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 China's anti-contagious policy on labor market outcomes in 2020. 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 in a 14-day observation window, 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. Unlike most large economies that suffered a serious health shock from the COVID-19 pandemic, China effectively contained the scale and the spread of the initial outbreak in 2020. This provides a special empirical setting to examine the policy effect of anti-contagious policies, and we show that the disruption on the labor market majorly comes from the zero-Covid containment measures, while health shocks are trivial on the labor market outcomes. 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 clearance during the first stage of the pandemic (ended by Feb 17, 2020) caused the negative impacts on the labor outcomes, while the subsequent dynamic clearance strategy did not generate significant disruption on the labor market outcomes in 2020.

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 has not been achieved. 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.[1]

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 disease prevention policies, thus leading to a persistent public health shock as well as the impacts of the mitigation policies (Goolsbee and Syverson, 2021).

China provides a suitable empirical setting to investigate the sole impact of the anti-contagious policies. 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 prefecture implements stringent anti-contagious measures, such as city lockdowns, human mobility will fall dramatically and business will cease not only within the focal

[1]For example, Gordon et al. (2021) find that Sweden experienced a more serious public health shock relative to its Nordic neighbours because of its decision not to impose an air border closure in the first half of 2020. They also find that a poorer public health performance in containing the COVID-19 pandemic is associated with a worse economic performance for OECD Europe in 2020.

region but also between it and other regions . This implies that economic activities in a prefecture could be influenced by zero-COVID policies of its nearby regions, if their economic connections were strong before the outbreak. We control the spillover effect by controlling every prefecture's nearby zero-Covid policy duration, and our results show that the estimated local policy effect is not driven by the spillover effect.

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. We provide the evidence of the associative economic cost of China's zero-Covid policy for eliminating the pandemic in 2020.

This paper relates to several strands of literature. First, it contributes to the increasingly large empirical literature on identifying causal impact of COVID-19 pandemic on labor market[2] [3]. Gupta et al. (2020) applies a Difference in Differences 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)

[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).

[3]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. 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.

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. 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.

Our choice of treatment, identification setting and unique context provide credibility to identify causal effects of COVID policies, by tackling down possible challenges.[4] First, our framework enables us to construct a conditional exogenous treatment — the duration that a prefecture was exposed to the zero-Covid policy and thereby we could causally interpret China's anti-contagious policy impact on labor outcomes. Second, our identification design allows us to analyze the disentangled impact of zero-Covid policy on labor market, rather than combined public health shock and derivative voluntary precautions.[5] There are no other studies, to our best knowledge, that conduct analysis on the impact of anti-contagious policies on labor market without the existence of the public health shock caused by the pandemic. Third, in the context of China, our result provides a benchmark for the marginal change in labor market outcomes where the region implements anti-contagious measures without a significant scale of the pandemic. Furthermore, the unprecedented anti-contagious policy launched by China leaves little chance for anticipation.

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 on the influence from city lockdowns, while this paper identifies the zero-Covid policy effect for a wide spectrum of containment measures at different intensities, e.g. lockdowns, regional quarantines, closure of public places, transportation restrictions, etc. During the early period of China's anti-Covid campaign, many prefectures implemented lockdowns to block the spread of the virus quickly and efficiently, while more prefectures which experienced mild outbreak of the epidemic chose less stringent measures to contain this public health crisis.

[4]Goodman-Bacon and Marcus (2020) discuss threats to the validity of DiD designs on identify causal effects of COVID policies, such as packaged policies, voluntary precautions, anticipation and spillovers.

[5]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.

Our estimations capture 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 contribute 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. Section 5 discuss our estimation results and potential threats to our baseline findings. Section 6 concludes this paper.

2 Background

China's zero-Covid policy [6] 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 resume work and production after Feb 17, 2020, zero-Covid policy became a

[6] Note that the term "zero-Covid" in Chen et al. (2022) only refers to the *stringent clearance* in our paper.

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 [7][8]. 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; Tian et al., 2020). Local governments reacted with *stringent clearance* policies in response to the unprecedented national emergency. The entire Hubei province implemented the 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 lockdown, 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[9]. 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.

It is noteworthy 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[10]. Following the central government's guidance, local governments soon implemented this 14-day observation window, which will be an

[7]According to Emergency Response Law of the PRC, the emergency events are classified into 4 levels, First as extreme important and Fourth as normal. The First level emergency response is coordinated by the central government, the Second level is led by province government, the 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), so this province level indicator is considered as a bellwether for province government's attitude towards COVID.

[8]Ironically, as 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.

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

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

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,[11] 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*[12] policies implemented at low risk areas, such as school closings, cancellation of public events and restaurant closures. The middle risk areas were defined as prefectures without an *outbreak*.[13] On average, the high risk areas were defined as those with more than 10 cases reported within 14 days.[14] 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.[15] 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

[11] "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)

[12] In 2020, "dynamic clearance" refers to implementing precise containment measures to control the spread of virus at small economic costs. However, this terminology is interpreted differently — to eliminate COVID at any cost — by Chinese propaganda in 2022, when Chinese government deals with Omicron variant. In this paper, we use the definition in 2020 for "dynamic clearance"

[13] An *outbreak* is defined as 2 to 5 or more emerging confirmed COVID-19 cases within 14 days.

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

[15] 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

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 switched to the Second level. Three days later, Hubei switched 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, 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 sample of 139,983 observations. To arrive at the sample used for analysis, 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), resulting in a sample of 112,670 observations. We further restrict attention to individuals whose ages were between 16 and 64, and the sample size reduces 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. Finally, we end up with a sample of 63,343 observations (20,006 individuals). Among these 63,343 observations collected from 125 prefectures, 25.6 percent were surveyed in 2014, 27.6 percent were survey in 2016, 29.0 percent were surveyed in 2018 and the rest 17.8 percent were surveyed in 2020.[16]

[16] We also report the distribution of samples across four waves in Table A1.

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 an interviewee answers "NO" for all questions above, the interviewee is unemployed; otherwise, the interviewee is on employment.[17]

Moreover, there is a question for employed interviewees rather than self-employed interviewees and business owners, "Including salary, bonus, cash benefit, material benefit, and excluding tax, insurances, and public housing, how much in total did you make from this job for the last 12 months?" We construct the outcome variable *Income* according to the answer of this question. Finally, the questionnaire has a question, "How many hours per week on average did you work for this job in the past 12 months?" We construct the outcome variable *Hours Worked* accordingly. To capture the responses of hours worked along the intensive and extensive margins, we also include unemployed workers and replace the missing values of hours worked with zero. Panel A of Table 1 presents a statistic summary for labor outcomes in our sample. Average unemployment is 0.173.[18] Among the employed workers, average labor income is 20,992 RMB and average hours worked per week is 46.3. Furthermore, we calculate length of subsistence as the ratio between cash or deposit and family's yearly expenditure.[19] For families located at the bottom 20% income distribution who are extremely vulnerable to unemployment, their saving could only maintain their

[17]Again, the definition of unemployment we apply here is similar to U-4 unemployment, which includes unemployed and discouraged workers.

[18]In Table A2, we report the different measures of unemployment in China and the United States. One could observe the U-4 unemployment rate collected from CFPS is higher than the official U-3 unemployment rate published by the Chinese government. The discrepancy between these two measures is also larger in China than in US.

[19]Figure A2 displays how many years interviewees' cash or deposit could afford their expenditure if they become unemployed and have no other income.

basic family expenditure for around 6 months.

To investigate heterogeneous effects of COVID-19, we use a series of basic demographic information from CFPS 2018. Specifically, we report the heterogeneous effects for the following dimensions: gender, age, education and the age of the youngest child in the household. Panel B of Table 1 provides a 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 was labeled as a middle or high risk region, thus potentially the zero-Covid anti-contagious measures were implemented in the region, we rely on the time-series data[20] of the daily new COVID-19 cases from Jan 23[21] to June 30.[22] 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-19 case, then the risk level will degrade to low. We locate each prefecture's middle or high risk period by excluding the low-risk period, i.e., the dates that have no COVID-19 positive cases and are not within a 14-day window of new COVID-19 case. Essentially, *Duration* measures how many days that a prefecture was exposed to mid- or high- risk under the national 14-day observation rule, accompany by a wide spectrum of anti-contagious measures under the zero-Covid policy. Panel C of Table 1 summarizes the statistics for zero-Covid policy duration and COVID-19 cases at prefecture level. Average zero-Covid policy duration is 37.128 days. Average number of confirmed cases and death is 451.697 and 31.432, respectively. 34.9 percent of the prefectures once implemented a (city level) lockdown policy. Finally, for regression estimation, we use *lnDuration*, the log of zero-COVID policy duration plus 1, as the major treatment variable.

