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RESEARCH ARTICLE

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Hit Hard but Recover Slowly: The Asymmetric Effects of Social Distancing Policies on the US Labor Market

Abstract This study employs a difference-in-differences approach to examine the US labor market response to two widely used social distancing policies, stay-at-home (SAH) order and non-essential business closure, with special attention paid to the asymmetric effect of the policies' imposition and lifting. Exploiting the variation across states and time, we find that state employment rates declined by 4.3% and 1.9% for the two policies respectively, within one month of the enaction of social distancing policies, but the recovery was slower after the policies were removed. We also highlight that the low-income group suffered the highest employment rate drop from the SAH enaction while presenting the mildest rebound. Self-employed workers were more affected by the policy impositions but recovered slightly faster than wage earners. Our results suggest persistent efforts must be made after the pandemic, especially for more vulnerable groups in the labor market.

Keywords COVID-19, social distancing policies, labor market, asymmetric effects
JEL Classification I1, J0, J1

1 Introduction

The COVID-19 pandemic has been a great shock in various countries, including the US. In an effort to control the spread of the virus, U.S. state governments

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enforced various non-pharmaceutical interventions (NPIs)[1]. Although these policies were shown to be effective in reducing the transmission of the virus (Flaxman et al., 2020) and significantly reduce the local and cumulative mortalities (Hatchett et al., 2007), they may have simultaneously caused negative and long-term effects on the labor market, even when they were lifted after the pandemic.

In this study, we use both daily real-time and monthly data to examine the labor market effect of the policy dynamics of two widely used social distancing policies, namely the stay-at-home (SAH) order and non-essential business closure (NBC) in the US, paying special attention to the potential asymmetric labor market effect of the policies' impositions and lifting. We first follow the literature to employ a difference-in-differences (DiD) approach to investigate the effect of both policies' dynamics on employment and working hours (Gupta et al., 2020; Lozano-Rojas et al., 2020). For the daily state-level analysis, we applied the DiD with staggered treatments to exploit the fact that different NPIs were enacted at different time points across states, to identify the aggregate employment market response to NPI dynamics. For the monthly data, we examine both the intensive and extensive margins of the labor market at an individual level, using the length of exposure to conceptualize the policy strength.

While the asymmetric effects of the policy effects in the COVID-19 economics study were first proposed by Cheng et al. (2020), who emphasize the asymmetry of employment inflow and outflow during a reopening, asymmetry in this study refers specifically to the labor market response differences when policies are enforced or lifted, measured by the effect's size and speed[2]. Using the above methods, we find strong and consistent results from the two sets of data; the implementation of both social distancing policies presents a quick and significant negative effect on the labor market, including on employment rates and individual working hours. Our evidence suggests that within one month of enforcing the policies, the two policies on average led to employment rate reductions of 4.3% and 1.9% respectively, compared to January levels, while the weekly working hours of individuals were expected to drop between 0.42 and

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1 *Non-pharmaceutical interventions* are actions, apart from getting vaccinated and taking medicine, that people and communities can take to help slow the spread of illnesses like a pandemic influenza (flu).

2 The signs of the policy effects are clearly anticipated to be opposite to each other with the imposition and lifting, so the focus here is on size and speed.

0.17 hours for every ten days after the implementation of the SAH and NBC policies. In contrast, the labor market recovered much slower and by only a marginal rate after state governments lifted these policies. The estimates show that there is only a 2% rebound in the relative employment rates due to the lifting of the SAH order, whereas the lifting of the NBC policy had limited impact.

We also observe heterogeneous policy effects on different income groups in the sense that lower income groups suffer more but recover less when faced with the policy dynamics. For the state-level employment rates, the low-income group suffered the highest drop of 5.8% from the SAH enaction, while the high- and middle-income employees' employment rates only reduced by 4.2% and 2.19%, respectively, compared to January levels. The corresponding recovery from the lifting of the policies is limited: the employment rate rebound only makes up for 29.6%, 42.0%, and 40.0% of the harm caused in each group respectively, which enlarges the gap in different income levels. This is also verified by the extensive margin: the working hours of those employed, for the low-income earners it reduced by 0.50 hours per ten days on average after the SAH order – the greatest drop among all the groups.

From the cohort features in the current population survey (CPS) monthly data, we further find that self-employed people suffer more in the face of NPI policies. Being equally exposed to the two policies, the self-employed are 28% less likely to work compared to hired workers with each of the policies, while their employment is stimulated by lifting the NBC policy, with a 1% recovery per ten-day exposure. Similar effects are also found in working hours, indicating that the self-employed group, who face less market friction, have more flexibility in employment and working decisions than those working for wages. This potentially supports market friction as an important factor in explaining the asymmetry between imposition and lifting.

We also conducted a robustness check to add credibility to our results. The first test is to verify the assumption of the difference-in-differences approach, that is, the common trend assumption, employing the event study framework. For the Economic Tracker data, the key variables include the overall employment level showing no pre-trend 21 days prior to the SAH and NBC policies, while the CPS data show that individual-level employment opportunities and average working hours also present no pre-trends for the three months prior to both the SAH and NBC policies. Further, the post-treatment estimates also provide additional insights into the lagged effect of the SAH policy.

This study builds on labor market research in the aftermath of COVID-19 by systematically comparing the asymmetric impacts of the impositions and removals of NPIs. We find asymmetric features in the response speed and strength of the SAH policy, which indicates that the employment market may take longer to recover from social distancing restrictions than the time taken to harm it. The study also highlights the cohort differences in sensitivity to NPI policies, featured by income levels and employment types. Not only have we found that lower-income earners are more vulnerable to restricting policies, but they also recover to a lesser extent. Finally, even for the high-frequency analysis, we use a rich set of direct measures of the labor market, such as relative employment levels, working hours, the self-employed and wage earners, whereas most of the existing research uses proxies of the labor market, such as Google search data and work-related mobility data (Kong and Prinz, 2020) or intervention policies on a coarse scale (Cheng et al., 2020).

The remainder of this paper is organized as follows: Section 2 provides background information on the US NPIs at the state level and the labor market trend during the time periods considered. Section 3 summarizes the relevant literature and, in particular, the major studies that we based our empirical analysis on, highlighting our improvements to these methods. Sections 4 and 5 illustrate the data used and empirical methods in detail, while the results and interpretations are discussed in Section 6. Finally, the conclusions of the study are presented in Section 7.

2 Background

The outbreak of COVID-19 in March 2020 not only caused an urgent public health crisis but also had labor market consequences in the US. Until March 26, ten weeks after the confirmation of the first local case in the US, 81,321 cases of infection were reported (Carter & May, 2020). By the end of April, the national unemployment rate had reached a historical high of 14.7%, and the proportion of unemployed (including those with a job but not at work) had increased by 14.1% (Bitler et al., 2020; Coibion et al., 2020). In an effort to contain the spread of the virus, the state government enforced various NPIs. Throughout early April to late May, various state-level NPIs[3], including SAH and NBC, were shown to be effective in reducing the transmission of the virus (Flaxman et al., 2020) and

[3] Other NPIs include travel restrictions, school closures, large gathering bans and restaurant and bar limitations.

significantly reduce local and cumulative mortality rates (Hatchett et al., 2007).

