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The effect of income support programs on job search, workplace mobility and COVID-19: International evidence

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A B S T R A C T

This study examines the effect of Income Support Programs (ISPs) on job search effort, workplace mobility, COVID-19 cases, and mortality growth rates. To identify ISPs’ causal effect, I use the variation in their introductions’ timing across countries and implement a difference-in-difference and multi-event analysis method. I find that ISPs led to a 4.4–8.29 percentage points reduction in workplace mobility and a 6.6–11.6 percentage points reduction in job search effort levels. They also caused a 21.8–47.7 and 171–29.7 percentage points reduction in the COVID-19 case growth rate and COVID-19 mortality growth rates, respectively. Using the event analysis estimates, I simulated the counterfactual job search effort, workplace mobility, and the number of COVID-19 cases and mortality without income support programs. The average global job search effort and workplace mobility without ISPs would have been 11.12 and 9.26 percent higher than the observed mean job search effort and workplace mobility. However, these would have come at the cost of 3.69 million and 166,690 additional COVID-19 cases and mortality than the cases and deaths registered by May 15th.

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1. Introduction

To contain the spread of the novel coronavirus disease of 2019 (COVID-19), policymakers introduced social distancing measures. Because compliance with these measures depends, among other things, on families’ ability to buffer against COVID-19 caused income loss, policymakers across countries designed interventions that can mitigate the economic consequence of the pandemic while increasing compliance with the social distancing measures. One of the policy responses is providing income support through the expansion of social insurance programs. These programs were expanded by changing the eligibility criteria and the generosity level of unemployment insurance programs, introducing new programs such as paycheck protection program loans through stimulus packages, incorporating cash transfer programs, or pairing existing and new social safety-net programs (Gentilini et al., 2020). While critics argue that these programs cause an efficiency loss due to reduced job search effort and labor supply, proponents justify these programs based on their consumption smoothing benefit. However, in the presence of a contagious respiratory disease, these programs’ benefits can go beyond consumption smoothing. To the degree they increase unemployment spell through workers reduced job search effort or workplace activities, social insurance programs can reduce workplace contacts and contain the virus’s spread in the community. This paper evaluated the trade-offs between the job search effort and workplace activities versus the COVID-19 cases and mortality growth rates effects of Income Support Programs (ISPs).

To identify their causal effects, I exploited the variation in the timing of the expansion of ISPs across countries and implemented a difference-in-difference and an event analysis method. Because countries expanded social insurance programs in a short time interval with other contemporaneous social distancing measures, identifying their causal effect using a single event analysis method is challenging. To trace out the trends leading up to the change in contemporaneous policies and partial out their effect, I used a multi-event analysis method with country and time fixed effects.

The study combines data from the Oxford university COVID-19 Government response tracker (OxCGRT), Google community mobility reports, and Google ‘job’ search trends from 178 countries between February 15–May 15th. First, I examined the effect of ISP on “job” search effort. Following Baker and Fradkin (2017) and Aaronson et al. (2020), I use information from the Google search engine that tracks the topic ‘jobs’. Although not all results are precise at the conventional level of statistical significance, I found that ISPs led to a 6.6–11.6 percentage points reduction in search for the topic ‘jobs’ at Google from the pre-pandemic baseline search level. These effects should represent the upper bound of the actual effect of ISP on "job" search effort since the reduction in "job" trend
at google.com could also be caused by the destruction of jobs, lower expectation of finding a job, or a higher risk of working in a pandemic.

Evaluating the "job" search effort effect of ISPs using google trend data, especially for samples that include developing countries, might bias their actual effect for at least two reasons. First, potential workers in developing countries might engage in traditional job search methods such as personal networks, newsletters, and other media. Second, a significant portion of workers in emerging and developing economies engage in self-employment and might not need to engage in the "job" search effort. To overcome these two challenges, I supplement the results on job search effort with additional analysis of ISPs' effect on workplace mobility (labor supply). To capture the effect of labor supply, I use the new Google community mobility report that tracks individuals’ activities in workplaces using their cell phone and other devices. I found that ISPs reduced workplace mobility in a range of 4.4–8.29 percentage points from the pre-pandemic baseline workplace mobility at a 5% level of statistical significance. The workplace mobility data does not differentiate whether these reductions in workplace mobility come from employers' decision to layoff or furlough (not to reopen) or workers' decisions to reduce labor supply or work from home. Therefore, the estimates on the workplace activities effects of ISPs also at best represent the upper bound workplace activity reduction effect if there is any of ISPs caused by workers’ reduced effort.

I also examined the effect of ISP on workplace mobility, ‘job’ search, COVID-19 cases, and mortality by the generosity of ISP and the country’s level of development. The COVID-19 cases and mortality effect of ISP is more substantial in countries that replaced more than 50% of lost income and are more advanced than others. However, the workplace mobility effect of ISP is more substantial in countries that replaced less than 50% of the lost income and are less developed than other countries. On the other hand, the "job" search effort effect of ISP is higher in countries that replaced more than 50% of the lost income and are less developed countries than others.