One possible concern is that our measure of zero-Covid policy duration is constructed from the COVID-19 case data following the guidance rule enforced by central government, instead of docu-

[20]The data source is from *Dingxiangyuan* website, which collects the official daily release of COVID-19 cases from each province.

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

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

menting the real duration of mid- or high- risk level in each prefecture. To test the validity of the treatment, the ideal way is to compare the documented prefecture-level zero-Covid policy duration between January to June, 2020 with our constructed treatment. However, there is no accurate measure for the timing and duration of the zero-Covid policy corresponding to this period.[23] He et al. (2020) and Zhang (2021) collect information of starting dates for lockdowns without ending dates, and thereby cannot provide accurate duration of lockdowns. Hale et al. (2022) generate stringent indices for China's COVID-19 responses, however, the policy stringency is 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 in China, nevertheless, their research collects data between April 2020 and January 2022, only 2 months overlapped with the period considered in this paper.

We therefore choose an alternative outcome, traffic mobility index from Baidu[24] (Hu et al., 2020), to validate our treatment. To be specific, we calculate the difference between the daily traffic mobility indices (including in-town traffic, out-town traffic and intra-town traffic) in 2020 and those of a comparable lunar date [25] a year ago in each prefecture.[26] We define *Exposed to Risk* as a dummy that equals to 1 if prefecture p is considered as mid- or high- risk on date t, under the national 14-day observation rule. We expect to observe a significant negative correlation between traffic mobility and the zero-Covid policy indicator. The period of traffic index we use is from January 1st to May 7th [27], which has 4 months overlapped with the period considered in our sample.

As shown in Equation (1), we further apply a DiD setting to examine the validity of our treatment — *LnDuration*, the log form *Duration* of the instrumented zero-Covid policy. ΔY_{pt} is the measure of difference in mobility for prefecture p on date t. Besides, *Jan23* is defined as 1 for the dates post January 23, 2020, and 0 otherwise , and θ_p and δ_t capture the prefecture and date

[23] The uniform national rule has not been launched by April 2020. Pre-April, it is hard to classify different local risk level rules into a general framework. For example, Zhejiang province use a five color system to classify risk level before April 2020.

[24] A counterpart of Google mobility index in China

[25] Since the research period is overlapped with Chinese Spring Festival, involving with high volatility in traffic mobility, we use Chinese lunar calendar as comparable date

[26] A similar calculation was used in Sim et al. (2022)

[27] Baidu stopped publishing the mobility index after May 8th, 2020.

fixed effects.

$$\Delta Y_{pt} = \beta(lnDuration_p \times Jan23_t) + \theta_p + \delta_t + \epsilon_{pt} \tag{1}$$

We report the correlation between *Exposed to Risk* and change in traffic mobility relative to 2019 in the first three columns of Table A3. In Panel A, among all the prefectures in China, columns (1) to (3) show negative and significant correlations between whether exposed to zero-Covid policy and the decline of in-town, out-town and intra-town traffic mobility. Columns (4) to (6) present the DiD estimation results which suggest a non-trivial decline in traffic mobility relative to the counterfactual change in mobility based on 2019 corresponding to the duration of zero-Covid exposure. In Panel B, we only keep the CFPS surveyed prefectures in sample, and the results stay robust. These stable significant negative correlations provide evidence for the validity of our treatment variable — as a prefecture receives a larger treatment, the more it is possibly exposed to the zero-Covid policy.

It is important to point out that our measure also captures less stringent zero-Covid interventions other than lockdowns.[28] As we argued in Section 2, for prefectures with mild increase of COVID-19 cases, less stringent policies are more likely to be implemented as they are enough to mitigate the spread of virus. In Figure A3, we plot the number of confirmed COVID-19 cases versus zero-Covid policy duration for each prefecture, while categorized by whether prefectures experienced lockdown or not.[29] We could observe that prefectures with similar situation in COVID-19 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.

Last 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 separate duration variables

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

[29] The lockdown information is adopted from He et al. (2020). 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."

corresponding to different time periods (using the same method described above): 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).[30] 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);[31] (3) Share of Service Sector in GDP. 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.

4 Identification

4.1 Baseline Model

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

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

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.

[30]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

[31]The minimum GDP is 15937.7 thousand RMB and round to 0 in billion.

The parameter of interest β captures the marginal effects of exposure to zero-Covid policies on the labor outcomes. In contrast with binary treatment DiD, 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,b). For robustness purpose, we also generate a binary treatment variable that assigns one if the $lnDuration_p$ is above the median.[32] We will discuss more details about the continuous treatment setting and potential challenges in Section 5.4.4.

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 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, including population, GDP and share of service industry in 2018. We include $X_p \times Year_t$ to let their effects differ across year, and thereby to address the concern that prefectures with different economic characteristics may response differently to the pandemic through other channels.[33] 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 DiD design relies on the 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.

$$Y_{ipt} = \sum_{t \in \{1,2,4\}} \beta_t (lnDuration_p \times Year_t) + \sum_{t \in \{1,2,4\}} (X_p \times Year_t)\lambda_t + \theta_i + \delta_{r,t} + \epsilon_{ipt} \quad (3)$$

[32] We split the sample at the median prefecture so that the number of treated and controlled prefectures is approximately balanced. This method is used by Jensen and Johannesen (2017)

[33] A detailed discussion of this method is made by Jensen and Johannesen (2017). One possible alternative is to control time variant characteristics, which mean to include post-treatment variables into regression. However, It will result in"Bad Control" problem (Angrist and Pischke, 2009). Moreover, the data of prefecture level controls in 2020 are not available yet.

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 a significant relative impact from the "placebo" treatments on the pre-treated outcomes. Same with previous section, we also use a binary treatment DiD setting as robustness check. In this case, 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 assumptions underlying our generalized DiD design. First, the potential disproportionately distributed spillover effects from neighbor units would bias 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 DiD setting — heterogeneous gains across different treatment doses, given the same counterfactual treatment dose (Callaway et al., 2021; Cunningham, 2021). We present evidences supporting our identification assumption and discuss these threats to identification in greater details in Section 5.4.

5 Results

5.1 Baseline Result

We first present our estimated zero-Covid policy effect on labor market outcomes using the baseline DiD specifications. In Table 2 Panel A, we provide estimates for unemployment. The interaction of log form of the zero-Covid policy duration with an indicator for post-treatment is our DiD 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 (log population, log GDP and share of service sector together) 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 based on a national rule, we are able to predict the counterfactual impact of anti-contagious policies with a shorter observation window. 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 perform a back-of-the-envelope calculation and 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 zero-Covid policy under a 5-day window would only increase the unemployment probability by 0.0324 , which is about a 12% decrease in the marginal policy effect.

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 0.1 hours per week, depending on the regression specification.

Comparing with the average hours worked per week, 46.54, the effect on hours worked is trivial. However, there are several points that could potentially explain this result worth noting. First, the hours worked per week are calculated from the past 12 months since the survey time (July to December, 2020) but not solely from post-COVID period. For instance, if half of the past year was under the pandemic and the other half was not, the estimated policy effect on hours worked should be doubled to adjust for the inappropriate averaging in the survey. Second, our results could underestimate the true policy impact due to the spillover effect. We will discuss the details in section 5.4.1, where we provide estimation of the policy effect controlling for the spillover effect. Third, with the increase of unemployment, some workers who remain employed might be assigned heavier workload to achieve production targets, offsetting the negative effect on intensive margin responses. Fourth, the trivial 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. Fifth, it is also possible that hours worked increase for workers who are quarantined at the working places, such as factories, hospitals and schools, and decrease for those who don't have access to the working places. The former scenario could partially offset the general negative policy impact on hours worked.

We also estimate a binary treatment DiD specification for robustness purpose: 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 DiD 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. Plausible reasons are analyzed above. Given all those above, the binary DiD 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 DiD settings, we could observe the magnitude of the policy effect increases compared to the intensive margin responses. Furthermore, in the binary DiD 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 DiD estimator is that prefectures 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 prefecture characteristics that are associated with the pandemic outbreak. We provide the test for pre-trends that might violate parallel trend assumptions of the DiD 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 observe that before the pandemic shock in 2020, prefectures 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 highly likely to hold 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 A4, 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 worker or other possible reasons.

