Unemployed



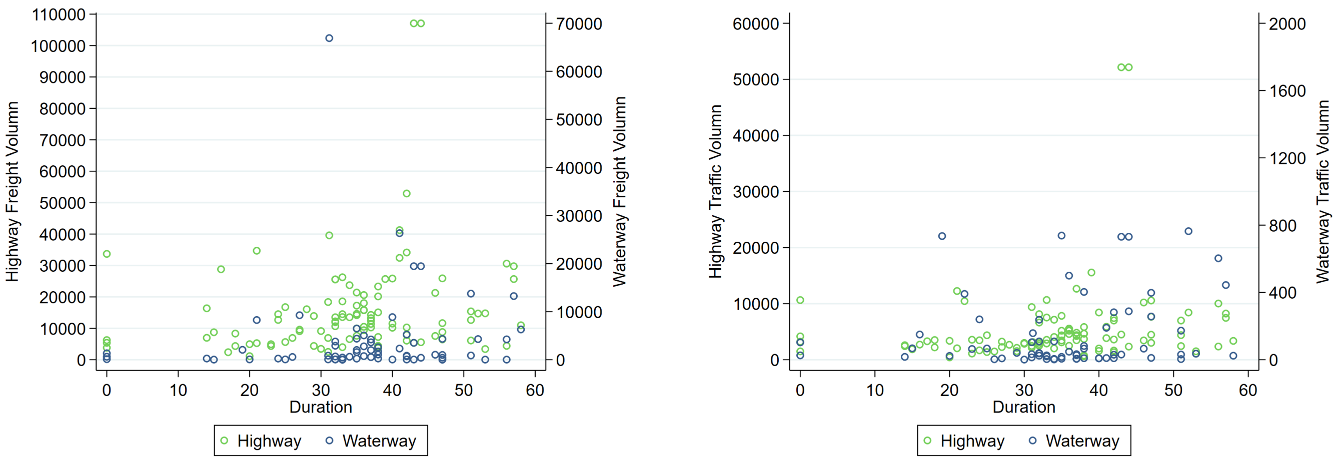
Notes: The figure shows coefficients and 95% confidence intervals from estimating the leads and lags regression in equation (2), where the dependent variable is the natural log of hours worked. All effects are relative to 2018.

Figure A5: Dynamic Effects: Hours Worked



*Notes:*The figure shows the estimated effect of zero-covid policy on probability of unemployment, from July to December. Reporting 90% confidence intervals.

Figure A6: Treatment Effect by Survey Month



(a) Freight Volume

(b) Traffic Volume

Notes: The figure shows correlation between duration and highway or waterway freight volume(a) or traffic volume(b).

Figure A7: Freight, Traffic and Duration

8.3 Appendix Tables

Table A1: Sample by Waves

Year	Prefectures	Obs	Share
2014	125	16246	0.26
2016	125	17453	0.28
2018	123	18379	0.29
2020	121	11265	0.18
Total	125	63343	1.00

Notes: The Table reports distribution of sample across waves (2014-2018).

Table A2: Comparison of Different Data Sources on Unemployment Status

Year	Unemployment	Unemployment(U-3)	Unemployment_US(U-4)	Unemployment_US(U-3)
2014	17.0	4.6	6.6	6.2
2016	17.0	4.5	5.2	4.9
2018	17.1	4.3	4.2	3.9
2020	17.9	5.0	8.4	8.1

Notes: The table shows unemployment we use (column 1), China Official U-3 unemployment(column 2), US officia U-4 unemployment(column 3) and US official U-3 unemployment(column 4).