After establishing the effect of ISPs on “job” search and workplace mobility, I analyzed their effect on COVID-19 case and mortality growth rates. To reduce the noise from the daily case and mortality data, I binned national level COVID-19 cases and mortality at a week level. I found that ISPs led to a 21.8–47.7 percentage point reduction in the COVID-19 case growth rate three weeks after their introduction. They also caused a 17.1–29.7 percentage point reduction on the COVID-19 mortality growth rate five weeks after their introduction. Finally, using the event analysis estimates, I generated the counterfactual job search effort, workplace mobility, COVID-19 case and mortality growth rates, and cumulative COVID-19 cases and mortality. I found that without ISPs, job search effort and workplace mobility would have been 2.61 and 2.29 percentage points higher than the observed job search efforts and workplace mobility reductions, implying that these programs explain 11.12 and 9.6 percent of the mean job search effort and workplace mobility reductions occurred since the beginning of the pandemic. On the other hand, these programs averted 3.69 million additional COVID-19 cases and 166,690 additional COVID-19 mortality by May 15th. These estimates are equivalent to 89.1%, and 59% additional COVID-19 cases and mortality avoided through these programs by the end of the sample period.

This article's findings contribute to three major strands of literature. The first literature is the job search effort and the spillover effect of social insurance programs. Previous studies documented the job search effort (Baker and Fradkin, 2017; Kroft and Notowidigdo, 2016; Rothstein, 2011), the consumption smoothing (Gruber, 1994) and the credit risk (Hsu et al., 2018) and the self-reported health status (Kuka, 2020) effects of these programs. My paper extends this literature by examining the "job" search effort, workplace mobility, and health trade-offs of ISPs (another social insurance program) in the presence of contagious respiratory disease.

This paper also contributes to the growing literature on the effect of governments' policy responses on COVID-19 cases and mortality. Previous studies evaluated the effects of social distancing policies on mobility (Gupta et al., 2020b) and COVID-19 case growths (Courtemanche et al., 2020; Dave et al., 2020), paid sick leave mandate on mobility (Andersen et al., 2020), perceived compensation, and preference to comply with social distancing measures (Bodas and Peleg, 2020) and income status on social distancing behavior (Chiou and Tucker, 2020). The current study extends this literature by providing the first evidence of social insurance programs' effect on job search effort, workplace mobility, and COVID-19 case and mortality growth rates.

This paper's findings also contribute to the literature that examines the effect of COVID-19 on labor market outcomes. Previous studies examined the effect of social distancing policies on labor market outcomes(Gupta et al., 2020a), working from home (Dingel and Neiman, 2020) and racial disparities (Montenovo et al., 2020). This paper adds to the literature by examining the effect of ISP on "job" search effort and workplace mobility.

The rest of this paper is structured as follows. In Section 2, I describe ISPs, data, and descriptive statistics, and in Section 3, I discuss the identification method. In Section 4, I present the empirical results and compare the observed and counterfactual outcomes. Finally, in Section 5, I discuss the findings and concludes.

2. Income support programs, data and descriptive statistics

This study combines information on country level COVID-19 cases and deaths, country-level changes in ISPs, social distancing measures, and community mobility measurements. I obtained the information on COVID-19 cases and deaths, ISPs, and social distancing policies from the Oxford COVID-19 government response tracker (OxCGRT). The data on ISP reports information on the availability, the exact date of the expansion, and the duration of the support of 178 countries. The ISPs are provided by expanding and activating unemployment benefits, social safety-net programs, paycheck protection programs loans for employers, cash transfer programs, or a combination of a variety of social safety-net programs.

Fig. B1 in the Appendix (Supplementary material) reports the number of countries that expanded or activated ISPs throughout February 15–May 15. By the end of the sample period, 126 (70.79%) of the countries in the sample expanded their ISPs. Fig. B2 in the Appendix (Supplementary material) displays the dates of the introduction of these programs across countries. The timing of the introduction of ISPs in countries included in the sample ranges from February 15 to June 15, and the ISP's introduction modal timing is between the last week of March and the first week of April. I include countries that did not expand the program during the pandemic in the control group.

The OxCGRT also reports a binary wage/ income replacement rate indicating whether the programs replace more or less than 50% of the lost salary income. It also reports whether the benefits include self-employed individuals in the informal sectors (Hale, 2020). Table B1 (Supplementary material) reports these figures. Close to 43.29 percent of countries in the sample expanded income support programs with a less than 50 percent replacement rate during the sample period. Simultaneously, 27.5 percent of countries in the sample provided income support with 50% and
higher replacement rates during the same period. Additionally, 45.5 percent of countries in the sample provided income support for individuals in the informal sector. While only 14.6 percent of countries replaced 50 percent and a higher share of the lost incomes for individuals in the informal sector, 30.9% of them replaced less than 50 percent of lost incomes for individuals employed in this sector.