In Figure 1, we summarize the enforcement and removal dates of the SAH and NBC policies in different states[4]. As shown in Figure 1, when COVID-19 initially broke out in March, the SAH and NBC were quickly enacted by 51 states within approximately 20 days from March to mid-April, in response to the rapid increase in COVID-19 cases. The enaction of NBC peaked on March 25 and led the enforcement of the SAH order, which was applied inconsistently in different states. From early May to mid-June, the two policies were gradually removed because the COVID-19 virus was, to some extent, under control, with large timing variances in different states. It is clear that the policy-lifting dates are much more spread-out throughout the second phase. As shown in Table 1, the first ending of the NBC policy happened on April 20 in South Carolina, after which 30 states gradually ended NBC restrictions and the removal peaked on May 8 with exceptions such as Missouri, Virginia and Pennsylvania. The removal of the SAH orders was generally later than that of the NBC order. From April 24 to June 15 they were almost evenly distributed, although concentrated in May. Among the 40 states where SAH restrictions were lifted, 27 lifted them in May.

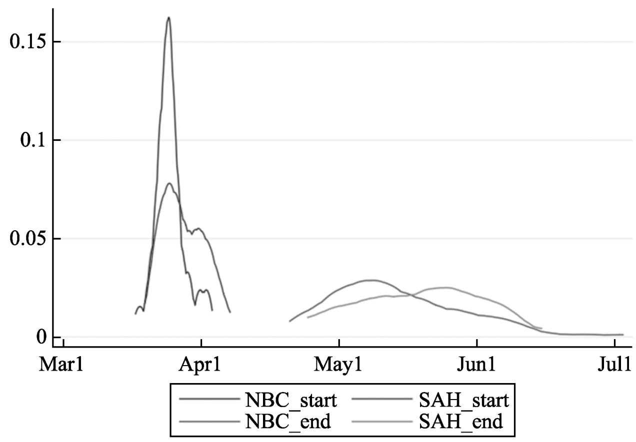

Figure 1 The Distribution of Policy Dynamics Timing

Note: The graph is a kdensity depicting the timing of different policies across 51 states. NBC stands for non-essential business closure, and SAH stands for the stay-at-home order. “start” and “end” mark the date on which the corresponding policy dynamics started or ended, respectively.

[4] This is also the policy timing measure *policy milestone* in the Economic Tracker dataset, which is discussed in Section 4.1.

Table 1 Policy Imposition and Lifting Dates

State Name	Policies			
	NBC_start	SAH_start	NBC_end	SAH_end
Alabama	03/28/2020	04/04/2020	05/01/2020	04/30/2020
Alaska	03/28/2020	03/28/2020	04/24/2020	04/24/2020
Arizona		03/31/2020	05/08/2020	05/15/2020
Arkansas			05/04/2020	
California	03/19/2020	03/19/2020	05/22/2020	
Colorado	03/26/2020	03/26/2020	05/09/2020	05/09/2020
Connecticut	03/23/2020	03/23/2020	05/20/2020	05/20/2020
Delaware	03/24/2020	03/24/2020	06/01/2020	05/31/2020
District of Columbia	03/25/2020	04/01/2020	05/29/2020	05/29/2020
Florida		04/03/2020	05/18/2020	05/18/2020
Georgia		04/03/2020	04/24/2020	04/30/2020
Hawaii	03/25/2020	03/25/2020	05/15/2020	05/31/2020
Idaho	03/25/2020/	03/25/2020	05/01/2020	04/30/2020
Illinois	03/21/2020	03/21/2020	06/03/2020	05/29/2020
Indiana	03/24/2020	03/24/2020	05/18/2020	05/18/2020
Iowa	03/17/2020		05/08/2020	
Kansas		03/30/2020	05/11/2020	05/22/2020
Kentucky	03/26/2020	03/26/2020	05/11/2020	05/11/2020
Louisiana	03/22/2020	03/23/2020	05/16/2020	05/16/2020
Maine	03/25/2020	04/02/2020	05/01/2020	05/31/2020
Maryland	03/23/2020	03/30/2020	06/01/2020	06/01/2020
Massachusetts	03/24/2020	03/24/2020	05/18/2020	05/18/2020
Michigan	03/23/2020	03/24/2020	05/11/2020	06/01/2020
Minnesota		03/27/2020	04/27/2020	05/17/2020
Mississippi	04/03/2020	04/03/2020	04/27/2020	04/27/2020
Missouri		04/06/2020	05/18/2020	05/18/2020
Montana	03/26/2020	03/28/2020	05/01/2020	04/26/2020
Nebraska			06/01/2020	
Nevada	03/21/2020	04/01/2020	05/09/2020	05/09/2020
New Hampshire	03/28/2020	03/27/2020	05/11/2020	06/15/2020
New Jersey	03/21/2020	03/21/2020	06/15/2020	06/09/2020
New Mexico	03/24/2020	03/24/2020	06/01/2020	05/31/2020

(*To be continued*)

(*Continued*)

State Name	Policies			
	NBC_start	SAH_start	NBC_end	SAH_end
New York	03/22/2020	03/22/2020	06/08/2020	05/28/2020
North Carolina	03/30/2020	03/30/2020	05/08/2020	05/22/2020
North Dakota			05/01/2020	
Ohio	03/23/2020	03/23/2020	05/04/2020	05/29/2020
Oklahoma	04/01/2020		05/01/2020	
Oregon		03/23/2020	05/15/2020	
Pennsylvania	03/23/2020	04/01/2020	06/05/2020	06/05/2020
Rhode Island		03/28/2020	05/09/2020	05/08/2020
South Carolina		04/07/2020	04/20/2020	05/04/2020
South Dakota				
Tennessee	04/01/2020	03/31/2020	05/11/2020	05/11/2020
Texas		04/02/2020	05/01/2020	04/30/2020
Utah			05/01/2020	
Vermont	03/25/2020	03/25/2020	05/11/2020	05/15/2020
Virginia	03/24/2020	03/30/2020	05/29/2020	06/10/2020
Washington	03/25/2020	03/23/2020	07/03/2020	05/31/2020
West Virginia	03/24/2020	03/24/2020	05/04/2020	05/03/2020
Wisconsin	03/25/2020	03/25/2020	05/26/2020	05/26/2020
Wyoming			05/11/2020	

Note: The data are from the *policy milestone* measure in the Economic Tracker dataset. The SAH start is the date on which the state government told residents to stay home other than for essential activities. The NBC start is the date on which the state government ordered all non-essential businesses to close. The "end" marks the ending of the two policies.

As employment declined during the imposition period, we also note that there was a corresponding labor market response to the slowdown in the spreading of the virus and social distancing policies. During the second period, the national unemployment rate gradually declined to 10.2% in July and the labor force participation rate also increased from 60.2% to 61.5%, which implies that the labor market had bounced back. The recovery of the labor market differs in both region and time. Some states, such as Nevada, experienced a greater scale of recovery (unemployment rate declined from 30.07% to 14.99%), while others such as Minnesota showed only a slight recovery that did not last long (Bitler et al., 2020; Coibion et al., 2020). This variation between states and time horizons led to the study of the imposition and lifting of NPIs' influences on labor market outcomes.

