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

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Abstract

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

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

JEL Codes: I12, J20, J18

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1 Introduction

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

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

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

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

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

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

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

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

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

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

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

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

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

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

2 Background

2.1 First Phase: Stringent Containment

To slow down the spread of COVID-19, China government imposed a series of unprecedented lockdown and quarantine policies since January 23, 2020 [3] [4]. As shown in Figure A1, by January 25, 30 out of 31 provinces of China enacted First level emergency response, measures taken including case isolation, suspension of public transportation and public space closure, etc. ((Qiu et al., 2020) and (Tian et al., 2020)). Under the unprecedented national emergency event, local governments reacted with stringent containment policies. The entire Hubei province locked down in Jan 24, residents were not allowed to leave their prefectures. Strict anti-contagious policies were also implemented in other provinces, including strategies of partial shutdown, banned traffic leaving and 14 days self-quarantine for visitors. According to Qiu et al. (2020), up to 14,000 health check points were set up at service centers on ferry and highway. By February 16, more than 250 prefectures rolled out policies 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

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

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

and quarantine close contacts of suspicious cases" and so on[5]. Under such stringent containment policies, during January to February, economic activities were rigorously suppressed (Fang et al., 2020). In Appendix section 4, we provided two anecdotal stories, which reflects the *Stringent Containment* during January 2020.

An important point is worth noting, that the 14-day observation window was already set as epidemiological criteria to define a suspected case since January 18, 2020 (Li Q, 2021) and were publicly mentioned in a guidance from National Health Commission on Jan 22 [6]. Following with central government, local governments soon updated this 14-day window as part of anti-contagious policies. The 14-day window will be an important instrument we use to construct our major treatment variable.

2.2 Second Phase: Precise Containment

After nearly one month stringent containment policies [7], the central government tried to reboost the economy and partly relax the lock down policy. In Feb 17, The State Council and National Health Commission of China issued *Prevention Guidance for Novel Coronavirus Pneumonia (version 5)*, local government were required to classify different risk levels for different regions. In the low risk areas, which are usually defined as 0 case counties, the travel from middle or high risk areas would be restricted. However, mobility within county and other low risk areas were permitted. Middle risk areas were defined as counties without a *outbreak* [8]. Averagely, High risk area were usually defined as a region reported more than 10 cases within 14 days [9]. Both the mid and the high regions would be imposed stringent public health strategies, including traffic restriction, centralized treatment, community isolation and forced stay at home order [10]. The government developed a Communication big data travel card application to monitor countries

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

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

[7] "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)

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

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

[10] Again, there is no general distinction between the quarantine strategies of the mid and the high. In some cases, residences in high risk regions were strictly required to stay at home, security patrols inspected on any violators and food could only be ordered from delivery

that people visited, or domestic cities that stayed longer than 4 hours within 14 days by using the phone signal. If these countries or cities are an area of medium or high risk, their application will be marked with a star and they cannot be allowed into other cities. Although this state issued Guidance left enough freedom for local government to manipulate the boundary between high and middle risk level, the Middle or High risk area could only change the level to Low risk after 0 case increase in 14 days, this threshold are considered as a clear distinction between low risk level areas and the other two levels.

Local governments followed the guidance from the central government immediately, by the end of February, half provinces in China were no longer in the First level reaction. Although there could be high or middle risk areas (counties) within a Third level province, the rest part of the province already resumed the economy activities, only with travel restrictions to high risk areas. Until April 30, the national daily cases were already smaller than 50. Beijing and its neighbor provinces lowered to the second level. Three days later, Hubei lowered to Second emergency response level and no province were in the First response level anymore.

3 Data

3.1 CFPS Data

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

We collect four waves of CFPS data, surveyed in 2014, 2016, 2018 and 2020, giving us a sample of 139,983 observations. To arrive at the sample used for estimation, we first exclude observations who (i) were surveyed by proxy mode which lacks information on labor outcome (16,696 observations); or (ii) were full-time student (10,617 observations), leaving us with a sample of 112,670 observations. We further restrict attention to individuals who were between age 16 and

64, which reduces the sample to 93,357 observations.

We utilize the CFPS data in 2018 to investigate the heterogeneous effect, thus we drop respondents who were not interviewed in CFPS 2018, i.e., 17,141 observations. We drop 8,654 observations whose county is not included in the county list provided by Peking University's Institute of Social Science Survey in 2010. Finally, we drop 811 observations that migrated to another county and 3,408 observations that appear only once in our sample and end up with an estimated sample of 63,343 observations (20,006 individuals). Among the 63,343 observations in our sample, 25.6 percent were surveyed in 2014, who belong to 124 cities. The observation in 2016, 2018 and 2020 is 27.6%, 29.0% and 17.8% respectively[11].

Our main outcome variable concerns individual unemployment status. There are several questions related to employment status in the CFPS questionnaire. Specifically, interviewees (excluding full-time students) are asked for the following questions: (1) "Including agricultural work, waged job, self-employment and private business (housework and unpaid help do not count), have you worked for at least one hour last week?" (2) "Do you have a job, but you are currently on temporary vacation, sick leave or other vacation, or on-the-job training?" (3) "Will you return to the original job position in a certain period or within six months?" (4) "Are you running your own business which is currently in an off-season, but will resume after a while?" (5) "Is your agricultural work (including cropping, managing orchard, collecting agricultural and forestry products, fish farming, fishing, raising livestock, selling agricultural products in market, etc.) in an off-season?" If all answers of an interviewee are "NO", the interviewee is on unemployment, otherwise, the interviewee is on employment [12].

Moreover, there are a question for employed people rather than self-employed people and business owners, "Including salary, bonus, cash benefit, material benefit, and excluding tax, insurances, and public housing, how much in total did you make from this job for the last 12 months?" An outcome variable called *Yearly Labor Income* is constructed accordingly. Finally, CFPS asked respondents a question, "How many hours per week on average did you work for this job in the past year?" An outcome variable called *Weekly Hours Worked* is constructed accordingly. Panel A of

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

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

Table 1 present summary statistics for labor outcomes in our sample. Average unemployment is 0.173. Among the 31,145 observations employed, average labor income is 20,558 RMB. Among the 41,441 observations employed, respondents work an average of 46.3 hours per week.Different from the traditional measures of intensive margins responses, we choose to keep employed respondents with 0 hours worked and 0 income in our sample, with the purpose of fully utilize the original data set.

To investigate heterogeneous effects of Covid-19, we construct a series of basic demographics variables by using CFPS 2018. Specifically, we consider the following dimensions: (1) Gender: a dummy variable taking value 1 if the interviewee is female and 0 otherwise; (2) Age: age of the interviewee; (3) Middle school or below: a dummy variable taking value 1 if the highest level of education the interviewee has obtained is middle school or below and 0 otherwise; (4) Age of the youngest child: age of the youngest child of the interviewee. Panel B of Table 1 summarizes statistics for characteristic variables of the respondents in 2018.

3.2 Covid-19 Cases Data

The *Duration* of zero-Covid policy implemented in each prefecture is our primary treatment variable. To document the days that a prefecture has a mid- or high- risk region, thus potentially the anti-contagious measures were implemented in the prefecture, we rely on the time-series data of the daily new Covid cases from Jan 23 [13] to June 30[14]. Based on the national guidance for Covid-19 containment, a region will remain in mid- or high- risk level until a consecutive 14-day without new confirmed Covid case, then the risk level will turn down to low. We locate each city's mid- or high- risk period by removing the days after a 14 zero-case window, i.e., low-risk period. Panel C of Table 1 summarizes the statistics for zero-Covid policy and Covid-19 at prefecture level. Average duration of policy duration is 37.128 days. Average number of confirmed cases and death is 451.697 and 31.432, respectively. 34.9 percent of these prefectures implemented lockdown policy. Finally, for regression estimation, we use the log of the policy duration as the major treatment

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

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

variable.

One potential concern about our construction of the treatment variable is that the intensity and the coverage of the anti-contagious measures during the early stages of the virus outbreak were more stringent compared to the later period when precise containment policies were recommended by the central government. To cope with this issue, we further construct two duration variables corresponding to different time periods: one for the period between Jan 23 and Feb 17; another for the period after Feb 17 till the starting date of survey collection (June 30) [15]. In this way, we are able to capture the effects of the anti-contagious policies on labor market outcomes in different phases of the pandemic.

Different from He et al. (2020) and Zhang (2021), our measurement of anti-contagious policy, instead of a dummy variable for prefecture lockdown[16], aims to document the existence of any mobility restriction measures, while prefecture-level lockdown is only one of the most restricted measures. For prefectures that experienced relatively small numbers of confirmed Covid cases and did not implement any recorded lockdown policy, it was unlikely that they made no reactions to the pandemic. Even measures were less intense, but efficient enough to mitigate the spread of the virus. In Figure A2, we plot the confirmed Covid-19 cases versus zero-Covid policy duration for each prefecture, while categorized by whether experienced lockdown or not. We could observe that prefectures with similar Covid cases and zero-Covid duration could vary in their lockdown decisions, which implies a dummy variable for lockdown could not fully capture the general anti-contagious measures that a prefecture implemented.

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 China City Statistical Yearbook. These variables include (1)Population: population of a prefecture in 2018; (2) GDP: gross domestic product (GDP) of a city in 2018; (3)

[15] 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

[16] He et al. (2020) and Zhang (2021) defined a city (prefecture) as locked down “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.”

Added value of the tertiary industry/GDP: the share of Added value of the tertiary industry in GDP; (4) Highway freight volume, Highway passenger traffic volume, Water freight volume, and Waterway passenger traffic volume of a city in 2018. Panel D of Table 1 summarizes statistics for city characteristics in 2018. Average population is 5.586 million in 2018 and average GDP in 2018 is 396.489 billion Yuan.

4 Identification

4.1 Baseline Model

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

$$Y_{ipt} = \beta \times lnDuration_{p,t} + (Year_t \times X_p)\lambda_t + \theta_i + \delta_{r,t} + \epsilon_{ipt} \tag{1}$$

where Y_{ipt} represents the outcomes of interests (e.g. unemployment and hours of worked) of individual i, in prefecture p surveyed in year t. $lnDuration_{p,t}$ is a continuous variable if year is 2020 and assumes the value of 0 for other years. A similar continuous treatment is also used by Gupta et al. (2020). The $lnDuration_p$ is constructed by the method mentioned in Section 3.2, which measures the duration of the zero-Covid policy in prefecture p in log form. The parameter of interest β captures the marginal effects of exposure to zero-Covid policies on the labor outcomes. In order to identify causality, our model needs to rely on a stronger parallel trends assumption comparing to the conventional DD specification, where treatment is a dummy variable (Callaway et al., 2021). We therefore generate a dummy variable that assigns one if the $lnDuration_p$ is larger than the 50th percentile cut off, as an alternative treatment. We will discuss more about the continuous treatment setting and potential challenges in Section 4.5 and Appendix 1. Putting aside this issue for now, our continuous treatment could estimate the marginal effect, with policy implication in real world and provide more comparative discussion with evidence from other countries (Gupta et al. (2020), Aum et al. (2021a) and Aum et al. (2021b)).

