Remote Work and the Heterogeneous Impact of COVID-19 on Employment and Health*

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Abstract

This paper examines the impact of the COVID-19 pandemic on employment and respiratory health for remote workers (i.e. those who can work from home) and non-remote workers in the United States. Using a large, nationally-representative, high-frequency panel dataset from March through July of 2020, we show that job losses were up to three times as large for non-remote workers. This gap is larger than the differential job losses for women, African Americans, Hispanics, or workers without college degrees. Non-remote workers also experienced relatively worse respiratory health, which likely occurred because it was more difficult for non-remote workers to protect themselves. Grouping workers by pre-pandemic household income shows that job losses and, to a lesser extent, health losses were highest among non-remote workers from low-income households, exacerbating existing disparities. Finally, we show that lifting non-essential business closures did not substantially increase employment.

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1 Introduction

The COVID-19 pandemic has been an unprecedented shock for the U.S. economy. From the first reported case on January 20 and the first death on February 29, the prevalence of the disease grew exponentially, spurring dramatic individual and policy responses. People avoided public spaces such as stores and restaurants, while policymakers closed schools and non-essential businesses, and ordered people to work from home. In combination, these responses led to a large economic contraction and extreme job losses in late March and early April (Brinca et al., 2020; Coibion et al., 2020). Several weeks later, policymakers lifted restrictions on non-essential businesses to encourage the resumption of economic activity.

Unlike previous recessions, the COVID-19 pandemic may have caused disproportionate employment and health losses for workers whose jobs could not be conducted remotely (e.g., by working from home). Social distancing is more difficult in non-remote jobs. As a result, individual and policy responses to limit contact may have caused larger job losses among non-remote workers than remote workers. Non-remote workers who remain employed may have faced a heightened risk of disease. Since non-remote workers have lower socioeconomic status on average, these differential effects could magnify existing economic and health disparities.

This paper studies how the impact of the COVID-19 shock on employment and respiratory health differs by remote or non-remote job type. A challenge to investigating this question is that existing data sources do not measure these job types precisely. To overcome this challenge, we designed and fielded a longitudinal survey of U.S. households as part of the Understanding America Study (UAS), eliciting their experiences with the COVID-19 pandemic.

Four features of our data make them especially suitable for this analysis. First, the data come from a large, high-frequency, nationally representative panel of approximately 7,000 U.S. adults surveyed every other week from mid-March to late July 2020 (Kapteyn et al., 2020). Secondly, we observe employment status and whether jobs could be performed remotely. This individual self-reported measure complements existing studies of remote work during COVID-19, which classify occupations on a continuum of remoteness (Dingel and Neiman, 2020; Montenovo et al., 2020). Thirdly, we measure two important facets of respiratory health: symptoms of respiratory illness and perceived risk of COVID-19 infection. While COVID-19 has made exposure to respiratory illnesses more costly, these illnesses already entail non-negligible health costs. Yelin et al. (2002) estimate that medical expenditures for respiratory health conditions amount to $12-31 billion per year. Lastly, we observe protective behaviors. We can therefore assess how employed and unemployed workers in remote and non-remote jobs practiced different types of protective behavior.

We find that job losses were up to three times as large for non-remote workers. We estimate that 24 percent of non-remote workers lost their jobs by early April, compared to 8 percent of remote workers ($p < 0.001$). This gap is larger than the gap for women, African Americans, or workers without a college degree.

We also examine patterns in respiratory health and protective behavior. Consistent with greater risk of exposure, non-remote workers exhibited more respiratory symptoms and perceived higher COVID-19 infection risk than their remote counterparts. This finding suggests a trade-off for many non-remote workers between employment and respiratory health. Supporting this conjecture, we show that non-remote workers practiced fewer work-related protective behaviors than remote workers, and that employed people were responsible for this difference. Both groups practiced similar numbers of non-work-related protective behaviors, which suggests differences are a function of job features, rather than innate differences in distancing preferences.

Next, we explore how job and health losses for remote and non-remote workers varied by household income. Job losses and, to a lesser extent, health declines were most severe among non-remote workers from low-income households. As a result, the pandemic has exacerbated pre-existing disparities.

Finally, we use two complementary approaches to investigate whether the removal of non-essential business closures around May was effective at increasing employment in general and specifically for non-remote workers. An event study approach examines whether reopenings boosted employment in the days and weeks after they were implemented. A difference-in-difference design assesses whether states that lifted closures sooner increased employment relative to states that did so later. Both approaches fail to show a substantial increase in employment from easing business restrictions.

This paper contributes to our emerging understanding of the COVID-19 pandemic, which caused the largest quarterly economic contraction in the U.S. since at least 1875. Using a new, nationally representative dataset that measures remote work directly, we show stark differences in the impact of the crisis for non-remote workers.[1] Observing both economic and health outcomes for a common set of respondents allows us to focus on the health/employment trade-off. Our results highlight the double burden borne by non-remote workers: these workers had substantially larger job losses, and those who kept their jobs faced elevated health risks. Since the losses were highest among remote workers from very low income households, these disparities exacerbated growing income inequality in the U.S. (Piketty et al., 2017).

We also contribute to the broader literature on remote work. This literature has identified higher job performance, work satisfaction, and worker retention as benefits of remote work (e.g. Bloom et al., 2015; Choudhury et al., 2019). We show that an additional benefit of remote work, which has not been documented previously, is the reduced exposure to pathogens and allergens in the environment. This pandemic has likely accelerated an ongoing reduction in on-the-job physical proximity, from contactless deliveries to telemedicine and videoconferencing. Beyond reducing the costs of future pandemics, an understanding of the broader impact of this changing landscape is important and policy relevant.

[1]This research complements the work of Montenovo et al. (2020) and Dingel and Neiman (2020), who classify occupations on a continuum of remoteness using O*NET database of occupational information.

2 Data

We rely on a nationally-representative online panel of 6,922 U.S. adults from the Understanding America Study (Alattar et al., 2018). Since 2014, panel participants have regularly answered surveys on a variety of health and economic topics. The UAS recruits respondents through probability-based sampling from postal addresses and provides tablets and internet access to people without other means to access the internet. Beginning on March 10, 2020, we implemented a high-frequency longitudinal survey of economic and health conditions related to COVID-19 (Kapteyn et al., 2020). While the survey remains in the field, this analysis uses nine waves from March 10 through July 21, 2020. We supplement this dataset with existing quarterly employment data from 2019 for the study sample. Our dataset has 13 rounds: Rounds 1-4 cover the four quarters of 2019 and Rounds 5-13 cover nine rounds of the COVID-19 specific survey.[2]

Employment is measured in all survey rounds.[3] We measure remote and non-remote job types in Round 5 (March 2020). Workers held remote jobs if they indicated that they have "the kind of job where working from home is an option." Importantly, our measurement of remote work preceded the large loss of jobs that occurred in late March and early April.

Rounds 5-13 include two complementary indicators of respiratory health. First, we measure whether the respondent has experienced a fever, cough, shortness of breath, nasal congestion, chest congestion, sore throat, or sneezing within the past seven days. These symptoms proxy for exposure to respiratory pathogens such as SARS-CoV-2. Because COVID-19 is less prevalent than other respiratory illnesses (e.g. the common cold), most reported symp-

[2]Over Rounds 5-13, 82 percent of respondents participated in at least 8 of 9 rounds. Incomplete participation is uncorrelated with job type ($p = 0.91$), but it is correlated with demographic characteristics ($p = 0.02$). However, these differences are at most 5 percentage points. As a robustness test, we re-estimate our main results on employment and respiratory symptoms while limiting the sample to respondents who participated in at least 8 of 9 rounds. Estimates (available from the authors) closely resemble the results we report below, differing by less than one percentage point.

[3]Employment is likely subject to errors in survey responses. This may be problematic since our main regression conditions on employment. An additional issue is that the phrasing of the employment question differs between Rounds 1-4 and 5-13. Appendix A.1 discusses how we address this concern.

toms do not arise from actual COVID-19 cases.[4] We create a respiratory symptom index by taking the average of the seven measured symptoms. Secondly, we elicit the subjective probability of COVID-19 infection within the next three months. This variable is measured on a probability scale from 0 to 100 percent. The correlation between this variable and the symptom index is 0.14 and statistically significant. Lastly, we measure several protective behaviors, grouping them into work-related and non-work-related categories. We provide more details in Section 4.

We benchmark the differential effect for non-remote workers to the differential effects by gender, race/ethnicity, and education. We distinguish between White, African American, Hispanic, and other races.[5] For education, we distinguish between respondents with and without college degrees in Round 5 (March 2020).

A comparison of demographic characteristics by job type shows that remote workers are better educated: 68 percent have a college degree, compared to 26 percent of non-remote workers. Remote workers are slightly less likely to be African American (9 percent, compared to 12 percent for non-remote workers) and more likely to belong to the "other race" group (14 percent, compared to 7 percent for non-remote workers), and as likely as non-remote workers to be either White or Hispanic. Remote workers show more respiratory symptoms in Round 5 (13 vs 11 percent) and higher subjective COVID-19 infection risk (25 vs 22 percent).[6] Finally, non-remote workers practice fewer work-related protective behaviors than non-remote workers (72 vs 57 percent), while they practice non-work-related protective behavior to a similar degree (61 vs 57 percent).