5.3 Disentangled Effect

5.3.1 Disentangled from Health Effect

China provides a suitable empirical setting to investigate the sole impact of the anti-contagious policies and our estimation presents the isolated effect of the zero-Covid 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 was arguably negligible in most parts of China. As the number of confirmed cases is trivial compared to the prefecture population, the infection probability is close to zero and the workers 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 (4), $lnCases_p$ 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.

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

We estimate the DiD treatment effect of the number of confirmed cases and dead cases and report the results in Table 3. In column (1) (2) and (3), besides the DiD treatment for the policy duration and other standard fixed effects, we further include the DiD treatments for the public health shock, which are the interaction terms 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 the public health shock estimators are positive or statistically significant, while the coefficient for policy duration does not change much. This implies that the potential public health shock does not measurably influence the local employment status as the extremely restricted containment policy eliminates the public health concern efficiently. In other words, the results suggest that our estimated policy effect is not driven by the public health shock and it majorly reflects the impact on the labor market from the zero-Covid policy.

5.3.2 Disentangled from Lockdown Effect

As we discussed in Section 2, 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 the pandemic, and disentangle the effect of policy intensity and policy duration, we include 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 level. The former is defined as inter-city travel restriction, and the latter is defined as intra-city mobility restriction. 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.

$$
\begin{aligned}
Y_{ipt} = &\beta(Duration_p \times Post_t) + \pi_1(Lockdown\_city_p \times Post_t) \\
&+ \pi_2(Lockdown\_comm_p \times Post_t) + \sum_{t\in\{1,2,4\}} (X_p \times Year_t)\lambda_t + \theta_i + \delta_{r,t} + \epsilon_{ipt}
\end{aligned} \tag{5}
$$

In Table 4 columns (1)(2) and (5)(6), we estimate the DiD treatment effects 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 effects of the DiD 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 fully explain the negative pattern observed in the labor market. We provide further evidences that the zero-Covid policy, disentangled from the city lockdown, made 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 is not only 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 (impacted by neighbors) in prefectures with relatively longer zero-Covid policy duration, and weaker spillover effect (impacted by neighbors) in prefectures with relatively shorter duration, the coefficient of local policy effect will be overestimated. Alternatively, if prefectures with relatively shorter zero-Covid policy duration experienced severe spillover from neighbors and prefectures with relatively longer zero-Covid policy duration experienced negligible spillover effects, the policy impacts will be underestimated.

In this section, we empirically assess the *Stable Unit Treatment Values Assumption* (SUTVA) by controlling the duration of zero-Covid policy in nearby prefectures. 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 first collect the zero-Covid policy duration for all neighboring prefectures of the surveyed prefectures in our sample. 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 model for the policy effect controlling for spillover effects is following:

$$\begin{aligned} Y_{ipt} &= \beta(lnDuration_p \times Post_t) + \alpha(lnDuration\_Nearby_p \times Post_t) \\ &+ \sum_{t\in\{1,2,4\}} (X_p \times Year_t)\lambda_t + \theta_i + \delta_{r,t} + \epsilon_{ipt} \end{aligned} \tag{6}$$

In Table 5, we estimate the effects 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 a 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 also helps understand that the trivial policy effect on hours worked in the baseline model could be due to the underestimation from spillover effect.

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. Nevertheless, when the COVID-19 virus initially outbroke at China, the 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 as well as the whole nation started implementing stringent anti-contagious policies soon after. As the time interval between the outbreak and the roll out of unprecedented policies is so narrow for labor market to anticipate, it addresses the concern of pre-anticipation bias.

5.4.3 Placebo Test

We employ the method suggested by Huntington-Klein (2021) as the placebo test. We use the truncated alternative version of the DiD model (drop the data in 2020 when the treatment actually happened) and choose year 2016 or 2018 as fake treatment periods. 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 is likely to hold and our baseline estimations are not contaminated by non-treatment influences.

5.4.4 Selection Bias

There are two sources of selection bias in the continuous DiD treatment setting — classic selection bias and differences in treatment effects across different treatment doses.[34] In Section 5.2 and 5.4.2, we have already resolved uncertainty on common trends, where is also referred as classic selection bias (Cunningham, 2021). In this section, we are going to discuss the later concern.

To identify causality with our continuous treatment DiD 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, among two prefectures with $lnDuration$ d_j and d_{j-1}, there might be 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 (7), which is cited from Callaway et al. (2021).

$$\frac{\partial \mathbb{E}[\Delta Y_t | D = d]}{\partial d} = \underbrace{ACRT(d|d)}_{\text{average causal responses}} + \underbrace{\left.\frac{\partial ATT(d|l)}{\partial l}\right|_{l=d'}}_{\text{selection bias}} \tag{7}$$

To be specific, our estimation might include Average Causal Responses (ACRT) and differences in Average Treatment Effect (ATT) across prefectures with differing $lnDuration$ at a given treatment level. 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 national level policy rule could alleviate the "select into different treatment dose" concern.

Given the number of days without 0 increase (absorbed by prefecture fixed effect), it is not easy for local governments to manipulate how many days with 0 new COVID-19 case in a 14-day window, as the time point of detecting a new case is quite random. We provide a hypothetical example in Figure A5: suppose there are two prefectures with identical characteristics and the

[34] According to recent discussion by Callaway et al. (2021),"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 DiD is contaminated by differences in different dosage groups own expected returns."

period to calculate *Duration* is from Jan 23 to Feb 29, 2020. For prefecture A and B, the total confirmed cases are both 40 and the number of days with new cases are 13 and 14, respectively. The almost same pattern in COVID-19 cases should not be surprising because these two prefectures are comparable in all dimensions. In fact, the only difference is that for prefecture A, there are 3 cases confirmed on Feb 05, and for prefecture B, there are 2 cases confirmed on Feb 05 and 1 case confirmed on Feb 09. According to the national 14-day observation rule, the *Duration* (shaded area) for prefecture A is 28 (the start day of *Duration* is Jan 23, the end day is 14 days after Feb 05) and for prefecture B is 32 (the start day of *Duration* is Jan 23, the end day is 14 days after Feb 09). We believe that the last case in prefecture B detected on Feb 09 instead of Feb 05, is mainly driven by some random factors such as the COVID-19 testing turnaround time or the incubation period but not correlated with prefecture characteristics or manipulated by local government. The variation in the treatment is very likely orthogonal to "self selection". Shown in Figure A6, conditional on the number of days with observed cases (X-axis),[35] we can observe large variation in our choice of treatment — *Duration* (Y-axis), which is driven by the random factors instead of prefecture characteristics.

The marginal treatment effect is less likely biased by the selection problem, given the fact that our treatment variable is exogenous conditional on prefecture fixed effect. However, there are still some chances that our baseline findings are influenced by outlier regions which are several largest prefectures 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 metropolises, including Beijing, Shanghai, Guangzhou, Chongqing and Tianjin, frequently detected new COVID-19 cases, resulting in very long 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 A7, 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

[35] which is correlated with prefecture factors and controlled by fixed effect in our econometric model

exclusions of these outlier regions.

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 A8, we only keep the individuals who stay in each 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 A9 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 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 A7, 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 does not have significant lag effect that 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 in 2020.

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* measures, such as lockdown and stay at home order, are more likely to be rolled out between January and late February for pandemic containment purpose. After February 17, *dynamic clearance* measures, such as public place closing and travel restriction between risk areas, became prevalent. Although we cannot measure this granular intensity difference 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 (8), 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 $lnDuration\_FebJun_p$.[36] Between

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

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.

$$\begin{aligned} Y_{ipt} &= \beta(lnDuration\_JanFeb_p \times Post_t) + \eta(lnDuration\_FebJun_p \times Post_t) \\ &+ \sum_{t\in\{1,2,4\}} (X_p \times Year_t)\lambda_t + \theta_i + \delta_{r,t} + \epsilon_{ipt} \end{aligned} \tag{8}$$

As mentioned in Section 2 and Section 3.2, 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 is 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 DiD treatment for policy duration before Feb 17, 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 significant positively related to unemployment, while column (2) show that the policy duration after Feb 17 is not. The results keep consistent while we include both *stringent clearance* and *dynamic clearance* into regression, shown in column (3). Column (4), (5) and (6) display a similar pattern that there are only significant correlations between hours worked and *stringent clearance*, which implies that the magnitude of *dynamic clearance* after Feb 17 is less significant compared to the early phase.

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 younger 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.