Table A3: Treatment Validation

	(1) Lockdown Days	(2) log Lockdown Days	(3) Lockdown Indicator	(4) Lockdown Indicator	(5) COVID Unmployment	(6) COVID Unmployment
Duration	0.0532**		0.00162**			0.0000795***
	(0.0269)		(0.000756)			(1.74e-18)
lnDuration		0.280***		0.0885***	0.0103***	
		(0.0641)		(0.0193)	(1.78e-16)	
R-squared	0.0831	0.0673	0.0538	0.0634	0.00366	0.00364
Observations	297	297	297	297	1866	1866
Mean of Outcome					0.0263	0.0263
Prefecture FE					✓	✓

Notes: Unit of observation is the prefecture (column 1-4) or individual (5-6). Log Lockdown Days is the natural log of the number of lockdown days. Lockdown Indicator is an indicator equal to 1 if the prefectures have ever locked down. COVID Unemployment is the number of the interviewee unemployed due to COVID. lnDuration (lnDuration) is the duration (natural log) of the zero-Covid policy. Robust standard errors in parentheses, clustered at the prefecture level. ***, **, and * indicate significance at the 1%, 5% and 10% levels.

Table A4: Baseline Results: Hours Worked (Overall)

	(1) log Hours Worked (Overall)	(2) log Hours Worked (Overall)	(3) log Hours Worked (Overall)	(4) log Hours Worked (Overall)	(5) log Hours Worked (Overall)
lnDuration × Post	-0.0533***	-0.0623***	-0.0710***	-0.0661***	
	(0.0176)	(0.0159)	(0.0163)	(0.0145)	
lnDuration_Dummy × Post					-0.110**
					(0.0472)
R-squared	0.459	0.461	0.460	0.461	0.461
Observations	48601	48601	48601	48601	48601
Mean of Hours Worked Overall	36.00	36.00	36.00	36.00	36.00
Individual FE	✓	✓	✓	✓	✓
Year FE	✓	✗	✗	✗	✗
Province-Year FE	✗	✓	✓	✓	✓
lnPop × Year FE	✗	✗	✓	✓	✓
lnGDP × Year FE	✗	✗	✗	✓	✓
ServiceShare × Year FE	✗	✗	✗	✓	✓

Notes: Unit of observation is an individual. The dependent variable is the natural log of hours worked. lnDuration is the natural log of the duration of the zero-Covid policy. lnDuration_Dummy is a dummy that is 1 if the duration of the zero-Covid policy is above median. Robust standard errors in parentheses, clustered at the prefecture level. ***, **, and * indicate significance at the 1%, 5% and 10% levels.

Table A5: Baseline Results: Income

	(1)	(2)	(3)	(4)	(5)
	log Income	log Income	log Income	log Income	log Income
lnDuration × Post	-0.106	-0.183***	-0.229***	-0.212***	
	(0.0679)	(0.0597)	(0.0606)	(0.0668)	
lnDuration_Dummy × Post					0.0616
					(0.176)
R-squared	0.440	0.443	0.443	0.443	0.443
Observations	28445	28445	28445	28445	28445
Mean of Income	20992.2	20992.2	20992.2	20992.2	20992.2
Individual FE	✓	✓	✓	✓	✓
Year FE	✓	✗	✗	✗	✗
Province-Year FE	✗	✓	✓	✓	✓
lnPop × Year FE	✗	✗	✓	✓	✓
lnGDP × Year FE	✗	✗	✗	✓	✓
ServiceShare × Year FE	✗	✗	✗	✓	✓

Notes: Unit of observation is an individual. The dependent variable is the natural log of income. lnDuration is the natural log of the duration of the zero-Covid policy. lnDuration_Dummy is a dummy that is 1 if the duration of the zero-Covid policy is above median. Robust standard errors in parentheses, clustered at the prefecture level. ***, **, and * indicate significance at the 1%, 5% and 10% levels.

Table A6: Placebo Test: Fake Treatment Period

	(1) Unmployment	(2) Unmployment	(3) log Hours Worked	(4) log Hours Worked
lnDuration × Post 2018	-0.00199		-0.00591	
	(0.00516)		(0.0187)	
lnDuration × Post 2016		0.00274		-0.0118
		(0.00445)		(0.0247)
R-squared	0.477	0.477	0.291	0.291
Observations	51033	51033	26286	26286
Mean of Outcome	0.175	0.175	46.46	46.46
Individual FE	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓
lnPop × Year FE	✓	✓	✓	✓
lnGDP × Year FE	✓	✓	✓	✓
ServiceShare × Year FE	✓	✓	✓	✓

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Unit of observation is an individual. The dependent variable is the natural log of income. lnDuration is the natural log of the duration of the zero-Covid policy. Robust standard errors in parentheses, clustered at the prefecture level. ***, **, and * indicate significance at the 1%, 5% and 10% levels.