3 Data

We use two datasets to capture labor market dynamics, the daily state-level employment data from Economic Tracker and individual-level data from the CPS.

3.1 The Economic Tracker Data

We compiled the real-time dataset from Economic Tracker[5] for February 24, 2020, to June 25, 2020. The dataset integrates real-time data from companies that capture changes in indicators during the COVID-19 pandemic, such as employment rates, consumer spending, and job postings across counties, industries, and income groups. The data we use are aggregated at a state level on a daily basis, and the total observations for the selected period amount to more than 6000.

As a basis for this study, we first constructed the NPI timing data according to the policy milestones measured in this dataset. Four dummy variables *NBC_inforce*, *SAH_inforce*, *NBC_lift*, and *SAH_lift* were constructed to reflect the dates on which SAH and NBC were imposed and lifted in each state, by marking the corresponding variables as one after a policy change was announced.

The most central variable is the relative employment rate, which measures the relative state-level employment rate compared to the period of January 4 to January 31. There are three main advantages of this employment measure. First, the high-frequency nature captures the policy effect promptly–it is updated in real time, while the traditional data is only available monthly. Second, the employment level is normalized to the January level, avoiding potential measurement errors. Lastly, the sample presents the population comprehensively,

[5] The Economic Tracker, led by Opportunity Insights at Harvard University, is a database that captures the real-time changes of indicators during the COVID-19 pandemic, such as employment rates, consumer spending, and job postings across counties, industries, and income groups. https://tracktherecovery.org

evenly including employees with different income levels. In fact, our study pays close attention to the variations between the different employment income levels.[6]

We also gathered two important control variables for our analyses. One is the relative consumption data from the seasonally adjusted credit and debit card data from Affinity Solutions. This is used as a proper proxy for general economic activities (Casado et al., 2020; Chetty et al., 2020). Another variable, for real-time COVID-19 infection cases, was also employed in our analyses to capture the general trend of the pandemic.

3.2 The Basic Monthly CPS Data

The CPS is a monthly survey of unemployment and labor force participation conducted in the US. It offers a panel structure of both household and individual levels, with interviews on housing unit data for four consecutive months[7] compiled on the 12th of each month and provides data on approximately 50,000 households (US Census Bureau, 2019). We use a sample from February to June 2020 that included more than 440,000 observations in total, for consistency with the previous dataset. Because we are only interested in the effect on the working-age population, we exclude the observations of people who are older or younger than the working age. In terms of measured variables, we investigate both the intensive and extensive margins of the labor market: the proportion of employment[8] and the working hours of those employed.

First, to examine the effect on the employment rate, we measure the employment status of each observation over the course of the policy implementations. Specifically, we account for those who actually worked, and those employed and separated from their jobs, whose working hours are denoted as zero. This treatment shadows that of previous studies that isolate the absent group, which has been shown to be critical in COVID-19 research (Gupta et al.,

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[6] The low-income employment level is defined as the bottom quartile of income distribution (approximate income below $27,000), while the middle-income is in the middle two quartiles and is approximately between $27,000 and $60,000 and the high-income in the top quartile is over $60,000.

[7] The sample will then be excluded for the next eight months before being reinstated in the following four months of the next year.

[8] This excludes those employed but absent from work.

2020; Lozano-Rojas et al., 2020; Montenovo et al., 2020).

We also investigate the labor market, from the perspective of working hours, by the measurement of hours, which refers to the number of hours worked in the week before the survey date, which is the 12th of each month. Because those who are forced to leave the employment market are also an important part of the policy impact, we count all unemployed people as working zero hours, which avoids selection bias.

4 Literature Review

This study is related to two strands of literature: studies investigating the economic effects of government interventions during the COVID-19 outbreak, and literature that examines how the general spread of epidemics impact labor economics.

In the first strand of recent COVID literature, the most relevant research evaluates the causal effects of NPIs in the US on unemployment reduction, although some also study the effect of restrictions being lifted. Gupta et al. (2020) examined the social distancing policies that states adopted from March to April in response to virus transmissions and found that about 60% of the employment rate decline was as a result of this policy. However, it does not consider the effect of removing the stimulation policies for the period that followed. In contrast, Cheng et al. (2020) discusses the labor market effect of the reopening policy, when policies were lifted, in detail. They use longitudinal CPS data until May, which shows that the state reopening policies significantly increase the re-employment probability, which is measured by the proportion of those employed in May from among the unemployed in April. However, their research was limited to the state reopening policies on a coarse scale, and the high-frequent variables, such as work-related mobility data and Google search data trends, are not direct measures of employment performance. Chetty et al. (2020) also evaluated the NPI policy to revitalize the economy after COVID-19 using low-income workers' earning data and the event study method. However, with a greater overall discussion on consumer spending, business revenues, and other key indicators, the investigation of unemployment, although inspirational, is limited to the economy related to low-income earners.

Although Cheng et al. (2020) suggest the asymmetry of closure and reopening policies, they do not formally compare the two using the same dataset and empirical methods. Therefore, despite the wide-ranging research centered on the study of the local labor market, there are still gaps in analyzing the specific intervention effects on the overall population, together with a systematic examination of the asymmetric effects of the imposition and lifting of the stimulation policy on the labor market.

Broadly speaking, economists are also interested in the more diversified aspects of the economy, with abundant variations in regions and empirical methods. Chetty et al. (2020) use the Economic Tracker dataset and a regression discontinuity estimator to study how the US spending responds to different NPIs, and find that high-and low-income households had reduced their spending by 17% and 4%, respectively, by June 10. Additional evidence from South Africa, based on the newly released panel dataset, the National Income Dynamics Study: Coronavirus Rapid Mobile Survey (NIDS-CRAM), shows that the lockdown policy decreased the active employment rate by 40% one month after its implementation (Budlender et al., 2020). Reichelt et al. (2020) focus on gender inequality in the labor market under COVID-19 in the U.S., Germany, and Singapore. Using survey data from the firm YouGov, they find that women were 7% more likely to transition to remote working than men and 5% more likely to reduce their working hours by more than ten hours. All of the studies above use real-time high-frequency data, despite the resource differences, which clearly capture the trend caused by the pandemic and related policies.

This study also relates to the broad literature on labor market responses to pandemics. Garrett (2009) focused on the wage growth in the US manufacturing sector across states from 1914 to 1919 and concluded that increased influenza mortalities were associated with higher increases in wages due to labor shortages. The role of NPIs in the 1918 influenza pandemic was analyzed by Correia et al. (2020), in which the timing and strength differences of NPIs across US cities were examined. They found that the effect of NPIs varied with time horizons. The rapid and prompt implementation of NPIs were associated with better economic outcomes in the medium term.

There are also studies that examine other epidemics or labor markets in other countries and regions. Arndt and Lewis (2001) found that the 2001 HIV/AIDS pandemic in South Africa greatly depressed the demand for labor, especially for sectors with unskilled or semi-skilled workers. Using the data for 48 countries, Barro et al. (2020) found that the proportion of economic decline attributed to flu led to a 6% GDP decrease and an 8% consumption drop in a typical country.