The OxCGRT provides detailed information on the exact date and the strength of social distancing measures that a given country introduced. These policy interventions are either suggested, recommended, or required. This study uses the variations in the dates of the introduction of the most substantial measure within a category for each country as a control to partial out the effects of contemporaneous policy changes.

The study uses information on mobility measures from Google community mobility reports. This data document the number of visits and activities around retail and recreation areas, grocery stores and pharmacies, parks, transit stations, workplaces, and residential areas of 135 countries (Aktay et al., 2020). Although the effect of ISP operates through mobilities in multiple locations, for brevity, this study focuses on mobility in workplace areas.¹ The reported values in the workplace mobility data are the percentage change of activities and visits in workplace areas relative to the median value of the corresponding day of the five weeks between January 3–February 6, 2020. This information is available at different geographical levels. To create a consistent sample, I collapsed the workplace mobility trends at a country level. The purpose of this data is to inform the work effort change caused by ISP. Because changes in workplace activities can be caused by factors other than workers’ willingness to work (such as lay-off, furlough, or working from home), the resulting reduction in workplace mobility should be interpreted as the upper bound of the effect of ISP on workers decision to work.

As noted above, I also supplement the analysis on workplace mobility with information on job search effort using the Google trends on ‘job’ search. Google Trends provides a time series representing search activity to query on the topic ‘jobs’ for a specific date range and geographical locations. I searched the topic ‘job’ for a given country between January 3 and June 1. The values of a Google trends series represent the number of searches for the topic ‘jobs’ relative to other searches at Google.com. Each series is normalized so that the highest value during query time is set to 100. The values of the series are always integers between 0 and 100. In this study, I calculated the weekly percentage change in the Google ‘jobs’ search relative to the weekly median ‘jobs’ search for the five weeks between January 3–February 6, 2020, using the formula Google used to generate the workplace mobility percentage change due to the pandemic.² This data is available for all countries in the sample. The ‘job’ search trend at google.com should be interpreted with caution as the change in trend could also result from “job” destruction and frustrated workers’ behavioral response to a low probability of finding a job or high risk of catching COVID-19. Here too, the change in “job” search should be interpreted as the upper bound of the effect of ISP on “job” search effort.

I restrict the sample period to February 15-May 15 for two reasons. First, Google’s workplace mobility data availability starts on February 15. Second, the job market season across countries can change with a longer sample period, which might introduce seasonality problems in the identification method.³ The total number of country-day observations of COVID-19 cases and mortality and Google’s ‘jobs’ search is 16198. The total country-days of observations of workplace mobility is 12150. To reduce the data’s noise, I binned the sample to every seven-day interval, which I refer to in this paper as weeks. Binning the sample to weeks reduces the country-days observation of COVID-19 cases and mortality, and Google’s ‘jobs’ search data to 2315. It also reduces the work mobility data to 1755. For a COVID-19 cases and mortality represented by y, I calculated the growth rate of COVID-19 cases and mortality as $\log(y_{t+1}) - \log(y_{t})$. Differencing the current and the lagged inverse hyperbola sine of COVID-19 cases and deaths count reduces the number of COVID-19 cases and mortality observations by 178. To protect privacy, Google reports mobility data as missing when a location shows significantly smaller activities. As a result, the final sample size is 2137 for COVID-19 case and mortality growth, 2254 for Google “jobs” search, and 1700 for workplace mobility data.

Table 1 presents the mean and standard deviation of the outcome and the proportion of countries that extended ISPs. While the mean cumulative COVID-19 cases and deaths are 7760.8 and 520, the average COVID-19 case and death growth rates are 0.59 and 0.34. More than 70 % of countries in the sample extended ISPs during the sample period. Due to inadequate testing capacity during the early periods of the pandemic, the true prevalence of COVID-19 cases and death could be higher than those reported in this paper.⁴ An additional 7.1 % of countries that did not extend income support programs between February 15 and May 15 also introduced them after May 15. I included these countries’ observations in the control group. The mean percentage reduction in ‘jobs’ search at Google.com and workplace mobility between February 15-May 15 from the median values of the five weeks between January 3-February 6, 2020, are −23.39 and −24.7, respectively.