[4]This use of respiratory symptoms is similar to the use of sexually-transmitted infections to proxy for risky behavior that could lead to HIV infection (Gong, 2015). Symptoms such as sneezing may also indicate seasonal allergies. Results are not sensitive to the inclusion or exclusion of sneezing or other specific symptoms.

[5]Following other studies of race/ethnicity, we code respondents of all races as Hispanic if they identify as Hispanic.

[6]The different prevalence in respiratory symptoms is consistent with the higher prevalence of allergic rhinitis among white-collar workers (Goldstein and Orris, 1964; Broder et al., 1974; Park et al., 2018).

3 Impact on Employment

3.1 Identification and Estimation

We are interested in estimating the reduced-form impact of the onset of the COVID-19 pandemic and the ensuing policy response on employment for U.S. adults, focusing first on the impact without conditioning on initial employment. We proceed by estimating the parameters of the following equation:

$$E_{it} = \alpha_i + \sum_{t \neq 5} \beta_t D_t + u_{it} \tag{1}$$

In this equation, i indexes the respondent and t indexes the survey round: $t = 1, 2, \ldots, 13$. The variable E_{it} is an employment dummy and D_t is a vector of survey round dummies. We exclude the Round 5 dummy to estimate differences relative to mid-March 2020. With individual fixed effects, α_i, estimates are based on individual changes over time.

The parameters $\beta_6 - \beta_{13}$ are the mean difference in employment in Rounds 6-13 (April through July) compared to Round 5. These parameters identify the causal effect of the pandemic on employment under two assumptions, which we discuss in turn.

The first assumption is that counterfactual employment would have remained unchanged in the absence of the pandemic. This assumption is plausible because the economy had been at full employment for at least the previous five quarters (Edwards and Smith, 2020) and because we consider a very short time horizon. Figure A1 plots employment by round: 62 percent of respondents were employed in Round 5 (March 2020) and this rate varies by no more than 1.5 percentage points over the previous four rounds and is not significantly different between Rounds 1-4 and Round 5 ($p = 0.31$).

The second assumption is that no other unobserved determinants of employment coincided with the onset of the COVID-19 crisis. There were no large-scale economic shocks, political shocks, or natural disasters that occurred during this period. A notable exception is the civil unrest that followed the police killing of George Floyd on May 25, 2020. Protests began on May 26 in Minneapolis and spread throughout the nation in the following week. While the economic ramifications of these protests are unclear, these events may have adversely affected employment by disrupting supply chains. We conjecture that this effect is relatively small and we do not attempt disentangle its separate contribution.

We are also interested in assessing whether the pandemic had a differential effect on employment for remote and non-remote workers. To do that, we estimate the parameters of the following equation for people with jobs in Round 5 (mid-March 2020):

$$E_{it} = \alpha_i + \sum_{t \neq 5} (\gamma_t D_t + \gamma_t^N D_t \cdot N_i) + \varepsilon_{it} \quad \text{if} \quad E_{i5} = 1 \tag{2}$$

The notation and variable definitions are the same as above. In addition, N_i is an indicator for people with non-remote jobs.

The parameters $\gamma_6^N - \gamma_{13}^N$ measure the differential employment patterns for non-remote workers during the pandemic. Because Equation (2) conditions on employment in Round 5, employment can only change in Rounds 6-13 if people lose their jobs. To interpret these parameters causally, we require the two identifying assumptions stated above, as well as two additional assumptions, which focus on job exits.

The first assumption is that counterfactual job losses would have remained unchanged and negligible in the absence of the pandemic. To consider this issue, Panel A of Figure 1 shows that employment for remote and non-remote workers grew by around 1 percentage point per quarter over Rounds 1-4. Since almost everyone with a job in March 2020 had been employed for at least the previous 15 months, it is plausible that they would have remained employed immediately after March 2020. Moreover, the lack of *differential* employment trends by job type before COVID-19 supports the claim that differential changes in employment after Round 5 are attributable to the pandemic.

Panel B of Figure 1 shows the transition out of employment from Round $t-1$ to Round

t by job type. This figure shows that job losses were negligible before Round 6. Specifically, around 0.1-0.8 percent of remote workers and 0.4-1.7 percent of non-remote workers exited employment in Rounds 2-5. From these quarterly data, we estimate a biweekly exit probability of at most 0.1 percentage points for remote jobs and 0.2 percentage points for non-remote jobs prior to COVID-19.[7] Therefore, we can assume that the April-July 2020 job losses would have continued to be negligible in the absence of the pandemic. Conversely, the biweekly exit probability was as high as 8.4 percentage points for remote workers and 25.6 percentage points for non-remote workers in the post-COVID-19 period. We conclude that the rate of labor market churn in 2019 cannot explain this pattern.

Secondly, we require that counterfactual job losses were not systematically different between workers with remote and non-remote jobs. *A priori*, it is possible that remote and non-remote jobs have different rates of job turnover. If the counterfactual job losses are relatively high in non-remote jobs, we may overestimate the differential effect of COVID-19 on non-remote workers. Figure 1 supports our identification assumption: pre-COVID-19 employment levels and employment exits do not systematically differ by job type. In joint significance tests, remote and non-remote workers do not have significantly different employment levels ($p = 0.55$) or employment exits ($p = 0.75$).

3.2 Employment Results

Table 1 shows the estimated employment changes for April 1 to 28 (Rounds 6-7), April 29 to May 26 (Rounds 8-9), May 27 to June 24 (Rounds 10-11), and June 25 to July 21 (Rounds 12-13), relative to mid-March. Column 1 shows that employment among all U.S. adults fell by up to 10 percentage points in April and May, implying a loss of around 26 million jobs over a few weeks. Employment then increased by 1.2 percentage points over Rounds 10-13.

Before proceeding with the analysis of our other results, it is important to establish that our estimates closely align with job loss reports from other sources. First, our estimated April

[7] Since Rounds 5-13 are spaced two weeks apart rather than 13 weeks (one quarter) apart, we multiply the quarterly exit probabilities by 2/13.

job losses closely align with the official statistics from BLS and from CPS estimates, once we account for the mis-classifications of workers known to exist in these data.[8] Secondly, we compare labor market outcomes in the UAS with those observed in the Census Household Pulse Survey and the NORC COVID Impact Survey. This comparison is limited to the weeks for which Census or NORC data are available. From the end of April to the end of July, the fraction of employed individuals in the UAS is very similar to the fraction of employed individuals in both the Census and NORC surveys. The differences never exceed 2 percentage points and are statistically insignificant. The close alignment between our estimates and job loss reports from other sources help to validate our empirical approach and the UAS as a data source.

Column 2 limits the sample to respondents with jobs in Round 5. Since the U.S. economy was near full employment prior to the COVID-19 crisis, this restriction is similar to limiting the sample to labor force participants. According to our estimates, 18 percent of people who were employed in mid-March of 2020 lost their jobs in Rounds 6-9 (April through late May). Employment then increased by one percentage point each in Rounds 10-11 and 12-13. The estimates from Column 2 are larger than those implied by the official change in unemployment rate, which rose by 10-11 percentage points between April and June 2020 (Bureau of Labor Statistics, 2020).

Column 3 examines the differential effects of the COVID-19 pandemic for remote and non-remote workers. Remote employment fell by 8.5 percentage points while non-remote employment fell by 24.5 percentage points in April (Rounds 6-7). Remote employment fell by an additional 1.5 percentage points in Rounds 8-13. Non-remote employment improved

[8]The BLS reports employment losses for 21.8 million workers between March and April. These employment numbers are known to undercount actual job losses (Bureau of Labor Statistics, 2020). For example, in April the number of workers classified as "with a job not at work" jumped by about 7.5 million compared to the year before. BLS states that "this group included workers affected by the pandemic response who should have been classified as unemployed on temporary layoff." The estimated job losses in the Basic Monthly Current Population Survey jump from 8.5 to 10.5 percent once we include workers classified as "with a job not at work" in the job loss count, implying a 23.5 percent undercount of job losses from the misclassified data. Adjusting BLS data for this 23.5 percent undercount implies that total job losses were closer to 27 million. Both the 10.5 percent job losses from the Basic Monthly CPS data and the 27 million job losses from revising the BLS data are very similar to our estimates

slightly, so that 19 percent of non-remote workers remained unemployed by July (Rounds 12-13).

Columns 4-6 compare these results with the differential job losses for women, people without a college degree, African Americans, and Hispanics, all of whom experienced differential job losses. To interpret the gender, race, and education gaps in job losses as the differential effects of the COVID-19 pandemic, we require parallel pre-trends in employment across these demographic subgroups. Estimates (available from the authors) validate these assumptions. Job losses were 6-7.5 percentage points higher for women than for men in Column 4, while job losses were 6.3-13 percentage points higher for workers without a college degree in Column 5. In Column 6, job losses were 6.6-8.2 percentage points higher for African Americans than for whites, and they were 1.4-5.9 percentage points higher for Hispanics than for whites (although estimates for Rounds 6-9 are not statistically significant).