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, $\delta_{r,t}$. X_p is a set of proxies for prefecture economic status, (i.e. population, GDP and share of service industry in 2018), we include the $Year_t \times X_p$ to let their effects differ across year [17].

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

We follow closely from the binary treatment case, employing a dynamic model to examine the parallel trends assumption. In our continuous treatment case, our assumption implies that after adjusting for controls and fixed effect, any treated units, with any treatment level (dose), would follow a common path of labor market outcomes that untreated units actually experienced (Callaway et al., 2021). Although this assumption is not guaranteed for an unbiased result, we will discuss more in the Section 4.5 that why the unbiased problem will not cause a serious issue to our identification. Furthermore, we also check robustness of the results with binary treatment.

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

In this model, $lnDuration_{p,t}$ is a continuous variable if year is 2014, 2016 or 2020 and assumes the value of 0 for year of 2018. 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 outcomes up to six years prior to actual treatment. The parallel trends test assume that we should not observe relative impact from the "placebo" treatments on the pre-treated outcomes.

[17] one possible solution is to control time variant characteristics, which mean to include post-treatment variables into regression. However, It will result in"Bad Control" problem.

4.3 Disentangle Effect

By June 30 2020, the total confirmed number in China was 83,534, around 50,000 cases were detected in Wuhan and another 18,000 cases were detected in Hubei province. Given the large population base, the health effect were negligible in most parts of China. 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. To disentangle the policy effect from the public health shock, we exploit the variation between zero-Covid policy duration and Covid severity measures: confirmed cases and death cases. In equation (3), $lnCases_p$ is the prefecture level total confirmed cases in log form. $lnDeaths_p$ is the prefecture level confirmed death cases in log form. Both variables are counted between Jan 23 to June 30, 2020. ω_1 and ω_2 capture the health effect and leave β as the isolated policy effect. The interpretations for other parameters are similar to previous models.

$$\begin{aligned} Y_{ipt} = \beta \times lnDuration_{p,t} + \omega_1 \times lnCases_{p,t} + \omega_2 \times lnDeaths_{p,t} \\ + (Year_t \times X_p)\lambda_t + \theta_i + \delta_{r,t} + \epsilon_{ipt} \end{aligned} \tag{3}$$

4.4 Spillover Effect

Our estimation also relies on the assumption that the prefectures in our sample were not affected by the anti-contagious policies of neighboring prefectures. Potentially, the labor market not only be affected by local anti-contagious policy, but also be influenced by spillover effects from nearby regions — the inter-region traffic and labor force mobility could be strictly controlled. If the zero-COVID spillover effect disproportionately drove up the unemployment probability between sample prefectures, our estimation could be biased. For example, larger spillover effect for longer duration prefecture and smaller spillover effect for shorter duration prefecture, the coefficient of policy effect will be overestimated. Alternatively, if the shorter duration prefectures experienced severe spillover from neighbors and longer duration prefectures experienced negligible spillover effects, the true policy impacts will be underestimated. In this section, we empirically assess the

Stable Unit Treatment Values Assumption (SUTVA) by controlling the zero-Covid policy in nearby prefecture. If we observe a negative correlation between local labor outcomes and zero-COVID policy duration of nearby prefectures, the estimates of local policy effect suppose to be overstated in magnitude.

To measure the duration of zero-Covid policy in nearby prefectures, we firstly collect the zero-Covid policy duration for all neighboring prefectures to the 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 controlling for spillover effects as following:

$$\begin{aligned} Y_{ipt} &= \beta \times lnDuration_{p,t} + \theta \times lnDuration\_Nearby_{p,t} \\ &+ (Year_t \times X_p)\lambda_t + \theta_i + \delta_{r,t} + \epsilon_{ipt} \end{aligned} \tag{4}$$

4.5 Threats to Parallel Trends Assumption

Our continuous treatment DD specification replies on a stronger parallel trends assumption than binary treatment DD estimation (Callaway et al., 2021). The parallel trends assumption we examine in the dynamic model shows that any treated units with any treatment intensity (dose) would have same outcome as the untreated group if they had not participated. However, this only ensures parallel trends between treated and untreated group, but cannot ensure the common trends within treated units with different treatment intensity (dose). In our case, the dynamic effect model only examines parallel trends between prefectures with *lnDuration* of 0 and *lnDuration* of a positive number. However, when we make comparisons between outcome variables across different values of *lnDuration*, it involves a causal effect and a selection bias term comes from the potential heterogeneous treatment effect (Callaway (2021) and Cunningham (2021)). For example, if prefectures with *lnDuration* of 6 days and 5 days would have different gains at a 5 days treatment [18], then the marginal treatment effect is biased. To eliminate such bias, we need a stronger parallel trends assumption indicates that for all possible levels of *Duration*, the expected change in outcomes over time across all prefectures if they had been experienced that level of *lnDuration* is the same as the expected change in outcomes for all prefectures that experienced that level of treatment.

Yet, there is not compelling method to assess this stronger parallel assumption, we try to see if there were nonlinearities in treatment effects as *lnDuration* increase. We will discuss this method in detail in Appendix 1. Alternatively, for robustness purpose, we use 50 percentile as cutoff and transfer the continues treatment into a binary treatment. In this case, the treatment effect would be interpreted as effect of higher *lnDuration* versus lower *lnDuration*. Specially, if the dynamic results shows a pre-treated common trends, we can conclude a casual effect from the COVID-zero policy.

There is another point worth noting. The constructed semi-exogenous treatment could alleviate the "select into treatment dose" concern. Given the number of days without 0 increase (absorbed by fixed effect), it is not easy for local governments to manipulate how many days with 0 cases increase in a 14 days window, as the time point of detecting an increase case is quit random. Shown in A3, given a number of days without 0 increase (X-axis), we can observe some variations of *Duration*, which could be driven by the random factors instead of prefecture characteristics. Since the selection of prefectures into doses (different levels of *lnDuration*) was highly likely unrelated with potential outcomes, the marginal treatment effect will be highly likely same for different levels of *lnDuration* and therefore eliminates selection bias (Cunningham, 2021).

Another challenge to our identification strategy is patterns of labor market outcomes change in anticipation of anti-contagious policy shock. This concern arises in Gupta et al. (2020) when the low-frequency dynamic effect model could not capture the high-frequency changing outcomes. However, as the origin country of COVID [19], the time interval between outbreak and the roll out of unprecedented policies is too narrow for labor market to anticipate, which address the concern

[18]which means, the counterfactual outcome for units of 6 days with 5 day-treatment, would be different with the outcome for units of 5 days with 5 day-treatment

[19]Chinese government did not admit that the coronavirus can be transmitted between humans until Jan 20, 2020. three days later, Wuhan locked down.

of pre-anticipation bias.

4.6 Placebo Test

5 Results

5.1 Baseline Result

In Table 2 panel A, we report our baseline estimation results. We estimate the two-way fixed effects regression of unemployment status on the DID policy duration treatment, controlling individual fixed effects and year fixed effects or other city-related factors. The DID estimation is the coefficient on the interaction of log form of the zero-Covid policy duration with a dummy variable for year 2020. 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 containment policy increases the individual unemployment probability by 0.08 percentage points, which is statistically significant at the 5% level. Instead of year fixed effect, we further control for the province by year fixed effect in column (2), the interaction term of prefecture characteristics[20] and dummy variable for year in column (3) and (4). Our estimation of the casual effect remains statistically significant in all these specifications. In Table 2 panel B, we report the estimated effect of policy duration on log hours worked. We find that the policy duration has a significant negative effect on the hours worked, as a 10% increase in policy duration would decrease the hours worked by around 0.2%, depending on the regression specification.

As mentioned in the section 4.4, our identification strategy has a concern that due to the construction of our treatment variable, the estimated coefficient of a continuous dosage is the average effect of different policy duration, while there might exist systematic heterogeneous effect for different lengths of the zero-Covid policy. For example, a containment policy might cause significant disruption in labor market after long term implementation while a short lockdown might have much less severe impact that is nonlinear to the decrease in policy duration. To

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

cope with this potential discrepancy of the continuous DID treatment, we also estimate a binary treatment DID specification: First categorize the cities 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 and the dummy variable for year 2020, using all specifications considered in the baseline model.

The result for this binary DID estimation is reported in column (5), and 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 is statistically insignificant using the binary DID treatment variable, while the magnitudes of the coefficients are even smaller compared to the continuous treatment variable setting. One plausible explanation is that policy effect on hours worked estimated in the continuous setting is majorly driven by the observations that are at the tails of the overall distribution, while a large mass of sample is distributed around the median of the policy duration which causes the average sample in the low and high dummy treatment group do not have a significant difference in the hours worked.

In Table 3, we report the estimated effect of policy duration on log labor income. The results indicate that a 10% longer policy duration could result in the income decrease by around 2% when we control the individual fixed effect and province or city-related factors interacted with year dummy variables, and the coefficient is statistically significant at 5% level. 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 to 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.

As our result estimates the impact of the policy duration on the unemployment status, we are able to compare the negative effect of policies with different restrictions. We construct the duration of zero-Covid policy in the counterfactual scenario where the required zero-Covid window reduces from 14 days to 5 days. Then we predict the policy effect on the labor market using the constructed data. We find that, compared to an increase of 0.0371 in the unemployment probability caused by the current policy, the less restricted 5-day zero-Covid policy would only increase the unemployment probability by 0.0324 in the average city, which is about a 12% decrease in the marginal policy effect.

Our finding implies that the negative impact of a very stringent pandemic containment policy on the labor market is more acceptable comparing to the US labor market where the unemployment rate rises into double digits after implementing lockdown in March and April of 2020. The back of envelope calculation implies that the isolated impact of Covid containment policy on the labor market should be less than the observed outcomes in US, implying the existence of a significant public health shock on the labor market, which is consistent with the existing literature (Gupta et al. (2020); Lozano Rojas et al. (2020); Aum et al. (2021a); Forsythe et al. (2020)).

5.2 Dynamic Effects

The underlying assumption for the DID estimator is that cities with different policy duration would have parallel trends in the employment situation before the policy is implemented. The observed increase in unemployment probability could be driven only by the pandemic containment measures, but not the unobserved city characteristics that are associated with the pandemic outbreak. We provide the test for pre-trends that might violate parallel trend assumptions of the 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 could observe that before the pandemic shock in 2020, cities that are associated with a longer policy duration display no trend in unemployment situation. The estimated coefficients for year 2014 and 2016 are not statistically different from zero and year 2018 is the base year. Only the coefficient for year 2020 is positive and significant, which implies the parallel trend assumption is not violated in our model. In Figure 2, we consider the dynamic effect for the binary treatment variable, which gives us similar results as in the continuous setting. We report the dynamic effect estimation for the hours worked in Figure A4, which also provides us a consistent pattern for parallel trends.