5 Empirical Methodology

We conduct three econometric analyses of the policy dynamics. First, we examine the impact of policy dynamics on employment rates, measured relative to January. The model, like many of the models in COVID-19 literature, uses real-time data at a state level and is a variant of the staggered difference-in-differences (DiD) method of Mitze et al. (2020). A DiD design is well-suited for capturing the dynamic treatment effects of various states, that is, those accumulated over time from different policy timings. Second, we study the causal effect on working hours and, at the same time, re-examine the employment indicators with the aid of monthly CPS data. With this low-frequency data at an individual level, the generalized difference-in-difference is employed to investigate whether different lengths of exposure to policy dynamics affected these labor market outcomes. This analysis moves from an aggregate to an individual level compared to previous cases, and is expected to offer some confirmations or new insights. Third, we test the validity of the DiD estimation by using the multiple-policy event study method. The differential NPI enaction dates, across the various states, allow us to compare their pre-treatment trends, and only a parallel trend justifies a counterfactual analysis in the DiD estimation.

5.1 The Effect on Aggregate Employment

To exploit the daily frequency data, we suggest the following specification to quantify the effects of the two most widely adopted NPIs, the SAH order and NBC (Chetty et al., 2020; Kong & Prinz, 2020):

$$\begin{aligned} Y_{it} = {} & \beta_1 SAH\_inforce_{it} + \beta_2 NBC\_inforce_{it} + \beta_3 SAH\_lift_{it} \\ & + \beta_4 NBC\_lift_{it} + \delta X_{it} + \gamma_i + \gamma_t + u_{it} \end{aligned} \tag{1}$$

Our main outcome variable Y_{it} is the relative employ β_1 to β_4 ment rate level of state i at time t, standardized to the pre-COVID-19 January level. The four policy indicators take the value of one from the day when the policy was enforced (or lifted) for state i. Their coefficients are the DiD parameters that reveal the effects of imposing or lifting the policies on employment levels. While their signs indicate the direction of the policies' influences, their magnitudes determine the size of the drift from January levels that are credited to the enaction or removal of the two policies. X_{it} is the vector for the control variables which include an economic activity indicator, spending, and the measurement of COVID-19. γ_i and γ_t are the state and time fixed effects, respectively, which account for the heterogeneity of different states and the time-specific dynamics in the evolution of employment levels, while standard errors are clustered at the area level.

We also note that both policies' dynamics are simultaneously included to avoid overstating their effects, as in a single-policy analysis. Such an approach may constitute a trade-off with the collinearity problem, given that NPIs are enforced closely in time. Nonetheless, by carefully selecting only the two most influential policies (Goodman-Bacon & Marcus, 2020; Kong & Prinz, 2020), the harm of the collinearity problem is minimized. As a robustness check, we also evaluate the policy effect using the multiple-policy event study analysis, which is further discussed in Section 5.3.

Our identifying assumption is that no other differences between the treated and control states, except for the policy imposition, systematically affect the trend of employment levels. Under this assumption, the β coefficients correctly compare the progression of states, with stable exposures, to certain policy dynamics over time against those that have not been treated and relate the differences to the employment level change. To account for the fact that the NPIs are not randomized, we include two important control variables to match the treatment group, the COVID-19 infection cases, and the overall strength of economic activity, which improves the accuracy of quasi-experimental estimations. We also include the calendar date and state-fixed effects in the DiD framework to eliminate bias from omitted variables. The former adds controls for the variables that determine the trend in employment levels, while the latter allows for the heterogeneity of each state. We will also formally test for a pre-treatment common trend, statistically, using the event study method, which is further illustrated in Section 5.3.

5.2 The Effect on Individual Labor Market Performance

By using daily data, the standard DiD approach, as described, captures the NPI effect on an aggregate employment level. To enrich the analysis, we examine the effect of the policy imposition and lifting by investigating monthly labor market outcomes at an individual level. While the finer observation units better represent the population, the monthly available data also offer a potentially different angle that may help account for conventional labor market rigidity in this context. To eliminate the policy effect in this case, we estimate a generalized DiD model that associates the length of policy exposure to labor market outcomes. The specification, as a variant of Gupta et al. (2020), takes the following form:

$$\begin{aligned} Y_{im} = {} & \beta_1 Exposure\_SAH\_inforce_{im} * D_m + \beta_2 Exposur\_NBC\_inforce_{im} * D_m \\ & + \beta_3 Exposure\_SAH\_lift_{im} * D_m + \beta_4 Exposure\_SAH\_lift_{im} * D_m \\ & + Self\_employed + Self\_employed * policy\_dynamic + \delta X_{im} \\ & + \gamma_i + \gamma_m + u_{im} \end{aligned} \tag{2}$$

where Y_{im} in Equation (2) are the outcome variables that are specific to each panel unit and month. The X_{im} vector consists of human capital and other individual-level characteristics, including education, gender, and race, while γ_i and γ_m are also the state and month fixed effects. The key to the model lies in the measure of the policy, that is, $Exposure\_XXX\_inforce_{im}$ and $Exposure\_XXX\_lift_{im}$. These variables denote individuals' *i* lengths of exposure to a certain policy dynamic in month *m*, and we multiply them by the month dummy D_m to restrict the entire term in the corresponding month. Specifically, let E_{SAH_i} be the date on which SAH started or was lifted in state *i*, $Exposure\_SAH\_inforce_{im}$ is the duration of such a policy change before $t_m^* =$ 12 (CPS survey day) in month *m*, that is, $t_m^* - E_{SAH\_i}$. For example, the SAH order was officially lifted on April 30 ($E_{SAH\_inforce\_Alabama}$) in Alabama, and $Exposure\_SAH\_inforce_{Alabama\_May}$ = 12 days and $Exposure\_SAH\_inforce_{Alabama\_June}$ is 43 days, which is the difference between April 30 and June 12.

This measure allows us to differentiate between people exposed for longer and shorter periods of time and associate this with the hours worked or employment status changes. Thus, in this generalized DiD framework, the β coefficients in front of the interaction terms are interpreted as the effect of an additional day's exposure to a certain policy's enaction or removal on labor market outcomes. We also note that for the outcome variable *Employed*, which takes the dichotomous form to indicate an individual's employment status, the regressions have the features of a linear probability model. Because the proportion of the employed in the sample is 61%, the estimation will be accurate, and the implication is the probability of being employed or absent from a job. We also cluster the variance to allow for heteroskedasticity and dependence between observations from the same state.

A key heterogeneity that we add to the model is self-employed people. Because this group of workers face less employment friction, we expect the presence of this variable to account for part of the potential asymmetric effects of a policy's enforcement and lifting. The interaction term $Self\_employed*policy\_dynamic$ measures how they respond to each of the four policy dynamics and indicates a positive response to the NBC lifting; for example, it supports that smaller unemployment friction boosts employment recovery compared to those working for wages. Lastly, we also comment on the advantages and shortcomings of the model. Although there are concerns about multicollinearity when considering multiple NPIs (Kong & Prinz, 2020), the nature of the panel data largely reduces the resulting error, and the span of days over which the different policies were lifted further relieves the problem. In addition, we compare this analysis to a high-frequency estimation outcome to obtain a more robust conclusion.

