The Impacts of Covid-19 Absences on Workers*

Gopi Shah Goda Evan J. Soltas

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Abstract

We show that Covid-19 illnesses and related work absences persistently reduce labor supply. Using an event study, we estimate that workers with week-long Covid-19 absences are 7 percentage points less likely to be in the labor force one year later compared to otherwise-similar workers who do not miss a week of work for health reasons. Our estimates suggest Covid-19 absences have reduced the U.S. labor force by approximately 500,000 people (0.2 percent of adults) and imply an average labor supply loss per Covid-19 absence equivalent to $9,000 in forgone earnings, about 90 percent of which reflects losses beyond the initial absence week.

Keywords: Covid-19, coronavirus, labor supply, work absence, participation
JEL Codes: I12, J17, J21, J22

*Goda: Stanford Institute for Economic Policy Research and NBER (gopi@stanford.edu). Soltas: MIT Economics (esoltas@mit.edu). We thank the editor Nathan Hendren and three anonymous reviewers for suggestions that improved the paper. We thank Alan Auerbach, David Autor, Justin Bloesch, David Cutler, Martha Gimbel, Daniele Girardi, Dev Patel, Jim Poterba, Brendan Price, Cecilia Rouse, Charlie Rafkin, Aaron Sojourner, Ben Sprung-Keyser, Michael Stepner, Ernest Tedeschi, and seminar participants at the NBER Summer Institute, the Congressional Budget Office, and the SIEPR Working Longer Conference for helpful comments. We gratefully acknowledge support from the National Science Foundation Graduate Research Fellowship (Grant No. 1122374, Soltas) and from the National Institute on Aging of the National Institutes of Health (P30AG012810, Goda and Soltas). The content is solely the responsibility of the authors and does not necessarily represent the official views of the National Institutes of Health.

1 Introduction

There have been over 65 million reported cases and approximately 250,000 deaths from Covid-19 among working-age U.S. adults through December 2022. An emerging body of medical research finds that many who fall ill but survive Covid-19 suffer from enduring health problems. Such research has measured the prevalence and severity of various post-acute conditions of Covid-19, such as chronic fatigue and organ damage.[1] Many in government and the media have speculated that such post-acute conditions have reduced labor supply, but data limitations have made it difficult to assess these impacts and the economic costs of Covid-19 illnesses more broadly.

This paper studies the impacts of work absences related to Covid-19 on the labor supply of U.S. workers using longitudinal data from the Current Population Survey (CPS). We first document that the rate of week-long health-related absences has been substantially elevated during the pandemic relative to pre-pandemic seasonal patterns, and that these excess absences appear to reflect Covid-19 illnesses as well as other related absences, such as quarantines and efforts to avoid exposure. In a typical pandemic week, about ten workers per thousand missed an entire week of work due to their own health problems, as compared to six health-related absences per thousand workers in an average week from 2010 to 2019. Excess absences covary with reported Covid-19 cases both in the national time series and a state panel, and they are higher among workers in occupations with greater likely exposure to Covid-19.

Second, we show that health-related absences generate persistent reductions in labor supply. Using an event study, we find that workers who miss an entire week due to a probable Covid-19 absence are approximately 7 percentage points less likely to be in the labor force one year after their absence compared to otherwise-similar workers who do not miss work for health reasons. These estimates appear representative for Covid-19 specifically, not only as an average over myriad health issues, and they are about the same as the average effects of pre-pandemic health-related absences. One reason why Covid-19 absences reduce labor supply is that they push older workers into retirement. Our results are thus best interpreted as identifying not "long Covid" in isolation but rather the overall labor-supply adjustment, across mechanisms, induced by Covid-19.

We next use our results to assess the aggregate impacts of Covid-19 absences on the U.S. labor force participation rate. Combining our event-study results with the excess rate of health-related absences, we estimate that Covid-19 absences have reduced the labor force participation rate by 0.2 percentage points, or by approximately 500,000 workers, through June 2022. This point-in-time impact is near our estimates of the steady-state impact of Covid-19 absences at the average rate of health-related absences in 2021. These losses fundamentally reflect an overall increase in the

[1] These conditions are formally known as post-acute sequelae of SARS-CoV-2 infection or post-acute coronavirus disease syndrome (PASC or PACS) and are often informally called "long Covid." See Groff et al. (2021) for a systematic review of the health research.

health-related absence rate with no change in the average long-run impact of an absence relative to before the pandemic. We also discuss additional considerations that would raise the total effect of Covid-19 absences on labor supply relative to our preferred estimate. Overall, the loss in U.S. labor supply from Covid-19 absences appears substantial, comparable to an additional year of U.S. population aging at its current pace.