5.3 Alternative Labor Market Outcomes

Previous results show the impact of zero-Covid policy on the employment status, while the CFPS data allows us to investigate more labor market outcomes, including the working hours and labor earning. In Table 3, we report the estimated effect of policy duration on log hours worked and log income in panel A and panel B, respectively. In columns (1) - (4), we replicate the different specifications used in the baseline model, and in column (5), we change the continuous treatment variable to the dummy treatment variable. It is noteworthy that we restrict our sample for regressions in this section on the individuals who reported positive hours worked and income in year 2020, which implies our estimation results could only apply to the employed population.

In panel A columns (1) - (4), we find that the policy duration has a significant negative effect on the working hour, as a 10% increase in policy duration would decrease the working hours for employed individuals by around 0.2%, depending on the regression specification. In panel B columns (1) - (4), the results indicate that a 10% longer policy duration could result in the major income decrease by around 2% when we control the individual fixed effect and province or city-related factors interacted with year dummy variables, and the coefficient is statistically significant at 5% level. 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 to the regional factors.

In column (5), the estimated effect on working hours and labor income are both statistically insignificant using the dummy DID treatment variable, while the magnitudes of the coefficients are even smaller compared to those in the continuous treatment variable settings. The results imply that policy effect estimated in the continuous setting is majorly driven by the observations that are at the tails of the overall distribution, while a large mass of sample is distributed around the median of the policy duration which causes the average sample in the low and high dummy treatment group do not have a significant difference in the working hours and labor income.

The insignificant effect could imply that the working schedule has a rigidity in response to the pandemic shock such that the employed workers' working hours do not change much in the short term. Our result implies that the working hour change is relatively small within cities that are comparable in population, GDP and share of service sector, while large working hour changes are observed in a cross-group comparison. Specifically, working hours in the service sector are more flexible and also the service vector could experience larger labor market disruption facing the pandemic shock compared to other sectors. Controlling cities with similar service sector share, the policy effect is not statistically significant because of the high with-in group variation in the working hour, while in the pooling estimation the policy effect is majorly driven by the cross-group comparison difference.

5.4 Robustness Checks

In Table A4, we keep the individuals who stay in the survey from 2014 to 2020 and estimate the policy effect with the remaining balanced panel data. The result has a relatively larger coefficient compared to the baseline model and is statistically significant, which implies that the individuals dropping out of the sample do not have a systematically relation with the unemployment change.

In Table A6, we keep the baseline regression specification and compute standard errors using the cluster robust variance matrix to adjust for heteroskedasticity and potential correlation within the county or the province. We calculate the standard errors clustered for county and year, province, and both province and year, respectively, and the coefficient remains statistically significant at 10% level.

In Table A2 and Table A3, we consider the potential labor outcome difference for workers in the agricultural sector versus non-agricultural workers. In Table A7 and Table A8, we drop the observations in Wuhan or in Hubei province, where the Covid-19 outbreak started. Our major results remain consistent and robust in these settings.

5.5 Competing Hypothesis

In this section, we test several competing hypothesizes. First, as we mentioned in section 4.3, our estimation presents the isolated effect of the anti-contagious policy on the labor market outcomes, without the influence of the public health shock. Our reasoning is that the pandemic was put under control very quickly after implementing the stringent disease preventive measures, thus there were few prefectures that experienced a considerable outbreak. As the number of confirmed cases is negligible compared to the prefecture population, the infection probability is close to zero and the consumers should have no behavioral change during the period.

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

We also want to ensure that the policy effect is generated from the duration of the zero-Covid policy, rather than the extreme anti-contagious measures implemented in a short period, such as the city level lockdown. In Table 5, 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 column (4) and (8), we only estimate the effect of the DID treatment for lockdown on the labor outcomes. In both settings, the lockdown coefficients are statistically insignificant, implying that whether a city implemented lockdown could not explain the negative pattern observed in the labor market.

5.6 Separate Phase: Stringent containment and Precise containment

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

To examine the policy effect during different time periods, we estimate the coefficients of the DID treatment for policy duration before Feb 17, the policy duration after Feb 17, and both of them, respectively. The results are reported in Table 6. Column (2) show that the policy duration before Feb 17 is significantly related to the labor outcome change, while column (3) show that the policy duration after Feb 17 is not. Column (4) displays a similar result that the correlation between the labor outcomes and policy duration after Feb 17 is not statistically significant, which implies that the magnitude of the policy effect after Feb 17 is less obvious compared to the early phase. The precise containment indeed limited the negative impact of the zero-Covid policy to an acceptable level.

5.7 Spillover Effects

In Table 9, we estimate the effect of both local policy duration and nearby policy duration on labor market outcomes. In column (1), we report the estimation of policy impact controlling for spillover effect on individual unemployment probability. The estimated local policy effect remains positive and statistically significant, while the spillover effect has an negative coefficient which is not significant. The coefficient for the local policy duration is also close to the estimates of policy effect in our baseline specification as shown in Table 2. These results imply that the spillovers are unlikely to be present as the nearby policy duration did not contribute to the increase, if not a decrease, in the individual unemployment probability.

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

5.8 Heterogeneous Effects

We estimate the heterogeneous impacts of policy duration on different sub-populations and the estimation results are shown in Table 7. 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 DID policy duration treatment and each of the sub-population dummy variables. We find that for groups such as female workers, employed workers above age 65, workers with education level less than middle school, the bottom 50 percent population in the income distribution, and parents whose children are less than 6 years old, they are more vulnerable to the zero-Covid policy impact on the labor outcomes.

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

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

However, this heterogeneity does not exist for the dimension of traffic volume, as shown in column (1) – (4). It is worth noting that the coefficients are not statistically significant for both high and low traffic volume groups. The potential reason for this significance reversal is that the traffic volume is highly correlated to the policy duration as a city in the central position of the transportation network has larger chance to find a positive Covid case than a city less connected to the rest of the region (as shown in Figure A6). When the positive correlation between traffic volume and policy duration is diluted in each of two separated groups, the policy effect is also diminished within each group.

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

5.9 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 A5, we report the coefficient and the standard error of the policy effect on unemployment estimated from the subsamples collected in each month from July to December. We could observe that the policy effect becomes insignificant as time goes, without clear trend of increasing or decreasing. Although this result is partially due to the sample size is smaller in the later month groups, it also implies that the impact of the zero-Covid policy on the unemployment has no significant lag effect which is not captured by our major estimation. The survey data we use for our estimation result are still valid in analyzing the policy effect on labor outcomes.

5.10 Placebo Test

6 Conclusions

During the Covid-19 pandemic, countries across the world took enormously different policies in mitigating the unprecedented public health crisis. While China was the first to implement harsh anti-contagious interventions nationwide, the effect of policy intervention on China's economy remains obscure until very recent. Using a DID design, we find that when a city's zero-Covid policy lasts for 10%(3.7 days) longer, the individual unemployment probability will increase by around 0.1, and employed workers will suffer a decrease in hours worked and income by 0.2% and 2%, respectively. Our estimation disentangles zero-Covid policy and the public health shock, where the latter has no significant impact on labor market outcomes. We also control for the spillover effect from the nearby cities, which does not contribute to the negative labor market impact. While the lockdown policy is widely investigated as the major non-pharmacological interventions in the recent literature, our paper examines the effect of the general zero-Covid policy, which includes not only the lockdowns but other potential anti-contagious interventions that are hard to observe due to the data limitation. Our result also implies that only the stringent anti-contagious policy implemented during the early stage of the pandemic caused a significant negative impact on the labor outcomes, while little evidence could support that the more precise containment policy implemented in the later phase contributes to the labor market disruptions.

The Covid-19 has caused millions of death and a global humanitarian crisis as many countries were unable to control the spread of the virus after the outbreak of the pandemic. Partially contributing to this catastrophic outcome is the fact that policymakers concerned about the potential economics loss from the human mobility restriction and were reluctant to adopt serious disease preventive measures immediately after the outbreak of the virus, and eventually resorted to herd immunity. Our work provides a systematic review on the labor market disruption caused by the most stringent Covid containment policy and helps measure the economics cost of the non-pharmacological interventions to shut down 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 harder to put under control and the zero-Covid policy has to last longer.[21] The economic cost of the anti-contagious policy would not grow linearly as the length of the policy increases, but exponentially. However, our work could still serve as the benchmark under such scenario, the scale of the pandemic was constrained soon after its outbreak and millions of lives were saved, how much it would cost economically? After all, we hope our work could be a helpful reference for future policymakers facing similar situations, where the decision over the tradeoff between public health and economic well-being is in their hands.

[21] This is indeed what happened to many China's cities after the emergence of Omicron in China. More stringent measures and city lockdowns are implemented since March 2022, as we are finishing this version of the paper.

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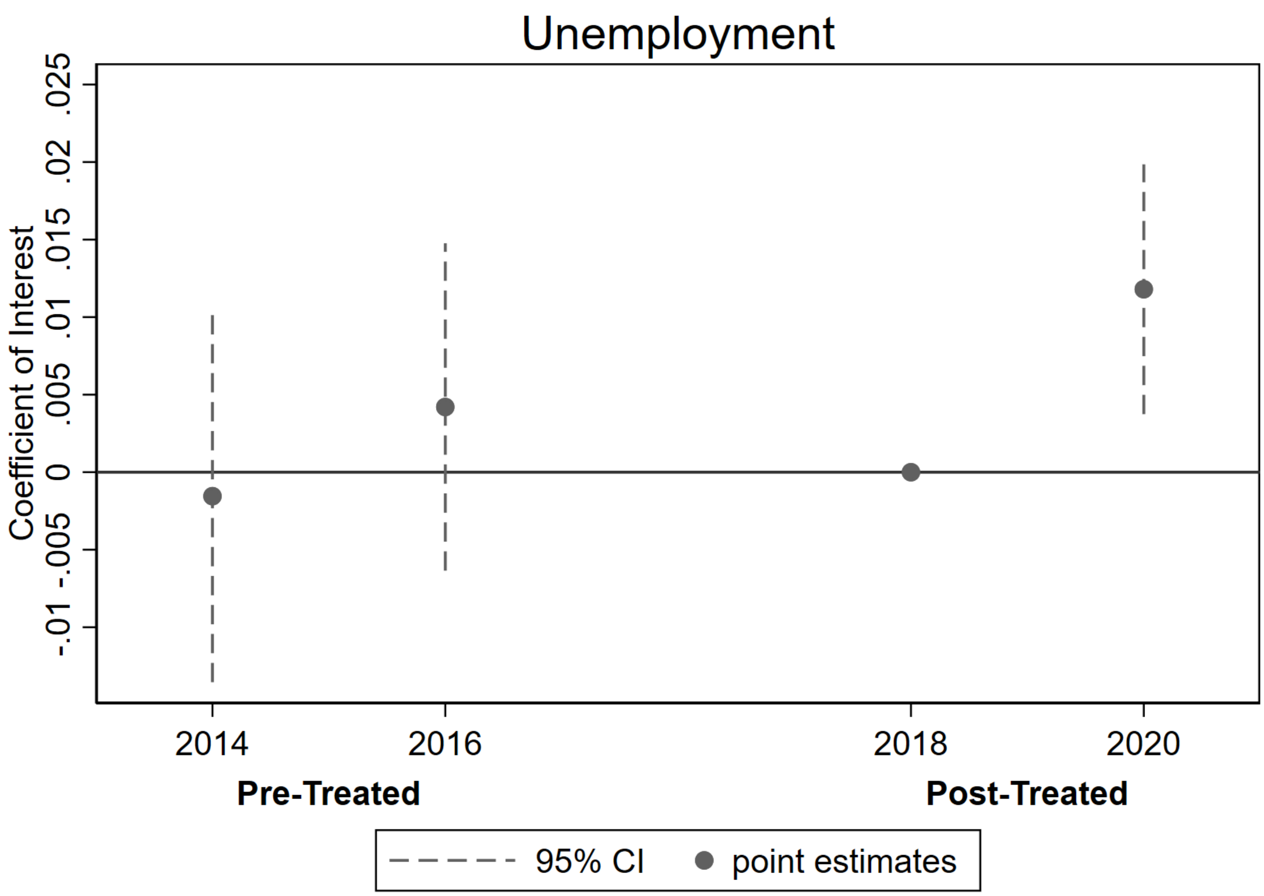
7 Figures and Tables