Although the differential effects for these groups are large, all estimates are smaller than the differential effect for non-remote workers. Seemingly unrelated regression (SUR) estimates show that non-remote job losses are statistically different from job losses for women ($p = 0.02$), non-college graduates ($p = 0.07$), and Hispanics ($p < 0.001$). They are not significantly different from estimated job losses for African Americans ($p = 0.29$).

Table A2 repeats this exercise while adding all of these covariates jointly. These estimates are closer to zero than the ones in Table 1, as one would expect since these covariates are positively correlated. However, the differential effect for non-remote workers remains larger than the other effects.

4 Respiratory Health, COVID-19 Risk, and Protective Behavior

By interacting directly with customers and coworkers, non-remote workers may face greater exposure to infectious diseases like COVID-19. Physical proximity is an especially costly job attribute during a pandemic. However, the ability to avoid threats to respiratory health may be another important benefit of remote work in general.

This section assesses the impact of the COVID-19 pandemic on respiratory health. As Section 2 explains, we measure the percent of observed respiratory symptoms experienced in the past seven days. The subjective COVID-19 infection risk is the perceived probability of contracting COVID-19 within the next three months. Both outcomes are available from March through July 2020 (Rounds 5-13).

We estimate versions of Equation (2) for these outcomes. The parameters of interest, $\gamma_6^N - \gamma_{13}^N$, identify the causal effects of the pandemic on remote workers and the differential effects for non-remote workers under the assumptions discussed in Section 3.1 applied to respiratory health. Since observations for these outcomes begin in Round 5, we cannot assess differential pre-trends here as we could for employment (although the recent emergence of COVID-19 suggests that pre-trends for subjective infection risk are zero).

Table 2 shows results for respiratory symptoms (Columns 1-4) and subjective infection risk (Columns 5-8). Columns 1 and 5 show differential trends in respiratory health by job type for both outcomes: in Column 1, respiratory symptoms became less prevalent for all workers from March through July, consistent with the well-know seasonal pattern of infectious diseases and allergies (Moriyama et al., 2020). However, this improvement was smaller for non-remote workers, particularly in April and June. In Column 5, perceived infection risk increased for both groups of workers in April, and more so for non-remote workers. It later fell for remote workers, but not for non-remote workers.

The remainder of Table 2 shows differential health impacts for women, workers without a college degree, African Americans, and Hispanics. In general, health worsened for African-American and non-college workers relative to Whites and college-educated workers. However, these differential health impacts were not larger than the differential effects by job type.[9]

[9] We cannot reject the hypothesis that the differential effects for non-remote workers are identical to the differential effects for African Americans ($p = 0.28$) or women ($p = 0.54$). However the differential effects for non-remote workers are significantly larger than the differential effects for non-college workers ($p = 0.02$) and Hispanics ($p = 0.04$).

The highlighted differences in respiratory health between remote and non-remote workers may reflect job-related differences in the ability to self-protect. An indirect way to gauge whether the patterns in Table 2 are linked to the nature of remote and non-remote jobs is to study the actions undertaken to reduce their risk of infection. People can improve respiratory health by engaging in protective behaviors such as social distancing, washing hands, and wearing face masks. Some behaviors, such as hand washing, are not job dependent. Others, such as social distancing, may be harder to implement for non-remote workers if their jobs force them to interact closely with customers or co-workers.

The survey measures whether respondents have engaged in several protective behaviors within the past seven days. We distinguish between behaviors that are related to work and those that are unrelated to work.[10] We use this information to test the hypothesis that non-remote workers, especially those who remained employed, practiced fewer work-related protective behaviors (but not fewer non-work-related protective behaviors).

To do that, we create two indices, ranging from 0 to 1, and measure the fraction of work-related and non-work-related protective behaviors that the respondent practiced.[11] Tables A3 and A4 estimate the simple and double differences in means of these two indices and their individual components by job type and employment status. There is not a significant gap by job type in protective behaviors that are not work related, regardless of the workers' employment status. Conversely, people with non-remote jobs in March practiced fewer work-related protective behaviors, especially if they remained employed in April-July. In this case, the double difference is 11 percentage points ($p < 0.001$), suggesting that the worse health outcomes for non-remote workers may relate to the nature of their jobs.

[10] The non-work-related behaviors include washing hands frequently, wearing a face mask for protection, avoiding bars, restaurants, and grocery stores, not visiting others' homes, not hosting others, and keeping physical distance (at least 6 feet) from household members. The work-related activities include avoiding contact with high-risk people, avoiding close contact (fewer than 6 feet) with non-household members, not sharing tools or towels, working or studying from home, sheltering in place, and avoiding public spaces. We count the fraction of non-missing activities that each person practiced in the previous seven days. Since some of these distinctions are somewhat fuzzy, we also consider the changes in specific protective behaviors.

[11] Since some items were not asked in Round 5, we restrict our analysis to Rounds 6-13 (April to July 2020). Results are similar if we add Round 5 data for items that were asked in that round.

5 Distributional Effects on Employment, Income, and Health

Besides its differential effects by job type, COVID-19 may have exacerbated existing disparities between low-income and high-income workers within each job type if economic and health impacts were concentrated among low-income households. We observe reported household income groups in the first quarter of 2020, before the start of the pandemic. Household income is strongly correlated with job type: around 55 percent of non-remote workers have household incomes below $60,000 (around the 2019 median household income), compared to 36 percent of remote workers; 29 percent of remote workers have household incomes below $30,000, compared to 11 percent of remote workers.

Figure 2 shows the evolution of the three study outcomes over time, by job type and household income group. Job losses and, to a lesser extent, health losses were highest among non-remote workers from the poorest households: 40 percent of non-remote workers with household incomes below $30,000 lost their jobs after March 2020, while only about 5 percent of remote workers from the wealthiest households lost their jobs. The difference in perceived infection risk between remote and non-remote workers is also particularly stark for workers in the poorest households, while patterns in respiratory symptoms are less clear.

6 Did Reopening Non-Essential Businesses Boost Employment?

Some policymakers have lifted non-essential business closures as a way to increase employment, especially for non-remote workers. This benefit could be small if consumers remained wary of patronizing these businesses. Moreover, reopening businesses could increase infection risk for non-remote workers.

This section assesses the impact of relaxing these restrictions on employment by job type.

We use data from the COVID-19 State Policy Database to compare states that lifted non-essential business closures earlier and later (Raifman et al., 2020). Between April 20 and May 5, 24 states lifted these restrictions, allowing some non-essential businesses to reopen.[12] The remaining states (except South Dakota, which never closed) reopened between May 6 and June 5. Using event study and difference-in-difference approaches, we provide evidence that business reopenings did not have sizable short-term effects on employment. The top three panels of Figure 3 show employment rates before and after reopening non-essential businesses. There are no statistically significant changes in employment right after reopenings for the full sample (all adults regardless of employment status; the left panel), or for remote and non-remote workers (the middle and right panels). The event study estimates in Table A5 confirm the lack of a statistically significant short-term effect of reopening.

The bottom panels of Figure 3 compare trends in employment between states that reopened early (between April 20 and May 5) and late (between May 6 and June 5). While the choice to reopen is endogenous, the employment trends were similar for these two groups in Rounds 1-4. However, the difference-in-difference exercise is inconclusive for the full sample (in the lower left) because employment pre-trends differed across early and late states starting from Round 7, violating the parallel trends assumption. By contrast, the parallel trends assumption is plausible for the remote and non-remote sub-samples in the lower middle and lower right graphs, because the trends did not differ before Round 9, when the early reopenings began to occur. The lower right panel suggests that reopening modestly improved non-remote employment. However, employment still remained 20 percent lower in July than in March. Moreover, the higher employment may have come at the cost of worse respiratory health: using the same methodologies, Figure A2 shows that respiratory health for non-remote workers worsened differentially in states that reopened early.

[12] States that lifted non-essential business closures on or before May 5 include Alabama, Alaska, Arkansas, Colorado, Georgia, Idaho, Kansas, Louisiana, Maine, Minnesota, Mississippi, Missouri, Montana, North Dakota, Ohio, Oklahoma, South Carolina, Tennessee, Texas, Utah, Vermont, Washington, West Virginia, and Wyoming. Our analysis excludes South Dakota, which did not pursue these policies. All other states reopened later.

Overall, the existing evidence suggests that lifting store closures was not sufficient to substantially boost employment over the short time horizon in our data and warns that lifting restrictions may worsen respiratory health.

7 Conclusions

Between mid-March and July 2020, 24 percent of non-remote workers and 8 percent of remote workers lost their jobs. The respiratory health and perceived COVID-19 risk of non-remote workers also worsened over time relative to remote workers. These changes are likely linked to the difficulty avoiding environmental pathogens in non-remote jobs.

The COVID-19 pandemic has hit low-income non-remote workers the hardest, exacerbating existing inequalities: both employment and, to a lesser extent, health losses were highest among non-remote workers from the poorest households.

Reopening businesses appears to be ineffective at substantially increasing employment in the very short term, and it may come at the cost of worse respiratory health for non-remote workers.

