


## RESEARCH ARTICLE

# The intensity of COVID-19 nonpharmaceutical interventions and labor market outcomes in the public sector

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**Abstract**

This paper examines whether the intensity of non-pharmaceutical interventions (NPIs) during the coronavirus disease 2019 (COVID-19) pandemic has differentially impacted the public sector labor market outcomes. This extends the analysis of the already documented negative economic consequences of COVID-19 and their dissimilarities with a typical economic crisis. To capture the intensity of the NPIs, we build a novel index (COVINDEX) using daily information on NPIs merged with state-level data on out-of-home mobility (Google data). We show that among individuals living in a typical state, NPI enforcement during COVID-19 reduces the likelihood of being employed (at work) by 5% with respect to the pre-COVID period and the hours worked by 1.3% using data on labor market outcomes from the monthly Current Population Survey and difference-in-difference models. This is a sizable amount representing the sector with the higher job security during the pandemic. Public sector workers in a typical state are 4 percentage points more likely to be at work than salaried workers in the private sector and 7 percentage points more likely to be at work than self-employed workers (the worst so far). Our results are robust to the endogeneity of the NPI measures and present empirical evidence

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of heterogeneity in response to the NPIs, with those in local employment being the hardest hit.

#### KEYWORDS

coronavirus, COVID-19, employment, essential worker, hours worked, public sector, remote work

## 1 | INTRODUCTION

During a traditional economic crisis in the last decades, the US public and private sector employment follow different paths in terms of the magnitude and timing of declines (Fontaine et al., 2020; Kopelman & Rosen, 2016; Laird, 2017). At least at the beginning, public sector employment (also called government employment) is less sensitive to economic recession. For example, from December 2007 to June 2009, the US government employment grew by 0.8% whereas private sector employment dropped by 6.6% (Fontaine et al., 2020; Goodman & Mance, 2011; Hatch, 2004). In the current coronavirus disease 2019 (COVID-19) pandemic crisis, US public and private sector employment have followed quite similar trajectories during the first wave of the pandemic, when the most restrictive non-pharmaceutical interventions (NPIs) to contain and mitigate the spread of the virus were applied. According to the Bureau of Labor Statistics, almost 5% of the job losses in April 2020 were in government employment (local, state, and federal employment [BLS April 2020]).<sup>1</sup> This is not a minor issue, as almost 1 in 30 public employments were lost in April in a sector that accounted for 14% of the US total nonfarm employment in the pre-pandemic period (February 2020).<sup>2</sup> To our knowledge, there is no prior research on how the NPIs impact public sector employment. We focus here on how the intensity of the NPIs which varied across US states affected public employment outcomes in the early stages of the pandemic and whether there are differences with the private sector.

The COVID-19 pandemic resembles other pandemics in the responses to contain and mitigate the spread of the virus through the use of NPIs such as the closure of schools and nonessential businesses, public gathering bans, and isolation/quarantine (Markel et al., 2007). NPIs restrict social interactions to save lives but they also cause a decline in economic activity through reductions in the demand for goods and services and disruptions in supply and production chains (Bootsma & Ferguson, 2007; Correia et al., 2020; Hatchett et al., 2007). There is a growing body of literature examining the short-run economic consequences of COVID-19 and NPIs (see, for an extensive review, Brodeur et al., 2020) concentrated mainly on employment outcomes (Baek et al., 2020; Béland et al., 2020; Cowan, 2020; Forsythe et al., 2020; Gupta, Montenegro, et al., 2020). For the United States, empirical findings point to the stay-at-home order (which is one of the NPIs) as a possible factor explaining 60% of the decline in the employment rate from January 2020 to April 2020. The rest (40%) should be due to the nationwide shock caused by COVID-19 (Gupta, Montenegro, et al., 2020). Is this really happening in public or private sector employment (or both) during the COVID-19 pandemic? Indeed, with the existent literature, the answer is: We do not know.

Because private sector firms are driven by profit maximization and face different constraints than the public sector, it cannot be immediately deduced that both respond in the same way to the NPIs from a theoretical point of view. Government employment is driven by other objectives, including budgetary targets, stabilization policies, and/or electoral objectives (Fontaine et al., 2020). On one hand, in the short run, the limited flexibility of public sector budgets generates difficulties to reduce public expenditures by, for example, cutting jobs in the face of an income shortfall (Craig & Hoang, 2011). This is especially problematic in the United States, as state and local

<sup>1</sup>[https://www.bls.gov/news.release/archives/empst\\_05082020.pdf](https://www.bls.gov/news.release/archives/empst_05082020.pdf) (Updated July 2020).

<sup>2</sup>[https://www.bls.gov/news.release/archives/empst\\_05082020.pdf](https://www.bls.gov/news.release/archives/empst_05082020.pdf) (Updated July 2020).

governments account for 90% of public jobs (BLS, 2019) and have annual basis restrictive budgets adjusted after September (Goodman & Mance, 2011).<sup>3</sup> Then, since March 2020, in the early stages of the pandemic crisis, the public sector could be less likely to quickly respond to the crisis by reducing jobs than the private sector. On the other hand, governments focus on public good problems whose demand normally increases during economic downturns, thereby generating an increase in government employment necessities without inevitably altering other categories of expenditure in the short-run (Craig & Hoang, 2011). In the current pandemic crisis, the public and private sectors have followed similar trajectories in the United States. We argue here that NPIs play a role in the public sector response to the public health crisis in a different way from what can expect from business cycle fluctuations.

Unlike what happens in an economic crisis, states of emergency were declared in several states and by the federal US government early in March 2020 to quickly respond to the pandemic. States of emergency declarations give access to additional resource and ease employment transitions by relaxing laws and regulations at the local, state, and federal levels in a short space of time, which may positively (e.g., increasing the number of job hires in the health care system) and negatively (e.g., reducing nonessential jobs) affect government employment.<sup>4</sup> With respect to the measures that can be taken to reduce the spread of the virus, NPIs such as school closures may directly damage public sector employment because of the employment composition of the public sector. This is especially pronounced in local and state governments in the United States, as over half of state and local workers are in education-related jobs (Hinkley, 2020). School closures may indirectly affect the employment of US health care workers because 15% of those workers require childcare (Bayham & Fenichel, 2020). The additional childcare burden is not limited to front-line health care workers but also is likely to penalize women (Amuedo-Dorantes et al., 2020; Biroli et al., 2020; Del Boca et al., 2020; Farré et al., 2020; Sevilla & Smith, 2020) who were 8 percentage points more likely to lose their jobs than men in April (Adams-Prassl et al., 2020). This is of interest in the US public sector employment, given that women—especially black women—are over-represented by 3–4 more percentage points (Laird, 2017). Also, NPIs such as the closure of nonessential public service and the lockdowns may negatively affect the level of employment and/or the hours of work of public sector employers who cannot complete their work tasks remotely (Gupta, Montenegro, et al., 2020). Even for the probably positively affected health care jobs in public services in a pandemic, the NPIs should reduce the need for new health care workers and/or more hours of work by alleviating the impact of the pandemic. Our work focuses on testing whether those US states with more intense NPIs, which theoretically appear to negatively affect public sector employment outcomes, are those that are losing more public jobs and/or reducing more hours of paid jobs.

We exploit the large differences in how states in the United States have tried to contain the spread of the COVID-19 virus. The United States is an interesting case study because, unlike other countries, each state in the United States is acting independently. Although the Centers for Disease Control and Prevention (CDC) makes some recommendations, states are free to make their own decisions.<sup>5</sup> It is also an attractive framework to study the response of public sector employment. The US public sector accounted for 14% workers in 2018, which is a sizable proportion in comparison to other sectors and countries (18% for OECD countries varying from 30% in Norway to 6% in Japan in 2015; OECD, 2017).<sup>6</sup> In our analysis, we use individual-level data on employment outcomes and socio-demographic characteristics from the monthly Current Population Survey, from January 2019

<sup>3</sup><https://www.bls.gov/emp/tables/employment-by-major-industry-sector.htm> (Updated July 2020).

<sup>4</sup>Emergency declarations include State of Emergency, Public Health Emergency, and Public Health Disaster declarations. These actions allow flexibility regarding a variety of state laws and regulations; they can vary greatly to include the waiver of procurement rules, the easing of licensing requirements, exceptions to transportation rules, and even the suspension of school attendance laws, as well as many other changes meant to facilitate a more efficient response.

<sup>5</sup>In some cases, there are also variations within the state on how counties/MSAs reacted. We check this in the robustness analysis.

<sup>6</sup>For the last annual data available in the BLS, see: <https://www.bls.gov/emp/tables/employment-by-major-industry-sector.htm> (Updated July 2020). This is not a minor sector in terms of employment in comparison to other sectors. For example, the number of workers in the retail trade is only 10.2% of the total workers or 13% in professional and business services.

to May 2020. We merge this information with a novel index (COVINDEX) that captures the timing and intensity of the NPIs by state and month. Using the assembled data, we estimate a difference-in-difference model aimed at gauging how NPIs affect the labor supply in the public sector and comparing it with the private sector for various population subgroups (heterogeneity analysis). The two employment outcomes of interest are individuals' employment propensity and the actual number of hours worked for those who report working.