3. Identification method

To identify ISP’s causal effect, I exploit the variation in the timing of the introduction of these programs across countries. I used a simple difference-in-difference method and multi-event analysis methods. I specified the difference-in-difference model as:

| Table 1 | Descriptive Statistics of the outcome variables. |
|-------------------------------------------|-----------------------------------------------|
| Google ‘job’ search                        | (1)                      | Standard Deviation | N  |
| Work place                                | −24.70                   | (24.19)             | 1700 |
| Covid-19 case growth                      | 0.59                     | (0.75)              | 2137 |
| Covid-19 death growth                     | 0.34                     | (0.80)              | 2137 |
| Cumulative covid-19 cases                 | 7760.8                   | (52600.48)          | 2137 |
| Cumulative covid-19 deaths                | 520.29                   | (3637.71)           | 2137 |

¹ I included the effect of ISP on other mobility categories in Appendix B (Supplementary material).
² I also made the same analysis on the raw index provided by Google and found qualitatively the same result. This result can be made available upon request for interested readers.
³ Due to the nature of the research question and lack of data on mobility, the seasonality problem cannot be addressed directly with the current identification method. Instead, I added a result with a shorter and longer sample ending on May 1 and June 1 in the Appendix (Supplementary material) as a robustness check, which produced qualitatively and quantitatively similar results.
⁴ However, the identification method used in this paper should offset the measurement problem between the control and the treatment countries. One might wonder less developed countries (or countries with lower ability to provide ISP) might be subject to higher measurement error than others. The results presented by the country’s level of development and by the degree of generosity show that ISP still affects COVID-19 cases and mortality, job search, and workplace mobility in these countries too.
\( Y_{cw} = a_{ISP} \cdot w_{ISP,cw} + \gamma \cdot Z_{cw} + \delta_{C} + \tau_{w} + \epsilon_{cw} \) \hspace{1cm} (1)

\( Y_{cw} \) represents the workplace mobility, ‘job’ search, COVID-19 case, or COVID-19 mortality growth rate of country \( c \) in a week \( w \). \( ISP_{cw} \) is a dummy variable equal to 1 if country \( c \) has an income support program in a week \( w \), and 0 otherwise. \( Z_{cw} \) controls for a vector of other contemporaneous policy changes introduced shortly before and after ISP’s introduction. These policy measures include stay-at-home orders, school closures, restrictions on mass gathering, workplace closures, public events cancellation, and public transportation restrictions. The country and week fixed effects are controlled by \( \delta_{C} \) and \( \tau_{w} \), respectively. The fixed effects control for country and week unobserved permanent heterogeneity. The parameter of interest is \( a \). As long as countries that introduced ISP and those countries that did not introduce ISP followed a parallel pre-trend, \( a \) will identify the causal effect of ISP on workplace mobility, job search, COVID-19 case growth, and COVID-19 mortality growth. I clustered the robust standard errors at a country level. The workplace mobility and job search outcome response observed in the data depends on the internet penetration rate across countries rather than population or density. To be consistent across outcome variables, the regressions are not weighted. As a result, the estimates reflect the effect of ISP on the average country, not on the average person.

To test whether the pre-ISP parallel trend assumption holds and to evaluate the effect of ISPs on the dynamics of the change in workplace mobility, job search effort, COVID-19 case and COVID-19 mortality growth rates, I estimated a multi-event analysis model, in which I identify the effect of ISPs after controlling for other contemporaneous policy changes. The multi-event analysis model is specified as:

\[ Y_{cw} = \sum_{i=2}^{6} \alpha_{i} \cdot 1(WISP_{cw} = a) + \sum_{i=0}^{6} \beta_{i} \cdot 1(WISP_{cw} = b) + \sum_{i=2}^{6} \gamma_{i} \cdot 1(WISP_{cw} = c) + \ldots \]

\[ = -a + \sum_{i=0}^{6} \theta_{i} \cdot 1(WISP_{cw} = b) + \delta_{C} + \tau_{w} + \epsilon_{cw} \] \hspace{1cm} (2)

\( W_{c} \) is the week ISP introduced in the country \( c \) and \( WISP_{cw} = w - W_{c} \). The number of weeks between week \( w \) and the week an ISP introduced. I set \( WISP_{cw} = -1 \) for countries that never introduced ISP during the sample period. Because the number of countries with COVID-19 cases and deaths 5 weeks before the introduction and 7 weeks after the introduction of ISP are small, I set \( WISP_{cw} = -1 \) if \( WISP_{cw} = w - W_{c} \geq 7 \) and \( WISP_{cw} = 5 \) if \( WISP_{cw} = w - W_{c} \leq -5 \). The parameters of interest are \( \alpha_{i} \) and \( \beta_{i} \). While \( \alpha_{i} \) identifies the differential pre-event trends in workplace mobility, job search, and COVID-19 outcomes that are associated with countries that go on to experience an ISP, \( \beta_{i} \) traces out the dynamics of the post-event trends in workplace mobility, job search, and COVID-19 outcomes that occur after a country introduced an ISP. Since it is not reasonable for an income support program that is not yet implemented to affect current COVID-19 cases and mortality growth, an \( \alpha_{i} \) that is statistically different from zero will imply a model misspecification or policy endogeneity problem. \( WISP_{cw} \) captures weeks since country \( c \) introduces one of the six contemporaneous policies. The coefficients of the pre trend of policy \( Z_{i} \) and its causal effect is captured by \( \kappa_{i,a} \) and \( \theta_{i,b} \), respectively. To capture changes in the outcome variables 5 weeks before and 7 weeks after the changes in the contemporaneous policies, I set \( WISP_{cw} = 7 \) if \( WISP_{cw} = w - W_{c} \geq 7 \) and \( WISP_{cw} = -5 \) if \( WISP_{cw} = w - W_{c} \leq -5 \). The reference week, both for the contemporaneous policy measures and ISP, is the week before their introduction. All other variables are as defined in Eq. (1).