6 Conclusions

During the COVID-19 pandemic, countries across the globe adopted drastically different strategies for mitigating the unprecedented public health crisis. While China was the first country to implement harsh anti-contagious interventions nationally, the zero-Covid policy's effect on the economy remained obscure until very recently. Based on a generalized DiD design, we find that when a prefecture's Zero-Covid policy lasts for 10% (3.7 days) longer, the individual-level unemployment probability increases by around 0.1, and employed workers lose 0.2% and 2% of their hours worked and income, respectively. Our estimation disentangles zero-Covid policy and the public health shock of COVID-19, where the latter has no significant impact on labor market outcomes. The impact of lockdown policy is widely discussed in recent literature, while our paper examines the effect of China's zero-Covid policy, a full spectrum anti-contagious policy which includes not only lockdown, but also less stringent anti-contagious interventions that have been difficult to observe due to data limitations. We also control for spillover effects from nearby prefectures, which do not significantly 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 in 2020.[37]

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 the potential economic and political outcomes of restricting human mobility deterred the policymakers from taking serious disease preventive measures immediately after the outbreak of the virus. 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.[38] The economic costs 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 by fast and stringent containment measures, 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 face the trade-off between health, freedom and economic well-being.

[37]Due to data limitation, we are not able to estimate the mid-term policy effect in 2021 — there was a strong rebound in the first half of 2021 and China's GDP growth rate reached 8.1 percent by the end of that year. It was partially credited to the zerro-Covid policy in 2020.

[38]This is indeed what happened to many Chinese cities after the emergence of Omicron in China. More stringent measures and longer zero-Covid policies including city lockdowns were implemented from March till June 2022.

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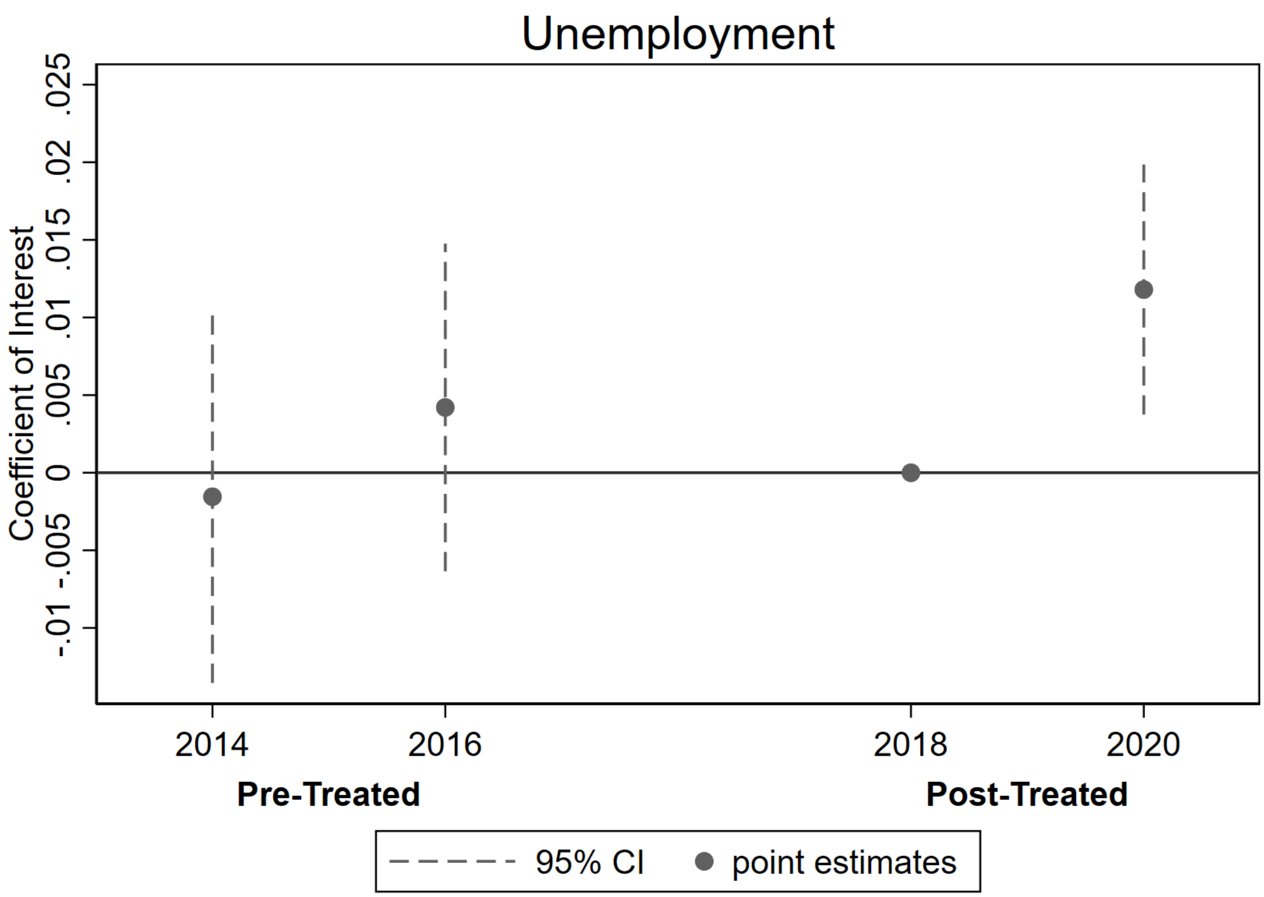
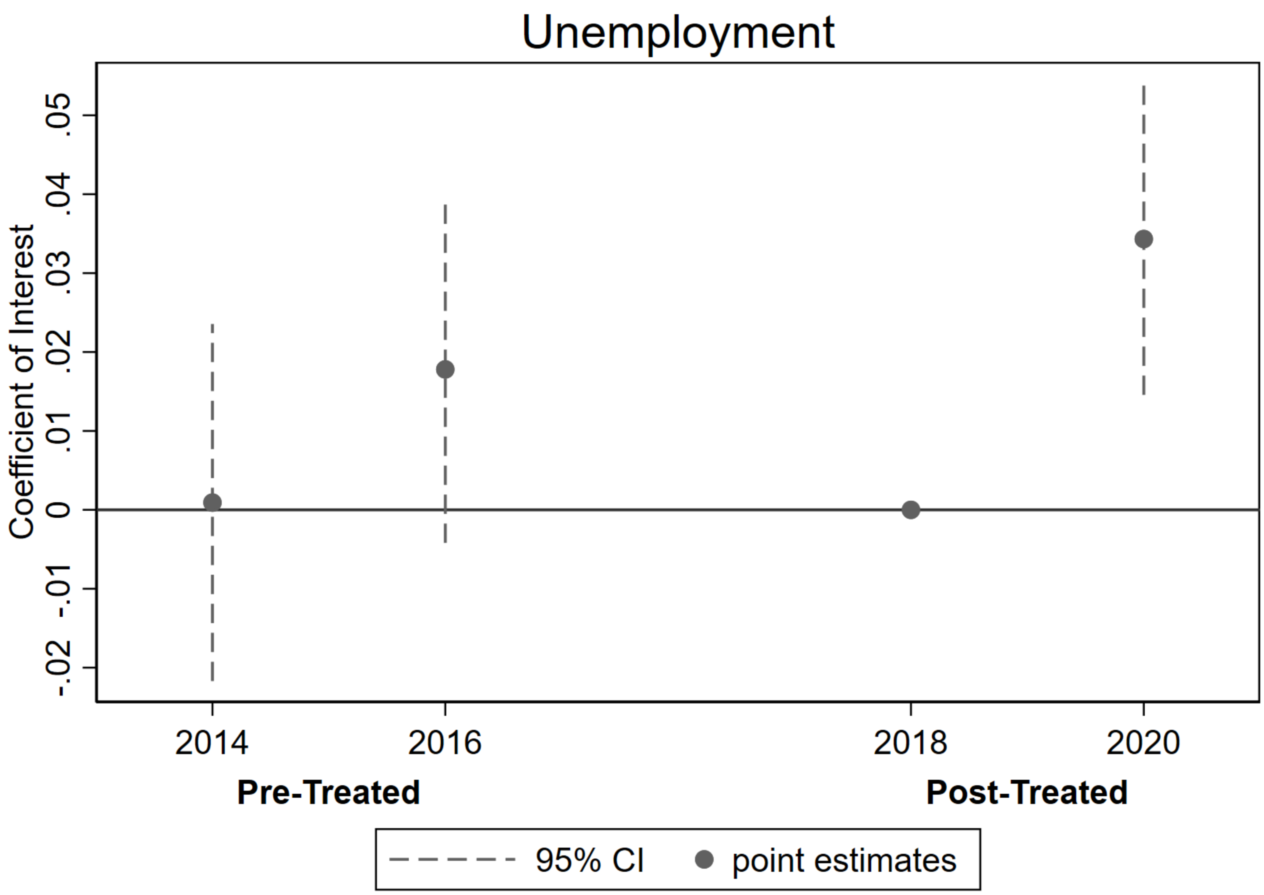
7 Figures and Tables

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. 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. All effects are relative to 2018.