Measuring the NPIs in the United States is not straightforward because it matters not only what NPIs are considered but also when those NPIs are applied.<sup>7</sup> To gauge this, we use a novel weighted index, COVINDEX. The definition of this index is an additional contribution of our work to the COVID-19 related literature. We collect daily information on the announcement of five NPIs and their expiration at the state level, if any (state of emergency, school closures, partial business closures, stay-at-home orders, and nonessential business closures). We combine this with the out-of-home mobility data provided by Google (Google LLC, 2020) to capture, in an easy way, the intensity of the NPIs by using one index for state and month. Prior literature on the impact of NPIs on employment considers only an indicator variable focusing mainly on stay-at-home orders and/or business closures (Béland et al., 2020; Gupta, Montenovov, et al., 2020), which is not able to capture the differences across states in the NPI enforcement. We add to this line of research by extending the number of NPIs and better measuring the enforcement of the NPIs in an easy way with one index for state and month.

The adoption of NPIs and the level of effectiveness and compliance could be correlated to the incidence of COVID-19 in a way that may bias the regression estimates. Also, the differences in the NPIs introduced in the states, which can be related to their Democratic or Republican control (Adolph et al., 2021; Allcott et al., 2020), may generate endogeneity concerns. We mitigate these concerns by using event studies and adding controls for the incidence of COVID-19. To provide additional evidence on the validity of our findings, we present an extensive number of robustness checks in the Supporting Information Appendix.

The paper is organized as follows. Section 2 describes the data used in the analysis, and Section 3 presents the empirical specification, including the description of the COVINDEX. Section 4 discusses the main findings, as well as identification and robustness checks. Section 5 presents a heterogeneity analysis and Section 6 concludes.

## 2 | DATA

We use the Basic Monthly Current Population Survey (CPS) from the Integrated Public Use Micro Samples (IPUMS; Flood et al., 2020). This is a household-level monthly survey of approximately 50,000–60,000 households and includes information about everyone in the surveyed households. The CPS is the main source of labor force statistics for the population in the United States, sponsored by the US Census Bureau and the BLS. Several papers examine the short-term impact of COVID-19 on US employment with the CPS (Béland et al., 2020; Gupta, Montenovov, et al., 2020) because it provides enough observations to obtain reliable estimations. COVID-19 impacted the data collection with the response rate dropping by 10 (March), 13 (April), and 15 (May) percentage points in 2020 with respect to the same months in 2019.<sup>8</sup> The concern about possible sampling error problems is mitigated by the BLS. That agency claims that this data set is “still able to obtain estimates that met [their] standards for accuracy and reliability.”<sup>9</sup>

<sup>7</sup>All the NPIs were taken in a short space of time (a few days or even on the same day). Disentangling the impact of each of the NPIs on employment outcomes is tricky because the impact of each of them depends on the prior NPIs applied. Because we do not have daily data, we consider that monthly intensity indices can better capture the effect of NPI enforcement on monthly employment outcomes.

<sup>8</sup>The BLS releases supplementary information about the impact of the COVID-19 pandemic: <https://cps.ipums.org/cps/covid19.shtml> (Updated July 2020).

<sup>9</sup>See Employment Situation Summary BLS of March, April, and May: <https://www.bls.gov/bls/news-release/empsit.htm>. (Updated July 2020). The data collection method was also affected because all interviews had to be conducted by telephone. Our results prove robust to controlling for whether the interview was done in-person or telephone (see Supporting Information Appendix B, Table B1).

For our main sample, we use data spanning from January 2019 through May 2020. This time period allows us to perform event studies assessing the exogeneity of the COVINDEX with respect to public employment outcomes, as well as to control for the seasonality of the data by including month fixed effects. Our main sample is restricted to working-age (18–64), noninstitutionalized civilians whose current or most recent job is in the public sector according to the type of ownership of the employing organization, as in Laird (2017).<sup>10,11</sup> It excludes those in the armed forces because CPS does not include military personnel. Our main sample consists of 129,502 individuals, representing 14% of salaried private/public and self-employed workers from the January 2019 through May 2020 period examined here.

## 2.1 | Labor market outcomes

We pay attention to two labor market outcomes in the main analysis: the respondent's employment status as captured by the variable *employed*, which takes the value 1 if the respondent reported being *at work* with a main job in the public sector and 0 otherwise (if he/she has a job but *did not work last week* and if he/she is *unemployed or not in the labor force* with his/her more recent main job being in the public sector).<sup>12</sup> We focus on those employees *at work* to mitigate a possible nonsampling error problem that can bias upward the possible negative impact of NPIs on employment propensity generated by the misclassification of some individuals in the category furlough absent from work. BLS analysis of the CPS data in March, April, and May suggests that this group included workers affected by the pandemic response who should have been classified as unemployed on temporary layoff.<sup>13</sup>

For those respondents at work during the last week, we examine, at the intensive margin, the number of actual hours worked in the public sector when this is their main job.<sup>14</sup> Although individuals could be working in secondary jobs in the public sector, we must limit our analysis to the main job because the use of the actual hours worked in all jobs may generate a measurement error problem. The CPS provides information on the class of worker by the type of ownership of the employing organization (salaried public, salaried private, or self-employed worker) only for the main job.<sup>15</sup> The hours worked are in logarithm in the main analysis.

For the labor outcome variables considered here, respondents are usually asked to report about them by the CPS for the 7-day calendar week (Sunday–Saturday) that includes the 12th of the month (or the 5th in the case of December).<sup>16</sup> Considering this, the March CPS refers to early March before the onset of the pandemic in most of

<sup>10</sup>Our sample does not include minors because the Fair Labor Standards Act (FLSA) sets specific wage, hours worked and safety requirements for minors (individuals under age 18), and this can vary at the state level. We probe the robustness of our findings to this sample selection including individuals aged 16–64 in Table B2 in Supporting Information Appendix B.

<sup>11</sup>According to the technical documentation of the CPS, current job is the job held in the reference week (the week before the survey) for employees and self-employed workers. Workers with multiple sources of employment were classified according to the job in which they worked the most hours. Respondents who were not employed during the previous week reported the most recent job. The unemployed are classified according to their latest full-time job lasting two or more weeks or by the job (either full-time or part-time). The most recent job is also reported by persons not in the labor force who are in the 4th and 8th months in the sample and who have worked in the last 5 years.

<sup>12</sup>Main job is the job at which the person usually works the most hours. If a person usually works the same number of hours at two jobs, the "main" job is the job at which the person has been employed the longest.

<sup>13</sup>People who indicated that they were under quarantine or self-isolating due to health concerns should be classified under "own illness, injury, or medical problem" in the category did not work at all during the survey reference week. People who were not ill or quarantined but said that they did not work last week "because of the coronavirus" should be entered as "on layoff (temporary or indefinite)." However, a significant part of them were included in the category did not work last week because "other reasons." For example, in April, if workers who were recorded as employed but absent from work due to "other reasons" had been classified as unemployed on temporary layoff, the overall unemployment rate would have been almost 5 percentage points higher than reported. See Employment Situation Summary BLS of March, April, and May.

<sup>14</sup>We trim the extremes without those below the 1st percentile (working less than 5 h in the public service) and above the 99th percentile (working over 70 h per week) as in Béland et al. (2020). Our findings are maintained without its exclusion, see Table B3 in Supporting Information Appendix B.

<sup>15</sup>We have showed the robustness of our findings by considering hours worked in all jobs and also by limiting the sample to individuals who do not spend time at second jobs, see Table B4 in Supporting Information Appendix B.

<sup>16</sup><https://www.census.gov/programs-surveys/cps/technical-documentation/methodology/collecting-data.html>.