4. Result

4.1. Empirical analysis

This section presents the difference-in-difference and multi-event analysis results. Table 2 shows the estimates of the effect of ISPs on workplace mobility, job search, COVID-19 case, and mortality growth rate. I measure workplace mobility by the percentage change in workplace activity from the baseline. The result presented in Column (1) shows that ISPs led to a 5.36 percentage point reduction (21.7 percent off of the mean) in workplace mobility at a 1% level of statistical significance. Column (2) describes the effect of ISP on job search effort. The job search effort is captured by the percentage change in search frequency for the topic ‘job’ at Google.com. The result shows that providing income support caused a 6.6 percentage point reduction in the job search at Google.com at 5% level of statistical significance, representing 28.22% of the mean reduction in the job search effort. Column (3) indicates that ISPs caused a 33.2 percentage point reduction (62.5% off of the mean) in the COVID-19 case growth rate at 0.1% level of statistical significance. Column (4) plays the effect of ISPs on the COVID-19 mortality growth rate. The simple difference-in-difference estimate shows that ISPs do not significantly affect the mortality growth rate.

To test whether the pre-policy change parallel trend assumption of the difference-in-difference method holds and evaluates ISPs’ effect on the dynamics of workplace mobility, COVID-19 case, and COVID-19 mortality growth rates, I estimated the multi-event analysis method presented in Eq. (2). Fig. 1 presents the point estimates and the 95% confidence interval of the effect of income support program on mobility, job search, COVID-19 case, and COVID-19 mortality growth rates. In all cases, the coefficients leading to the income support program introduction are not different from zero, implying that the pre-income support parallel trend assumption holds up, and the identification method is valid. Panel A displays the effect of ISPs on workplace mobility. The result shows that the effect of ISPs on workplace mobility continuously magnified. Relative to one week before their introduction, ISPs led to a statistically significant reduction in workplaces mobility of 4.63 percentage point after two weeks, 4.4 percentage

Table 2

| ISP | Workplace Mobility | 'jobs' search | Case growth | Mortality growth |
|-----|---------------------|--------------|------------|----------------|
| Controls | Yes | Yes | Yes | Yes |
| Country Fixed Effect | Yes | Yes | Yes | Yes |
| Week Fixed Effect | Yes | Yes | Yes | Yes |

N = 1700 2254 2137 2137

Notes: *p < 0.05, **p < 0.01, ***p < 0.001.
point after three weeks, 6.38 percentage point after four weeks, 7.01 percentage point after five weeks, 8.29 percentage point after six weeks, 7.72 percentage point after seven weeks and onwards.

Panel B presents the effect of ISPs on job search effort. ISP reduced the frequency of search for ‘jobs’ at Google.com by 11.6 percentage points on week four of their introduction from the median of the five-week search between January 3rd–February 6, 2020. Despite similar magnitude reductions on job search behavior due to ISP after the 4th week of their introduction, these effects are not statistically significant.

Panel C reports the effect of ISPs on the COVID-19 case growth rate. Within the first two weeks of their introduction, ISPs did not

affect the COVID-19 case growth rate. Relative to a week before their introduction, ISPs caused a statistically significant reduction of COVID-19 case growth rates of 23.4 percentage points after three weeks, 21.8 percentage points after four weeks, 25.1 percentage points after five weeks, 34.5 percentage points after six weeks, and 47.7 percentage points after seven weeks onwards. ISPs' null effect on the first two weeks of their introduction does not come as a surprise since the virus’s incubation period spans 2–14 days after infection.

Panel D shows the effect of ISPs on the COVID-19 mortality growth rate. Here too, ISPs have a delayed effect on COVID-19 mortality growth rates. The programs did not have a statistically

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**Fig. 1.** The effect of Income support on job search, COVID-19 and mobility: estimates of event analysis.

**Table 3**
The Effect of income support on covid-19 cases, covid-19 mortality and mobility by income support generosity.

|                  | Workplace Mobility | ‘jobs’ search | Case growth | Mortality growth |
|------------------|--------------------|---------------|-------------|-----------------|
| Panel A: ≥ 50% replacement versus no ISP | -4.102 (2.223) | -9.071 (5.606) | -0.741*** (0.113) | -0.0530 (0.0745) |
| ISP              | 925                | 1170          | 1185        | 1185            |
| Panel B: < 50% replacement versus no ISP | -5.888** (2.003) | -7.020 (3.620) | -0.183* (0.0747) | -0.0297 (0.0525) |
| ISP              | Controls           | Yes           | Yes         | Yes             |
| Country Fixed Effect | Yes               | Yes           | Yes         | Yes             |
| Week Fixed Effect   | Yes               | Yes           | Yes         | Yes             |
| N                | 1195               | 1664          | 1568        | 1568            |

Notes: *p < 0.05, **p < 0.01, ***p < 0.001.
significant effect on the COVID-19 mortality growth rate in the first five weeks of their advent. Relative to a week before their introduction, ISPs led to a statistically significant reduction of COVID-19 mortality growth rate of 0.352 percentage point six weeks after and 29.7 percentage points seven weeks after and onward.