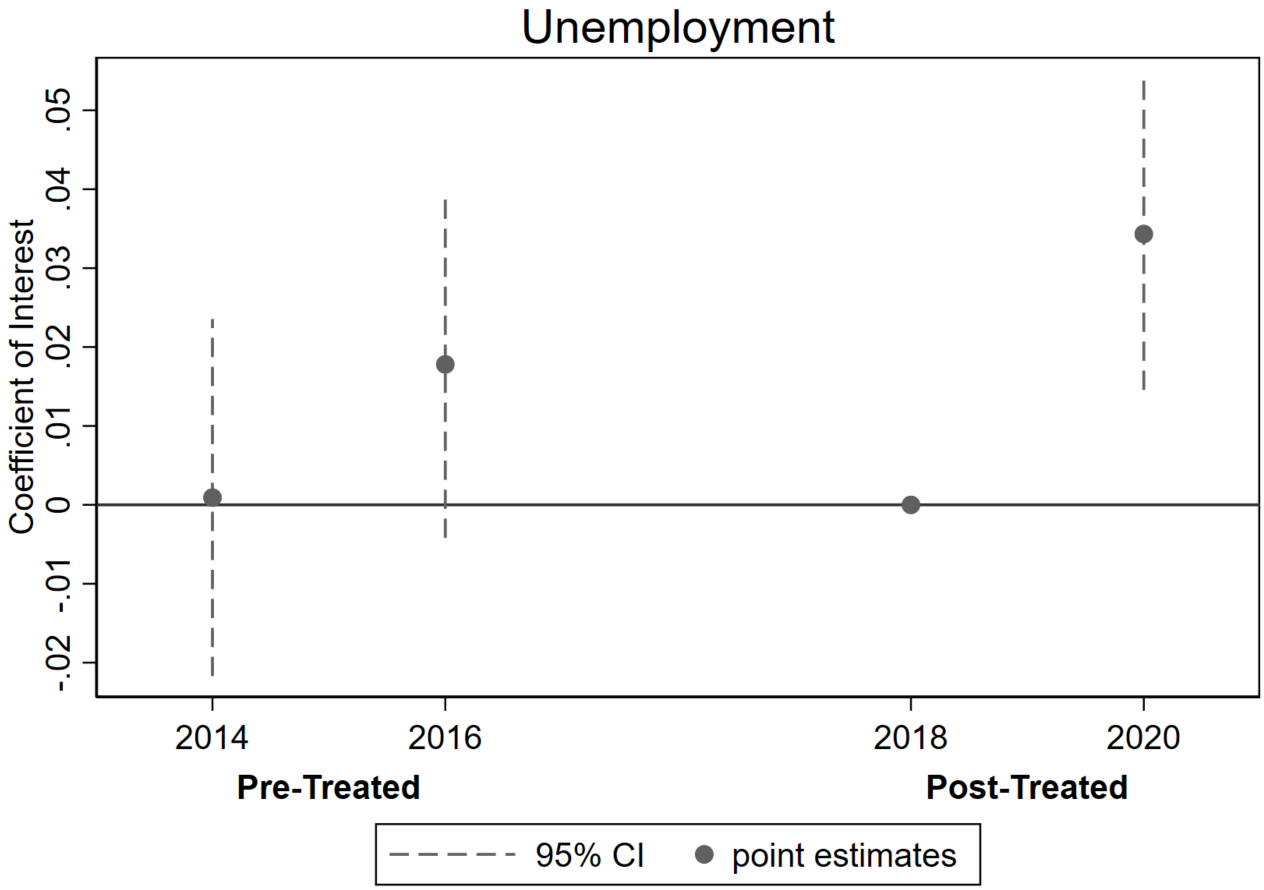
7.1 Figures

Figure 1: Dynamic Effects Unemployment (continuous treatment)



Note:

Figure 2: Dynamic Effects Unemployment (dummy treatment)



Note:

7.2 Tables

Table 1: Statistic Summary

	Obs	Mean	Std.Dev	Min	Max
Panel A: Individual D.V.					
Unemployed (Non Agr)	40232	0.227	0.419	0	1
Unemployed (Full Sample)	63343	0.173	0.378	0	1
Hours Worked	41441	46.294	21.974	0	133
Income	31145	20558.582	24960.902	0	100000
COVID Unemployed	1866	0.026	0.160	0	1
Panel B: Individual Characteristics					
Gender	63343	0.517	0.500	0	1
Age	63343	45.808	11.870	11	69
Education (middle school or less)	63343	0.732	0.443	0	1
Agricultural Worker	63343	0.365	0.481	0	1
Private Sector Worker	63343	0.874	0.332	0	1
Youngest Child Age	57690	19.120	11.394	0	47
Panel C: Prefecture Treatments					
ZC duration	126	37.128	21.411	0	158
ZC duration Feb Jun	126	18.349	19.158	0	135
ZC duration Jan Feb	126	18.779	5.095	0	24
Covid duration	126	13.921	13.054	0	102
Confirmed Cases	126	451.691	4481.225	0	50340
Confirmed Deaths	126	31.432	344.629	0	3869
Lockdown (General)	126	0.349	0.479	0	1
Lockdown (City Level)	126	0.349	0.479	0	1
Lockdown (Community Level)	126	0.183	0.388	0	1
Panel D: Prefecture Controls					
Population 2018 (Thousand)	126	5586.448	4662.472	430	34040
GDP 2018 (Billion)	126	396.489	557.217	0	3268
Share of Service Sector in GDP	126	48.090	8.518	31	81

Table 2: Labor Market Outcomes: Unemployment and Hours Worked

	(1) Outcomes	(2) Outcomes	(3) Outcomes	(4) Outcomes	(5) Outcomes
Panel A: Unemployment					
Duration × Post	0.00831**	0.0109**	0.0125***	0.0109**	
	(0.00398)	(0.00442)	(0.00449)	(0.00446)	
Duration_Dummy × Post					0.0284***
					(0.0100)
R-squared	0.469	0.470	0.470	0.470	0.470
Observations	63343	63343	63343	63343	63343
Mean of Hours Worked	0.173	0.173	0.173	0.173	0.173
Panel B: log Hours Worked					
Duration × Post	-0.0165*	-0.0183**	-0.0215***	-0.0239***	
	(0.00844)	(0.00746)	(0.00776)	(0.00772)	
Duration_Dummy × Post					-0.00793
					(0.0269)
R-squared	0.312	0.315	0.315	0.315	0.315
Observations	37914	37914	37914	37914	37914
Mean of Outcome	46.29	46.29	46.29	46.29	46.29
Individual FE	✓	✓	✓	✓	✓
Year FE	✓	✗	✗	✗	✗
Province-Year FE	✗	✓	✓	✓	✓
lnPop × i.Year	✗	✗	✓	✓	✓
lnGDP × i.Year	✗	✗	✗	✓	✓
SverviceShare × i.Year	✗	✗	✗	✓	✓

Standard errors in parentheses

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

Table 3: Income

	(1)	(2)	(3)	(4)	(5)
	log Income	log Income	log Income	log Income	log Income
Duration × Post	-0.105	-0.175***	-0.214***	-0.195***	
	(0.0635)	(0.0649)	(0.0645)	(0.0680)	
Duration_Dummy × Post					0.0511
					(0.181)
R-squared	0.441	0.444	0.444	0.444	0.444
Observations	27679	27679	27679	27679	27679
Mean of Income	21214.8	21214.8	21214.8	21214.8	21214.8
Individual FE	✓	✓	✓	✓	✓
Year FE	✓	✗	✗	✗	✗
Province-Year FE	✗	✓	✓	✓	✓
lnPop × i.Year	✗	✗	✓	✓	✓
lnGDP × i.Year	✗	✗	✗	✓	✓
SverviceShare × i.Year	✗	✗	✗	✓	✓

Standard errors in parentheses

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

Table 4: 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
Duration × Post	0.0149**	0.0125**	0.0141*	-0.0357**	-0.0222**	-0.0399**
	(0.00709)	(0.00481)	(0.00732)	(0.0155)	(0.00928)	(0.0170)
LnConfirmedXPost	-0.00363		-0.00182	0.0112		0.0204
	(0.00555)		(0.00737)	(0.0146)		(0.0215)
LnDeadXPost		-0.00479	-0.00364		-0.00552	-0.0184
		(0.00624)	(0.00844)		(0.0153)	(0.0232)
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	46.29	46.29	46.29
Individual FE	✓	✓	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓	✓	✓
Population-Year FE	✓	✓	✓	✓	✓	✓
lnPop × i.Year	✓	✓	✓	✓	✓	✓
lnGDP × i.Year	✓	✓	✓	✓	✓	✓