TABLE 1 Summary statistics

Descriptive statistics by type of ownership of the employing organization					
	Jan. 2019-Feb. 2020	March 2020	April 2020	May 2020	All period
<b>Public sector</b>					
% Employed (at work)	92.60	91.93	84.60	88.88	91.87
% Employed (Did not work last week)	4.49	5.17	6.00	3.64	4.57
% Unemployed	2.12	2.00	8.27	6.70	2.75
% Out of labor force	0.78	0.90	1.13	0.78	0.82
Mean work hours	38.64	38.72	38.05	38.29	38.59
Sample size	109,183	7056	6769	6494	129,502
<b>Private sector</b>					
% Employed (at work)	92.90	91.08	76.80	80.34	91.17
% Employed (did not work last week)	2.50	3.12	5.65	4.10	2.81
% Unemployed	3.73	4.87	15.66	14.04	5.06
% Out of labor force	0.86	0.93	1.89	1.52	0.97
Mean work hours	38.58	38.26	37.99	38.12	38.51
Sample size	576,385	35,698	33,295	32,201	677,579
<b>Self-employed workers</b>					
% Employed (at work)	92.22	89.59	72.21	77.85	90.5
% Employed (did not work last week)	5.03	7.00	18.59	14.14	6.48
% Unemployed	2.04	2.36	7.63	7.02	2.68
% Out of labor force	0.71	1.05	1.58	0.99	0.79
Mean work hours	38.37	37.22	35.30	36.00	38.05
Sample size	71,034	4517	4356	4211	84,118
<b>COVINDEX</b>					
Number of states with a nonzero index	0	34	51	51	
Mean	0.000	-0.014	-1.448	-1.592	
SD	(0.000)	(0.034)	(0.392)	(-0.588)	

Note: Weighted percentages are presented. The sample is restricted to individuals aged 18–64. Sector refers to the respondent's job at the time of the survey if the respondent is employed. For those who are unemployed or out of the labor force, sector refers to the respondent's most recent job. Number of states with the COVINDEX different from zero by the day 12th of each month. The COVINDEX range from -2.67 to 0.055.

the US states and, so, to the application of hard NPIs. As can be seen in Table 1, which displays the composition of the labor force by sector for pre-COVID (January 2019 to February 2020) and post-COVID periods (separately for March, April, and May), the disparities in labor outcomes are much more evident in April (the percentage of individuals being at work is significantly reduced by 8.6% in the public sector and the work hours by 1.5%) and there is not much of a change in March 2020 in the employment outcomes.<sup>17</sup> In any case, the drop observed here is sizable in comparison to the percentage of people employed in the public sector during the period 2003–2013 which is above 92%, even including the last Great Recession (Laird, 2017). May shows a significant recovery of 4 percentage points with respect to April in the percentage of individuals at work, whereas the hours worked changes slightly, which can be related to the reopening process. This pattern is maintained in all the worker

<sup>17</sup>Table A1 in Appendix A shows when the differences are statistically significant.

categories (salaried public, salaried private, or self-employed worker). The main difference is observed in the category having a job but absent, which significantly increased in April for self-employed workers. This is also due, in part, to how the CPS processes data. The “on layoff (temporary or indefinite)” response option is not available for business owners who have no other job, so they are included in the category furlough absent from work for “other reasons” when they do not have a job. Because some authors suggest the importance of learning about those individuals having a job but being absent (Montenovo et al., 2020), we have incorporated this into the secondary analysis when separating the sample by the class of worker category. We define a variable, *did not work last week*, for whether an individual has a job but did not work last week. In any case, this should be taken with caution because of the aforementioned misclassification of some workers in the category of having a job but being absent rather than unemployed.

## 2.2 | The intensity of NPIs

In this study, we explore which part of the changes in the labor outcomes in the public sector can be explained by the intensity of NPIs. There are variations in the space, scale, and timing of the NPIs as well as their effectiveness and compliance across US states. In any state, individuals may be exposed to a multiplicity of NPIs which make no straightforward analysis of each of the NPIs individually (Gupta, Montenovo, et al., 2020). For this reason, we build a weighted index to gauge the intensity of the NPIs using daily information on the announcements of five NPIs, and their expiration, if any (state of emergency, school closures, partial business closures, stay-at-home orders, and nonessential business closures), and daily real-time data on the mobility of the population.<sup>18</sup> We start by calculating the number of days in which the NPIs have been applied each month. This daily information is weighted by the estimated average impact of each NPI on mobility using data from the COVID-19 Google Mobility Reports (Google LLC, 2020).

Real-time data on mobility have been used in the last months to learn about the COVID-19 pandemic (Chetty et al., 2020; Gupta, Nguyen, et al., 2020; Painter & Qiu, 2020). The effectiveness of NPIs in containing the spread of the virus by reducing social interactions is strongly supported in this pandemic (Amuedo-Dorantes et al., 2021; Badr et al., 2020; Dave et al., 2020; Prem et al., 2020) with a 9% reduction in the daily case growth rate after the implementation of stay-at-home orders and restaurant closures in the United States (Courtemanche et al., 2020). Although much of the literature concentrates on the impact of stay-at-home orders on mobility, there is some evidence of the early impact of several NPIs finding that the estimated average effect of state-of-emergency declarations resulted in approximately a 10% reduction in mobility away from places of residence; each of the additional partial closures resulted in an additional 25% reduction, and the stay-at-home orders add a 29% drop until March 29 (Wellenius et al., 2020). We extend this analysis by separately examining this issue for each state as follows:

$$Mobility_{ist} = \sum_{i=1}^{MC} \sum_{j=1}^N (\beta_{ijs} NPI_{jst}) \theta_i + \delta_m + \theta_i + \varepsilon_{ist}, \quad (1)$$

where  $Mobility_{ist}$  measures relative changes (in percentage) in the total number of visitors of the category of mobility  $i$  ( $i = 1, \dots, MC$  [ $MC = 5$ , five categories of mobility]) in state  $s$  in Day  $t$  with respect to the baseline,

<sup>18</sup>Emergency declarations include state of emergency, public health emergency, and public health disaster declarations. Partial business closures incorporate partial closures without specifying the closures of nonessential business, such as the restriction or limitation of restaurants, casinos, gyms, fitness centers, and entertainment venues, among others. Nonessential business closures are mandates to close all nonessential businesses. Stay-at-home orders refer to mandates for individuals to stay at home for all nonessential activities. These definitions come from Fullman et al. (2020). We combine several sources of information (for the NPI announcements: Fullman et al. (2020), Education Week, and the National Governors Association [NGA]; for the reopenings: NGA, The New York Times, and Johns Hopkins Coronavirus Resource Center). See Table E2 in the Data Appendix for detailed information. As for robustness, we have calculated the index at the county level with the same NPIs (National Association of Counties [NACo], Education Week, Fullman et al., 2020, and NGA) with the dates of the reopening being established at the state level. See Table B5 in Appendix B, which maintains our findings. It should be noted that in the case of county data, we have limited the sample to counties with at least 200 observations for all mobility categories to obtain reliable estimations. This reduces the number of counties with available data on mobility from 2829 to 2749.



which is the median value during the 5-weeks from January 3 to February 6, 2020 for the corresponding day of the week.<sup>19</sup> We considered the five out-of-place-of-residence categories of mobility provided in the Google Mobility Reports (retail and recreation, grocery and pharmacy, parks, transit stations, and workplaces) for the period February 15–May 12, 2020.<sup>20</sup>  $NPI_{jst}$  is a dummy variable that takes the value 1 since state  $s$  declares a NPI  $j$  in day  $t$  until its expiration date, if any, with  $j$  varying from 1 to  $N$ , being  $N$  the maximum number of NPIs applied in state  $s$ .<sup>21</sup>  $\theta_i$  is a set of mobility fixed effects that are meant to capture fixed differences in the level of outcomes across categories of mobility that are stable over the study period.  $\delta_m$  is a set of month fixed effects, which capture trends in the outcome that are common across all categories of mobility such as seasonality.  $\varepsilon_{ist}$  is the error term. We run this regression for the 50 states plus the District of Columbia. Our estimates are based on a panel of categories of mobility by state, which allows us to have enough observations (440 for each state) to run reliable estimations.  $\beta_{ijs}$  are the coefficients of interest. Those coefficients capture the average effect of each NPI on the relative change of visitors for each category of mobility. Those  $\beta_{ijs}$  that are statistically significant at least at the 10% level are used to calculate the COVINDEX. These estimations are based on the idea that human mobility patterns show a high degree of temporal and spatial regularity with the pandemic prevention/containment being able to break the inherent mobility patterns (González et al., 2008). In any case, our estimates can be considered as a lower bound of the average negative effect of the NPIs on mobility because partly could be explained by voluntary changes in behavior due to the evolution of the pandemic (Gupta, Nguyen, et al., 2020).<sup>22</sup>

An additional caveat of the data set is that reports use data from individuals who have opted-in to Location History for their Google Account, so the data represents a sample of their users, not the whole population.<sup>23</sup> Additionally, it may not be able to account for people who spend time near a location. To lessen possible sampling error problems, Google shows only reports on regions from which statistically significant data is obtained.<sup>24</sup>

Following the recent literature that developed indices weighted by the exposed population on policy enforcement (Amuedo-Dorantes & Lopez, 2015; Amuedo-Dorantes et al., 2018), we calculate the COVINDEX by considering the timing of the NPIs weighted by the average estimated effect of the NPIs on the relative change in the total number of visitors to five sets of locations. The COVINDEX of state  $s$  in a given month  $m$  is

$$COVINDEX_{sm}^{2020} = \sum_{i \in MC} \sum_{j \in N} \frac{1}{D} \sum_{d=1}^D 1(NPI_{i,jd}) W_{ij}, \quad (2)$$

with MC being the categories of mobility.  $W_{ij}$  equals  $\hat{\beta}_{ijs}$  when the estimated effect of the NPI  $j$  on the category of mobility  $i$  is statistically significant at least at the 10% level, and 0 otherwise.<sup>25</sup> This is measured as a relative

<sup>19</sup>This data is obtained from <https://www.google.com/covid19/mobility/>.