In sum, ISPs reduce mobility in workplaces, job search effort, COVID-19 case, and COVID-19 mortality growth rates. While the effect of ISPs on workplace mobility appears instantaneously and magnified continuously, the effect on job search occurs only on week five of their introduction. The effect of ISPs on COVID-19 case and COVID-19 mortality growth rates was observed two to five weeks after their introduction. The model held the effect of the stay-at-home order, school closure, workplace closure, and public transportation restriction and cancellation of public event constant. Therefore, these estimates are the additional effects of ISPs above and beyond these policies' effects.5

4.2. Heterogeneity

This section presents the effect of ISP on workplace mobility, ‘job’ search, COVID-19 case growth, and COVID-19 death growth by the degree of the income support generosity and the proportion of the population engaged in the informal sector. Table 3 describes the effect of ISP on the outcome variables by income support generosity. While Panel A shows the effect on countries that replaced greater than or equal to 50% of the lost income, Panel B indicates the effects on countries that replaced less than 50% of the lost income. The results indicate that the effect of ISP on COVID-19 cases growth (74.1 percentage point versus 18.3 percentage point), death growth (5.3 versus 2.9 percentage points), and “job” search (9.0 percentage point versus 7.1 percentage point) is more substantial in countries that replaced more than 50% of the lost income than countries that replaced less than 50% of the lost income. However, the effect on workplace mobility is higher in countries that replaced less than 50% of the lost income than countries that replaced more than 50% of the lost income. Figs. 2 and 3 present the multi-event estimates by the amount of replacement rate. The pre-trend between the control group and the treatment group is parallel, although it is more stable in countries that replaced more than 50% of the lost income than others. Here too, the effect of ISP on COVID-19 case, mortality growth rates is higher in countries that replaced more than 50% of the lost income than countries that replaced less than 50% of the lost income. The effect of ISP on workplace mobility is stronger on countries that replaced less than 50% of the lost income than countries that replaced more than 50% of the lost income.

I also evaluated the effect of ISP on the same outcome variables by the proportion of the population employed in the informal sector. I chose the level of employment in the informal sector for two reasons. First, the degree of informal employment correlates with the country’s level of development. As the number of people employed in the informal sector increases in a given country, the country’s level of development decreases, and vice versa.

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5 In Appendix B (Supplementary material), I included results on the effect of social distancing policies on the outcome variables discussed here. I found that school closure, stay at home order and restriction on transportation have a statistically significant negative effect on workplace mobility at 5% or less level of statistical significance. Their effects on COVID-19 cases, mortality, and job search, however, are not statistic; 1;ally significant at 10% level of statistical significance.
Evaluating the effect by the level of development informs us whether institutional factors influence the result. Second, it also informs us whether the effect of ISP on workplace mobility is stronger in countries where the nature of works requires physical contact. I present the results in Table 4 and Figs. 4 and 5. Panel A and Pane B of Table 4 show that the effect of ISP on workplace mobility (−7.87 versus −3.89 percentage points) and “job” search (−17.5 versus 1.7 percentage points) is higher in countries with a high level of informal sector employment than in countries with a lower rate of informal sector employment. However, the effect of ISP on the COVID-19 case growth rate is greater in countries with a lower rate of informal sector employment than countries with a higher rate of informal sector employment. Figs. 4 and 5 also show the effect of ISP on COVID-19 case growth and mortality growth. The results indicate that the effect of ISP on COVID-19 case and mortality for countries in both categories. As is the case with the simple difference-in-difference estimates, the effect of ISP on COVID-19 case growth is stronger among countries with lower than 50 % employment in the informal sector than others. The effect on workplace employment and “job” search, on the other hand, is higher in countries with higher than 50 % employment in the informal sector than other countries.

In sum, as is the case with the general sample, ISP affects workplace mobility, “job” search, COVID-19 cases, and mortality growth rates in countries that provided ISP when the sample is stratified by the generosity of ISP and the level of development of the country. The effect on COVID-19 case and death growth rate is stronger in countries that provided relatively more generous income support and have lower employment in the informal sector than others. While the effect on workplace mobility is stronger in
countries that provided less generous income support and relatively less developed countries, the “job” search effect of ISP is more substantial in countries that provided more generous income support and less developed countries than others.