Table A7: Robustness: Balanced Panel

	(1) Unemploymeny	(2) Unemploymeny	(3) log Hours Worked	(4) log Hours Worked	(5) log Income	(6) log Income
lnDuration × Post	0.0120**		-0.0379***		-0.181**	
	(0.00471)		(0.00837)		(0.0813)	
lnDuration_Dummy × Post		0.0378***		-0.0303		-0.122
		(0.0112)		(0.0311)		(0.211)
R-squared	0.451	0.451	0.331	0.331	0.457	0.457
Observations	32368	32368	21047	21047	14162	14162
Mean of Outcome	0.148	0.148	45.98	45.98	21090.4	21090.4
Individual FE	✓	✓	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓	✓	✓
lnPop × Year FE	✓	✓	✓	✓	✓	✓
lnGDP × Year FE	✓	✓	✓	✓	✓	✓
ServiceShare × Year FE	✓	✓	✓	✓	✓	✓

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Unit of observation is an individual. The dependent variable is an indicator for unemployment (columns 1-2), the natural log of hours worked(columns 3-4), or the natural log of income(column 5-6). lnDuration is the natural log of the duration of the zero-Covid policy. lnDuration_Dummy is a dummy that is 1 if the duration of the zero-Covid policy is above median. Robust standard errors in parentheses, clustered at the prefecture level. ***, **, and * indicate significance at the 1%, 5% and 10% levels.

Table A8: Robustness: Clustering Choices

	Unemployment			log Hours Worked		
	(1) Prefecture Year Clustering	(2) Province Level Clustering	(3) Province Year Clustering	(4) Prefecture Year Clustering	(5) Province Level Clustering	(6) Province Year Clustering
lnDuration × Post	0.0109**	0.0109*	0.0109**	-0.0239*	-0.0239**	-0.0239
	(0.00466)	(0.00568)	(0.00485)	(0.0127)	(0.0110)	(0.0151)
R-squared	0.470	0.470	0.470	0.316	0.315	0.315
Observations	63343	63343	63343	37914	37914	37914
Mean of Outcome	0.173	0.173	0.173	46.54	46.54	46.54
Individual FE	✓	✓	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓	✓	✓
lnPop × Year FE	✓	✓	✓	✓	✓	✓
lnGDP × Year FE	✓	✓	✓	✓	✓	✓
ServiceShare × Year FE	✓	✓	✓	✓	✓	✓

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Unit of observation is an individual. The dependent variable is an indicator for unemployment (columns 1-3) or the natural log of hours worked(columns 4-6). lnDuration is the natural log of the duration of the zero-Covid policy. Column 1-2 drop Wuhan, column 3-4 drop Hubei province, and column 5-6 drop big cities. Robust standard errors in parentheses, clustered at the prefecture level. ***, **, and * indicate significance at the 1%, 5% and 10% levels.

Table A9: Robustness: Drop Wuhan, Hubei and Big Cities

	Drop Wuhan		Drop Hubei		Drop Big Cities	
	(1)	(2)	(3)	(4)	(5)	(6)
	Unemployment	log Hours Worked	Unemployment	log Hours Worked	Unemployment	log Hours Worked
lnDuration × Post	0.0135***	-0.0235***	0.0117**	-0.0200**	0.0132***	-0.0214***
	(0.00440)	(0.00810)	(0.00466)	(0.00820)	(0.00478)	(0.00811)
R-squared	0.470	0.315	0.469	0.314	0.452	0.313
Observations	63073	37747	62379	37353	57505	34674
Mean of Outcome	0.173	46.55	0.173	46.57	0.166	46.66
Individual FE	✓	✓	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓	✓	✓
lnPop × Year FE	✓	✓	✓	✓	✓	✓
lnGDP × Year FE	✓	✓	✓	✓	✓	✓
ServiceShare × Year FE	✓	✓	✓	✓	✓	✓

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Unit of observation is an individual. The dependent variable is an indicator for unemployment (columns 1, 3, and 5) or the natural log of hours worked(columns 2, 4, and 6). lnDuration is the natural log of the duration of the zero-Covid policy. Robust standard errors in parentheses, double clustered at the prefecture and year level (column 1 and 4) , clustered at the province level (column 2 and 5), or double clustered at the province and year level (column 3 and 6) . ***, **, and * indicate significance at the 1%, 5% and 10% levels.