5.3 Test for a Common Trend

The DiD framework relies on the assumption of a common trend. As we treat the control group as a counterfactual estimation for the treatment group, we need them to exhibit common trajectories if the treatment had not occurred. Unfortunately, such an assumption cannot be perfectly tested because we are unable to observe the trend of a treatment group that had not been treated, which leaves us to test in the pre-treatment period, using the event study method. We examine the common trend assumption for the DiD methods used in the two datasets. We note that the total sample was separated into two windows (February 24 to April 19 and April 8 to June 25), corresponding to the imposition and removal, respectively. This window selection not only covers the time points in each state, but also reserves the longest possible period before and after the policy, to account for the lagged policy effect as much as possible.

First, for the high-frequency Economic Tracker data, we construct a multiple-policy event study regression model for the 21 days before and after the policy implementation date. By regressing the employment levels on the prior time variables and predicting the effect on the post-policy dates, we observe any differences on the labor market prior to the policy's enforcement. The model specifications are as follows:

$$Y_{it} = \sum_{p \in P} \sum_{r=-21}^{21} \alpha_{p,r} \times 1\{\tau p = r\} + \delta X_{it} + \gamma_i + \gamma_t + u_{it} \tag{3}$$

where r is the number of days prior to, or after, the policy enforcement date and $\alpha_{p,r}$ is the interested coefficient that measures the marginal change in the relative employment level for each day before the policy $p \in P = \{SAH, NBC\}$ while controlling for other influential variables. For the common trend assumption to hold, we anticipate that $\alpha_{p,r}$ is insignificant for $r < 0$. γ_i and γ_t are the state and time fixed effects, respectively.

Second, for the CPS data, based on the event study specification of Gupta et al. (2020) on generalized DiD, we expand the model to accommodate the analysis for both the imposition and lifting of policies and the specifications are as follows:

$$\begin{aligned} \Delta Y_{im} = {} & \sum_{\tau=0}^{m} \alpha_r Exposure_{SAH\,im} * M_\tau + \sum_{\tau=0}^{m} \mu_r Exposure_{NBC\,im} * M_\tau + \delta X_{im} \\ & + \gamma_i + \gamma_m + u_{im} \end{aligned} \tag{4}$$

In the model, γ_m is the month fixed effect, capturing the time trend that is invariant across states, and γ_i is the state fixed effect to account for the heterogeneity between states. len_SAH_{im} is the length of the policy until the survey date. It interacts with the month dummy M_r and is added across all the previous months until m, where $\tau \in [0,\ m-1]$ are control months and the m^{th} month is the treatment month. The coefficients of the interaction terms α_r and μ_r measure any difference in the time trend in the r^{th} month of the year. For example, in the case of imposition, the coefficients of the control month show the difference in labor market outcomes between states that will go on to have more versus fewer days of SAH exposure in January, February, and March 2020. We expect the coefficients to be insignificant since the policies, which are at the core of the common trend, had not been implemented in the first three months.

We also comment on the event study coefficients after the treatment (the imposition in April and the lifting in May or June, depending on the state), which theoretically serves as a check on the policy effect. Ideally, we expect the coefficient to be significantly different from zero after treatment.

6 Results

6.1 The Result of State Level Employment

Table 2 summarizes our DiD estimates in the two-way fixed-effect regressions for the policies' impositions and lifting. *SAH_inforce* and *NBC_inforce* are dummy signals for the states and days when the SAH and NBC policies are in force and lift marks the corresponding lifting days.

The first column shows that the imposition of the SAH policy for a state is associated with an employment level reduction of 4.3%, compared to the January level at a 5% significance level. Notably, the change in the average employment level in March and April was only -11.4%, which indicates that around 37.7% of the employment decline is due to the imposition of the SAH policy while controlling for economic activity. In other words, the employment level decreased on average by 4.3% with this policy for all state employees. The NBC policy is indicated to have a milder negative impact of only 1.9% on the overall employment level but is as statistically significant as the previous policy (*t*-ratio= −1.78). We also find that the coefficient of *SAH_lift* shows that there is only a 2% rebound in the relative employment rate led by the lifting of the SAH order, whereas the NBC policy has marginal impact.

Table 2 The Impact of NPIs on the State Relative Employment Level – State Level

	(1)	(2)	(3)	(4)
VARIABLES	Employed	Employed_Low	Employed_Mid	Employed_High
SAH_inforce	−0.0427***	−0.0580***	−0.0426***	−0.0219**
	(−3.29)	(−3.01)	(−3.51)	(−2.18)
NBC_inforce	−0.0189*	−0.0201	−0.0194	−0.0060
	(−1.78)	(−1.27)	(−1.55)	(−0.79)
SAH_lift	0.0204***	0.0218**	0.0219***	0.0131**
	(3.88)	(2.19)	(4.42)	(2.32)

(*To be continued*)

(*Continued*)

VARIABLES	(1) Employed	(2) Employed_Low	(3) Employed_Mid	(4) Employed_High
NBC_lift	0.0090	0.0020	0.0149**	0.0104*
	(1.34)	(0.18)	(2.47)	(1.84)
Spend_all	0.1616***	0.2118***	0.1763***	0.1763***
	(6.36)	(5.44)	(5.51)	(9.66)
Case_count	–0.0000**	–0.0000***	–0.0000**	–0.0000***
	(–2.39)	(–3.14)	(–2.66)	(–3.27)
Constant	0.0164***	–0.0020	0.0196***	0.0299***
	(3.51)	(-0.27)	(3.91)	(8.72)
Observations	6,477	6,477	6,477	5,969
R-squared	0.859	0.858	0.832	0.799
Number of State FIPS	51	51	51	47

Note: Column (1) shows the effect on employment rates for all workers relative to January 4-31. Column (2) shows the effect on employment levels for workers in the bottom quartile of income earners and columns (3) and (4) show the middle two quartiles and the top quartile, respectively. *SAH_inforce* and *NBC_inforce* are dummies signaling the days when the *stay-at-home* and *non-essential business closure* policies are in force, and *SAH_lift* and *NBC_lift* are when they are lifted, respectively. The calendar day fixed effect and state fixed effects are included. Controls are the average daily spending and confirmed cases in each state. Robust t-statistics in parentheses are calculated with state-clustered standard errors; *** p<0.01, ** p<0.05, * p<0.1.

Columns two to four further show the estimation of the policy effect when it is separated by income levels. In terms of the influence of being exposed to the SAH and NBC policies, all three groups were severely impacted by the previous one, with the estimates almost twice the magnitude and with stronger significance levels. In particular, the low-income group suffered a drop in their employment rate of 5.8%, which is the highest among the three groups. The last two columns, however, indicate that the enforcement of the SAH policy caused the employment levels of high- and middle-income employees to drop by 4.2% and 2.19%, respectively, and significantly (p-value<5%) from the January levels. Considering the average total relative employment level of 19.6%, 10.0%, and 5.47% for the three groups in these months, the negative impact of the SAH policy amounts to only 29.6%, 42.0%, and 40.0% in each group, respectively. In contrast, the enaction of the NBC policy does not have any significant impact on the employment rate in any of the income groups.