To counter the adverse effects for this group of workers, policymakers should try to reduce infection risk. Nevertheless, it appears unlikely that non-remote workers will face dramatic improvements in their employment opportunities in the short run. Adequate financial assistance for the unemployed, therefore, appears to be an essential policy tool. This combined approach would likely alleviate the higher economic and health losses experienced by non-remote workers, potentially increasing consumer demand without worsening health.

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Table 1: The Impact of COVID-19 on Job Losses by Subgroup

	Current Employment					
	(1)	(2)	(3)	(4)	(5)	(6)
Rounds 6-7	-0.10***	-0.18***	-0.085***	-0.15***	-0.11***	-0.18***
	(0.0063)	(0.0094)	(0.011)	(0.012)	(0.011)	(0.011)
Rounds 8-9	-0.10***	-0.18***	-0.098***	-0.14***	-0.11***	-0.17***
	(0.0063)	(0.0092)	(0.011)	(0.012)	(0.011)	(0.010)
Rounds 10-11	-0.091***	-0.17***	-0.11***	-0.14***	-0.13***	-0.16***
	(0.0066)	(0.0089)	(0.012)	(0.012)	(0.011)	(0.0100)
Rounds 12-13	-0.082***	-0.16***	-0.10***	-0.12***	-0.12***	-0.14***
	(0.0065)	(0.0088)	(0.011)	(0.012)	(0.011)	(0.0098)
Non-remote × Rounds 6-7			-0.16***			
			(0.017)			
Non-remote × Rounds 8-9			-0.13***			
			(0.017)			
Non-remote × Rounds 10-11			-0.10***			
			(0.017)			
Non-remote × Rounds 12-13			-0.089***			
			(0.017)			
Female × Rounds 6-7				-0.075***		
				(0.019)		
Female × Rounds 8-9				-0.073***		
				(0.018)		
Female × Rounds 10-11				-0.060***		
				(0.018)		
Female × Rounds 12-13				-0.067***		
				(0.018)		
$<$ College × Rounds 6-7					-0.13***	
					(0.018)	
$<$ College × Rounds 8-9					-0.12***	
					(0.017)	
$<$ College × Rounds 10-11					-0.074***	
					(0.017)	
$<$ College × Rounds 12-13					-0.063***	
					(0.017)	
African American × Rounds 6-7						-0.082**
						(0.036)
African American × Rounds 8-9						-0.071**
						(0.034)
African American × Rounds 10-11						-0.077**
						(0.035)
African American × Rounds 12-13						-0.066*
						(0.034)
Hispanic × Rounds 6-7						-0.014
						(0.028)
Hispanic × Rounds 8-9						-0.033
						(0.029)
Hispanic × Rounds 10-11						-0.051*
						(0.030)
Hispanic × Rounds 12-13						-0.059**
						(0.030)
Must be employed in 2020 Q1	No	Yes	Yes	Yes	Yes	Yes
Observations	73,070	38,023	38,023	38,023	38,023	38,023

Note: All regressions cover Rounds 1-13 (2019 Q1 to July 2020) and are weighted to be nationally representative in Round 5 (March 2020). Columns 1-2 follow Equation (1) and Columns 3-6 follow Equation (2). Column 1 shows effects for all respondents and Columns 2-6 limit the sample to people who were employed in 2020 Q1. Estimates include respondent fixed effects and respondent-clustered standard errors. Estimates for Rounds 1-5 and Column 6 estimates for "other race" are available from the authors. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: The Impact of COVID-19 on Respiratory Symptoms and Perceived COVID-19 Risk by Subgroup

	Respiratory Symptom Index				Perceived COVID-19 Infection Risk			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Rounds 6-7	-0.0081	-0.0012	0.00055	0.0071	0.027***	0.043***	0.028***	0.047***
	(0.0078)	(0.0068)	(0.0076)	(0.0055)	(0.0093)	(0.0088)	(0.0086)	(0.0069)
Rounds 8-9	-0.039***	-0.036***	-0.038***	-0.030***	-0.014	-0.013	-0.019**	-0.0090
	(0.0077)	(0.0068)	(0.0070)	(0.0055)	(0.0099)	(0.0086)	(0.0085)	(0.0066)
Rounds 10-11	-0.058***	-0.049***	-0.052***	-0.045***	-0.051***	-0.046***	-0.061***	-0.047***
	(0.0084)	(0.0072)	(0.0074)	(0.0058)	(0.010)	(0.0086)	(0.0085)	(0.0067)
Rounds 12-13	-0.047***	-0.039***	-0.053***	-0.040***	-0.033***	-0.025***	-0.045***	-0.028***
	(0.0079)	(0.0072)	(0.0072)	(0.0059)	(0.011)	(0.0091)	(0.0092)	(0.0069)
Non-remote × Rounds 6-7	0.018*				0.046***			
	(0.0098)				(0.012)			
Non-remote × Rounds 8-9	0.010				0.036***			
	(0.0097)				(0.012)			
Non-remote × Rounds 10-11	0.019*				0.043***			
	(0.010)				(0.013)			
Non-remote × Rounds 12-13	0.0053				0.044***			
	(0.010)				(0.013)			
Female × Rounds 6-7		0.0086				0.024**		
		(0.0094)				(0.012)		
Female × Rounds 8-9		0.0069				0.044***		
		(0.0094)				(0.012)		
Female × Rounds 10-11		0.0048				0.044***		
		(0.0099)				(0.012)		
Female × Rounds 12-13		-0.0092				0.040***		
		(0.0097)				(0.013)		
$<$ College × Rounds 6-7			0.0040				0.046***	
			(0.0097)				(0.012)	
$<$ College × Rounds 8-9			0.0088				0.047***	
			(0.0095)				(0.012)	
$<$ College × Rounds 10-11			0.0092				0.064***	
			(0.0099)				(0.012)	
$<$ College × Rounds 12-13			0.017*				0.068***	
			(0.0098)				(0.013)	
African American × Rounds 6-7				0.022				0.030
				(0.014)				(0.019)
African American × Rounds 8-9				0.037***				0.068***
				(0.014)				(0.021)
African American × Rounds 10-11				0.038**				0.11***
				(0.015)				(0.021)
African American × Rounds 12-13				0.014				0.096***
				(0.012)				(0.021)
Hispanic × Rounds 6-7				-0.035**				0.030*
				(0.015)				(0.018)
Hispanic × Rounds 8-9				-0.031**				0.048***
				(0.015)				(0.019)
Hispanic × Rounds 10-11				-0.030*				0.058***
				(0.015)				(0.019)
Hispanic × Rounds 12-13				-0.021				0.078***
				(0.016)				(0.021)
Observations	27,851	27,851	27,851	27,851	27,851	27,851	27,851	27,851

Note: All regressions cover Rounds 5-13 (March to July 2020) and are weighted to be nationally representative in Round 5 (March 2020). All regressions follow Equation (2) and limit the sample to people who were employed in Rounds 5. Estimates include respondent fixed effects and respondent-clustered standard errors. Column 6 estimates for "other race" are available from the authors. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