<sup>20</sup>Retail and recreation covers visits to restaurants, cafes, shopping centers, theme parks, museums, libraries, movie theaters, and similar locations. Grocery and pharmacy covers markets, food warehouses, farmer markets, specialty food shops, drug stores, and pharmacies. Parks covers public beaches, marinas, dog parks, plazas, and other public spaces. Transit stations covers subway stops and bus and train stations. Workplaces covers places of work. A residential category is not included in the analysis because it differs from the other categories in the unit of measure. The residential category shows a change in duration, see <https://support.google.com/covid19-mobility/answer/9825414?hl=en>.

<sup>21</sup>We have considered announcement dates because the exact date of the announcement is supposed to be exogenous, although there are no significant differences in the date of the implementation, only a gap between 1 and 3 days (Gupta, Nguyen, et al., 2020). We revisit this issue below.

<sup>22</sup>Some works suggest that all 50 states (plus D.C.) display similar quantitative pattern previous to the declaration of stay-at-home orders (Farboodi et al., 2020; Gupta, Nguyen, et al., 2020). They point to voluntary changes in the mobility patterns. We recognize that voluntary changes in the mobility could partly explain the decrease in the mobility but it could be more plausible that at the early stages of the pandemic (second-third week of March) before the declaration of the stay-at-home orders, the state-wide implementation of state of emergency, school closures, and partial business closures could drive that similar pattern in the mobility. We have also introduced a sub-index in the main regression to control for the possible impact of the evolution of the pandemic by using the daily information on the announcement of first COVID-19 death in each state and results are robust to the inclusion of this control. See Supporting Information Appendix B, Table B6.

<sup>23</sup>According to the Kantar Worldpanel ComTech, US sales of Android, the Google system for mobile devices, exceed 50% of the total sales in the United States. This should mitigate concerns about the representativeness of the data used. See <https://www.kantarworldpanel.com/global/smartphone-os-market-share/> (Updated July 2020).

<sup>24</sup>There are 3 days without data in the category parks in Delaware and 1 in Idaho.

<sup>25</sup>In the unlikely scenario that NPIs cause no physical interactions in a specific location,  $i \sum_{j \in N} W_{ij}$  should reach the value  $-100$ .



change in the total number of visitors to the five sets of locations, which can be interpreted as the portion of the population exposed to the NPI, as in the case of policy enforcement indices.  $NPI_{ljd}$  is an indicator function that takes value one because NPI  $j$  is implemented on day  $d$  of month  $m$  until its expiration, if any, whereas  $D$  is the total number of days in month  $m$ . To ensure the NPIs refer to the same period for which the CPS was collected, each month expands from the 13th of the month to the 12th of the next month. To easily interpret the estimated coefficients in the ulterior regressions, we divide this index by 100.

The COVINDEX is, by definition, bounded below to a value of  $-5$  when, as a consequence of the NPIs, there are no visitors to the five sets of locations in state  $s$  (maximum enforcement) during the entire month  $m$ . When the COVINDEX takes the value of  $0$  (no enforcement), this means that NPIs are not implemented or, if declared, the NPIs have no statistically significant effect on the categories of mobility (no change in the social interactions). Negative values should be interpreted as a reduction of social distancing. The more intense (effective) the NPIs are at reducing social interactions, the closer the value that the COVINDEX is to  $-5$ . The COVINDEX can take positive values when at least one of the NPIs encourages social interaction (this happens when the total number of visitors exceeds that of the baseline period as a consequence of the NPI implementation) and none of the other NPIs has statistically significant effects or, if significant, they cannot compensate for the estimated positive effect.<sup>26</sup>

As shown in Table 1, the COVINDEX over the post-COVID period (March, April, and May 2020) averaged  $-1.02$  and fluctuated between  $-2.6$  and  $0.05$ .<sup>27</sup> To provide a sense of the evolution of the NPI enforcement during the post-COVID period, Panels A–C in Figure 1 show the evolution of the COVINDEX over that time. Lighter colors correspond to lower levels of NPI enforcement (higher levels of the COVINDEX means low effectiveness of the NPIs in reducing social interactions) in each state and month. Enforcement levels in the United States have multiplied by a hundred during this period. In the first month of our post-COVID sample, 17 states had a COVINDEX equal to zero (i.e., no enforcement). Among the rest, 23 have a COVINDEX lower than zero (reducing social interactions) and 11 had a COVINDEX higher than zero (encouraging social interactions). By April and May, all states had an NPI enforcement index lower than  $0$ . In addition, the COVINDEX for most states decreased over time (the greater the intensity, the lower the COVINDEX). It decreased for all states from March to April and for 36 states from April to May.<sup>28</sup> Although some states started with relatively highly intense NPIs in comparison to the rest of the states, the states with a higher intensity of NPIs (lowest COVINDEX) are concentrated mainly in the North-East, East-North Central, and Pacific (California).

Depending on the use of the location places (recreation, shopping, business, transportation...), one can foresee a differential impact of the NPIs examined on the labor outcomes. This can be problematic in the case of the category of mobility park, which appears to respond in a different way to the NPIs. For example, after the school closures, a positive and statistically significant impact of this measure in this category of mobility is observed in 20 states. Although the higher the mobility in parks, the lower the effectiveness of the NPIs, it can be argued that this simply reflects the closure of other out-of-home places. Then, the higher the mobility in parks, the lower the employment and the hours worked. To address this issue, we have built our COVINDEX without the category Park and the results are maintained (see Table B7 in Supporting Information Appendix B).<sup>29</sup>

<sup>26</sup>This happens only in March, when, for instance, the emergency declarations appear to increase mobility to shops and school closures appear to increase the visitors to parks and groceries. Because human mobility patterns show a high degree of temporal and spatial regularity, the COVINDEX should not exceed the value  $0$  without NPIs implemented. That is what we obtain. The maximum value determined here is  $0.05$ .

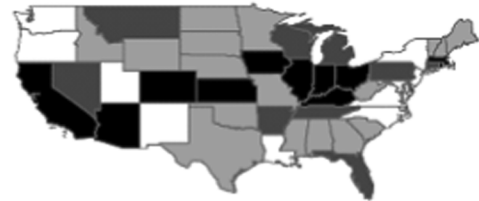
<sup>27</sup>The greater differences across states are observed in May. We obtain only positive values in March but not in April and May; see Figure A2 in Supporting Information Appendix A. States with positive values in March are Alaska, Louisiana, Maryland, New Jersey, New Mexico, New York, North Carolina, Oregon, Utah, Virginia, and Washington.

<sup>28</sup>The COVINDEX increases from April to May for Alabama, Alaska, Georgia, Iowa, Maine, Minnesota, Montana, Nebraska, Nevada, North Dakota, Ohio, South Carolina, Texas, Utah, and Wyoming.

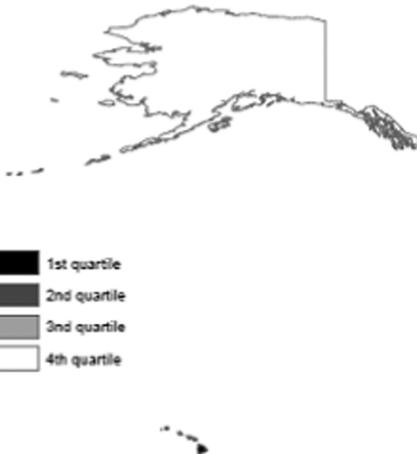
<sup>29</sup>The Pearson correlation of both indices is  $0.87$ . We have also tested whether the decrease in mobility has a different effect on the labor outcomes depending on the category of mobility considered. We have used the minimum value for each category of mobility and run regressions on the labor outcomes of interest. The results are presented in Table C1 in Appendix C. We observe the same direction in the relationship. We have also checked this using the average minimum drop in mobility by state and month considering all categories together, see Panel F in Table C1 in the Appendix. Once again, the direction of the relationship is maintained, which gives us confidence in including all categories of mobility together in the COVINDEX.