Figure 2: Dynamic Effects Unemployment (binary treatment)

7.2 Tables

Table 1: Statistic Summary

	Obs	Mean	Std.Dev	Min	Max
Panel A: Individual Dependent Variables					
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
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					
Policy Duration	126	37.128	21.411	0.0	158
Policy Duration Feb Jun	126	18.349	19.158	0.0	135
Policy 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 outcome 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 binary variable that equals 1 if the duration of the zero-Covid policy is above median and 0 otherwise. 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 binary variable that equals 1 if the duration of the zero-Covid policy is above median and 0 otherwise. Lockdown_city is an indicator that equals to 1 if the prefecture has ever experienced lockdown and 0 otherwise. Lockdown_comm is an indicator that equals to 1 if communities in the prefecture have ever experienced lockdown and 0 otherwise. 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. 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 are binary variables that equal to 1 if the interviewee is female, above age 65, with education level less than middle school, works in private firm, the bottom 50 percent population in the income distribution, has any child that is younger than 6 years old, has any child that is younger than 18 years old, and 0 otherwise. 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 are binary variables that equal to 1 if the interviewee is female, above age 65, with education level less than middle school, works in private firm, the bottom 50 percent population in the income distribution, has any child that is younger than 6 years old, has any child that is younger than 18 years old, and 0 otherwise. 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 [39].

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 [40], 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[41].), 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 [42].

[39] 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;

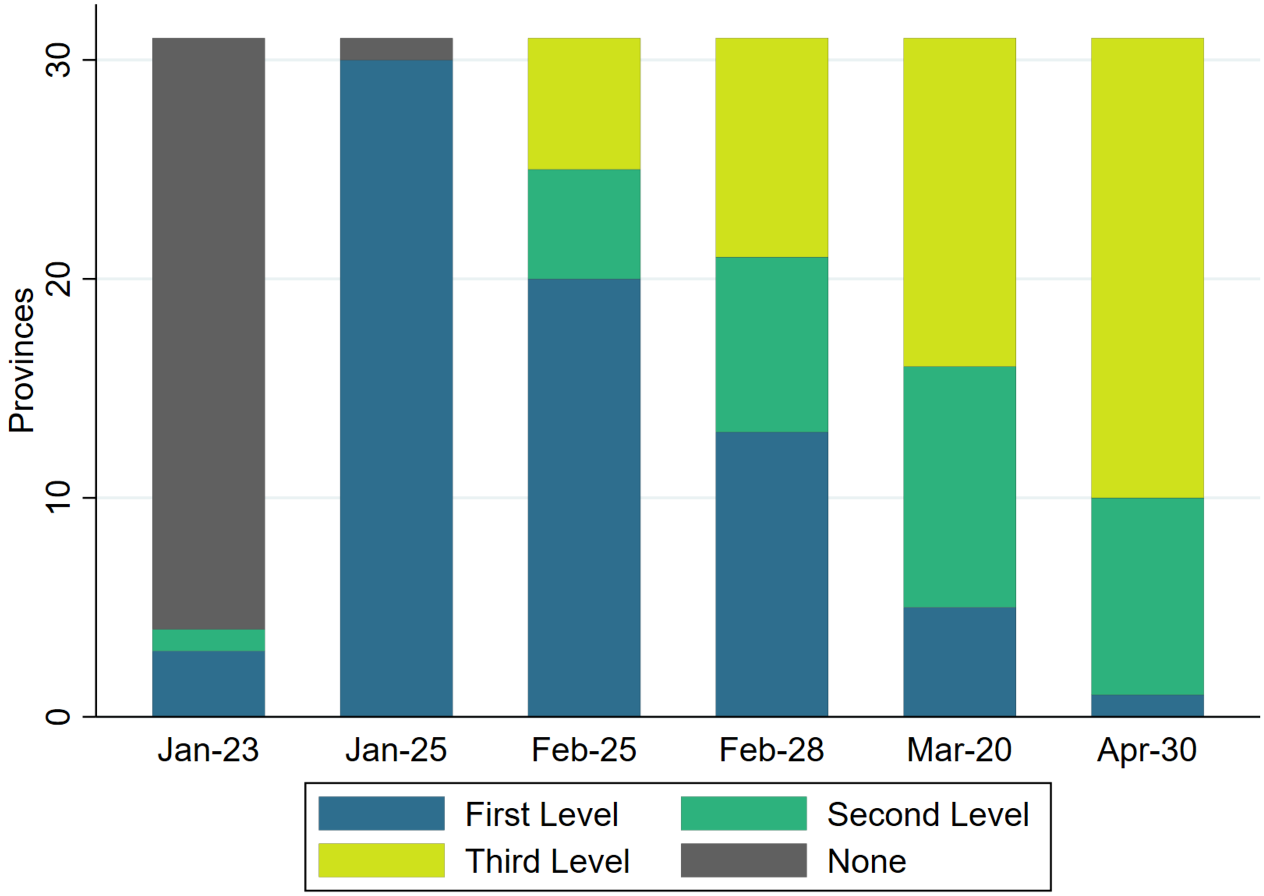
[40] Source: https://www.bilibili.com/video/BV1Y741167Yp/?spm$_{i}d_{f}rom = autoNext$.

[41] Source: http://www.moa.gov.cn/xw/qg/202002/t20200224$_{6}$337603.htm.

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

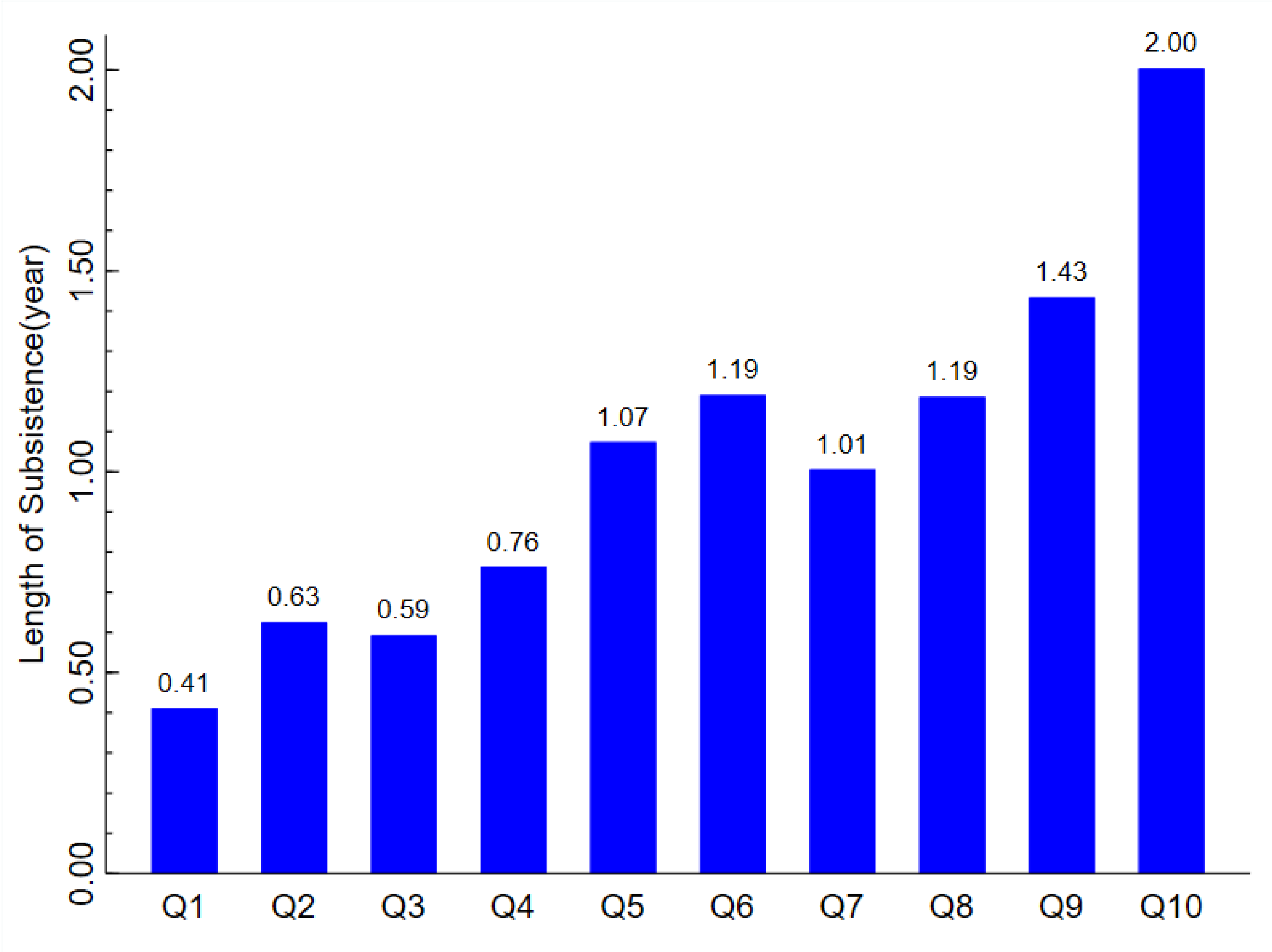
-still-not-over-NY4ba2qOaY/index.html; https://www.wsj.com/video/truck-driver-stuck-on-highway-since-chinas--coronavirus-lockdown/F7097DE9-FCEB-4E13-BCAB-9D715AF84D0B.html; https://www.sohu.com/a/3717-66675_617717.