We then look at the estimates for the three groups when these two policies were cancelled sequentially. Similar to the findings in the imposition case, all the estimates from columns two to four show that the influence of lifting the NBC policy does not have as strong an effect as the lifting of the SAH policy on the employment level recovery for any of the income levels. The magnitudes of estimations are around 1% and the *t*-ratios are only 0.18 for the low-income group. For the SAH policy, the result shows that its removal has a strong impact on employment, despite the impact being lesser when it is imposed. The coefficients in column two are only 2.18% with a p-value > 5%, and the estimates for the low-and high-income employment groups were also insignificant, with values of 2.19% and 1.31%, respectively. Compared to the first row, this recovery only makes up for 37.5% of the harm that low-income earners suffered in April, and this effect is slightly higher for the middle (51.4%) and high- income groups (59.8%).

Our interpretation of the results of the policy dynamics is that the aggregate state employment markets are not resilient as the imposition of SAH has a much quicker and stronger negative effect on employment rates than the positive effect of lifting it. Despite a shorter in-force period after the policy dynamic (around one month), for both general employment rates and income-specific cases, the undermining effects of the NPI are almost twice as large as when it is lifted, which covers approximately two months. We think this is evidence for friction in the employment market, that is, searching and participating in the job market takes time, and we use micro data to further verify the economic reason. Another potential insight is that overall state employment markets are not as sensitive to the NBC policy as to the SAH order. In addition, the lower the income level, the more vulnerable they are to NPIs. The magnitude of the effect on the low-income group is the highest in the imposition period, but the recovery from the lifting of the policy makes up for the smallest part of the harm.

6.2 The Result of Individual Labor Market Response

Using the low-frequency CPS data, the regression results of the analysis for policy imposition and lifting are presented in Table 3. The first column shows the estimates of the baseline model for the entire population, without a special investigation of self-employed people. The coefficient of *Exposure_SAH_inforce* shows that being exposed to the SAH policy for an extra ten days reduces an individual's possibility of being employed by 0.4% which is significant (p-value<1%), whereas this effect is much stronger with the constraints of the NBC policy (0.8%). In addition, the removal of these two policies does not seem to strongly correlate with opportunities of finding employment. For the income segmented estimates, in columns three to five, the effects are of a similar magnitude as the previous results. If anything, we observe a slightly greater impact of the SAH policy on the high-income group (0.5%/ten days) but this is compensated for by an equally lesser effect of the NBC policy (0.6%/ten days).

Table 3 The Effects of Imposing NPIs on Labor Market Outcomes – Individual Level

VARIABLES	(1) Employed_ Total	(2) Employed_ Total	(3) Employed_ Low	(4) Employed_ Mid	(5) Employed_ High
Exposure_SAH_inforce	–0.0004***	–0.0004***	–0.0004***	–0.0004***	–0.0005***
	(–2.95)	(–2.71)	(-2.71)	(–2.94)	(–4.12)
Exposure_NBC_inforce	–0.0008***	–0.0007***	–0.0007***	–0.0007***	–0.0006***
	(–6.51)	(–6.16)	(–6.28)	(–5.93)	(–6.04)
Exposure_SAH_lift	0.0002	0.0002	0.0002	0.0002	0.0002
	(1.12)	(0.97)	(1.05)	(1.01)	(0.91)
Exposure_NBC_lift	–0.0001	–0.0002	–0.0002	–0.0002	–0.0001
	(–0.33)	(–0.85)	(–0.84)	(–0.67)	(–0.33)
Women	–0.0255***	–0.0267***	–0.0267***	–0.0260***	–0.0190***
	(–10.28)	(–10.74)	(–10.92)	(–10.82)	(–9.66)
Self_employed		–0.0279***	–0.0277***	–0.0288***	–0.0193***
		(–5.19)	(–5.21)	(–5.45)	(–5.18)
SAH_inforce_self		–0.0004	–0.0004	–0.0003	–0.0006*
		(–1.28)	(–1.24)	(–0.92)	(–1.92)
NBC_inforce_self		–0.0004	–0.0004	–0.0005	–0.0005*
		(–1.21)	(–1.21)	(–1.53)	(–1.76)
SAH_lift_self		0.0001	0.0001	0.0000	–0.0001
		(0.11)	(0.12)	(0.00)	(–0.17)

(*To be continued*)

(*Continued*)

VARIABLES	(1) Employed_ Total	(2) Employed_ Total	(3) Employed_ Low	(4) Employed_ Mid	(5) Employed_ High
NBC_lift_self		0.0012**	0.0012**	0.0013***	0.0011***
		(2.56)	(2.53)	(2.76)	(2.58)
Constant	0.8301***	0.8341***	0.9139***	0.9154***	0.8806***
	(19.78)	(19.77)	(21.81)	(22.63)	(26.67)
Observations	242,082	242,082	245,269	247,119	293,328
R-squared	0.036	0.037	0.038	0.040	0.039
Number of ids	162,702	162,702	163,846	164,027	176,002

Note: Column (1) is the baseline model, showing the effects on the total employed working individuals without factoring in the self-employed group and column (2) adds interaction terms based on that. Columns (3) – (5) are income-specific models for the low-, middle-, and high-income groups, respectively. *Exposure_SAH_inforce* and *Exposure_NBC_inforce* are the number of days the policies are enforced before the survey day (12^{th}) of each month, and *Exposure_SAH_lift, Exposure_NBC_lift*, are the days after they are lifted . The control variables (education and race) are not listed on the table. Robust t-statistics are shown in parentheses. *** *p<0.01,* ** *p<0.05,* * *p<0.1*

However, when we consider the effect on the self-employed group in column two, we find that throughout the pandemic, this group suffers 28% more than those who work for wages, in finding employment. However, the interaction terms show that among all the policy dynamics, only the lifting of NBC positively affects the re-employment of the self-employed significantly, in the sense that employment is stimulated by the ending of NBC with a 1% recovery per ten-day exposure. Another effect is that when SAH is imposed, the chance of working for the low-income, self-employed people are significantly lower (0.6%) than that of hired workers due to the SAH policy. Our insights from the data are that, in general, self-employed workers are likely to lose their jobs during the pandemic. Few differences in response to social distancing policies are found, except that the removal of the NBC policy largely boosts their employment recovery.

We then consider the estimates in Table 4, which shows that the working duration of those still employed is also more significantly undermined by the

SAH policy instead of the NBC policy. As shown in column two, the weekly working hours of an individual is expected to drop further by 0.40 hours for every ten days after the implementation of the SAH policy, while the estimates for NBC are only 0.15 hours. For those who are self-employed, this negative effect on working hours increases by 32 hours, although strong effects are not attributed to the policy dynamics other than the SAH implementation. In particular, the coefficients for the lifting of the policies were not significant. For the three income groups in columns three to five, the effects of imposing the NBC policy are generally smaller than those of SAH. Similar to the influence on job losses, we find that low-income earners have the greatest reduction in working hours (0.50 hour/ten days) after the SAH, while high-income earners suffered the slightest with a 0.29-hour reduction per ten days.