(a) 13th February to 12th March (2020)



(b) 13th March to 12th April (2020)



(c) 13th April to 12th May (2020)



**FIGURE 1** Geographic variation in the COVINDEX over time. Lighter colors correspond to lower levels of NPIs enforcement (higher levels of the COVINDEX means low effectiveness of the NPIs on reducing social interactions) in each state and month

### 3 | EMPIRICAL STRATEGY AND IDENTIFICATION

Our objective is to explore the extent to which NPIs, adopted to curb the spread of COVID-19, paused public sector employment, and to examine whether the public sector was more/less affected than the private sector. We analyze heterogeneous impacts across different groups by race, gender, and level of education and the channels through which the NPIs are more likely to be operating in job traits. We examine fluctuations both in the propensity of being employed (at work) and in hours worked (measured in logarithm), in this last case for individuals reporting working during the reference week. Our model exploits the temporal and geographic variation as well as the differences in the intensity of the NPIs to identify their impact on labor outcomes as follows:

$$Y_{ismt} = \alpha + \beta \text{COVINDEX}_{sm}^{2020} + X_{ismt} \gamma + \delta_t + \varphi_m + \theta_s + \varepsilon_{ismt} \quad (3)$$

with  $Y_{ismt}$  being the labor outcome of interest, that is, whether individual  $i$  is employed (or the logarithm of weekly hours worked) in state  $s$ , month  $m$ , and year  $t$ . The variable  $\text{COVINDEX}_{sm}^{2020}$  is the index capturing the intensity of the NPIs measured in terms of the duration of the NPIs and weighted by the estimated share of the population that changes mobility patterns as a consequence of the NPIs at the state and month levels in 2020. All specifications include demographic characteristics ( $X_{ismt}$ ) known to affect the labor force status, including gender, age, educational attainment, marital status, and whether there are children in the household.<sup>30</sup> When focusing on those reporting to be employed, the vector  $X_{ismt}$  also includes controls for the occupation held.<sup>31</sup> Additionally, it includes a set of state and time (year, month) fixed-effects ( $\delta_t$ ,  $\varphi_m$  and  $\theta_s$ ) that control for unobserved factors potentially affecting employment outcomes.<sup>32</sup>  $\varepsilon_{ismt}$  is the error term. Data Appendix explains in detail how these variables are defined.

We focus our attention on the coefficient  $\beta$ , which captures the impact of the COVINDEX on the labor outcomes of the public sector. It can be surmised that this coefficient can be biased due to the unlikely random declaration (and expiration, if any) of NPIs throughout the United States. The spread of the COVID pandemic as well as the party affiliation of governors of US states and the division of their partisans can be factors that determined the implementation of the NPIs (Adolph et al., 2021; Allcott et al., 2020; Gupta, Nguyen, et al., 2020).<sup>33</sup> For instance, states with more employees in the public sector could delay or even avoid the closure of some activities due to fear of decreasing their future revenues.<sup>34</sup> If that were the case, the estimated impact of the COVINDEX might be downward biased. While recognizing likely nonrandom adoption of NPIs, what matters here in terms of inference purposes is the possible endogeneity with regard to the outcomes of interest. To tackle this issue, we conduct an event study for each of the labor market outcomes being examined. This allows us to gauge whether any impact of NPIs on labor market outcomes in the public sector predated the adoption of

<sup>30</sup>Results prove robustness to the exclusion/inclusion of these controls.

<sup>31</sup>For the propensity of being at work, occupation might be correlated with the error term because, for example, the same unobservable characteristics that affect occupational choice in the public sector also influence the likelihood that an individual will lose his/her job (Gittleman & Pierce, 2012). Being aware of this possible problem, we did not include these controls in the analysis of the propensity of being employed, although we include them in the case of hours worked. To probe this further, we have run simple robustness checks, adding/deleting occupation controls, and the results do not vary, see Appendix B Table B8.

<sup>32</sup>Table B9 in Appendix B presents the estimates after including state by year and month controls. The results are quite similar.

<sup>33</sup>As mentioned above, to assess whether we partly capture voluntary changes in the mobility patterns of individuals with our COVINDEX, we have controlled for the evolution of COVID-19 by adding an indicator variable for the first COVID-19 death by state to calculate the weights of the COVINDEX. Our findings are not undermined.

<sup>34</sup>It is also possible that access to some funds, such as rainy day funds, can affect the decision to cut jobs. To lessen this possible concern, we have repeated the analysis, controlling for rainy day funds as a percent of general fund expenditures in 2019, see Table B10 in Appendix B. Similar results are found when the sample is separated between the 10 states with the largest 2019 rainy day funds and the rest of states (Wyoming, Alaska, North Dakota, New Mexico, Texas, West Virginia, California, Vermont, Connecticut, and Oregon). Data comes from the Pew Charitable Trust. Pew's analysis is based on data from "The Fiscal Survey of the States," which the National Association of State Budget Officers publishes each fall and spring (<https://www.pewtrusts.org/en/research-and-analysis/articles/2020/03/18/states-financial-reserves-hit-record-highs>). We have incorporated region fixed effects in these regressions to control for unobservable characteristics at some geographical level.



effective (in terms of mobility) policies (Goodman-Bacon & Marcus, 2020). Because our empirical strategy is based on mobility pattern changes in the share of the population affected by the NPIs, we conduct an event-study model that defines the leads as the periods before the COVINDE<sub>X</sub> first turned to a nonzero value. The lags are interacted with the COVINDE<sub>X</sub> to capture the intensity of NPIs, using a similar methodology to those works that consider continuous treatment variables (Clemens et al., 2018; Goodman-Bacon, 2018). Formally, the event-study model is defined as follows:

$$Y_{ismt} = \sum_{r=-15}^{-2} \tau_r 1\{t^m - t_e^m = r\} + \sum_{r=0}^2 \rho_r \left[ 1\{t^m - t_e^m = r\} \cdot \text{COVINDE}_{sm}^{2020} \right] + X_{ismt} \gamma + \delta_s + \varphi_m + \theta_t + \varepsilon_{ismt} \quad (4)$$

where  $Y_{ismt}$  is the outcome for individual  $i$  in state  $s$ , month  $m$ , and year  $t$ . Pre/post-event is defined by dummy variables  $1\{t^m - t_e^m = r\}$  that measure the time ( $t^m = 1$  [January 2019], ...,  $t^m = 17$  [May 2020]) relative to the  $\text{COVINDE}_{sm}^{2020}$  first turned to a nonzero value ( $t_e^m$ ). The reference period in all event studies is the period before the event occurred (in our case when the index first turned to a nonzero value), when  $r = -1$ . We examine the existence of pretrends by coefficients  $\tau_r$ . The coefficients  $\rho_r$  measure the dynamics of NPI effects, and are interacted with the  $\text{COVINDE}_{sm}^{2020}$  to capture the intensity effects. The rest of the variables are defined as in Equation (3). The length of the event-time “window” is not so long in comparison to those papers using data since 2015 or 2016 (Béland et al., 2020) to avoid bias in the coefficients because of the composition change of groups in the pre-event.<sup>35</sup>

To further test whether adoption of the NPIs is not endogenous to the labor market outcomes of interest (reverse causality), we also examine whether our outcomes of interest can predict the number of days that elapse from the first death until the first NPI. To that end, we collapse our data at the state level and use, as the dependent variable, the number of days between the first COVID-19 death in the United States and the first NPI in each state. Formally:

$$\text{DaysuntilfirstNPI}_s = Y_s \vartheta + \rho_R + \varepsilon_s \quad (5)$$

The vector  $Y_s$  represents the average level of labor outcomes (namely, the employment rate in the pre-COVID period (January 2019–February 2020), or hours worked in logarithm) in state  $s$ . The model includes fixed effects,  $\rho_R$ , for each of the nine US regions (i.e., New England, Middle Atlantic, East North Central, West North Central, South Atlantic, East South Central, West South Central, Mountain, Pacific) because we cannot include state fixed effects.  $\varepsilon_s$  is the error term.

## 4 | LABOR MARKET IMPACTS OF NPIs

### 4.1 | Main findings

Table 2 presents the estimation of Equation (3), which shows the impact of NPIs on public sector labor outcomes.<sup>36</sup> We observe that the increase in the intensity of the NPIs (the lower the values of the COVINDE<sub>X</sub>, the higher the intensity of NPIs) that occurred from March 2020 did significantly affect the public sector labor outcomes, through a reduction in the propensity of being a public sector employee and a drop in the hours worked. Among individuals living in a typical state with the average COVINDE<sub>X</sub> (−1.02, post-COVID), the propensity of being at work significantly falls by 5% with respect to the pre-COVID period.<sup>37</sup> Meanwhile, the weekly hours of work decreased

<sup>35</sup>A long window might be problematic in the case of the US public sector because the last Great Recession has significantly lagged the private sector in recovery. Total public sector employment did not recover its 2008 levels until 2019 (Hinkley, 2020).

<sup>36</sup>All coefficients incorporated in the specifications can be seen in Appendix C in Table C2.

<sup>37</sup>Indiana has a COVINDE<sub>X</sub> that is quite close to the average. This state passed the five NPIs, with out-of-home mobility decreasing on average to a minimum of −4% in March, −41% in April, and −32% in May.