8.2 Appendix Figures



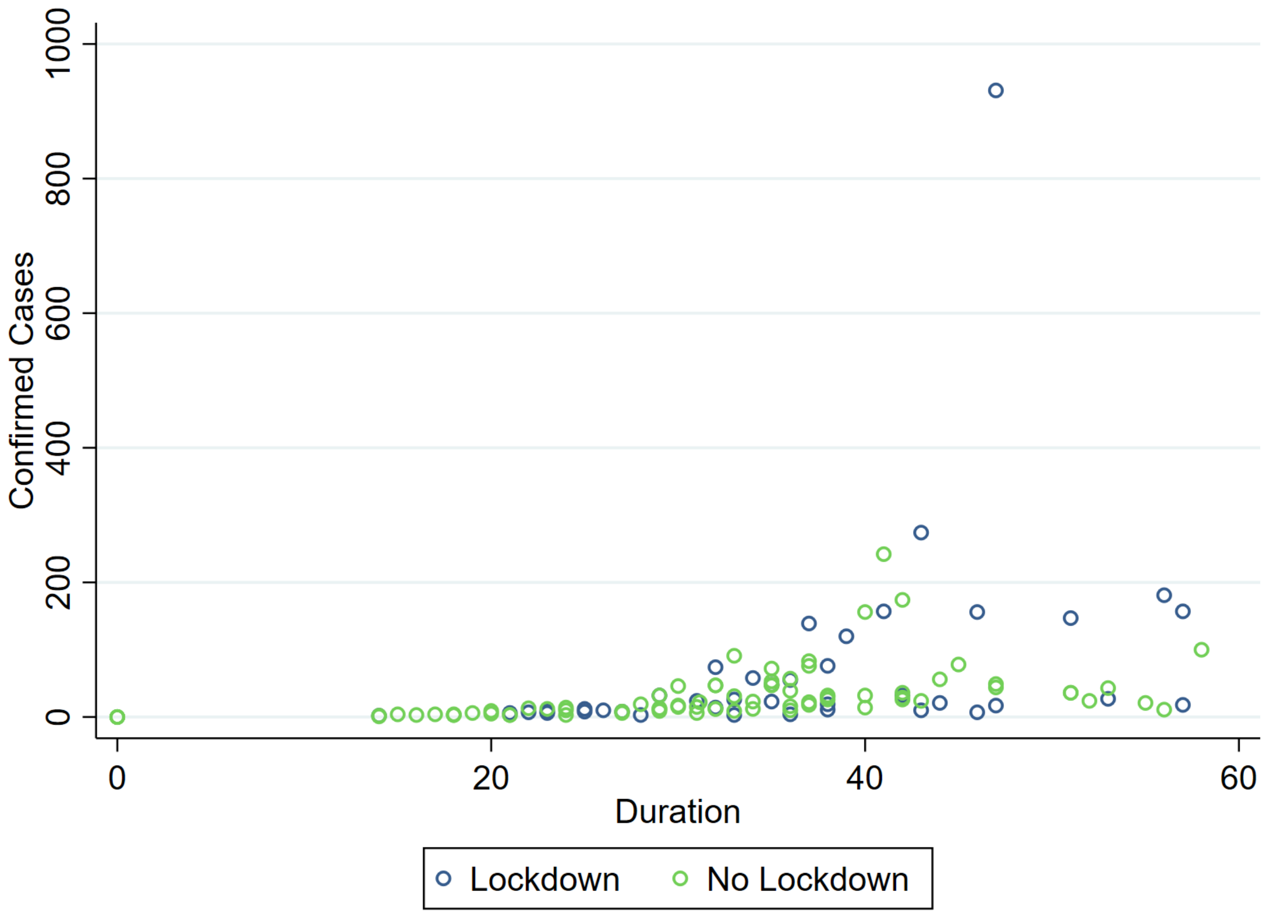
Notes: The figure shows the timeline of provincial emergency reaction level.

Figure A1: Province Emergency Reaction Level Time Line



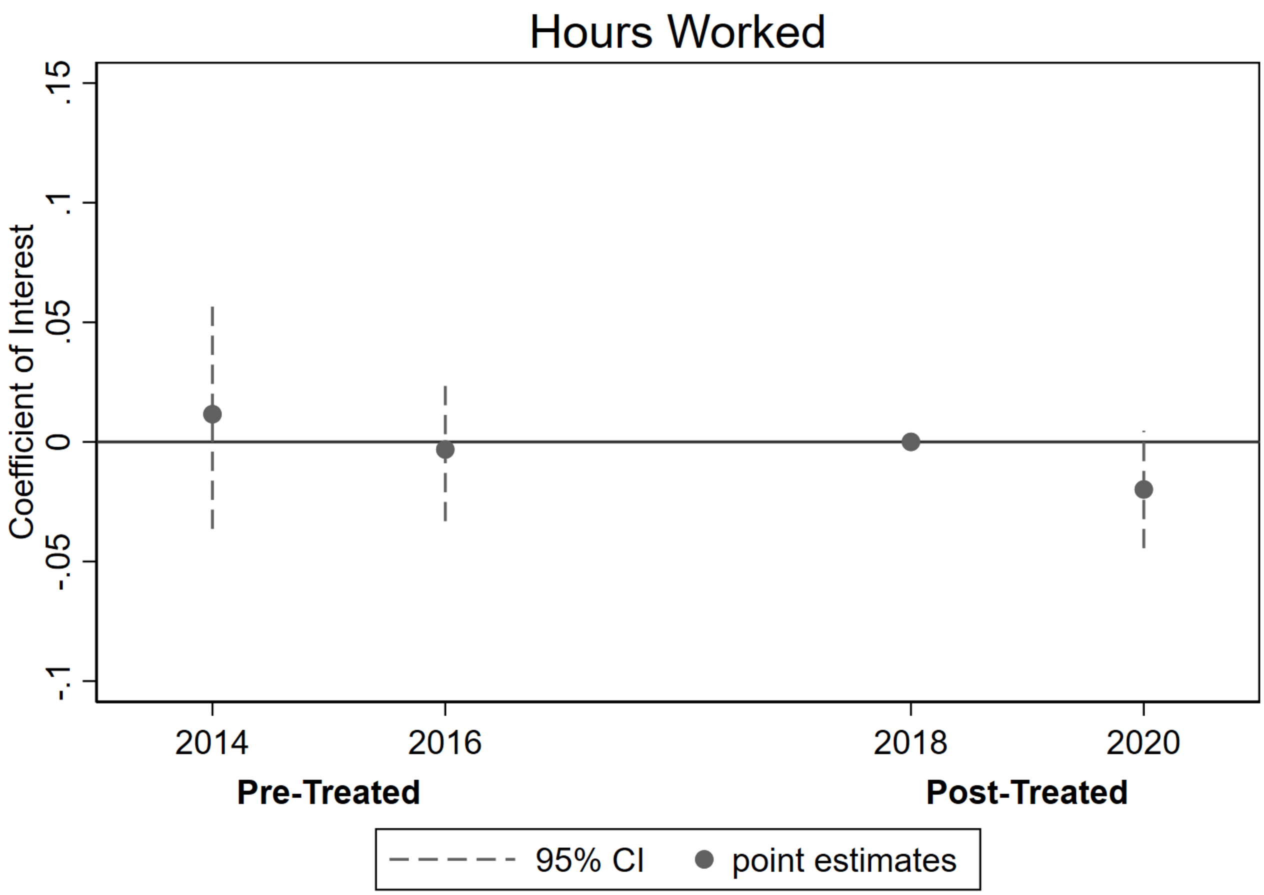
Notes: The figure shows how many years interviewees' cash or deposit could subsist their expenditure if they become unemployed. Y-axis represents length of subsistence = cash or deposit/ family's expenditure. X-axis represents deciles at income distribution.