Table 4 The Impact of NPIs on Individual Working Hours – Individual Level

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Hours_Total	Hours_Total	Hours_Low	Hours_Mid	Hours_High
Exposure_SAH_inforce	–0.0424***	–0.0397***	–0.0496***	–0.0460***	–0.0290***
	(–5.52)	(–5.12)	(–5.45)	(–5.22)	(–3.56)
Exposure_NBC_inforce	–0.0172***	–0.0153**	–0.0115	–0.0191***	–0.0160**
	(–2.78)	(–2.45)	(–1.56)	(–2.65)	(–2.42)
Exposure_SAH_lift	0.0073	0.0104	0.0175	0.0163	0.0107
	(0.71)	(0.96)	(1.37)	(1.32)	(0.95)
Exposure_NBC_lift	0.0018	–0.0066	–0.0091	–0.0104	–0.0134
	(0.15)	(–0.52)	(–0.62)	(–0.71)	(–1.03)
Women	–4.9737***	–5.1262***	–5.3201***	–4.8726***	–5.1599***
	(–39.15)	(–40.44)	(–36.35)	(–33.61)	(–39.05)
Self_employed		–3.2418***	–3.3185***	–2.9767***	–2.4254***
		(–10.39)	(–9.93)	(–8.94)	(–7.59)
SAH_inforce_self		–0.0387**	–0.0306*	–0.0365**	–0.0190
		(–2.25)	(–1.66)	(–1.99)	(–1.07)
NBC_inforce_self		–0.0222	–0.0248	–0.0090	–0.0165
		(–1.36)	(–1.42)	(–0.52)	(–0.98)
SAH_lift_self		–0.0313	–0.0279	–0.0512	–0.0202
		(–0.93)	(–0.77)	(–1.43)	(–0.60)
NBC_lift_self		0.0838***	0.0962***	0.0996***	0.0311
		(3.00)	(3.20)	(3.34)	(1.10)

(*To be continued*)

(*Continued*)

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Hours_Total	Hours_Total	Hours_Low	Hours_Mid	Hours_High
Constant	34.2991***	34.7508***	34.1507***	34.7604***	43.5104***
	(16.85)	(16.95)	(14.57)	(15.08)	(20.20)
Observations	242,082	242,082	210,509	212,707	169,619
R-squared	0.052	0.056	0.063	0.055	0.050
Number of ids	162,702	162,702	148,126	149,926	128,887

Note: Column (1) is the baseline model, showing the effects on the working hours of the whole sample without factoring in the self-employed group and income divisions and column (2) adds the self-employed interaction terms based on that. Columns (3) – (5) are income-specific models for the low-, middle-, and high-income groups, respectively. *Exposure_SAH_inforce* and *Exposure_NBC_inforce* are the number of days the policies are enforced before the survey day (12th) of each month, and vice versa for *Exposure_SAH_lift, Exposure_NBC_lift*, and the lifting days. The control variables (education and race) are not listed on the table. Robust t-statistics are shown in parentheses. *** *p<0.01,* ** *p<0.05,* * *p<0.1*

The estimations not only reinforce the observation of employment from a high-frequency analysis, but also provide more insights on how working hours are influenced by policy dynamics. This labor market measure is mainly harmed by the enaction of the SAH policy during April and the lifting of either policy does not aid recovery. However, unlike the estimates on the proportion of employment, the self-employed group, who face less friction in the labor market, does not respond strongly to the policy changes. This suggests that their labor supply decisions are not directly affected by the social distancing policy.

In contrast to the results in Tables 3 and Tables 4, although we find that self-employed people suffer more in terms of employment and working hours over the course of the pandemic, we find consistent evidence that their employment recovery is positively boosted by the lifting of the NBC policy. This distinguishes the effect on self-employed people from the rest of the population in the sense that they are more prone to the effect of the cancellation of the NBC policy, which could be justified by the nature of their jobs. Second, for self-employed people who face less market friction, it is easier for them to find employment compared to those working for wages when released from the NBC policy.

6.3 The Examination of the Common Trend

Figure 2 presents our study estimates for the two policies using Economic Tracker data. With the sample window from February 24, 2020, to April 19, 2020, Figure 2 suggests that there is not a differential trend in relative employment prior to the announcement of both policies, because all the estimates for 21 days prior to the enforcement dates are statistically insignificant at the 90% level. For the post-treatment estimates, there are clearly stronger downward trends, especially in the first three days, but the estimates present a lagged effect: the employment level does not become significant until 15 days after the policy. According to Goodman-Bacon and Marcus (2020), this could be due to the incubation period of the virus and the lagged economic effect, which led to the short-term post-policy event studies being less likely to capture obvious and prompt changes in the dependent COVID-19 cases reported and, accordingly, the economic outcomes. However, for the post-treatment estimates of the NBC policy, there is no effect on the employment level in the entire 21-day period, which is also consistent with the DiD results in Section 6.1.

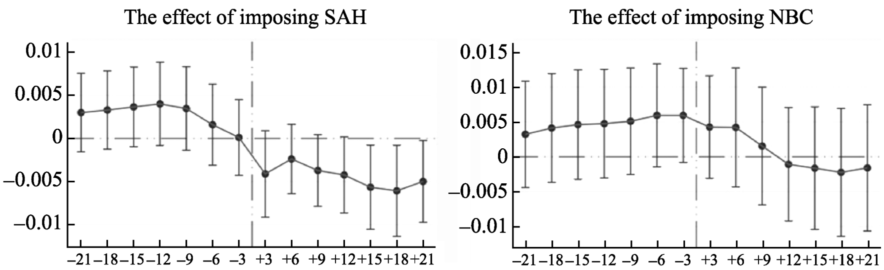

Figure 2 The Effect of SAH and NBC on Employment Levels – State Level

Note: Estimates are the coefficients of employment levels with 90% confidence intervals. The x-axis represents the number of days prior to, or after the event. Regressions include spending, infection levels and their quadratic terms, state fixed effects, and time fixed effects. The sample window is from February 24, 2020, to April 19, 2020, when the NPIs in all the states are enforced. Standard errors are clustered at the state level.

As for the event study estimates using the monthly CPS survey, the results for the imposition are summarized in Table 5 and graphically presented in Figure 3. Panel A of Table 5 does not show a significant difference at a 5% level for both the employment level and working hours before the SAH policy is imposed. However, in the relative first month following the announcement date, April, there is an approximately −0.5% decrease in the probability of finding employment (column one), and a 0.32 decline in hours worked per week in total by employees (column two). In Panel B, the NBC policy does not have a significantly dissimilar trend throughout January to March (as in Figure 3), and the post-policy month is almost unaffected as well. For the analysis of lifting the policies (Table 6), significant estimates are not found prior to the lifting of both policies, which also, on average, indicates the validity of parallel trends for all the states.