Table A10: Heterogeneity: Labor Outcomes by Job Class

	Unemployment		Unemployment		log Hours Worked		log Hours Worked	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Non Agri	Angricultural	Non Agri	Angricultural	Non Agri	Angricultural	Non Agri	Angricultural
lnDuration × Post	0.0136**	0.00843*			-0.0217**	-0.0254*		
	(0.00644)	(0.00453)			(0.00891)	(0.0150)		
lnDuration_Dummy × Post			0.0332**	0.0260**			-0.0215	0.0161
			(0.0135)	(0.0128)			(0.0207)	(0.0702)
R-squared	0.338	0.212	0.338	0.212	0.220	0.290	0.220	0.290
Observations	34217	25998	34217	25998	22114	15800	22114	15800
Mean of Outcome	0.158	0.0947	0.158	0.0947	51.39	39.74	51.39	39.74
Individual FE	✓	✓	✓	✓	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓	✓	✓	✓	✓
lnPop × Year FE	✓	✓	✓	✓	✓	✓	✓	✓
lnGDP × Year FE	✓	✓	✓	✓	✓	✓	✓	✓
ServiceShare × Year FE	✓	✓	✓	✓	✓	✓	✓	✓

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Unit of observation is an individual. The dependent variable is an indicator for unemployment (columns 1-4) or the natural log of hours worked(columns 5-8). lnDuration is the natural log of the duration of the zero-Covid policy. lnDuration_Dummy is a dummy that is 1 if the duration of the zero-Covid policy is above median. The sample is restricted to inverviewee worked non-agricultural (column 1, 3, 5, and 7) or agricultural sector (column 2, 4, 6, and 8). Robust standard errors in parentheses, clustered at the prefecture level. ***, **, and * indicate significance at the 1%, 5% and 10% levels.

Table A11: Heterogeneity: Unemployment by Traffic and Freight

	Unemployment		Unemployment		Unemployment		Unemployment	
	(1) Traffic.V HWY Low	(2) Traffic.V HWY High	(3) Traffic.V WTWY Low	(4) Traffic.V WTWY High	(5) Freight.V HWY Low	(6) Freight.V HWY High	(7) Freight.V WTWY Low	(8) Freight.V WTWY High
lnDuration × Post	0.0153	0.00271	0.0218	0.00336	0.0115**	-0.00324	0.0341***	-0.0671*
	(0.00963)	(0.00684)	(0.0222)	(0.00305)	(0.00536)	(0.00701)	(0.00909)	(0.0382)
R-squared	0.486	0.458	0.452	0.526	0.454	0.486	0.459	0.513
Observations	26229	30331	18219	14510	30771	25162	18726	13614
Mean of Unemployment	0.170	0.171	0.183	0.185	0.159	0.188	0.171	0.199
Individual FE	✓	✓	✓	✓	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓	✓	✓	✓	✓
lnPop × Year FE	✓	✓	✓	✓	✓	✓	✓	✓
lnGDP × Year FE	✓	✓	✓	✓	✓	✓	✓	✓
ServiceShare × Year FE	✓	✓	✓	✓	✓	✓	✓	✓

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Unit of observation is an individual. The dependent variable is an indicator for unemployment. lnDuration is the natural log of the duration of the zero-Covid policy. The sample is restricted to the interviewee in the prefecture with lower highway traffic volume (column 1), higher highway traffic volume (column 2), lower waterway traffic volume (column 3), higher waterway traffic volume (column 4), lower highway freight volume (column 5), higher highway freight volume (column 6), lower waterway freight volume (column 7), higher waterway freight volume (column 8). Robust standard errors in parentheses, clustered at the prefecture level. ***, **, and * indicate significance at the 1%, 5% and 10% levels.