Standard errors in parentheses

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

Table 5: 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: Duration								
Duration × Post	0.0110**	0.0105**	0.0110**		-0.0234***	-0.0256***	-0.0234***	
	(0.00445)	(0.00466)	(0.00445)		(0.00781)	(0.00830)	(0.00781)	
Lockdown_city × Post	-0.00264				-0.0165			
	(0.0135)				(0.0301)			
Lockdown_comm × Post		0.00885				0.0364		
		(0.0192)				(0.0337)		
Lockdown_general × Post			-0.00264	-0.00157			-0.0165	-0.0188
			(0.0135)	(0.0135)			(0.0301)	(0.0300)
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: Duration_Dummy								
Duration_Dummy × Post	0.0285***	0.0279***	0.0285***		-0.00955	-0.00955	-0.00955	
	(0.00977)	(0.0104)	(0.00977)		(0.0267)	(0.0263)	(0.0267)	
Lockdown_city × Post	0.000548				-0.0197			
	(0.0131)				(0.0301)			
Lockdown_comm × Post		0.00884				0.0305		
		(0.0179)				(0.0341)		
Lockdown_general × Post			0.000548	-0.00157			-0.0197	-0.0188
			(0.0131)	(0.0135)			(0.0301)	(0.0300)
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	46.29	46.29	46.29	46.29
Individual FE	✓	✓	✓	✓	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Population-Year FE	✓	✓	✓	✓	✓	✓	✓	✓
lnPop × i.Year	✓	✓	✓	✓	✓	✓	✓	✓
lnGDP × i.Year	✓	✓	✓	✓	✓	✓	✓	✓

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

Table 6: Disentangled Effect: Separate Phase

	(1) Outcomes	(2) Outcomes	(3) Outcomes	(4) Outcomes
Panel A: Unemployment				
Duration × Post	0.0109**			
	(0.00446)			
Duration_JanFeb × Post		0.0146**		0.0130*
		(0.00570)		(0.00703)
Duration_FebJun × Post			0.00570	0.00203
			(0.00421)	(0.00486)
R-squared	0.470	0.470	0.470	0.470
Observations	63343	63343	63343	63343
Mean of Unemployment	0.173	0.173	0.173	0.173
Panel B: log Hours Worked				
Duration × Post	-0.0239***			
	(0.00772)			
Duration_JanFeb × Post		-0.0304***		-0.0267*
		(0.00826)		(0.0140)
Duration_FebJun × Post			-0.0125	-0.00473
			(0.0104)	(0.0131)
R-squared	0.315	0.315	0.315	0.315
Observations	37914	37914	37914	37914
Mean of Hours Worked	46.29	46.29	46.29	46.29
Individual FE	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓
Population-Year FE	✓	✓	✓	✓
lnPop × i.Year	✓	✓	✓	✓
lnGDP × i.Year	✓	✓	✓	✓

Standard errors in parentheses

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

Table 7: Heterogeneity: Unemployment

	(1) Gender	(2) Elder	(3) Middle School	(4) Private Sector	(5) Bottome Income Distribution	(6) Young Child
Duration × Post	0.00528	0.00489	0.000878	0.00140	-0.00428	0.0135**
	(0.00467)	(0.00466)	(0.00678)	(0.00935)	(0.00819)	(0.00599)
Duration × Post × Female	0.0106**					
	(0.00507)					
Duration × Post × Old		0.0122**				
		(0.00591)				
Duration × Post × edu_middle			0.0132*			
			(0.00753)			
Duration × Post × Private				0.0103		
				(0.00888)		
Duration × Post × income_bottom					0.0382***	
					(0.0117)	
Duration × Post × child_6						-0.0166*
						(0.00942)
Duration × Post × child_18						-0.0000539
						(0.00821)
R-squared	0.470	0.471	0.470	0.470	0.543	0.471
Observations	63343	63343	63343	63343	27651	63343
Mean of Outcomes	0.173	0.173	0.173	0.173	0.173	0.173
Individual FE	✓	✓	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓	✓	✓
Population-Year FE	✓	✓	✓	✓	✓	✓
lnPop × i.Year	✓	✓	✓	✓	✓	✓
lnGDP × i.Year	✓	✓	✓	✓	✓	✓

Standard errors in parentheses

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

Table 8: Heterogeneity: Traffic and Freight

	Unemployment		Unemployment		Unemployment		Unemployment	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Traffice.V HWY Low	Traffice.V HWY High	Traffice.V WTWY Low	Traffice.V WTWY High	Freight.V HWY Low	Freight.V HWY High	Freight.V WTWY Low	Freight.V WTWY High
Duration × Post	0.0153	0.00271	0.0218	0.00336	0.0115**	-0.00324	0.0341***	-0.0671
	(0.00971)	(0.00648)	(0.0229)	(0.00758)	(0.00521)	(0.00736)	(0.00934)	(0.0404)
R-squared	0.486	0.458	0.452	0.526	0.454	0.486	0.459	0.513
Observations	26229	30331	18219	14510	30771	25162	18726	13614
Mean of Outcomes	0.170	0.171	0.183	0.185	0.159	0.188	0.171	0.199
Individual FE	✓	✓	✓	✓	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Population-Year FE	✓	✓	✓	✓	✓	✓	✓	✓
lnPop × i.Year	✓	✓	✓	✓	✓	✓	✓	✓
lnGDP × i.Year	✓	✓	✓	✓	✓	✓	✓	✓

Standard errors in parentheses

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

Table 9: spill over effect

	(1) Unemployment	(2) log Hours Worked
lnDuration × Post	0.0116***	-0.0424**
	(0.00437)	(0.0179)
lnDuration_nearby × Post	-0.0168	0.0350
	(0.0202)	(0.0341)
R-squared	0.470	0.315
Observations	63343	37914
Mean of Outcome	0.173	46.29
Individual FE	✓	✓
Province-Year FE	✓	✓
Population-Year FE	✓	✓
lnPop × i.Year	✓	✓
lnGDP × i.Year	✓	✓

Standard errors in parentheses

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

8 Appendix

8.1 More Discussions on Identification

As discussed is Section 4.5, we could only conclude a Average Treatment Effects on Treated (ATT), but not the Average Treatment Effects (ATE) from the continues treatment setting. Pointed out by Equation (6) (Callaway et al., 2021), the second term on the right hand side is the selection bias, comes from the different counterfactual treatment effect of does $d^{'}$ between treated group of dose d and treated group dose $d^{'}$. To eliminate such bias, we need a stronger parallel trends 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".

under binary treatment DD parallel assumption

$$ATT(d|d) - ATT(d^{'}|d^{'}) = \underbrace{ATT(d|d) - ATT(d^{'}|d)}_{\text{ATE}} + \underbrace{ATT(d^{'}|d) - ATT(d^{'}|d^{'})}_{\text{selection bias}} \tag{5}$$

To Examine the selection bias term comes from the potential heterogeneous treatment effect conditional on *Duration* (non-linearities), in equation 7, we model these with three indicator variables based on *Duration*: a 15-30 days term, a 30-45 days term and a greater than 45 days term. Units with *Duration* smaller than 15 is set as control group. This specification is analogous to the one used by Lindo et al. (2020).

$$\begin{aligned} Y_{ipt} = &\beta_1 \mathbf{I}(15 \leqslant Duration_p < 30) \times Post_t + \beta_2 \mathbf{I}(30 \leqslant Duration_p < 45) \times Post_t \\ &+ \beta_3 \mathbf{I}(45 \leqslant Duration_p) \times Post_t + (Year_t \times X_p)\lambda_t + \theta_i + \delta_{r,t} + \epsilon_{ipt} \end{aligned} \tag{6}$$

The β are explained as the ATTs for those who belong to the corresponding group, compared to those who had experienced less than 15 days treatment. If we observe linearity in ATTs, the selection bias problem will be eased. Additionally, we also categorized the *Case Duration*, number of days with 0 case increase, [22] into three dummies: a 7 to 14 days term, 14 to 21 days term, and a greater than 21 days term. Noticing that, the *Case Duration* is contaminated more by unobserved factors and the estimators supposed to be less linearity than those of *Duration*.

In Table A9 , we report the estimates in which Unemployment and Hours Worked are measured as a step function in *Duration* and *Case Duration*. The estimates in Column 1 indicate that relative to having the Zero-Covid policy duration less than 15 days), having the Zero-Covid policy 15 to 30 days increase the unemployment rate by 0.008 percentage point, having the Zero-Covid policy 30 to 45 days increase the unemployment rate by 0.018 percentage point, and having the Zero-Covid policy more than 45 days increase the unemployment rate by 0.038 percentage point. Although the first two estimates are not significant, the effect size gradually increase with the *Duration*. Comparing to the results in Column (3), which use *Case Duration* as treatment, we have a more linearized ATT with *Duration*. Unfortunately, we cannot observed the similar pattern shows up in Column (2) and Column (4). In conclusion, by comparing with a treatment (*Case Duration*) fully contaminated by unobserved factors, the treatment we choose shows less non-linearized ATT across groups of different treatment level. Although the evidence is not compelling enough to resolve the selection bias problem, comparing to a less interpretable dummy treatment specification, our continuous treatment approach is to present evidence on marginal effect with more meaningful policy implication.

[22] the main difference between this measure with *Duration* is no 14-day window restriction.

8.2 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 [23].

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 [24], 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[25].), 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 [26].

[23] e.g. link1; link2; link3; link4;