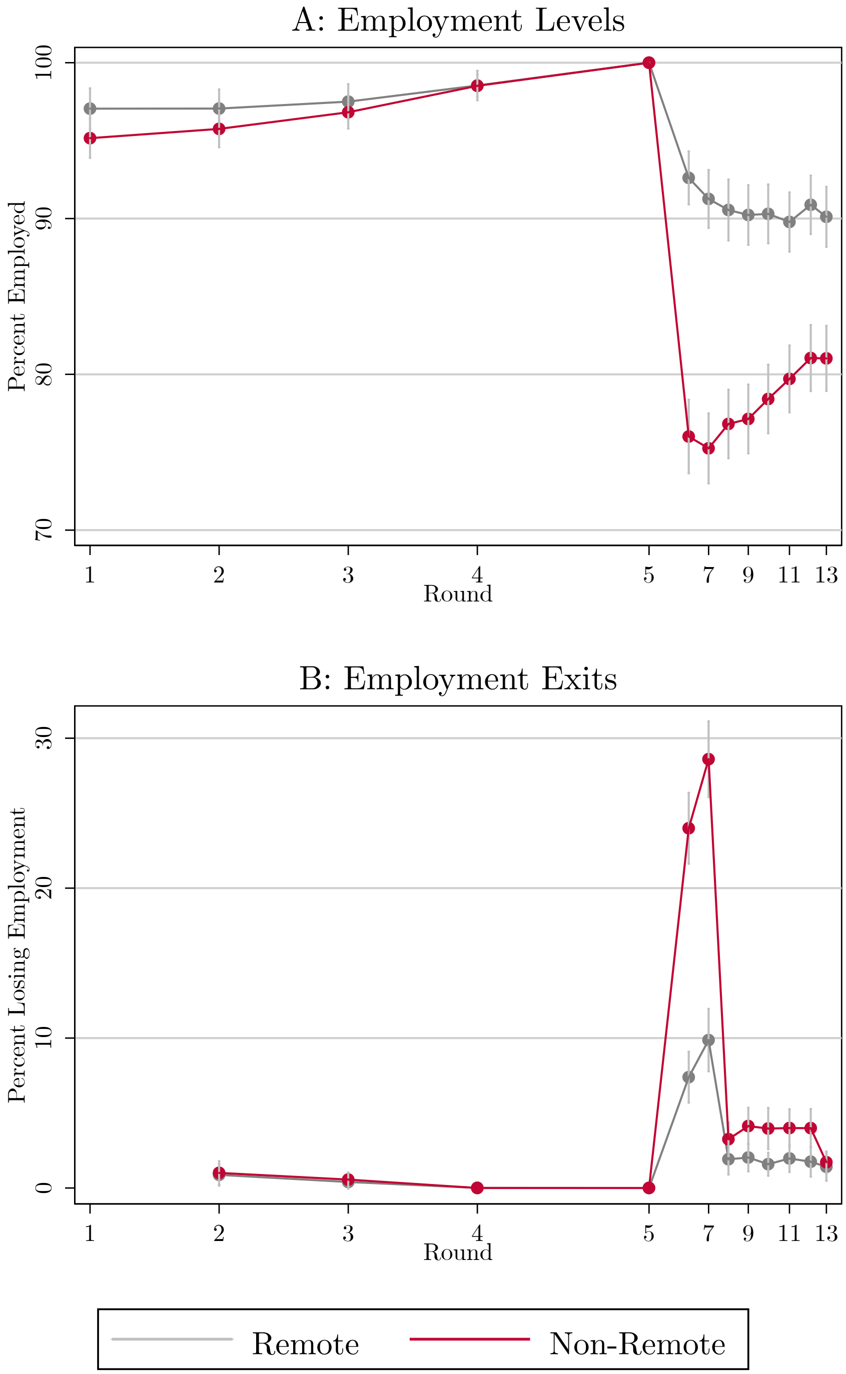

Figure 1: Employment Levels and Exits for People Employed in Round 5, by Job Type.

Note: the figure shows employment levels (top panel) and losses of employment (bottom panel) by round and remote or non-remote job type. Estimates are weighted to be nationally representative in Round 5 (March 2020). Error bars indicate 90 percent confidence intervals based on OLS regressions with respondent-clustered standard errors.

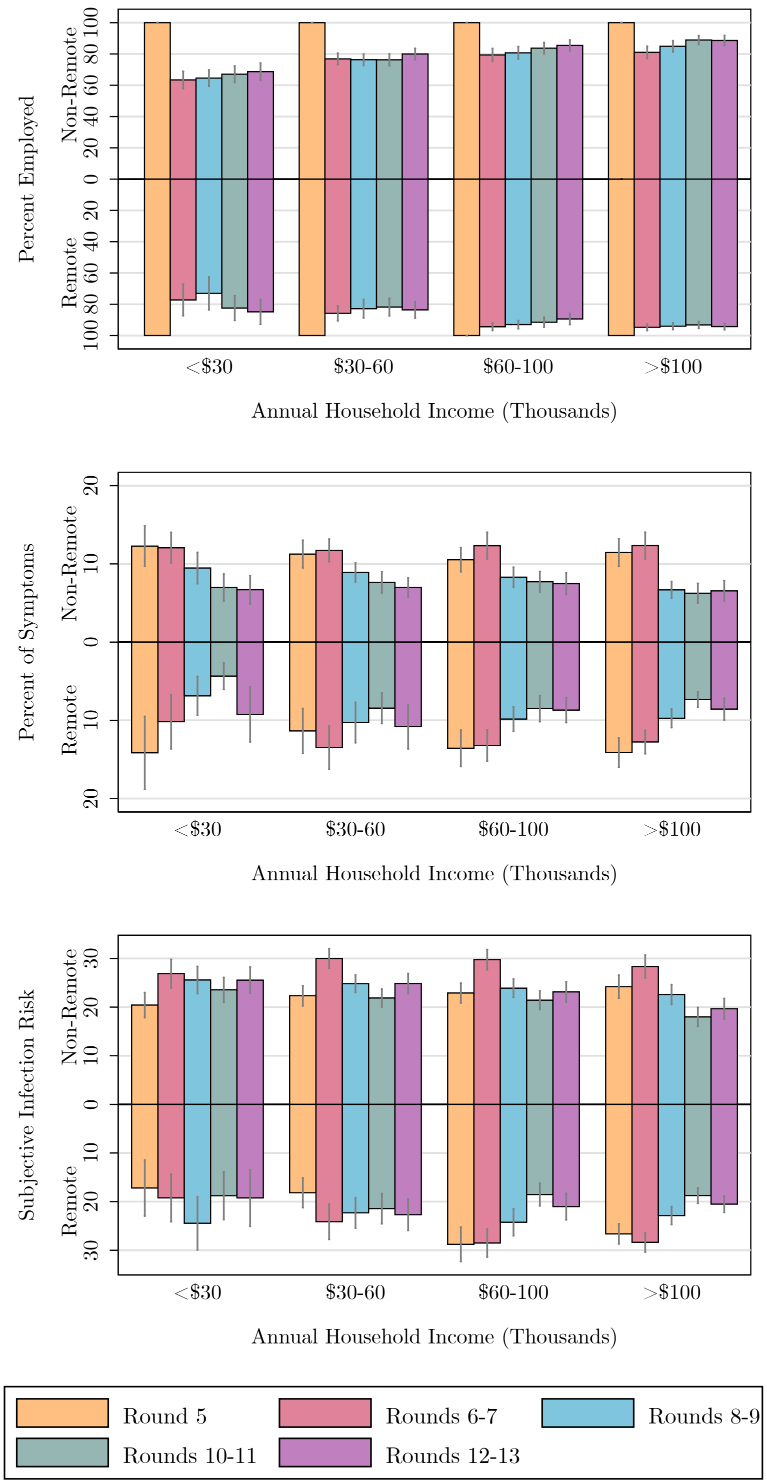

Figure 2: Impacts on Employment, Respiratory Symptoms, and Subjective COVID-19 Infection Risk, by Household Income and Job Type

Note: the figure shows impacts on employment (top panel), respiratory symptoms (middle panel), and subjective COVID-19 infection risk (bottom panel) for remote and non-remote workers in four annual household income bins. Error bars show 90 percent confidence intervals. We restrict the sample to people who were employed in Round 5 (March 2020).