Finally, we calculate the average loss in labor supply resulting from a Covid-19 absence in terms of its earnings equivalent through fourteen months after the absence, incorporating both extensive and intensive margins of adjustment. The average Covid-19 absence results in a loss equivalent to at least $9,000 in earnings, about 90 percent of which is due to persistent labor-supply reduction beyond the week-long absence itself. In aggregate, we calculate that the per-year value of the lost labor supply is approximately $62 billion, which is about half of estimated losses from cancer or diabetes (Yabroff et al., 2011; American Diabetes Association, 2018).

This paper relates to several strands of literature in labor and health economics. Much research has quantified the impacts of ill-health, using similar event-study approaches to study hospitalizations (García-Gómez et al., 2013; Dobkin et al., 2018; Stepner, 2019), cancer (Gupta et al., 2017), mental-health conditions (Biasi et al., 2021), and abortion denials (Miller et al., 2020). Such analyses find large, persistent declines in employment and earnings after health shocks.

Economists have also thoroughly examined the macroeconomic effects of the pandemic on labor markets (e.g., Adams-Prassl et al., 2020; Chetty et al., 2020). A related literature has examined variation in the pandemic's impact across demographic groups but do not distinguish between health-related effects and the contemporaneous macroeconomic shock (Montenovo et al., 2022; Coibion et al., 2020; Quinby et al., 2021; Goda et al., 2022). Several analyses have conjectured that losses from Covid-19 illnesses may be significant. Medical researchers have documented a variety of long-term adverse health consequences among infected adults relative to a control group, including kidney outcomes (Bowe et al., 2021), long Covid (Ayoubkhani et al., 2021), mental health (Xie et al., 2022a), and cardiovascular outcomes (Xie et al., 2022b). Early in the pandemic, Cutler and Summers (2020) projected that the monetized quality-of-life losses from complications of Covid-19 could amount to $2.6 trillion.

Recent survey evidence has also connected long Covid with labor force exit: Davis et al. (2021), Evans et al. (2021), Ziauddeen et al. (2022), and Ham (2022) report that approximately 20 percent of their respective respondents were not working due to health issues related to Covid-19. Combining these surveys with Covid-19 case rates and prevalence of long-Covid symptoms, authors have inferred labor force losses of 1.5 million workers or more (Bach, 2022; Domash and Summers, 2022; Cutler, 2022).[2] However, the existing survey evidence has several limitations, including the

[2]Some estimates are as high as 4 million, which would imply that but for Covid-19 illnesses, the labor force would have grown faster than pre-pandemic.

absence of control groups, self-reported attribution of nonemployment to long Covid, and in some cases unrepresentative samples. While Fischer et al. (2021) finds that Covid-19 reduces worker performance in the context of professional soccer, these results may not apply generally.

We contribute to this existing literature by providing, to the best of our knowledge, the first empirical analysis of Covid-19's direct impacts on labor supply for a representative population of workers. To do so, we propose and evaluate health-related absences as a proxy for probable Covid-19 illness that is available in many labor force surveys.[3] Our approach can enable additional research on the direct impacts of probable Covid-19 illnesses for a wide range of outcomes. It also suggests real-time survey-based monitoring strategies for future epidemics and public-health management.[4] Finally, our approach contributes to the broader literature on the effects of health shocks on labor market outcomes. In particular, we quantify impacts of less-severe health-related events than hospitalizations or other serious conditions, which is the primary focus of existing research. We also make full use of the detail and high-frequency nature of our data to explore the mechanisms by which health shocks affect labor supply.