Figure A2: Subsistence Years After Unemployed



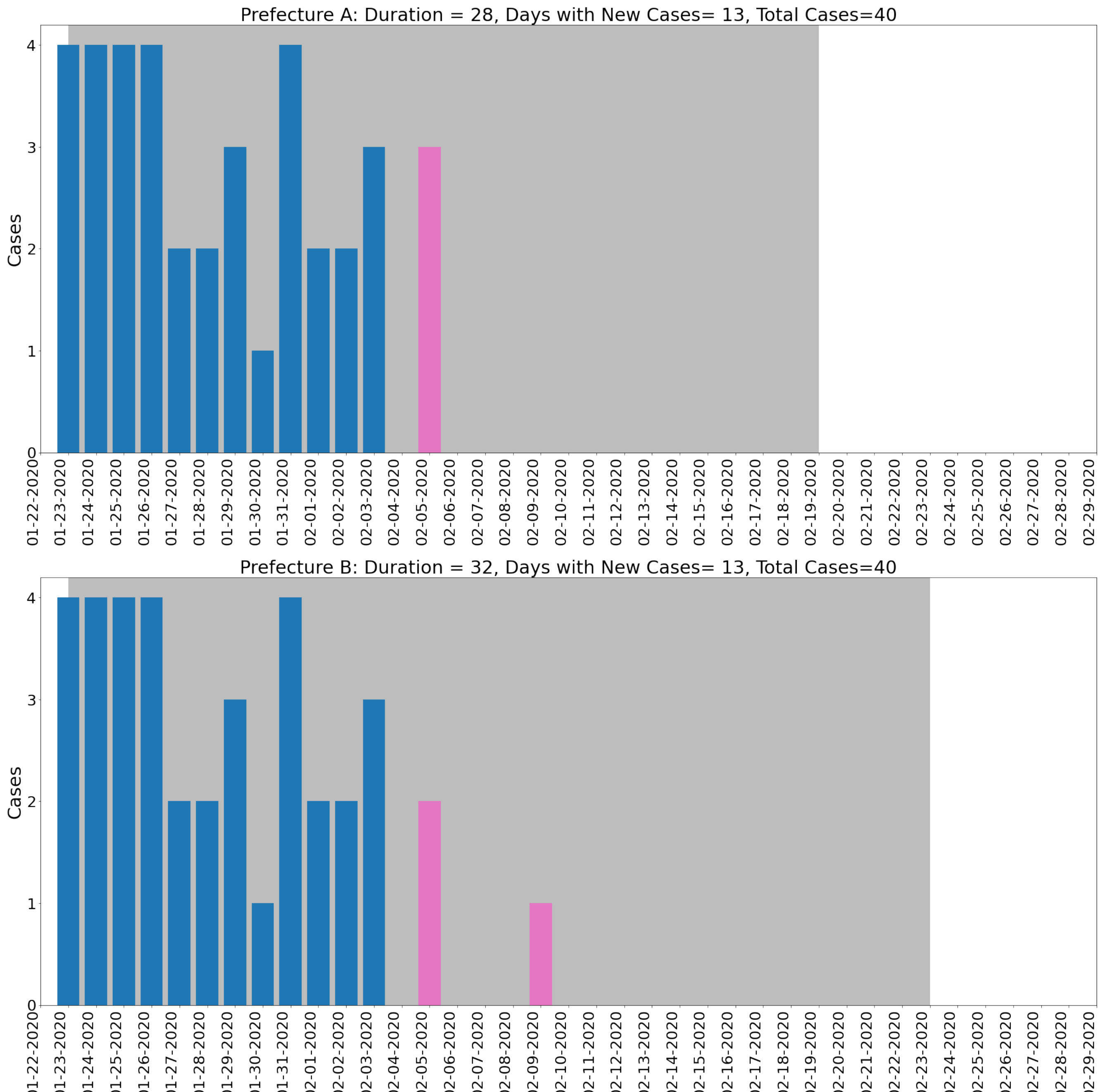
Notes: The figure shows the number of confirmed cases and the zero-Covid duration for prefectures by lockdown status. Duration outliers (95 percentile) are dropped from this graph.

Figure A3: Duration and Confirmed Cases by Lockdown



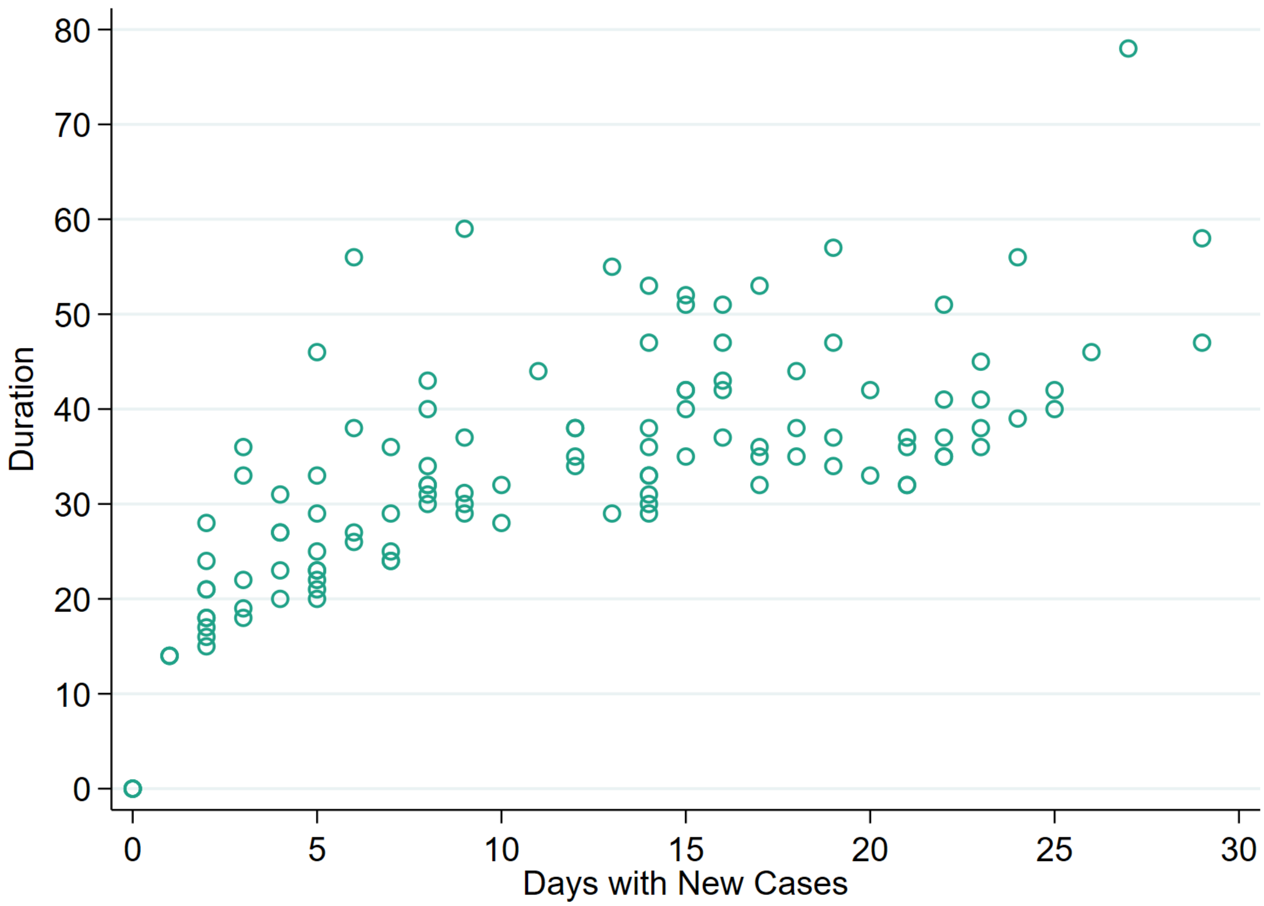
Notes: The figure shows coefficients and 95% confidence intervals from estimating the leads and lags regression in equation (2), where the dependent variable is the natural log of hours worked. All effects are relative to 2018.