[24] link5

[25] link6

[26] link7 link8 link9

8.3 Appendix Figures

Figure A1: Province Emergency Reaction Level Time Line

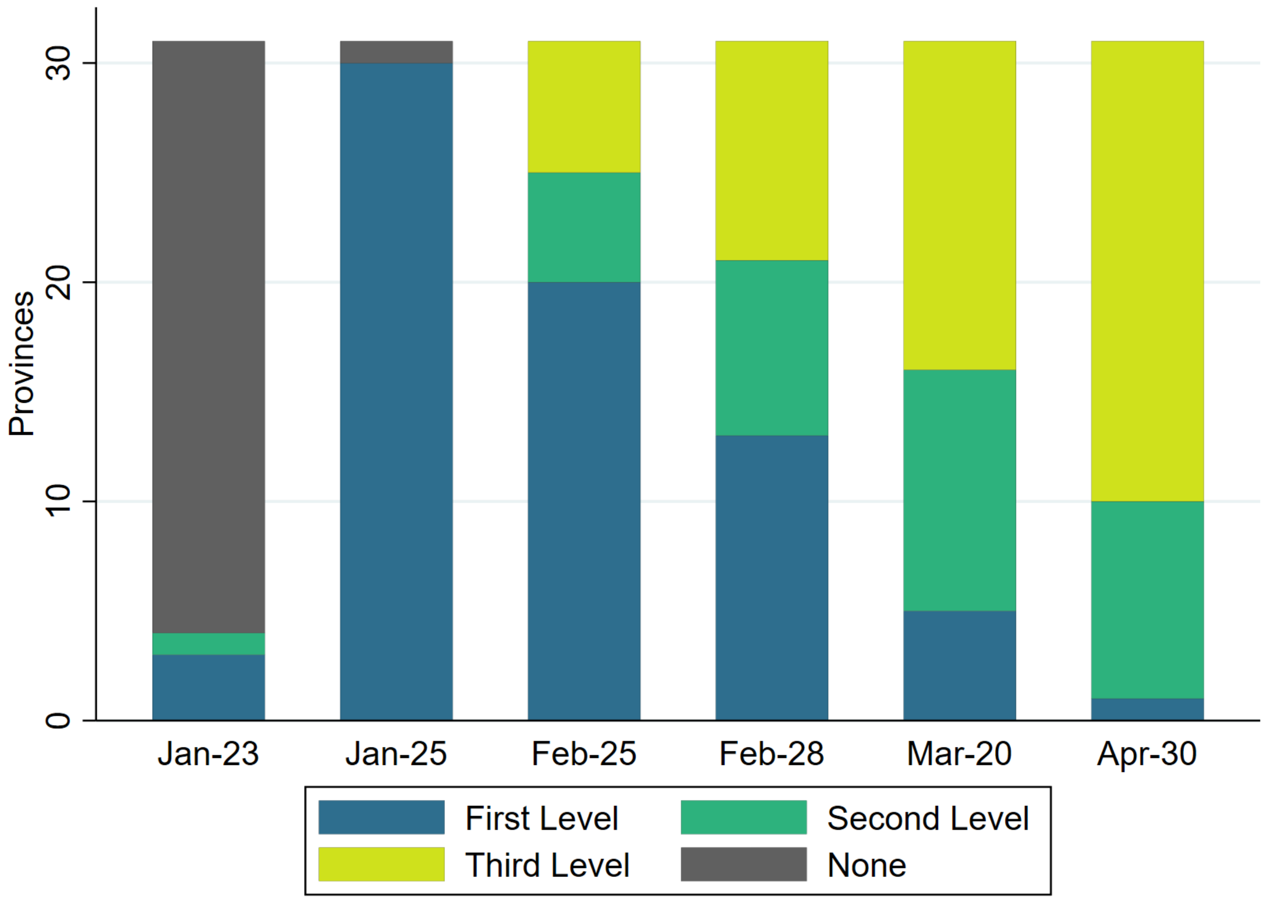
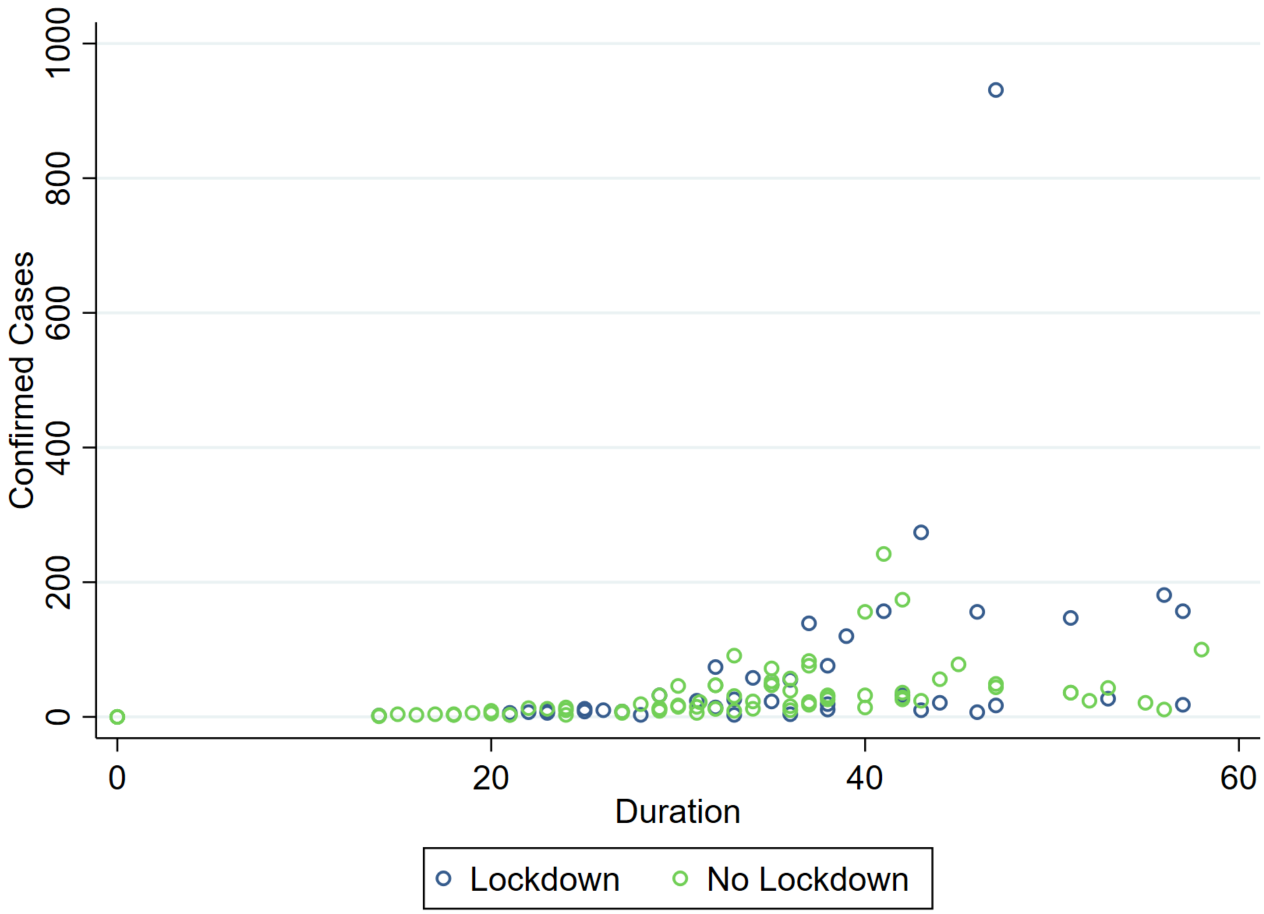
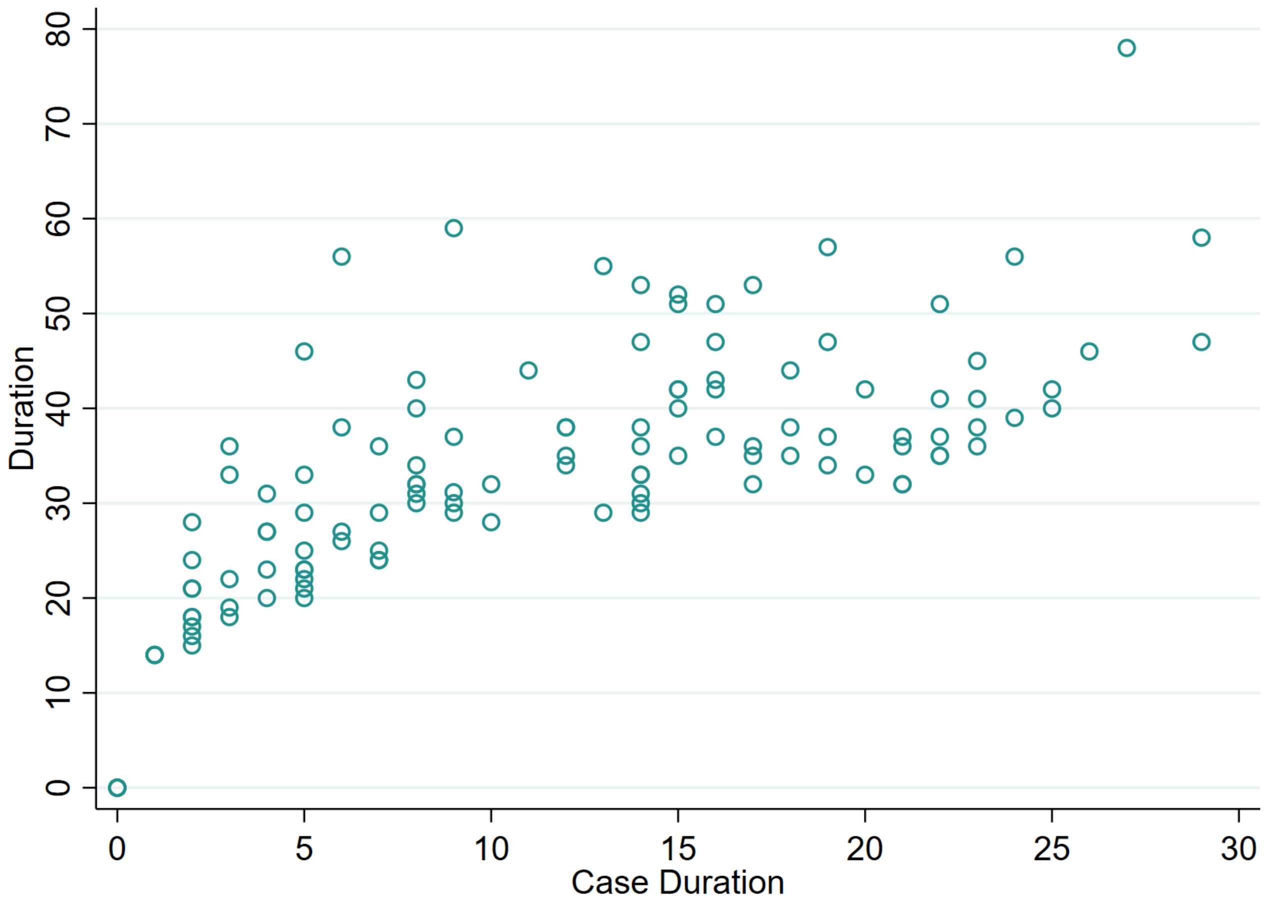