**TABLE 2** Main results

Dependent variable	(1) Employed	(2) Log (work hours last week)
COVINDEX	0.046*** (0.004)	0.013*** (0.004)
Observations	129,502	116,022
R <sup>2</sup>	0.041	0.106
D.V. Mean 01/2019–02/2020	0.93	3.61
For all		
Month FE	Yes	Yes
State FE	Yes	Yes
Year FE	Yes	Yes

*Note:* The sample in all columns includes public employees (current job or most recent job) between 18 and 64 years old. We estimate Equation (3). All regressions include a constant, as well as demographic controls for age, gender, marital status, parental status, and educational attainment. We also control for the type of occupation in Column (2). Please refer to Table E1 in the Data Appendix for a detailed description of each variable. The sample in Column (1) is civilian, not institutionalized individuals from January 2019 to May 2020 Monthly CPS data. The sample in Column (2) are individuals who report being at work during the prior week. Estimates are weighted using CPS weights. Robust standard errors are clustered at the state level and reported in parentheses.

\*\*\*Significant at the 1% level.

by only 1.3% for those at work. NPIs appear to mainly lower the propensity of being an employee (at work) in the public sector but the hours worked (for a sample of individuals working pre/post-COVID) does not change much.<sup>38</sup>

An aforementioned concern with the results reported in Table 2 refers to the possibility that our coefficient of interest, which captures the impact of the COVINDEX, might be biased due to the unlikely random implementation of the NPIs. To mitigate this, we examine whether any impact of NPIs on labor market outcomes in the public sector predated the adoption of effective NPIs in terms of mobility (Goodman-Bacon & Marcus, 2020). We made a generalization of the model presented in Equation (3) that can reveal biases caused for reverse causality and/or voluntary precautions (Goodman-Bacon & Marcus, 2020), see Equation (4). Event-study estimates are presented in Table 3. There is little evidence of significant differential pre-trends. All estimates for the months before the COVINDEX first turns into a nonzero value (which happens when NPIs are effective in changing mobility patterns) are not statistically significant and close to zero, especially in the case of hours worked, strongly supporting the assumption of no pretrends.<sup>39</sup> Postevent coefficients also capture variations in the intensity of the NPIs, the break (evaluated in the average value of the COVINDEX), as in the case of the estimates shown in Table 2, appears to be clearer in the case of employment than in the case of hours worked. In that case, we do not observe a coefficient statistically different from zero in the last period (2 months after the event). The rest of the postevent coefficients are all statistically different from zero, even 1 and 2 months after the NPI enforcement was effective.

<sup>38</sup>Because we are using state-level data, it can be argued that some local orders (which, in some cases, imposed more restrictions) are driving our estimations. However, our findings do not vary when we include Metropolitan Statistical Area fixed effects in Table B11. Similarly, results are unchanged when we use the COVINDEX calculated at the county level in Table B5, as we mention before. To explore possible regional differences, we have rerun estimates in Table 2, separately, for each of the four US major regions (the Northeast, the Midwest, the South, and the West) in Table B12. The estimated coefficient on the COVINDEX results appear to be statistically significant and with a similar impact in all regions with the exception of that capturing the impact of weekly work hours which, albeit positive, appear to be more imprecisely estimated in the Northeast region.

<sup>39</sup>Similarly, considering together all the pre-event months also reveals nonstatistically significant coefficients and quite close to zero, which again reinforces our findings on the nonexistence of pretrends, see Table C4 in Appendix C.

TABLE 3 Event study

Dependent variable	(1) Employed	(2) Log (work hours last week)
15 months before the event	0.239 (0.253)	0.045 (0.146)
14 months before the event	0.216 (0.232)	0.043 (0.134)
13 months before the event	0.205 (0.212)	0.017 (0.124)
12 months before the event	0.232 (0.218)	0.030 (0.125)
11 months before the event	0.213 (0.199)	0.039 (0.119)
10 months before the event	0.182 (0.183)	0.056 (0.113)
9 months before the event	0.214 (0.163)	0.077 (0.105)
8 months before the event	0.218 (0.143)	0.084 (0.093)
7 months before the event	0.137 (0.119)	0.073 (0.079)
6 months before the event	0.114 (0.102)	0.070 (0.061)
5 months before the event	0.099 (0.079)	0.052 (0.048)
4 months before the event	0.085 (0.062)	0.037 (0.036)
3 months before the event	0.058 (0.044)	0.011 (0.019)
2 months before the event	0.028 (0.023)	−0.006 (0.012)
The month of the event × COVINDEX	0.051* (0.009)	0.026** (0.012)
1 month after the event × COVINDEX	0.055* (0.010)	0.012*** (0.007)
2 months after the event × COVINDEX	0.042** (0.016)	0.0003 (0.009)
Observations	129,502	116,022
R <sup>2</sup>	0.043	0.106
D.V. Mean 01/2019–02/2020	0.93	3.61

**TABLE 3** (Continued)

Dependent variable	(1) Employed	(2) Log (work hours last week)
For all		
Month FE	Yes	Yes
State FE	Yes	Yes
Year FE	Yes	Yes

*Note:* The sample in all columns includes public employees (current job or most recent job) between 18 and 64 years old. We estimate Equation (4). All regressions include a constant, as well as demographic controls for age, gender, marital status, parental status, and educational attainment. We also control for the type of occupation in Column (2). Please refer to Table E1 in the Data Appendix for a detailed description of each variable. The sample in Column (1) is civilian, not institutionalized individuals from January 2019 to May 2020 monthly CPS data. The sample in Column (2) are individuals who report being at work during the prior week. Estimates are weighted using CPS weights. Robust standard errors are clustered at the state level and reported in parentheses.

\*Significant at the 1% level.

\*\*Significant at the 5% level.

\*\*\*Significant at the 10% level.

The possible reverse causality, because of the potential endogenous nature of NPIs with respect to the public sector labor market outcomes considered here, can also be checked by modeling the timing of NPIs as a function of the state's public-sector activity before COVID-19 (January 2019 to February 2020). This allows us to examine whether, while possibly nonrandom, the NPI adoption can be predicted by our labor outcomes of interest. The results on the estimation of Equation (5) are shown in Table C3 of the Appendix. As can be observed, there is significant evidence to reject the notion that the timing of NPIs is explained by the pre-COVID employment rate of those reporting working/or last work in the public sector or the average weekly worked hours for those at work in the public sector. We feel comforting with our findings because all this empirical evidence suggests that the adoption of the NPIs, while likely nonrandom, does not appear to be correlated with the labor outcomes that our work examines.

Because the impact of the NPIs is not limited to the public sector, we wonder whether those salaried workers in the public sector (the group examined here) are differentially affected by the NPIs as compared to those working for a wage in the private sector or/and self-employed workers.<sup>40</sup> This is interesting due to the traditional job security of the public sector jobs during an economic recession (Farber, 2010). The results are reported in Table 4, classifying the Panels by class of worker (public, private, and self-employed). As explained above, the propensity of being at work in the public sector significantly decreases by 5% with respect to the pre-COVID period for individuals residing in a typical state. This reaches the 9% of reduction for those in the private sector and 12% in the case of self-employed workers. For the weekly worked hours of those at work, the reduction was 1.3% in the public sector, which is not statistically different from the reduction in the private sector (1.6%). The self-employed workers appear to be significantly in danger, considering the average COVINDEX by a 6%. This is in line with the works suggesting that self-employed workers have been particularly hit not only in the United States but also in other countries such as the UK (Adams-Prassl et al., 2020; Blundell & Machin, 2020; Kalenkoski & Pablonia, 2020). Adams-Prassl et al. (2020), using a real-time data survey, find that employees in salaried jobs are 6 percentage points less likely to lose their jobs relative to nonsalaried employees as a consequence of the COVID-19 pandemic. Unlike in an economic crisis, the public sector

<sup>40</sup>Self-employed workers have been considered separately because there is some prior evidence suggesting that those workers are hardly hit during the pandemic because of the work activities in which they are involved, such as construction (Kalenkoski & Pablonia, 2020). Additionally, because self-employed workers are more likely to have control of their work hours as compared to wage-salary workers, self-employed workers can easily reduce their work hours, decide to telework, or stop working to provide childcare (Kalenkoski & Pablonia, 2020).



**TABLE 4** Results by type of ownership of the employing organization

Dependent variable	(1) Employed	(2) Did not Work Last Week	(3) Log (Work Hours Last Week)
<i>Panel A: Public employees</i>			
COVINDEX	0.046*** (0.004)	-0.015*** (0.003)	0.013*** (0.004)
Observations	129,502	125,051	116,022
R <sup>2</sup>	0.041	0.049	0.106
D.V. Mean 01/2019–02/2020	0.93	0.05	3.62
<i>Panel B: Private employees</i>			
COVINDEX	0.082*** (0.005)	-0.020*** (0.002)	0.016*** (0.002)
Observations	677,579	639,454	605,353
R <sup>2</sup>	0.93	0.03	0.129
D.V. Mean 01/2019–02/2020	0.93	0.03	3.61
<i>Panel C: Self-employed workers</i>			
COVINDEX	0.108*** (0.007)	-0.080*** (0.007)	0.060***
Observations	84,118	81,466	69,705
R <sup>2</sup>	0.051	0.036	0.099
D.V. Mean 01/2019–02/2020	0.92	0.05	3.57
For all			
Month FE	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Note: The sample in all columns includes individuals between 18 and 64 years old. We estimate Equation (3).