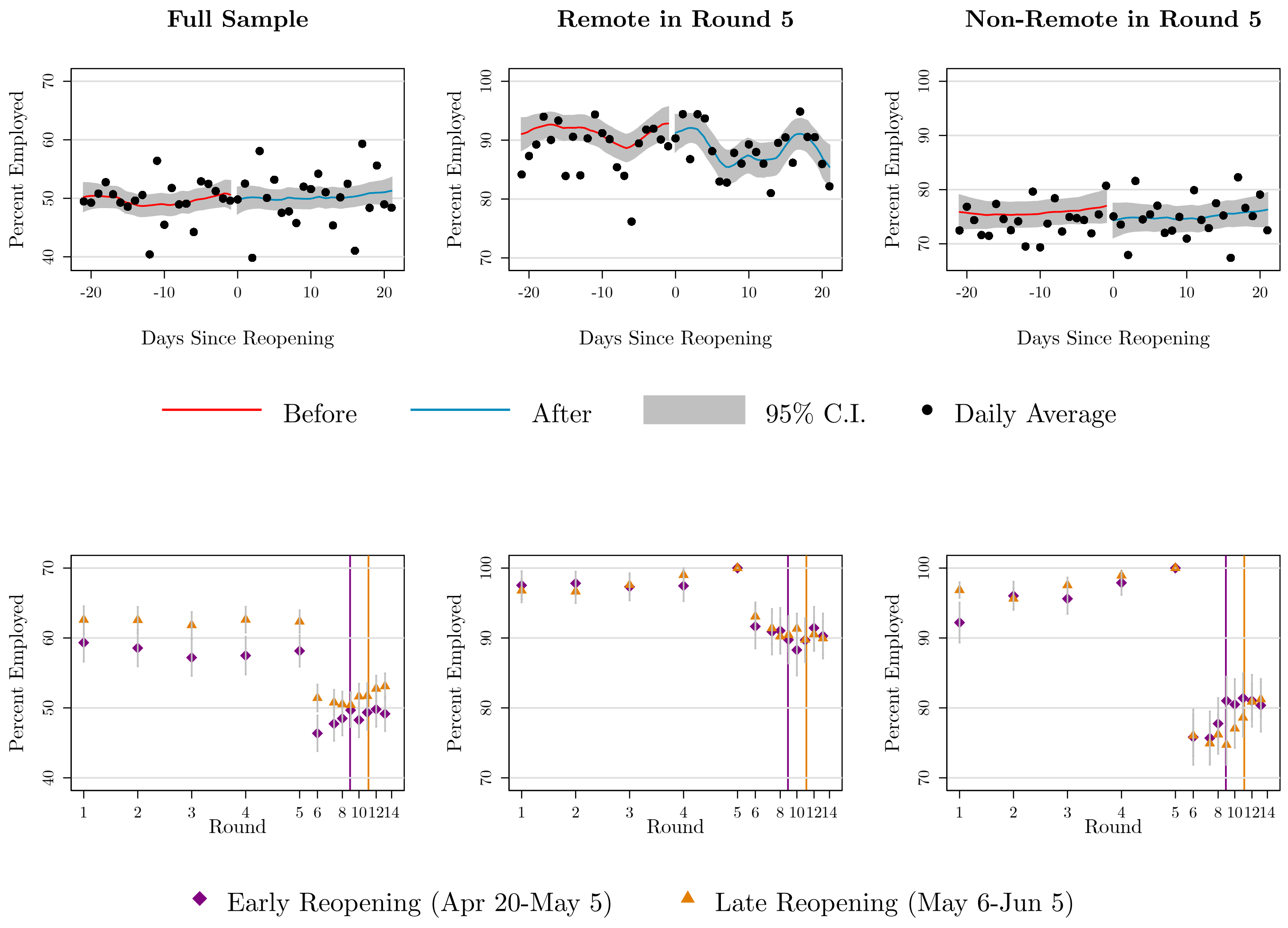

Figure 3: The Impact of Non-Essential Business Reopenings on Employment

Note: the figure shows event study (top panel) and difference-in-difference estimates of the impact of lifting non-essential business restrictions on employment. We show patterns for the full sample regardless of employment status (left) and people who were employed in March 2020 in remote jobs (middle) and non-remote jobs (right). The top panel uses local linear regressions to estimate a break in employment around the day of the reopening. The bottom panel plots employment patterns by round for states that reopened early (April 20-May 5) and states that reopened late (May 6-June 5). The purple vertical bars indicate the latest reopening date in "early" states and the orange vertical bars indicate the latest reopening dates in "late" states.

A Online Appendix – Not for Publication

A.1 Measurement Error in Employment Status

As in other employment surveys, employment may be mismeasured for some respondents. Because Equation (2) restricts the sample to people who were employed in Round 5, measurement error may bias downward (away from zero) our estimates of the impact of the pandemic on employment: this could occur if we erroneously count people who misreport being employed in Round 5 as subsequently losing their jobs.

A change in the phrasing of the employment question between Rounds 1-4 and Rounds 5-13 may exacerbate this issue. Surveys in Rounds 1-4 ask "What is your labor force status? Please choose all that apply: 1 Currently working; 2 On sick or other leave; 3 Unemployed - on layoff; 4 Unemployed - looking; 5 Retired ; 6 Disabled; 7 Other," while surveys in Rounds 5-13 ask "Do you currently have a job?". We consider people to be employed in Rounds 1-4 if they picked Options 1 or 2 and to be employed in Rounds 5-13 if they answered "yes".

To investigate, we compare employment responses in Round 5 (fielded between March 10-31, with 85 percent of responses happening by March 17) with responses to the quarterly survey in the first quarter of 2020 (fielded uniformly throughout the quarter). Specifically, we measure discrepancies in reported employment for people who answered the two surveys on the same date (16 percent of the Round 5 sample). The employment means are similar in the two surveys, consistent with classical measurement error. However, 3 percent of the remote sample and 5 percent of the non-remote sample (all of whom were employed in Round 5 by definition) did not report being employed in the quarterly survey. The implication of this finding is that not addressing mismeasurement may lead us to overestimate both total job losses from March 2020 and the differential job losses for non-remote workers.

To minimize the amount of measurement error in employment, we classify respondents as employed in Round 5 if they also reported being employed in the quarterly survey from the first quarter of 2020. This conservative approach reduces the number of people whose subsequent unemployment we might attribute to COVID-19. To assess the implications this classification, we reproduce all results without conditioning on employment in the first quarter of 2020. Impacts on employment are 1-2 percentage points higher while impacts on respiratory health are similar to the results in Table 2. These estimates are available upon request.

A.2 Regression Discontinuity Estimates of the Effect of Reopening on Employment

We use a sharp regression discontinuity design to measure whether reopening non-essential businesses led to employment gains in the days and weeks after the lifting of the restrictions. We have 3 different samples: (1) all US adults, (2) remote workers in Round 5, and (3) non-remote workers in Round 5. For each of these groups, we obtained RD estimates using bandwidths of 7, 14, and 21 days before and after reopening. These bandwidths approximately correspond to the bandwidths selected through the one common mean squared error (MSE)-optimal bandwidth selector from Calonico et al. (2014) if we restrict the running variable to 50 days before and after reopening (in which case the optimal bandwidths vary between 12 and 17 days) or if we do not restrict the running variable (in which case the optimal bandwidths vary between 19 and 22 days).[13] We use a local linear regression estimator, a uniform kernel, weights to make the data nationally representative, and standard errors clustered by respondent.

Table A5 shows that, regardless of bandwidth choice, the estimates are small and statistically insignificant. Moreover, the point estimates for non-remote workers are always negative. We find no evidence of an increase in employment in the 1-3 weeks following business reopenings.

[13] We also produced RD estimates using each of these bandwidths. The results are robust to the bandwidth choice.

Table A1: Summary Statistics for Respondents in Remote and Non-Remote Jobs

	Overall	By March 2020 Job Type		Significance
		Remote	Non-Remote	(2) minus (3)
	(1)	(2)	(3)	(4)
A: Demographic Characteristics				
Female	0.52	0.49	0.50	
White	0.63	0.60	0.62	
African American	0.12	0.09	0.12	***
Hispanic	0.17	0.18	0.19	
Other Race	0.09	0.14	0.07	***
Bachelors Degree or Higher	0.34	0.68	0.26	***
B: Outcomes				
Employed				
Rounds 1-4	0.61	0.98	0.97	
Round 5	0.61	1.00	1.00	
Rounds 6-7	0.50	0.92	0.76	***
Rounds 8-9	0.50	0.90	0.77	***
Rounds 10-11	0.51	0.90	0.79	***
Rounds 12-13	0.51	0.90	0.81	***
Respiratory Symptoms Index				
Round 5	0.12	0.13	0.11	***
Rounds 6-7	0.12	0.13	0.12	*
Rounds 8-9	0.10	0.10	0.09	***
Rounds 10-11	0.08	0.08	0.07	
Rounds 12-13	0.08	0.09	0.07	***
Perceived COVID-19 Infection Risk				
Round 5	0.21	0.25	0.22	***
Rounds 6-7	0.26	0.27	0.29	***
Rounds 8-9	0.23	0.23	0.24	***
Rounds 10-11	0.21	0.19	0.21	***
Rounds 12-13	0.23	0.21	0.24	***
Protective Behavior (Unrelated to Work)				
Rounds 6-7	0.60	0.60	0.61	***
Rounds 8-9	0.65	0.65	0.66	
Rounds 10-11	0.66	0.66	0.66	
Rounds 12-13	0.67	0.67	0.67	
Protective Behavior (Related to Work)				
Rounds 6-7	0.64	0.72	0.57	***
Rounds 8-9	0.56	0.64	0.49	***
Rounds 10-11	0.50	0.57	0.42	***
Rounds 12-13	0.51	0.58	0.42	***
Weighted Number of Respondents	6932	1400	2217	–

Note: The table shows sample characteristics and key outcomes overall (Column 1) and by job type (Columns 2 and 3) as of Round 5 (March 2020). "Remote" jobs can be carried out remotely, while "non-remote" jobs must be carried out at the employer's office or work site. Estimates are weighted to be nationally representative as of Round 5. Stars in Column 5 indicate statistically significant differences between the remote and non-remote jobs. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A2: The Impact of COVID-19 on Employment by Subgroup

	Current Employment	Respiratory Symptom Index	Perceived COVID-19 Infection Risk
	(1)	(2)	(3)
Rounds 7-8	-0.029*	-0.0067	0.30
	(0.015)	(0.0098)	(1.24)
Rounds 9-10	-0.039**	-0.040***	-5.64***
	(0.015)	(0.0096)	(1.23)
Rounds 11-12	-0.064***	-0.058***	-9.96***
	(0.015)	(0.010)	(1.29)
Rounds 13-14	-0.061***	-0.046***	-8.09***
	(0.015)	(0.0097)	(1.29)
Non-remote × Rounds 7-8	-0.13***	0.019*	3.23**
	(0.019)	(0.010)	(1.30)
Non-remote × Rounds 9-10	-0.097***	0.0060	1.92
	(0.020)	(0.010)	(1.35)
Non-remote × Rounds 11-12	-0.086***	0.017	1.80
	(0.019)	(0.011)	(1.34)
Non-remote × Rounds 13-14	-0.076***	-0.0011	1.67
	(0.019)	(0.011)	(1.42)
Female × Rounds 7-8	-0.061***	0.0081	1.80
	(0.018)	(0.0095)	(1.16)
Female × Rounds 9-10	-0.061***	0.0050	3.52***
	(0.018)	(0.0096)	(1.17)
Female × Rounds 11-12	-0.049***	0.0028	3.06**
	(0.018)	(0.010)	(1.20)
Female × Rounds 13-14	-0.058***	-0.0084	2.56**
	(0.018)	(0.0100)	(1.25)
< College × Rounds 7-8	-0.072***	-0.0024	2.84**
	(0.020)	(0.010)	(1.29)
< College × Rounds 9-10	-0.073***	0.0059	3.42***
	(0.020)	(0.010)	(1.30)
< College × Rounds 11-12	-0.028	0.0031	4.85***
	(0.020)	(0.011)	(1.28)
< College × Rounds 13-14	-0.018	0.017	5.37***
	(0.020)	(0.011)	(1.36)
African American × Rounds 7-8	-0.051	0.019	2.19
	(0.036)	(0.014)	(1.98)
African American × Rounds 9-10	-0.042	0.035**	5.67***
	(0.035)	(0.014)	(2.12)
African American × Rounds 11-12	-0.058*	0.036**	10.1***
	(0.035)	(0.015)	(2.09)
African American × Rounds 13-14	-0.053	0.015	9.64***
	(0.035)	(0.012)	(2.10)
Hispanic × Rounds 7-8	-0.00065	-0.036**	2.38
	(0.028)	(0.016)	(1.83)
Hispanic × Rounds 9-10	-0.019	-0.033**	4.04**
	(0.029)	(0.016)	(1.89)
Hispanic × Rounds 11-12	-0.043	-0.031*	4.93***
	(0.029)	(0.016)	(1.91)
Hispanic × Rounds 13-14	-0.055*	-0.024	6.98***
	(0.031)	(0.016)	(2.11)
Must be employed in 2020 Q1	Yes	Yes	Yes
Observations	41,159	27,323	27,323