4.3. Counterfactual simulation

Using the baseline estimates from event analysis regression, I simulated the counterfactual workplace mobility, job search, COVID-19 cases, mortality growth rate, cumulative COVID-19 cases, and mortality with no ISP ever being implemented. The counterfactual outcomes are calculated as the observed value net of the estimated value of the post-treatment period coefficients (i.e., \( Y_{cw} - \sum_{b=0}^{6} \beta_b I(WSISPcW = b) \)).

Fig. 6 compares the observed outcomes and their counterfactual of no ISPs. Panel A displays that the counterfactual minimum point of the workplace mobility reduction coincided with the observed timing on April 7. It also reports that the maximum workplace activity reduction would have been three percentage points less than the observed workplace mobility reduction. Additionally, Table 5 shows that the mean workplace mobility would have been 2.21 percentage points higher than the observed mean workplace mobility, implying that ISPs explain 9.26 percent of the mean reduction in workplace mobility since the beginning pandemic.

Panel B shows the actual and counterfactual percentage change of search for the term ‘job’ at Google.com without ISP. Before the week of March 29, the counterfactual and the observed frequency of search for the term ‘job’ show no differences. From the week of March 22, the counterfactual frequency of search for the term ‘job’ diverges from the observed frequency of search. The gap between the two widens from 2.5 percentage points in the week of March 29 to 5.5 points at the end of the sample period. Table 5 also compares the mean actual and counterfactual frequency of search for the term ‘job’ at the site. Without ISPs, the mean search for ‘jobs’ at google.com would have been 2.61 percentage points higher than the observed frequency of search, implying that ISPs explain 11.16 percent of the mean reduction in ‘job’ search at Google.com since the beginning of the pandemic.

Panel C delineates the same comparison between the observed and the counterfactual COVID-19 case growth rate. While the COVID-19 case growth rate of the observed and the counterfactual outcome occurs on March 25, the gap between the two expands from 6 percentage points on March 31 to 27 percentage points by the end of the sample period (May 15). Table 5 shows that without ISPs, the mean COVID-19 case growth rate would have been ten percentage points more than the observed COVID-19 case growth rate, equivalent to a 17.6 % increase in the COVID-19 case growth rate. Panel D juxtaposes the observed and the counterfactual COVID-19 mortality growth rate without ISPs. Here too, the observed and the counterfactual COVID-19 mortality growth rate peaked at the same time, and the gap between them remained low late until April 5. The gap between the observed and the actual COVID-19 mortality growth rate expanded from 5 percentage points on April 21 to 14.9 percentage points by the end of the sample period. Similarly, Table 5 shows that had it not been for the ISPs, the mean COVID-19 mortality growth rate would have been three percentage points higher than the actual COVID-19 mortality growth rate. The mean mortality growth rate avoided through ISPs represent 9.1 % of the observed mortality growth rate during the sample period.

Panel D and E put the observed and the counterfactual cumulative COVID-19 cases and mortality under the assumption.

![Fig. 4. The effect of Income support on job search, COVID-19 and mobility of countries with less than 50% of the population in the informal sector employment.](image-url)
of no ISPs. I calculated the week $w$ counterfactual cases and mortality of country $c$ by multiplying the lagged COVID-19 cases or mortality and the exponential of their respective predicted growth rate and residuals. Once I predicted the number of cases and mortality under the counterfactual of no ISPs, I summed the observed and the counterfactual cases by week to create a weekly global (for 178 countries) level observed and counterfactual case and mortality. Panel D displays the global trends of the observed and the counterfactual COVID-19 cases. The cumulative COVID-19 cases without ISPs would have been 7,827,937 by the end of the sample period. 89.1% more than the actual cumulative COVID-19 cases reported by May 15. Panel E also compares the actual and counterfactual cumulative COVID-19 mortality trends. With no ISPs, the cumulative COVID-19 mortality by May 15 would have been 454,114, 58% more than the observed cumulative COVID-19 caused mortality.

4.4. Robustness check

This section presents the robustness check results. First, to see whether the result is sensitive to changes of the sample period, I evaluated the effect of ISP on workplace mobility, "job" search, COVID-19 case, and mortality growth rate for the sample period ending May 1st and June 1st. I present the results in Tables A1 and A2 and Figs. A1 and A2 (Supplementary material). As was the case  

\[ \text{case}_w = \text{case}_w - \exp \left( \frac{\text{diff}_w}{\text{case}_w} \right) \]  

in the baseline sample, here, too, ISP reduces workplace mobility, "job" search effort, COVID19 case, and death growth rates. Although the magnitude of the effects of ISP is slightly stronger for the sample period ending May 1st than June 1st, the results are similar in all cases.