All regressions include a constant, as well as demographic controls for age, gender, marital status, parental status, and educational attainment. We also control for the type of occupation in Columns (2)–(3). Please refer to Table E1 in the Data Appendix for a detailed description of each variable. The sample in Column (1) is civilian, not institutionalized individuals from January 2019 to May 2020 Monthly CPS data. The sample in Column (2) are individuals currently employed. In Column (3) we use those individuals in Column (2) who report being at work during the prior week. Estimates are weighted using CPS weights. Robust standard errors are clustered at the state level and reported in parentheses.

\*Significant at the 10% level.

\*\*Significant at the 5% level.

\*\*\*Significant at the 1% level.

appears to behave in a pro-cyclical way during this pandemic but it still maintained significantly higher job security than did those salaried private employees and especially self-employed workers.

Up to now, we have left out the analysis of how NPIs impact workers who report having a job but being absent. Being aware of the problematic misclassification of some workers in this category, we have extended our work to this issue because other works consider their analysis important (Montenovo et al., 2020). It should be noted that, in this case, comparison among categories (salaried vs. self-employed workers) is tricky and we avoid it here because, for self-employed workers who are business owners without having another job,

the “on layoff (temporary or indefinite)” response option is not available according to the CPS criteria (BLS June 2020).<sup>41</sup> They are included in the category having a job but not at work for reasons related to the coronavirus, which can explain the huge previously described differences observed in the raw data, Table 1. For individuals living in a typical state having applied NPIs with an intensity equal to the average COVINDEK in the post-COVID period, the propensity of having a job but being absent increased by 30% among salaried public sector workers, 68% among the private sector, and over double in the case of self-employed workers with respect to the pre-COVID period.

## 5 | HETEROGENEOUS IMPACTS

### 5.1 | Gender, race, and education

To explore the differential effects of NPIs on the public sector labor outcomes of different subgroups of the population, we repeat our analyses, separating the sample by gender, race, and educational attainment. This allows us to study whether the historical role of the public sector in the United States as an equalizing institution (Laird, 2017), through job opportunities for minority workers, especially black people and women, has been altered as a consequence of the pandemic. What literature shows is that, in the aftermath of the last Great Recession, the job security of public sector work appears to be substantially reduced for minority workers (Laird, 2017).

Cuts to the public sector workforce might be of particular concern for women, who traditionally have been less exposed to job losses than men during an economic recession. During the pandemic, prior evidence on real-time surveys points to the opposite. Women are hardly hit (Adams-Prassl et al., 2020), in part because they are more penalized by some of the NPIs, such as school closures which increase the necessity of childcare (Biroli et al., 2020; Del Boca et al., 2020; Farré et al., 2020; Sevilla & Smith, 2020). Table D1 in Appendix D shows the results after differentiating between men and women. Not surprisingly, among women living in a typical state with the average COVINDEK (−1.02), the propensity of being employed (at work) in the public sector falls by 5.6%, whereas among men by 4.4% relative to the pre-COVID period while the opposite occurs in the case of worked hours. The labor impact of NPIs appears to occur on a double side, reducing the propensity of being at work and hours worked conditional on working (1.5% vs. 1.2%), though there are no statistically significant differences between men and women in the public sector. It is interesting, however, that men and women in the public sector are statistically less heavily hit than their counterparts in private and self-employed works. The magnitude of the impact of the NPIs on the gender differential response is lower than observed during the pandemic; only in April was being a woman associated with a 3.3 percentage point increase in layoff rates relative to men according to Montenegro et al. (2020). This may indicate that other reasons apart from the intensity of NPIs are driving the different responses to the COVID-19 pandemic among men and women.

The double punishment of the NPIs on the public sector labor outcomes is also observed among white and black individuals but not among individuals of other races, for which the readjustment appears to occur only through a reduction in the propensity of being at work, see Table D2 in Appendix D. It is interesting to see that in an equalizing institution such as the public sector, where all races have a similar propensity of being at work in the pre-COVID period, the pandemic has not broken this trend. The apparent dissimilarities in the reduction of their propensity of being at work relative to the pre-COVID period (5.8% vs. 4.7%) between black and white are not statistically different. This lack of a significant difference is also true regarding hours worked (1.9% vs. 1.2%). In any case, this is sizable in comparison to the estimated drop of 4% in the likelihood of being employed in the public

<sup>41</sup>[https://www.bls.gov/news.release/archives/empsit\\_07022020.pdf](https://www.bls.gov/news.release/archives/empsit_07022020.pdf) (Updated July 2020).

sector from 2008 to 2011 among black individuals but also for white individuals, for which the estimated drop was around 1% (Laird, 2017). Black self-employed workers were three times (only two times in the case of white people) more likely to report not being at work than those in the public sector relative to the pre-COVID period in a typical state (5.8% vs. 18.7%), which may suggest higher job security for this collective in the public sector.

We also observed notable statistically significant differences between people with and without a university education. Labor outcomes of low educated individuals (less than a college degree) have been the largest hit so far by the NPIs regardless of being a salaried worker in the public/private sector or a self-employed worker, Table D3 in Appendix D. This is in line with the findings of Adams-Prassl et al. (2020). What is again noticeable is that those in the public sector are again less affected by the NPIs, especially among those with some college or more (the largest group [81%] of workers in the public sector), who are almost three times less likely to report not being at work than self-employed workers, and the NPI impact on hours worked is almost 0.

In sum, NPIs appear to doubly impact the labor situation of men and women without differences in the public sector. The impact of the NPIs is not more remarkable among minorities, but it is remarkable among the low educated in the public sector. In any of the subpopulations considered, workers in the public sector are less heavily hit by the NPIs in comparison to salaried workers in the private sector or self-employed workers (the worst so far). This heterogeneity may indicate that workers in the public sector are in occupations with more opportunities for remote work or that they work in essential jobs. We revisit this issue in the next section.

## 5.2 | NPIs and job traits

Once we have analyzed whether some of the well-known labor market inequalities caused by characteristics of the population can be exacerbated by the NPIs, surely policymakers would be concerned about how NPIs differentially hit the three levels of the US public sector (federal, state, and local government). Two huge sticking points arise: the composition of the employment at each government level and government funding. Although the objectives of the stimulus packages passed under the US state of emergency (March 13th) are focused on *protecting the American people from the public health and economic impacts of COVID-19* (US Department of the Treasury), the vast majority of local governments (cities and counties) have received inadequate or no federal aid, according to the NACo and the NLC.<sup>42</sup> The over \$2 trillion economic relief package operating under the Coronavirus Aid, Relief, and Economic Security (CARES) Act (March 27th) established a \$150 billion Coronavirus Relief Fund (CRF) for state, county, and municipal governments with populations of more than 500,000 people to use for necessary expenditures incurred due to the COVID-19 public health emergency, but not to fill shortfalls in government revenue to cover expenditures that would not otherwise qualify under the statute. Less than 5% of the counties were eligible for direct payments and nearly 70% of cities have not yet received funding through the CARES Act, with 24 states without any plan to allocate the CRF to local governments by June 2020.<sup>43</sup>

With regard to our analysis, because local governments appear to be incurring unbudgeted COVID-19-related expenditures with no or inadequate funding and supporting a large public workforce (payroll, retirement, and workers compensation account for nearly half of city budgets in 2017, according to the NLC), we would expect that NPIs' intensity hard-hit our outcome of interest at this level of government.<sup>44</sup> This is what we find in Table D4 in

<sup>42</sup><https://home.treasury.gov/policy-issues/cares>.

<sup>43</sup>See: <https://www.naco.org/resources/counties-matter-covid-19> (Updated July 2020). Data at the city level come from a survey conducted between June 8 and June 16, 2020 in a total of 1117 cities, towns, and villages from all 50 states, the District of Columbia and Puerto Rico. <https://www.nlc.org/sites/default/files/users/user52651/CAE-Local-Impact-Survey-One-Pager.pdf>.

<sup>44</sup>[https://covid19.nlc.org/wp-content/uploads/2020/06/What-Covid-19-Means-For-City-Finances\\_Report-Final.pdf](https://covid19.nlc.org/wp-content/uploads/2020/06/What-Covid-19-Means-For-City-Finances_Report-Final.pdf) (Updated July 2020).

Appendix D; local employee appears to be doubly hit. Their propensity of being at work declined by 6.3% in a typical state (with the average COVINDEX) with respect to the pre-COVID period, whereas by only 3.7% and 4% among state and federal employees, respectively. Only local employees reduce the worked hours by 2.1% (conditional on working). In contrast, our estimates for state and federal employees are close to zero and not statistically significant, suggesting no change.