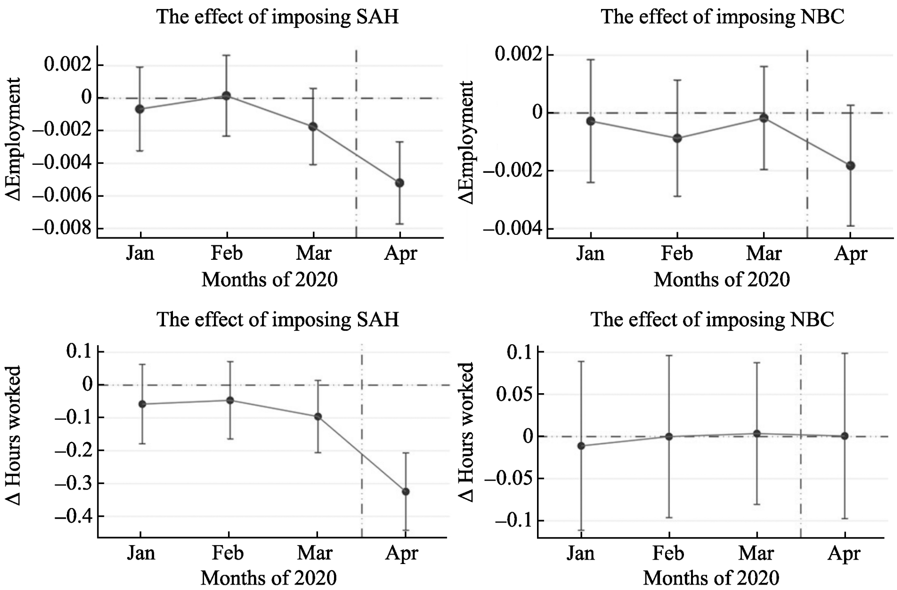

Figure 3 The Effect of SAH and NBC on Labor Market Outcomes – Individual Level

Note: The graphs on the left are the estimated coefficients of the stay-at-home orders on the change in employment levels with 95% confidence intervals, and the non-essential business closure policy on the right-hand side. Regressions include education level, gender, and race. Data on the left of the vertical reference line is before the policy enforcement, and to the right (April) is after certain cumulative days of the policies being active. The sample window was from January 2020 to April 2020. Standard errors are clustered at the state level. The estimation results are presented in Table 5.

Table 5 Parallel Trend Test Result of Imposing NPIs – Individual Level

	(1)	(2)
VARIABLES	Employment	Working hours
Panel A. SAH		
January	–0.001	–0.058
	(–0.50)	(–0.94)
February	0.000	–0.046
	(0.13)	(–0.77)
March	–0.002	–0.096*
	(–1.46)	(–1.71)
April	–0.005***	–0.324***
	(–4.04)	(–5.40)
Panel A. NBC		
January	–0.000	–0.011
	(–0.26)	(–0.22)
February	–0.001	–0.000
	(–0.85)	(–0.00)
March	–0.000	0.004
	(–0.19)	(0.08)
April	–0.002*	0.001
	(–1.71)	(0.01)
Constant	–0.141*	–0.840
	(–1.94)	(–0.27)
Observations	142,218	142,218
R-squared	0.056	0.066
Number of ids	86,089	86,089

Note: Each column summarizes one regression on the change in employment levels, but is presented in two panels, where the effects of SAH and NBC are listed separately. January, February, and March are before the policy enforcement, while April is after certain cumulative days of the policies being active. The sample window is from January 2020 to April 2020. Standard errors are clustered at the state level. Robust t-statistics are shown in parentheses. *** *p<0.01, ** p<0.05, * p<0.1*

Table 6 Parallel Trend Test Result of Lifting the NPIs – Individual Level

	(1)	(2)
VARIABLES	Employment	Working hours
SAH_prior	0.001	0.050
	(1.63)	(1.36)
SAH_crnt	0.001	0.046
	(0.87)	(1.02)
NBC_prior	0.000	0.024
	(0.10)	(0.44)
NBC_crnt	0.001	0.034
	(0.44)	(0.57)
Constant	–0.142	3.001
	(–0.93)	(0.46)
Observations	77,032	77,032
R-squared	0.066	0.076
Number of ids	55,762	55,762

Note: The dependent variable is the change in labor market outcomes estimated by OLS. *Policy_prior* and *policy_crnt* are the interaction terms of the treatment duration variable and month dummies, with both May and June treatments accounted for. Control variables include family and month fixed effects, as well as education, race, and so on. Standard errors are clustered at the individual level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

7 Conclusion

In response to the outbreak of COVID-19 in March, different state governments in the US consistently carried out a series of NPIs, within 21 days, to contain the spread of the virus. Taking advantage of the timing variation across states, we examine the policy effects on the levels of employment and working hours, focusing on contrasting the effects of policy imposition and lifting in two influential NPI policies: the SAH order and NBC.

Our results imply that the implementations of the SAH and NBC policies have quick and significant negative effects on multidimensions of the labor market, whereas the revitalizing effects of removing them are relatively small and slow. On the state-aggregate level for employment rates, within one month of enforcing the policies, the two policies, on average, led to relative employment rate reductions of 4.3% and 1.9%, respectively, compared to January levels. In contrast, lifting the SAH policy only caused a significant rebound of 2.0% in the one to two months that followed and there was not a significant result for lifting the NBC policy. The day-by-day event study further sheds light on the lagged effect of imposing NPIs at around 15 days. At an individual level, the CPS data suggest that the effect of lifting the policies is stronger when the policies were imposed, in the sense that there is a 0.4% (0.8%) increase in employment opportunities for ten additional days' relief from the SAH (NBC) restriction, but not when they are lifted. Similar results were found in the CPS analysis on working hours.

We also gain insights into policy differences, which show that when the dynamics of the polices are active, workers are more prone to the SAH effects than those of NBC. This effect difference between the two policies could potentially be justified economically - SAH directly constrained the labor supply side of the market, which is more closely related to workers' decision, whereas NBC limited labor demand more. Nonetheless, we still find that when it comes to the effects on extensive margins, the dependent variable *hours worked* is more responsive than the dependent variable *employment*, because people could preserve their jobs while their working hours were largely reduced according to Lemieux et al. (2020). This is why the indirect effect of NBC on workers affected the workers' labor supply more, that is, NBC only has a significant impact on working hours, and not on employment.

For the heterogeneity analysis across groups, our estimates suggest that the lower-income groups suffer more job losses from the NPI and have lower recovery rates when the policies are removed. However, for individual working hours, low-income earners are forced to reduce 0.50 hours per ten days after the SAH policy, the greatest drop among all the groups. This means that the low-income group suffers in both dimensions of the labor market measures (percentage of employment and working hours). As for the self-employed group, due to the nature of their jobs, they are more prone to be affected by the NBC policy instead of the SAH policy, during the pandemic. When the NBC policy was lifted, we found that it was easier for them to find employment and to increase their working hours, compared to hired workers, because of less market friction, in line with the findings of Martinez Dy and Jayawarna (2020).

These results provide several policy implications. While social distancing policies are set in attempts to balance health and the economy, our findings suggest that government bailouts or re-employment priorities should give more weight to the low-income group, who are more prone to the harm of policy dynamics and have less resilience to recover as a result of market friction. In addition, in facing the trade-off between the economy and health, and considering policy effect differences, it would be useful to strengthen the part of social distancing that causes less economic harm. This could be done by strengthening the SAH order but relaxing the NBC policy in areas where hired workers are predominantly employed.

We also note some potential limitations of our study. One is that our research, similar to many of the COVID-19 economic studies, may suffer from certain reverse causality problems, in a sense that different job characteristics may influence people's willingness and abilities to comply with social distancing policies (Ge & Zhou, 2020). Another concern is that our investigations on the labor market are limited to the overall employment performance and the labor supply measure of working hours, therefore, research on the demand side is important but lacking. Future studies could justify the differences in the effects of NBC and SAH policies from a labor supply and demand point of view, focusing on variables such as job vacancies or job postings to reveal the effects on the demand side. Nonetheless, this finding provides almost consistent evidence from both macro and micro data, bringing more robust support for policy recommendations when balancing health and the economy.

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