Second, in the dynamic treatment effects, the use of early adopters and late adopters of ISP in the control group can underestimate the actual effect of ISP. To see whether the baseline results are attenuated, I ran two regressions. The first regression examines the effect of ISP using early adopters (countries that introduced ISP before April 1st) as a treatment group and non-adopters as a control group. Fig. A3 and Panel A of Table A3 (Supplementary material) present the results. The effect of ISP on all outcome variables is stronger on early adopters than the total baseline sample that included the late adopters in the control group, suggesting that the baseline estimates are attenuated. Second, I also examined the effect of ISP on the same outcome variables using late-adopters (countries that introduced ISP after April 1st) as a treatment and non-adopters as a control group. Fig. A4 and Panel B of Table A3 (Supplementary material) present the results. I found that the effects of ISP on late-adopters' outcomes are less than its effects on the baseline sample outcomes.

Third, the Google trend "job" indexes limit the search effort’s comparability in a cross country setting, magnifying the effect of search effort change in countries with lower internet access/penetration. To address this problem, I dropped countries below the lowest five percentile of the internet access distribution. I present the results in Table A4 and Fig. A5 (Supplementary material). I found that the effect of ISP on "job" search is higher in a sample that excludes countries with low internet access than others.
5. Conclusion and discussion

This paper examined the tradeoffs between the job search effort and workplace activities reduction versus the COVID-19 case and mortality growth rates effects of income support programs. To identify ISPs' causal effect, I use the timing of their introduction across countries as exogenous sources of variations. I found that ISPs reduced workplace mobility and increased COVID-19 case and mortality growth rates. Their effect on job search at Google.com appears only on week four of their introduction. I also showed that the effect of ISP on COVID-19 cases and mortality are higher in countries that provided generous income support and are relatively more developed countries than others. The workplace mobility effect is stronger in countries that provided less generous income support and are less developed countries than others. The “job” search effect, on the other hand, is greater in countries that provided more generous income support and are less developed countries than others. Countries that saw a greater reduction in workplace mobility and “job” search experienced relatively less COVID-19 case and mortality reduction. The potential reason is the less adequate social distancing home environment in developing countries.

Using the observed and the counterfactual COVID-19 cases and mortality growth rates, I also simulated and compared the cumulative COVID-19 cases and mortality. With no ISPs, the cumulative COVID-19 cases and mortality would have been 3.6 million and 166,690 higher than the observed counts. The mean workplace activity and ‘job’ search effort reduction, on the other hand, would have been 2.21 and 2.61 percentage points lower than the observed workplace activity and ‘job’ search effort. These are equivalent to an 89.1 %, and 59 % additional cases and deaths avoided at the cost of 9.26 and 16.83 percent additional reduction in ‘job’ search effort and workplace mobility. In the lens of social distancing policy measures, the number of COVID-19 cases and deaths averted through ISPs is significantly smaller. A closely related study, Courtemanche et al. (2020), for example, showed
that without stay-at-home orders and any social distancing measures, the number of COVID-19 cases in the US, not globally, would have been ten times (10, 224,598) and 35 times higher (35,257,098).

The averted cases and deaths should be taken with great caution since these estimates are subject to uncertainties that are difficult to include in the current model. In the absence of ISPs, families could still use other coping mechanisms (such as credit cards, savings, loans from friends and families) to smooth their consumption and practice social distancing, reducing physical contacts in workplaces and other areas. These behavioral responses could significantly reduce the predicted COVID-19 cases and mortality with a small change in job search effort and workplace mobility. On the other hand, ISPs can also improve general health and reduce non-COVID mortality through their direct income effects. Income loss due to unemployment, for example, can affect mental health and increase deaths by suicide. Therefore, to the degree ISPs improve general health and reduce non-COVID-19 mortality, the effects of the programs documented here underestimate the lower bound of their total health benefits.

There are two caveats. First, the job search measure might bias the actual job search reduction caused by income support since the Google search data do not capture the job search effort change via traditional methods. Additionally, the “job” search trend also does not differentiate whether the reduction in the “job” search trend is caused by the lower expectation of finding a job or job destruction and higher risk of working in a pandemic. Workplace mobility, on the other hand, measures activities in workplaces. ISPs can reduce workplace mobility due to workers’ decision to reduce labor supply or employers’ decision to layoff or furlough workers, or employees’ decision to work from home. In some cases, this change could be due to the program’s nature that allowed employers to keep their employees. For example, programs similar to paycheck protection allow employers to keep their workers without requiring workplace attendance. If these programs caused any moral hazard cost in the job market, the workplace mobility reduction should represent the upper bound of the Moral hazard effect of ISPs. Second, the dates the income support programs became active can differ from the actual dates individuals obtained actual supports. Therefore, these estimates should be interpreted as the total effect of the implementation and the announcement of ISP’s availability, which might have induced anticipatory behavioral responses, on COVID-19 cases and mortality above and beyond the effect of social distancing policies.

This finding implies that in addition to the consumption smoothing and other spillover effects of these programs, policymakers should consider the disease containing and mortality reduction benefits when evaluating the total values of social insurance programs.

Author statement

This is solo authored work produced without any research assistance. All aspects of the paper are my own work.

Appendix A. Supplementary data

Supplementary data associated with this article can be found, in the online version, at https://doi.org/10.1016/j.ehb.2021.100997.

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