Figure A2: Duration and Confirmed Cases by Lockdown



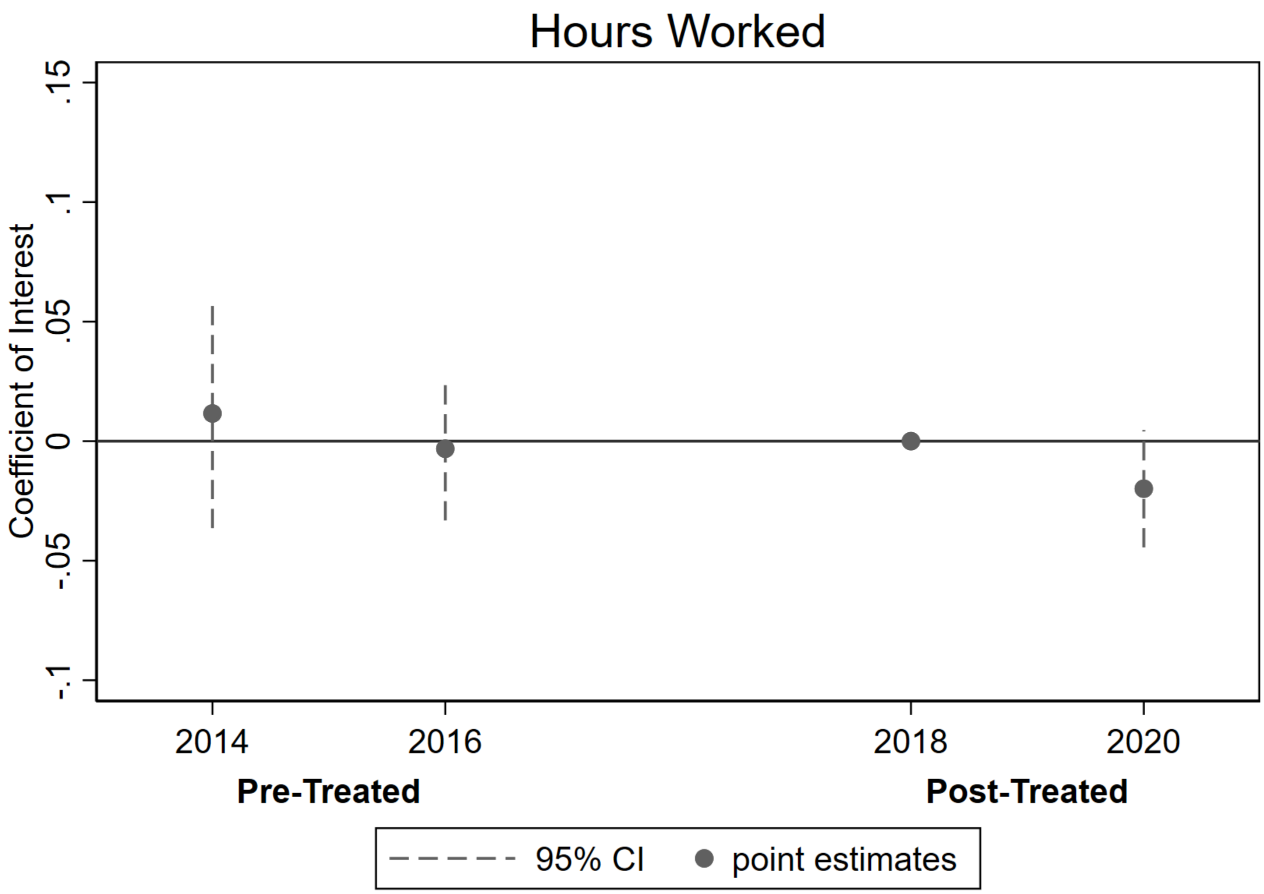
Note: Duration outliers (95 percentile) are dropped from graph

Figure A3: Examination: Randomized Treatment



Note: Caes Duration outliers (95 percentile) are dropped from graph

Figure A4: Dynamic Effects: Hours Worked



Note:

Figure A5: Treatment Effect by Survey Month

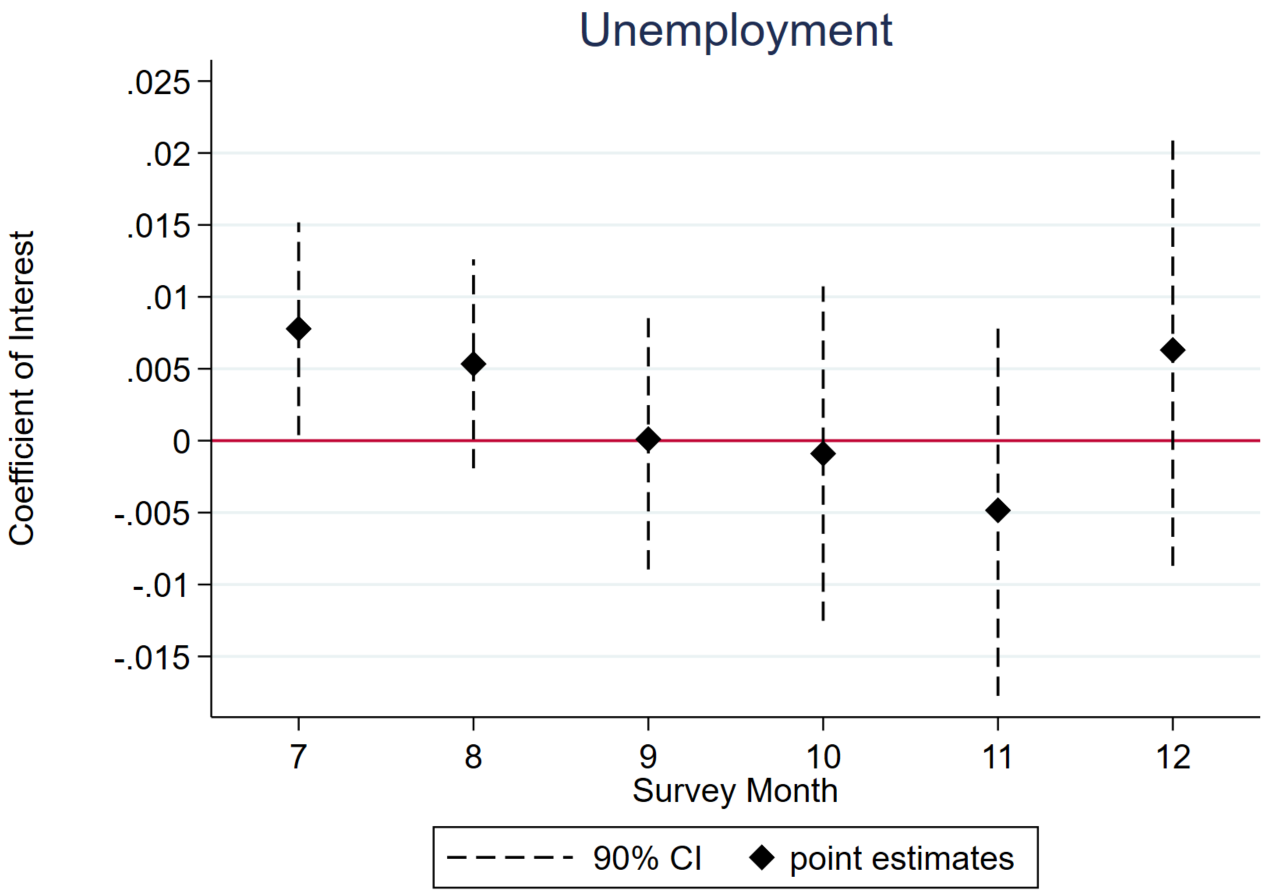
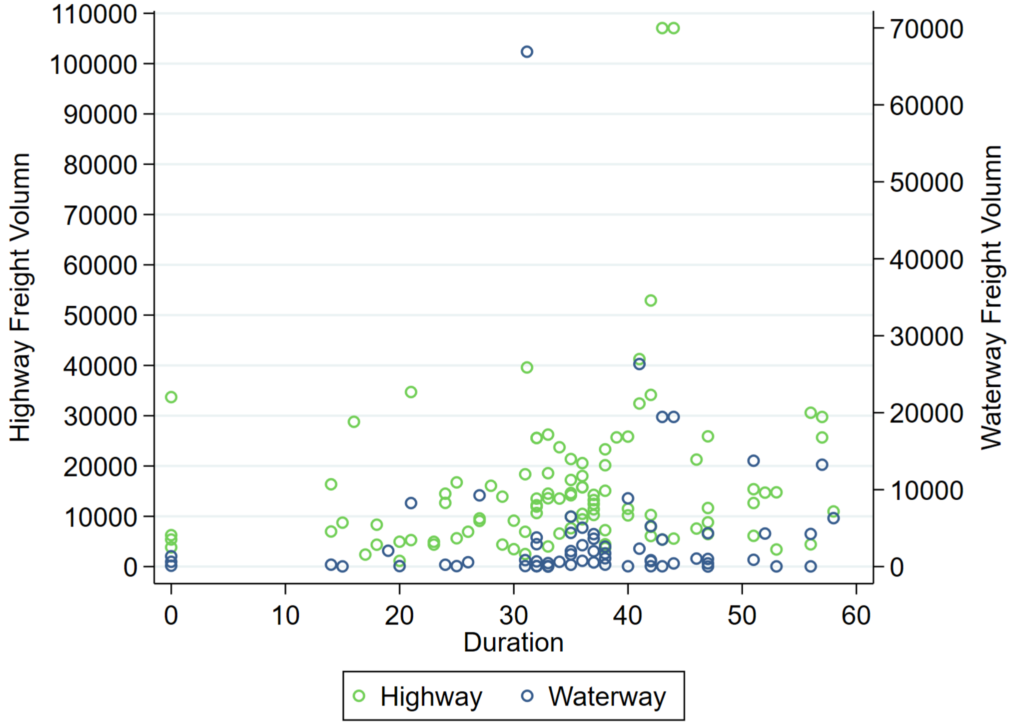
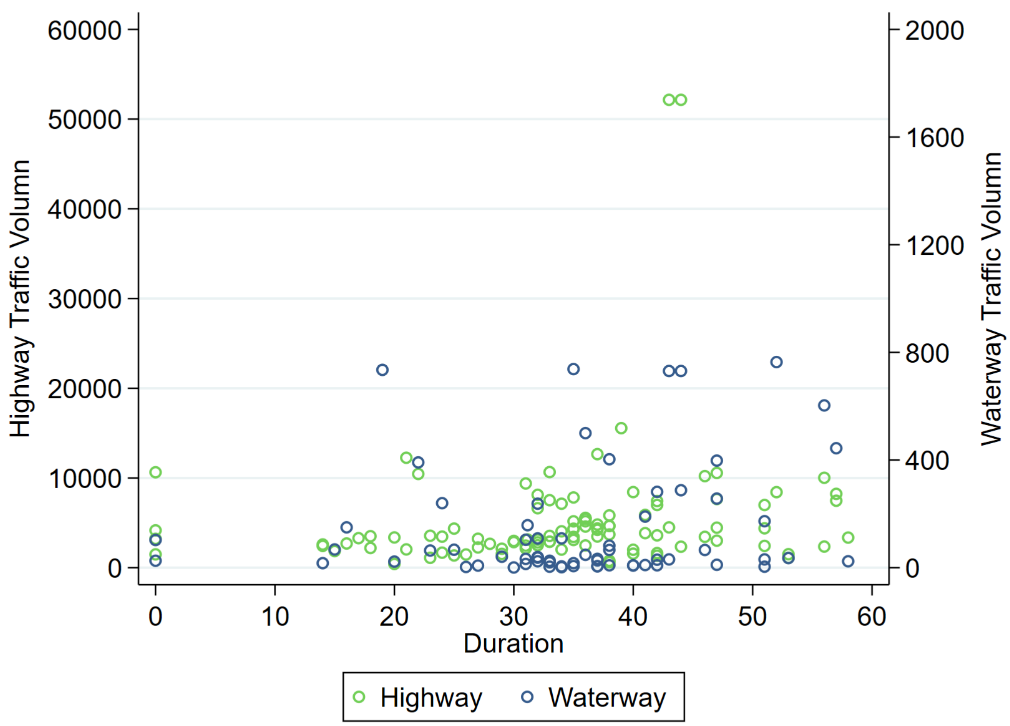


Figure A6: Freight, Traffic and Duration





Note:Duration outliers (95 percentile) are dropped from graph.