Figure A4: Dynamic Effects: Hours Worked



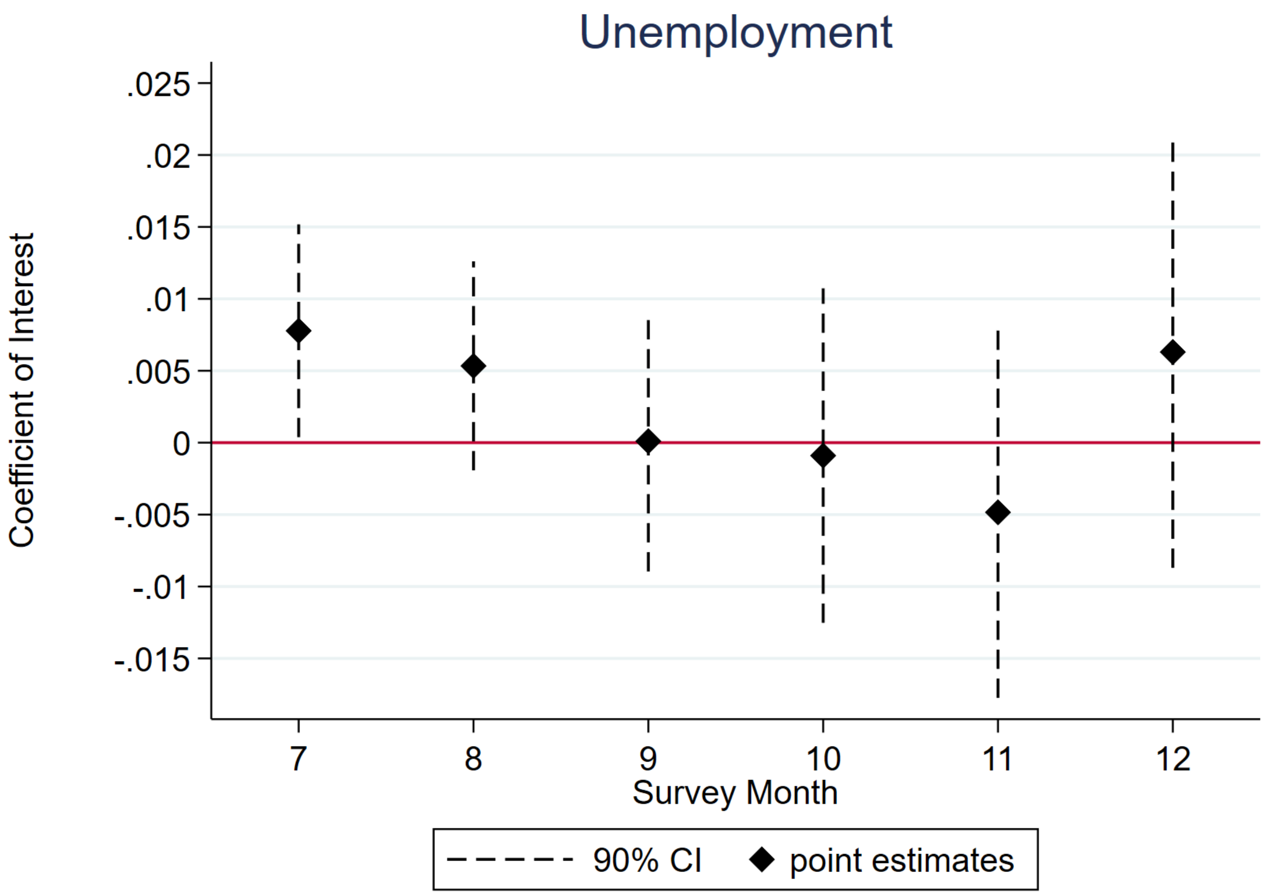
Notes: In this figure we demonstrate the exogeneity of treatment conditional on prefecture characteristics. Blues bars denote the similarity between prefecture A and B. Pink bars denote different timing of case report, which is highly likely driven by random factors. The shadowed area denotes zero-Covid duration.

Figure A5: Conditional-Exogenous Treatment



Notes: The figure shows that conditional on the the number of days with confirmed cases (X-axis), the Duration (Y-axis) is highly likely driven by random factors. Outliers (95 percentile) of Days with New Cases are dropped from graph.

Figure A6: Conditional-Exogenous Treatment 2



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

Figure A7: Treatment Effect by Survey Month

8.3 Appendix Tables

Table A1: Sample by Waves

Year	Prefectures	Obs	Share
2014	125	16246	0.256
2016	125	17453	0.276
2018	123	18379	0.290
2020	121	11265	0.178
Total	125	63343	1.00

Notes: The table reports the distribution of sample sizes across four waves (2014, 2016, 2018 and 2020).

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 U-4 unemployment from CFPS (column 1), China Official U-3 unemployment (column 2), US official U-4 unemployment(column 3) and US official U-3 unemployment(column 4).

Table A3: Treatment Validation

	(1) Out Town Traffic	(2) In Town Traffic	(3) Intra Town Traffic	(4) Out Town Traffic	(5) In Town Traffic	(6) Intra Town Traffic
Panel A: All Prefectures						
Exposed to Risk	-0.266***	-0.246**	-0.296***			
	(0.0927)	(0.0981)	(0.0640)			
lnDuration × Jan23				-0.137***	-0.129***	-0.114***
				(0.0443)	(0.0463)	(0.0264)
R-squared	0.521	0.522	0.803	0.528	0.527	0.804
Observations	27087	27084	26788	27087	27084	26788
Mean of Outcomes	-0.425	-0.425	-0.787	-0.425	-0.425	-0.787
Panel B: CFPS Prefectures						
Exposed to Risk	-0.107**	-0.0410	-0.280***			
	(0.0501)	(0.0518)	(0.0760)			
lnDuration × Jan23				-0.807*	-0.785*	-0.519***
				(0.478)	(0.468)	(0.193)
R-squared	0.541	0.541	0.826	0.566	0.557	0.830
Observations	8782	8732	8732	8782	8732	8732
Mean of Outcomes	-0.547	-0.566	0.465	-0.547	-0.566	-0.800
Prefecture FE	✓	✓	✓	✓	✓	✓
Date FE	✓	✓	✓	✓	✓	✓

Standard errors in parentheses

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

Notes: Unit of observation is at daily-prefecture level. Exposed to Risk is a dummy that equals to 1 if prefecture p is considered as mid- or high- risk on date t, under the 14-day observation rule. LnDuration is the duration (natural log) of the instrumented zero-Covid policy. Jan23 is defined as 1 for the periods post January 23, 2020 , and 0 otherwise. 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 binary variable that equals 1 if the duration of the zero-Covid policy is above median and 0 otherwise. 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 binary variable that equals 1 if the duration of the zero-Covid policy is above median and 0 otherwise. 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	✓	✓	✓	✓

Notes: Unit of observation is an individual. The 2020 data (where treatment is actually applied) is dropped from the sample. Post2018 is an indicator if the observation is from year 2018. Post2016 is an indicator if the observation is from 2016 or 2018. 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: Outliers: 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	✓	✓	✓	✓	✓	✓

Notes: Unit of observation is an individual. The observations from Wuhan (Column 1 and 2), Hubei province (Column 3 and 4) and big cities (Column 5 and 6) such as Beijing, Shanghai, Guangzhou, Chongqing and Tianjin are dropped from the sample, respectively. 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 A8: 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	✓	✓	✓	✓	✓	✓

Notes: Unit of observation is an individual. The sample is restricted to balanced panel. 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 binary variable that equals 1 if the duration of the zero-Covid policy is above median and 0 otherwise. Robust standard errors in parentheses, clustered at the prefecture level. ***, **, and * indicate significance at the 1%, 5% and 10% levels.

Table A9: 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	✓	✓	✓	✓	✓	✓

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 A10: Heterogeneity: Labor Outcomes by Job Class

	Unemployment		Unemployment		log Hours Worked		log Hours Worked	
	(1) Non Agri	(2) Agricultural	(3) Non Agri	(4) Agricultural	(5) Non Agri	(6) Agricultural	(7) Non Agri	(8) Agricultural
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	✓	✓	✓	✓	✓	✓	✓	✓

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 binary variable that equals 1 if the duration of the zero-Covid policy is above median and 0 otherwise. 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.