Our paper also relates to policy discussions regarding "missing" workers from the labor force after the pandemic, most prominently by Federal Reserve Chair Jerome Powell.[5] While many factors likely contributed to changes in the participation rate over this period, including population aging, our study suggests that long-term health consequences from Covid-19 illness can explain a sizable share of the reduction in labor force participation (Sheiner and Salwati, 2022; Abraham and Rendell, 2023).

2 Covid-19 Illnesses and Work Absences

This section describes our data and the relationship between Covid-19 illnesses and health-related absences. We present three pieces of evidence that, taken together, suggest that a large fraction of all health-related absences during the pandemic were due to Covid-19.

2.1 Work Absences in the Current Population Survey

Throughout this paper, we study absences in public microdata from the monthly U.S. Current Population Survey (CPS, Flood et al., 2021). A worker is recorded absent if they are currently employed but worked zero hours in the CPS reference week, which is the calendar week that includes the twelfth of the month. All absent workers are further asked for the "main reason" for their absence. Among the fourteen reasons that workers may provide is their "own illness/injury/medical

[3] E.g., the labor force surveys of Australia, Canada, the European Union, and the United Kingdom include questions about health-related absences.

[4] For instance, the U.S. Centers for Disease Control and Prevention have used the health-related absence rate to monitor seasonal influenza (Groenewold et al., 2019).

[5] Speech by Chair Powell on "Inflation and the Labor Market," 30 November 2022, available at `https://www.federalreserve.gov/newsevents/speech/powell20221130a.htm`.

problems," what we refer to in this paper as a *health-related absence*.

We focus our analysis on non-institutionalized civilian adults age 16 and older who do not have any indications of pre-existing health issues before their absence.[6] Health was the second most common main reason for absences in this population, representing about 18 percent of absences, following vacations (see Appendix Figure A1).

Survey enumerators were repeatedly instructed to classify some circumstances related to Covid-19 other than illness as health-related absences.[7] These include quarantines as well as absences to avoid Covid-19 exposure. We therefore interpret our results as reflecting several reasons for absence related to Covid-19, including but not only illness.

Other features of this definition of absences merit emphasis in interpreting our results. First, the CPS does not record absences outside of the reference week. An important implication is that the CPS likely understates the monthly share of workers in its sample with week-long absences, an issue to which we return in Section 4. Second, we study week-long absences, not workers who work fewer hours than usual during the reference week. Appendix Figure A2 shows that health-related hours reductions do not appear to be significantly elevated during the pandemic, except in January 2022. Third, the definition of absence refers explicitly to the worker's own health. It thus excludes absences related to the health of others. Finally, the category of health-related absences inherently includes many health problems, not only Covid-19. However, we show in this section that a substantial share of health-related absences during the pandemic are likely to reflect Covid-19. In Section 3, we seek to isolate the effects of Covid-19 absences from those of non-Covid health-related absences.

2.2 Health-Related Absences as a Proxy for Covid-19

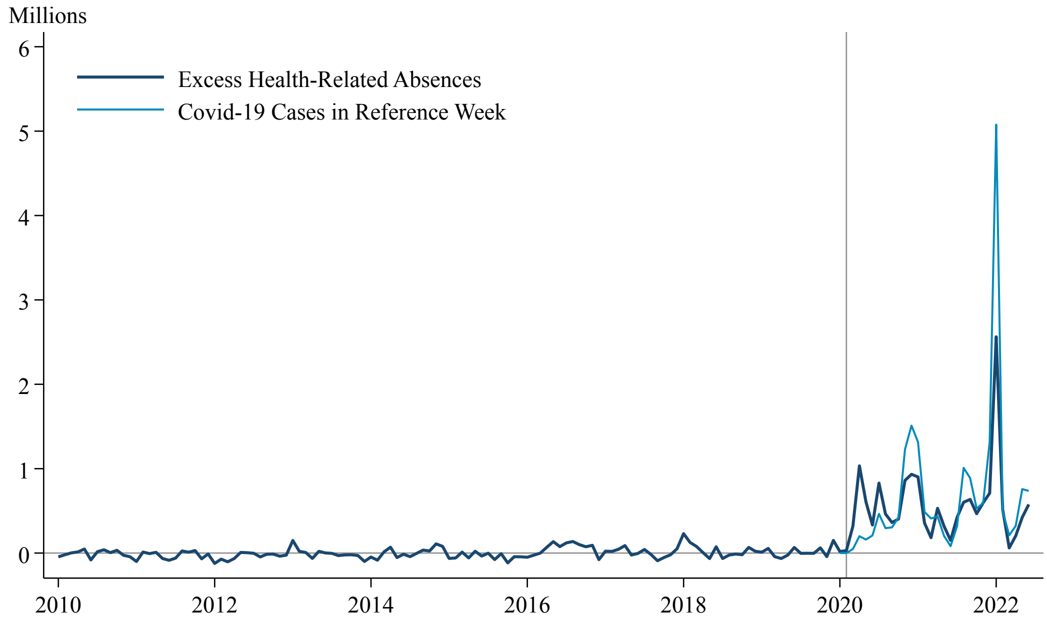
Health-related absence rates rise and fall with Covid-19 case and death rates, both in the national time series and in state- and county-level panels. Panel A of Figure 1 shows total confirmed Covid-19 cases nationally during the reference week of the CPS and excess health-related absences in each month between January 2010 and June 2022. Excess health-related absences are computed as the actual number of such absences less the average number in each month of the year before the pandemic (January 2010 – February 2020). Approximately ten workers per thousand missed a week of work for health reasons in the pandemic, on average, up from six health-related absences per thousand before the pandemic. Monthly fluctuations in such absences track fluctuations in reported Covid-19 cases, moving roughly one-for-one overall during the pandemic.