Note: Regressions use data from Rounds 1-13 (Quarter 1 of 2019 to July 2020) and are weighted to be nationally representative in Round 5 (March 2020). Regressions include respondent fixed effects. Standard errors are clustered by respondent. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Protective Behaviors **Unrelated to Work**, by Employment Status and Job Type

	Percent of Behaviors	No Close Contact w/ HH Members	Wear Face Mask	Wash Hands Frequently	Avoid Restaurants	Avoid Bars	Avoid Grocery Stores	No Home Visits to Others	No Visitors at Home
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A: Unemployed Sample									
Remote	0.65	0.21	0.81	0.91	0.84	0.95	0.79	0.35	0.39
Non-Remote	0.65	0.23	0.80	0.95	0.77	0.95	0.81	0.38	0.34
Remote−Non-Remote	0.000055	0.024	-0.015	0.040	-0.064**	0.00024	0.024	0.036	-0.045
	(0.013)	(0.038)	(0.040)	(0.030)	(0.031)	(0.014)	(0.040)	(0.046)	(0.044)
B: Employed Sample									
Remote	0.65	0.15	0.80	0.95	0.77	0.93	0.80	0.39	0.37
Non-Remote	0.65	0.17	0.79	0.94	0.68	0.91	0.88	0.42	0.40
Remote−Non-Remote	0.0034	0.024	-0.013	-0.015*	-0.093***	-0.020**	0.080***	0.030*	0.035**
	(0.0054)	(0.016)	(0.015)	(0.0084)	(0.016)	(0.0082)	(0.014)	(0.018)	(0.018)
Difference in Difference	0.0034	0.00043	0.0013	-0.055*	-0.029	-0.021	0.056	-0.0063	0.080*
	(0.014)	(0.040)	(0.042)	(0.031)	(0.034)	(0.016)	(0.041)	(0.048)	(0.047)
Observations	22,148	22,148	22,148	22,148	22,148	22,148	22,148	22,148	22,148

Note: the table shows the use of protective behaviors unrelated to work in Rounds 6-13 by job type (remote or non-remote) and employment. Column 1 shows the percent of the behaviors utilized and Columns 2-9 show the utilization of specific behaviors. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Protective Behavior **Related to Work**, by Employment Status and Job Type

	Percent of Behaviors	Work/Study From Home	Stay Home	Avoid Public Places	Avoid Sharing Items	Avoid High Risk People	No Close Contact w/ Non-HH Members
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A: Unemployed Sample							
Remote	0.64	0.63	0.65	0.87	0.30	0.85	0.53
Non-Remote	0.57	0.40	0.61	0.82	0.23	0.86	0.53
Remote−Non-Remote	-0.062***	-0.23***	-0.035	-0.055**	-0.073*	0.015	0.0069
	(0.019)	(0.048)	(0.039)	(0.027)	(0.042)	(0.031)	(0.044)
B: Employed Sample							
Remote	0.63	0.84	0.54	0.84	0.26	0.87	0.44
Non-Remote	0.46	0.27	0.44	0.73	0.22	0.78	0.32
Remote−Non-Remote	-0.17***	-0.57***	-0.10***	-0.11***	-0.040**	-0.088***	-0.12***
	(0.0089)	(0.017)	(0.017)	(0.015)	(0.017)	(0.014)	(0.017)
Difference in Difference	-0.11***	-0.34***	-0.068	-0.054*	0.034	-0.10***	-0.13***
	(0.020)	(0.050)	(0.042)	(0.031)	(0.044)	(0.033)	(0.046)
Observations	22,148	22,148	22,148	22,148	22,148	22,148	22,148

Note: the table shows the use of protective behaviors related to work in Rounds 6-13 by job type (remote or non-remote) and employment. Column 1 shows the percent of the behaviors utilized and Columns 2-7 show the utilization of specific behaviors. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Regression Discontinuity Estimates of the Effect of Business Reopening on Employment

	Current Employment		
	(1)	(2)	(3)
Bandwidth:			
7 Days	-0.015	-0.0062	-0.033
	(0.034)	(0.037)	(0.053)
N	*6406*	*1388*	*1970*
14 Days	-0.0083	0.016	-0.019
	(0.027)	(0.029)	(0.040)
N	*12,423*	*2708*	*3795*
21 Days	-0.00077	0.0066	-0.011
	(0.013)	(0.015)	(0.021)
N	*18,300*	*3981*	*5591*
Sample	Full	Remote	Non-Remote

Note: Estimates use a local linear regression estimator, a uniform kernel, weights to make the data nationally representative, and standard errors clustered by respondent (in parentheses). Each row reports the coefficient for the "post-reopening" indicator in a regression with current employment as the dependent variable. Regression sample sizes appear in italics below standard errors. Each row provides estimates for a specific bandwidth. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

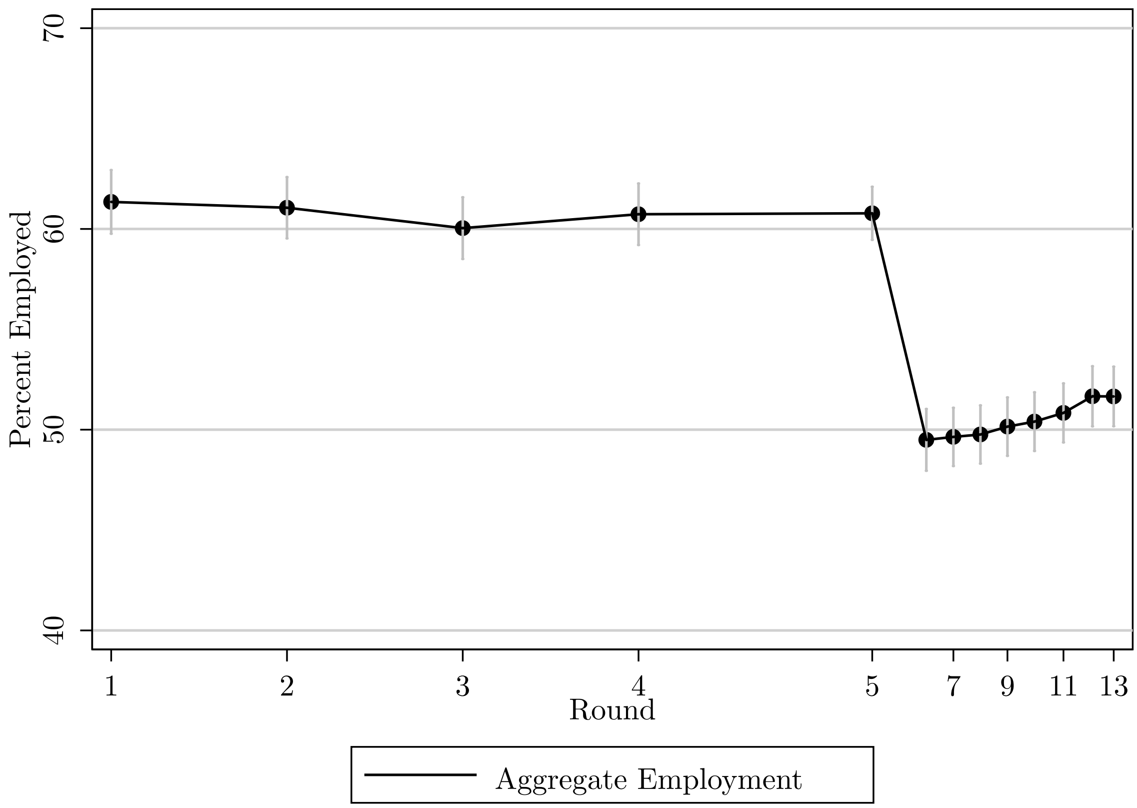

Figure A1: Aggregate Employment by Survey Round

Note: the figure shows the percent of all respondents who were employed in each survey round. Rounds 1-4 correspond to Quarters 1-4 of 2019, Round 5 corresponds to Quarter 1 of 2020, Rounds 6-14 are high-frequency panel survey from March 10-June 21, 2020. The figure shows 90 percent confidence intervals based on OLS regressions with respondent-clustered standard errors.