It is also arguable that much of the distinct impact of NPIs on the public sector, in comparison to salaried private and self-employed workers, is probably driven by the particular traits of government jobs if, for example, many of their workers are in essential activities (such as public health services), or/and if there are more possibilities for remote work in the public sector (education). As a first step to examine this, Table D5 in Appendix D accounts for the type of jobs held by the CPS's respondents. We split the sample between essential/nonessential workers.<sup>45</sup> Not surprisingly, a less dramatic picture is observed with those classified as essential for all salaried public/private and self-employed workers. It is interesting, however, that NPIs do not appear to have heavily impacted essential workers in the public sector, with three (two) times less probability of not being at work than self-employed workers (salaried private workers) and, for those working, with a negative impact (albeit almost zero) in their worked hours. Similarly, once again, non-essential workers appear to have great job security in the public sector at least to be at work, with salaried private employees and self-employed workers nearly doubling the reduction of the propensity of not being at work. In any case, we recognize that this should be taken with caution because the classification of essential workers does not distinguish between the public/private sector and the composition of the public workforce differs from that of the private sector.

To deeply explore whether part of the public workforce is being hardly hit, we distinguish between workers in Health Care and Social Assistance, Education, and Others.<sup>46</sup> An additional job descriptor is added to this analysis, which has played an important role during the pandemic: occupations that are allowed to telework or are not.<sup>47</sup> We divide the sample between this characteristic (telework) and then incorporate interactions of our variable of interest (COVINDEX) with the industries mentioned above. Table D6 in Appendix D presents the results. The most notable finding, although not unexpected, is the lack of a significant (and close to zero) impact of NPIs on labor outcome variables for those who are not able to telework in Health Care and Social Assistance inside the public sector. Then, the different intensity in the NPIs is not driving the evolution of Health Care and Social Assistance workers who cannot telework, at least in the public sector (which is expected to be on the front line of the pandemic). In contrast, in Education, those who are not allowed to telework are double hardly hit in the propensity of being at work and in the worked hours in the public sector. In other industries, the readjustment is observed only in the propensity of being at work but not

<sup>45</sup>The official industry guidelines issued by the Department of Homeland Security through the Cybersecurity and Infrastructure Security Agency (CISA) provided advisory guidance to identify the critical infrastructure sectors and essential workers. However, the CISA classification (without any official codification) cannot be easily merged with the detailed Industry Classification Codes of the CPS. For this reason, we opt for the classification of essential workers of two states—Pennsylvania and Delaware (this information is provided by the NGA)—that use the official NAICS codes which can be easily matched with the CPS Codes using BLS equivalence for the years 2019 and 2020. We define essential workers as those working in an industry classified as essential by both states, and as nonessential otherwise. We admit likely measurement error because not all states use the same classification of essential workers, but this is a much more precise way of determining essential industries than a possible subjective partial classification made manually from the CISA.

<sup>46</sup>Although the rest of the industries represent a lower percentage of the public sector labor force than those considered here (Education and Health Care), they constitute a substantial part of the private sector labor force. Because some of them, especially tourism, have been hardly hit by the NPIs in the pandemic, state and/or local governments can predict a higher negative effect in their future income revenues if many of their workers are involved in jobs related to, for example, tourism, which surely can affect their decisions on cutting jobs. To check whether having an industrial composition focused on tourism matters, we have tested whether our results are maintained after including controls for the pre-COVID importance of the tourism labor force in each state in Table B13. We add the proportion of individuals employed in "Hotel, motel, and resort desk clerks" and "Tour and travel guides" during the pre-COVID period at the MSA level (this allows us to maintain the state fixed effects). Our results do not change.

<sup>47</sup>We classify the feasibility of working at home (telework) for all occupation categories following the classification of Dingel and Neiman (2020), merging the Standard Occupational Classification (SOC) codes and the CPS occupational codes with the equivalence provided by the BLS in 2019 and 2020.

in the hours worked. Again, those in the public sector are less heavily hit or similarly hit (no significant difference from zero) regardless of the industry and ability to telework.

## 6 | CONCLUSIONS

Unlike recent economic recessions in the United States, the public sector has quickly responded to the pandemic crisis by furlough and/or temporarily laid off their workers, behaving similarly to the private sector. This is doubtless by simply looking at the raw data on the evolution of employment in the public/private sector; however, it is not so clear to what extent the distinct intensity of the NPIs, implemented to contain the spread of COVID-19, has hit the public sector labor outcomes. In this work, we document that the NPI enforcement in a typical state during COVID-19 reduces the likelihood of being employed (at work) by 5% with respect to the pre-COVID period and the hours worked by 1.3%. Despite being hit, we have shown evidence pointing to the public sector as still maintaining job security for salaried employees because the propensity of being at work is significantly reduced by 4 percentage point less than in the case of wage-salaried workers in the private sector and by 14 percentage points less than among self-employed workers with respect to the pre-COVID period.

To determine these findings, we have had to develop a novel index (COVINDEX) that allows us to easily gauge the intensity of the NPIs by using one intensity measure for each state and month. Because NPIs have varied in space, scale, and time, we have used high-frequency-real-time data on mobility from Google to proxy the intensity of NPIs without basing our findings on the assumed homogeneity and effectiveness of NPIs. The COVINDEX is an aggregate weighted index of the daily average effect of each NPI on the relative change of the total number of visitors in five sets of locations out of the place of residence.

Although the adoption of NPIs can likely be nonrandom, we feel confident in our estimates because we do not observe significant evidence of the existence of pre-COVID trends by conducting event-study models on our outcomes of interest. Additionally, pre-COVID differences across states in the labor outcomes of interest do not appear to predict the date on which the NPIs are adopted. We also further probe our results with an extensive amount of robustness.

In terms of heterogeneity analysis, NPIs appear to doubly impact (on employment propensity and hours worked) the labor situation of men and women without significant differences between both of them. The impact of the NPIs is more remarkable among the lower educated in the public sector but no differences are observed between white and black people. In all cases, regardless of the demographic traits or level of education, workers in the public sector appear to be less heavily hit than other salaried workers in the private sector and self-employed workers (the worst so far).

We document the impact of NPIs on the public sector workforce only in the short run. Admittedly, it is too early to determine whether the negative consequences of the pandemic crisis on the public sector workforce will be lasting and/or whether the economic stimulus packages can play a mitigating role in the long run. The CRF could be used only for necessary expenditures incurred due to the COVID-19 public health emergency, but not to fill shortfalls in government revenue to cover expenditures that would not otherwise qualify under the statute. It appears that the CARES Act affected public sector employment because without that economic stimulus package, state and local governments would have laid off an additional 401,000 workers—40% more workers than realized in April 2020 (Green & Loualiche, 2021). This could explain, in part, the higher job security in the public sector observed in our paper in response to the NPIs' intensity, with those public sector workers in Health Care and Social Assistance (at the front lines of the pandemic) who cannot telework being the only ones for which the intensity of the NPIs has had no effect. However, in June 2020, government employment was still 1.5 million below its February level, representing a decline of 7% (BLS June 2020).<sup>48</sup> While there were substantial job gains in several sectors (retail trade,

<sup>48</sup>[https://www.bls.gov/news.release/archives/empsit\\_07022020.pdf](https://www.bls.gov/news.release/archives/empsit_07022020.pdf) (Updated July 2020).

education and health services, other services, manufacturing, and professional and business services), this was not observed in the public sector (BLS June 2020). This pattern of late recovery in public sector jobs is still observed despite the nearly \$3 trillion that has been made available by various federal government agencies to assist state and local governments in their ongoing response to and recovery from COVID-19 (NACo February 2021). A new economic stimulus package is expected to be approved (Coronavirus Relief Package with Essential Pandemic Aid for Frontline Workers and Assistance) but it is not clear whether this is enough to lessen the impact on public sector employment of the huge budget shortfall (around \$360 billion at the local government level (which is the most significantly affected) and \$1 trillion if one considers the rest of the government levels for the year 2020 according to the NLC). If the adjustment in public budgets takes place with the destruction of public sector employment, this could lead to more pronounced negative effects of the NPIs on the private sector in the long run. The public sector crisis can reduce the multiplier effect of public employment, especially in the tradable sector (Faggio & Overman, 2014; Jofre-Monseny et al., 2020). Decreasing the number of public employees living in a city may decrease the demand for services such as housing and restaurants, which, in turn, reduces private employment, although this might be partly compensated for by decreases in local wages and prices that might follow the public employment reduction. It is also true that the framework under which US local and state governments are operating is much more flexible (with early responses on public employment), which could, in part, improve their possible budget constraints. This is not happening in other countries (such as Spain) at least as badly hit as the United States by the pandemic where there has been no readjustment in public employment.

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## DATA AVAILABILITY STATEMENT

Our manuscript includes data that are publicly available.

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## SUPPORTING INFORMATION

Additional Supporting Information may be found online in the supporting information tab for this article.

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