8.4 Appendix Tables

Table A1: Sample by Waves

Year	Prefectures	Obs	Share
2014	125	16246	.26
2016	125	17453	.28
2018	123	18379	.29
2020	121	11265	.18
.	.	63343	1

Table A2: Unemployment: Agricultural versus Non Agr

	Unemployment		Unemployment		Unemployment		Unemployment		Unemployment	
	(1) Non Agri	(2) Angricultural	(3) Non Agri	(4) Angricultural	(5) Non Agri	(6) Angricultural	(7) Non Agri	(8) Angricultural	(9) Non Agri	(10) Angricultural
Duration × Post	0.0102*	0.00715*	0.0126**	0.00957*	0.0150***	0.0111**	0.0133**	0.0106**		
	(0.00604)	(0.00373)	(0.00579)	(0.00499)	(0.00570)	(0.00499)	(0.00606)	(0.00486)		
Duration_Dummy × Post									0.0335***	0.0271**
									(0.0128)	(0.0124)
R-squared	0.511	0.190	0.512	0.191	0.512	0.191	0.512	0.191	0.513	0.191
Observations	40232	23111	40232	23111	40232	23111	40232	23111	40232	23111
Mean of Unemployment	0.227	0.0797	0.227	0.0797	0.227	0.0797	0.227	0.0797	0.227	0.0797
Individual FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Province-Year FE	✗	✗	✓	✓	✓	✓	✓	✓	✓	✓
Population-Year FE	✗	✗	✗	✗	✓	✓	✓	✓	✓	✓
lnPop × i.Year	✗	✗	✗	✗	✗	✗	✓	✓	✓	✓
lnGDP × i.Year	✗	✗	✗	✗	✗	✗	✓	✓	✓	✓

Standard errors in parentheses

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

Table A3: Hours Worked: Agricultural versus Non Ag

	log Hours Worked		log Hours Worked		log Hours Worked		log Hours Worked		log Hours Worked	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Non Agri	Angricultural	Non Agri	Angricultural	Non Agri	Angricultural	Non Agri	Angricultural	Non Agri	Angricultural
Duration × Post	-0.0133	-0.0172	-0.0181**	-0.0189	-0.0185*	-0.0261*	-0.0187*	-0.0302*		
	(0.00828)	(0.0151)	(0.00820)	(0.0146)	(0.00946)	(0.0156)	(0.00973)	(0.0160)		
Duration_Dummy × Post									-0.0372*	0.0438
									(0.0211)	(0.0742)
R-squared	0.226	0.294	0.232	0.298	0.233	0.298	0.234	0.298	0.234	0.298
Observations	22967	14947	22967	14947	22967	14947	22967	14947	22967	14947
Mean of Hours Worked	50.62	39.44	50.62	39.44	50.62	39.44	50.62	39.44	50.62	39.44
Individual FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Province-Year FE	✗	✗	✓	✓	✓	✓	✓	✓	✓	✓
Population-Year FE	✗	✗	✗	✗	✓	✓	✓	✓	✓	✓
lnPop × i.Year	✗	✗	✗	✗	✗	✗	✓	✓	✓	✓
lnGDP × i.Year	✗	✗	✗	✗	✗	✗	✓	✓	✓	✓

Standard errors in parentheses

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

Table A4: Robustness: Balanced Panel - Labor Outcomes

	(1) Unemploymeny	(2) Unemploymeny	(3) log Hours Worked	(4) log Hours Worked
Duration × Post	0.0120**		-0.0379***	
	(0.00464)		(0.00846)	
Duration_Dummy × Post		0.0378***		-0.0303
		(0.0112)		(0.0310)
R-squared	0.451	0.451	0.331	0.331
Observations	32368	32368	21047	21047
Mean of Outcome	0.148	0.148	45.89	45.89
Individual FE	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓
Population-Year FE	✓	✓	✓	✓
lnPop × i.Year	✓	✓	✓	✓
lnGDP × i.Year	✓	✓	✓	✓

Standard errors in parentheses

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

Table A5: Robustness: Balanced Panel - Income

	(1) log Income	(2) log Income
Duration × Post	-0.181**	
	(0.0841)	
Duration_Dummy × Post		-0.122
		(0.211)
R-squared	0.457	0.457
Observations	14162	14162
Mean of Outcome	20696.3	20696.3
Individual FE	✓	✓
Province-Year FE	✓	✓
Population-Year FE	✓	✓
lnPop × i.Year	✓	✓
lnGDP × i.Year	✓	✓

Standard errors in parentheses

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

Table A6: Robustness: Clustering Choices

	Unemployment			log Hours Worked		
	(1)	(2)	(3)	(4)	(5)	(6)
	County Year Clustering	Province Level Clustering	Province Year Clustering	County Year Clustering	Province Level Clustering	Province Year Clustering
Duration × Post	0.0109**	0.0109*	0.0109*	-0.0239	-0.0239**	-0.0239***
	(0.00301)	(0.00568)	(0.00365)	(.)	(0.0110)	(6.96e-10)
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	46.29	46.29	46.29
Individual FE	✓	✓	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓	✓	✓
Population-Year FE	✓	✓	✓	✓	✓	✓
lnPop × i.Year	✓	✓	✓	✓	✓	✓
lnGDP × i.Year	✓	✓	✓	✓	✓	✓

Standard errors in parentheses

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

Table A7: Robustness: Drop Wuhan and Hubei

	Drop Wuhan		Drop Hubei	
	(1)	(2)	(3)	(4)
	Unemployment	log Hours Worked	Unemployment	log Hours Worked
Duration × Post	0.0135***	-0.0235***	0.0117**	-0.0200**
	(0.00433)	(0.00825)	(0.00458)	(0.00835)
R-squared	0.470	0.315	0.469	0.314
Observations	63073	37747	62379	37353
Mean of Outcome	0.173	46.30	0.173	46.32
Individual FE	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓
Population-Year FE	✓	✓	✓	✓
lnPop × i.Year	✓	✓	✓	✓
lnGDP × i.Year	✓	✓	✓	✓

Standard errors in parentheses

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

Table A8: Heterogeneity: Hours Worked

	(1) Gender	(2) Elder	(3) Middle School	(4) Private Sector	(5) Bottome Income Distribution	(6) Young Child
Duration × Post	-0.0219**	-0.0230**	-0.0385***	-0.0275	-0.0176	-0.0275**
	(0.00998)	(0.0108)	(0.0127)	(0.0184)	(0.0125)	(0.0116)
Duration × Post × Female	-0.00378					
	(0.0184)					
Duration × Post × Old		-0.00274				
		(0.0144)				
Duration × Post × edu_middle			0.0186			
			(0.0140)			
Duration × Post × Private				0.00349		
				(0.0198)		
Duration × Post × income_bottom					0.0231	
					(0.0285)	
Duration × Post × child_6						-0.000175
						(0.0210)
Duration × Post × child_18						0.0144
						(0.0236)
R-squared	0.315	0.316	0.315	0.315	0.231	0.316
Observations	37914	37914	37914	37914	16552	37914
Mean of Outcomes	46.29	46.29	46.29	46.29	46.29	46.29
Individual FE	✓	✓	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓	✓	✓
Population-Year FE	✓	✓	✓	✓	✓	✓
lnPop × i.Year	✓	✓	✓	✓	✓	✓
lnGDP × i.Year	✓	✓	✓	✓	✓	✓

Standard errors in parentheses

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

Table A9: Examination: Heterogeneity in ATT across Duration

	Duration		Case Duration	
	(1)	(2)	(3)	(4)
	Unmployment	log Hours Worked	Unmployment	log Hours Worked
I(15 ⩽ Duration < 30)	0.00855	-0.0878***		
	(0.0182)	(0.0257)		
I(30 ⩽ Duration < 45)	0.0180	-0.0421		
	(0.0189)	(0.0311)		
I(45 ⩽ Duration)	0.0377**	-0.109***		
	(0.0185)	(0.0362)		
I(7 ⩽ Case_Duration < 14)			0.00137	-0.0279
			(0.0134)	(0.0305)
I(14 ⩽ Case_Duration < 21)			0.0103	-0.0415
			(0.0160)	(0.0346)
I(21 ⩽ Case_Duration)			0.0108	-0.00711
			(0.0160)	(0.0457)
R-squared	0.470	0.316	0.470	0.315
Observations	63343	37914	63343	37914
Mean of Outcomes	0.173	46.29	0.173	46.29
Individual FE	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓
Population-Year FE	✓	✓	✓	✓
lnPop × i.Year	✓	✓	✓	✓
lnGDP × i.Year	✓	✓	✓	✓

Standard errors in parentheses

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

Notes: *Duration* is defined as number of days with 0 increase in a 14 days window, between Jan 23 to Jun 30. *Case Duration* is defined as as number of days with 0 increase between Jan 23 to Jun 30.

Table A10: Examination: Heterogeneity in ATT across log Duration

	(1)	(2)
	Unmployment	log Hours Worked
I($2 \leqslant$ Log Duration < 3)	0.0343*	-0.102***
	(0.0193)	(0.0348)
I($3 \leqslant$ Log Duration < 4)	0.0353**	-0.0756***
	(0.0154)	(0.0240)
I($4 \leqslant$ Log Duration)	0.0551**	-0.0798*
	(0.0234)	(0.0446)
R-squared	0.470	0.315
Observations	63343	37914
Mean of Outcomes	0.173	46.29
Individual FE	✓	✓
Province-Year FE	✓	✓
Population-Year FE	✓	✓
lnPop × i.Year	✓	✓
lnGDP × i.Year	✓	✓

Standard errors in parentheses

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

Notes: Table A 10 reports estimates in which treatment are measured as a step function in log duration. The results indicate the relative effect comparing to group which log duration is smaller than 2.

Table A11: Placebo Test: Fake Treatment Period

	(1) Unmployment	(2) Unmployment	(3) log Hours Worked	(4) log Hours Worked
lnDuration × Post 2016	-0.00199		-0.00591	
	(0.00516)		(0.0187)	
lnDuration × Post 2014		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 Outcomes	0.175	0.175	46.14	46.14
Individual FE	✓	✓	✓	✓
Province-Year FE	✓	✓	✓	✓
Population-Year FE	✓	✓	✓	✓
lnPop × i.Year	✓	✓	✓	✓
lnGDP × i.Year	✓	✓	✓	✓

Standard errors in parentheses

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

Table A12: First Stage

	(1) Lockdown_Days	(2) log Lockdown Days	(3) Lockdown Indicator	(4) Lockdown Indicator
Duration	0.176***		0.00162**	
	(0.0391)		(0.000756)	
lnDuration		0.472***		0.0885***
		(0.168)		(0.0193)
R-squared	0.301	0.110	0.0538	0.0634
Observations	34	34	297	297

Standard errors in parentheses

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