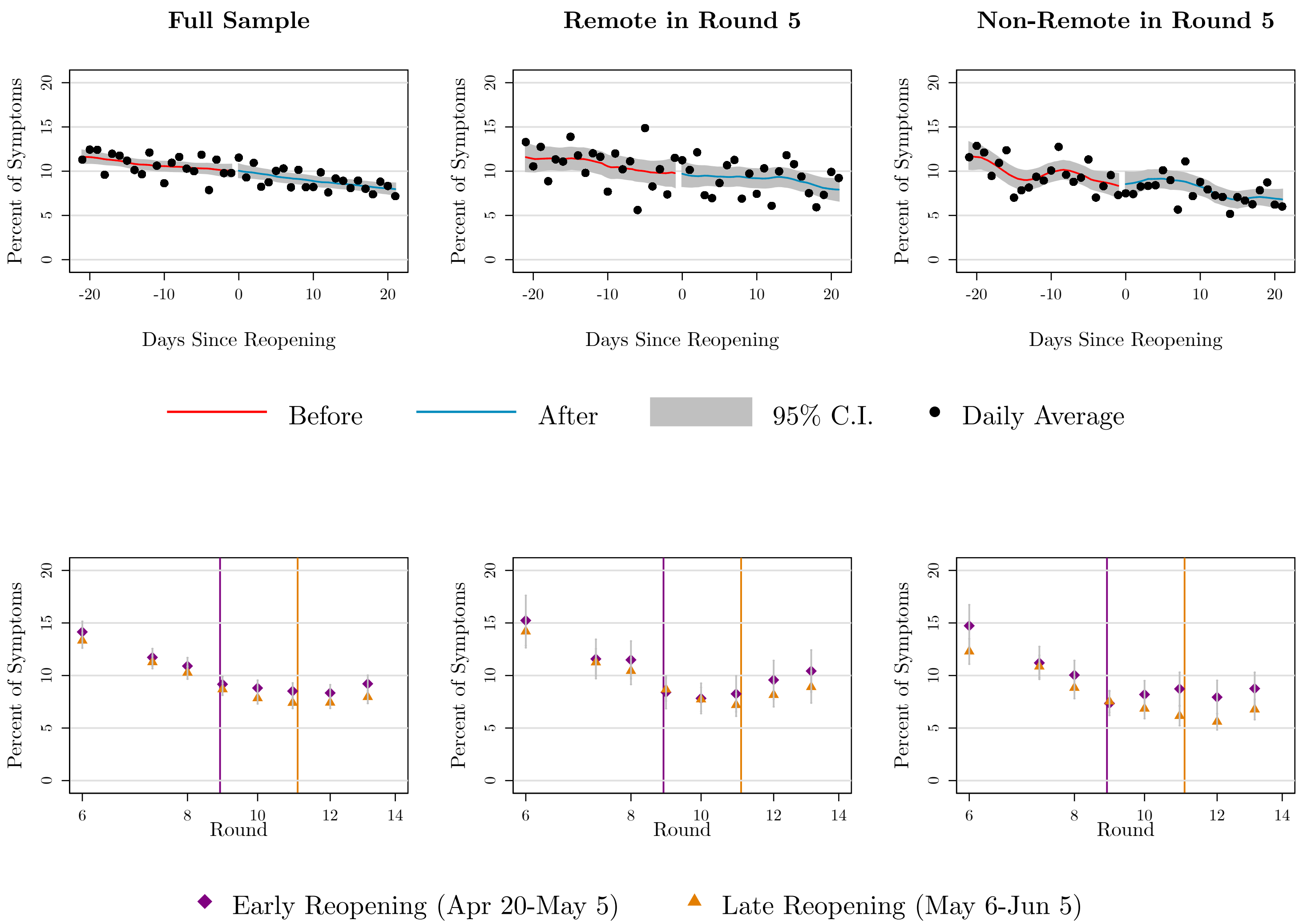

Figure A2: The Impact of Non-Essential Business Reopenings on Respiratory Illness Symptoms

Note: the figure shows event study (top panel) and difference-in-difference estimates of the impact of lifting non-essential business restrictions on symptoms of respiratory illness. We show patterns for the full sample regardless of employment status (left) and people who were employed in March 2020 in remote jobs (middle) and non-remote jobs (right). The top panel uses local linear regressions to estimate a break in symptoms around the day of the reopening. The bottom panel plots symptoms patterns by round for states that reopened early (April 20-May 5) and states that reopened late (May 6-June 5). The purple vertical bars indicate the latest reopening date in “early” states and the orange vertical bars indicate the latest reopening dates in “late” states.

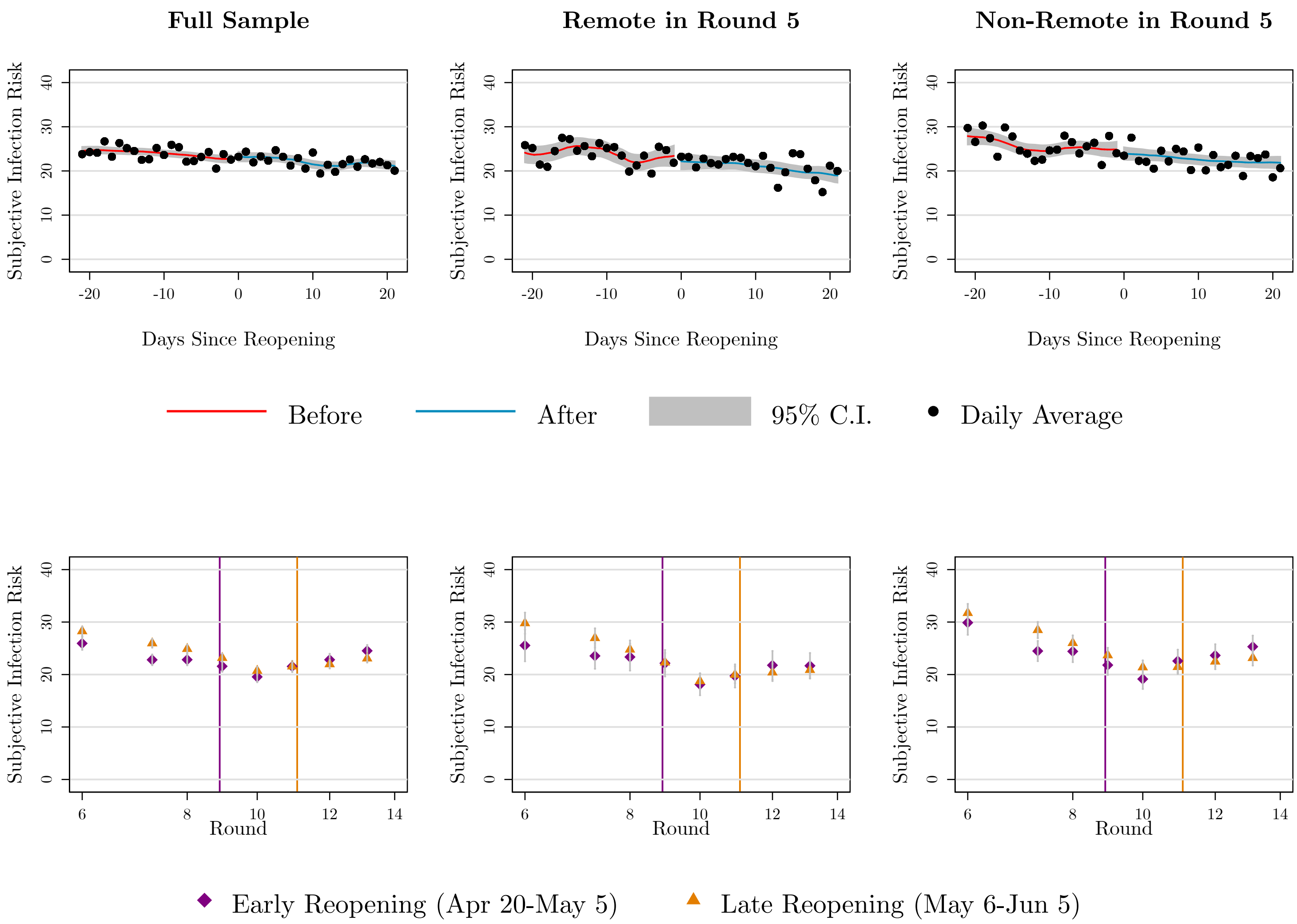

Figure A3: The Impact of Non-Essential Business Reopenings on Subjective COVID-19 Infection Risk

Note: the figure shows event study (top panel) and difference-in-difference estimates of the impact of lifting non-essential business restrictions on subjective COVID-19 infection risk. We show patterns for the full sample regardless of employment status (left) and people who were employed in March 2020 in remote jobs (middle) and non-remote jobs (right). The top panel uses local linear regressions to estimate a break in infection risk around the day of the reopening. The bottom panel plots infection risk patterns by round for states that reopened early (April 20-May 5) and states that reopened late (May 6-June 5). The purple vertical bars indicate the latest reopening date in “early” states and the orange vertical bars indicate the latest reopening dates in “late” states.