[6] In particular, we exclude workers who ever report having a physical disability as well as those who, before their absence, ever report that they did not participate in the labor force or worked fewer hours due to illness or disability. Health-related absences among such workers are especially likely to reflect health issues other than Covid-19. We present results including these workers in Appendix B.

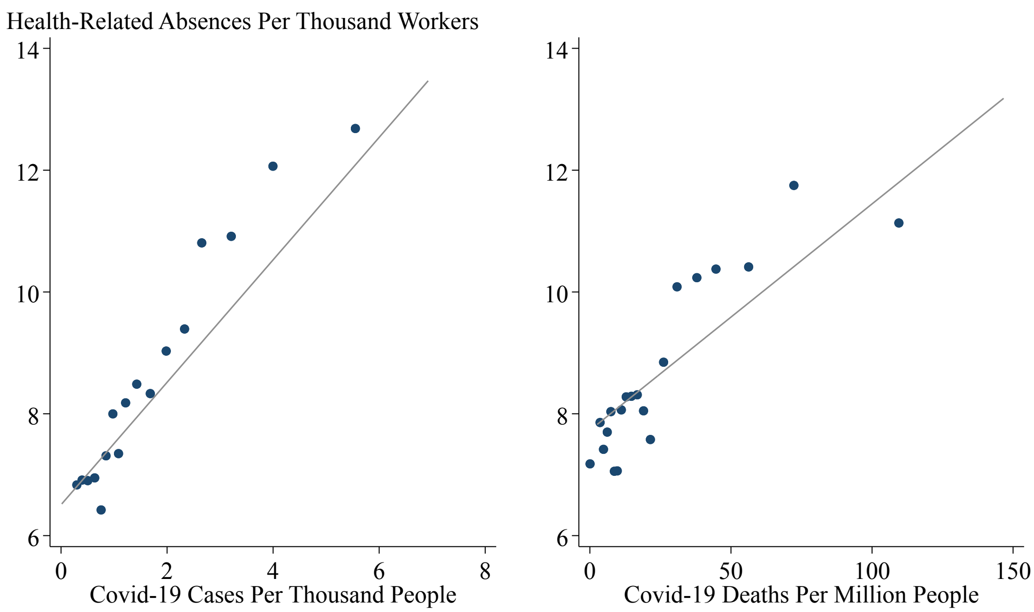
[7] See https://www.bls.gov/covid19/effects-of-covid-19-pandemic-and-response-on-the-employment-situation-news-release.htm#ques6. We thank Kyra Linse, the CPS survey director, for additional helpful conversations.

Figure 1: Health-Related Absences Versus Covid-19 Reported Cases

Panel A: National Time Series



Panel B: State–Month Panel, Removing State and Month Fixed Effects



Notes: In Panel A, this figure displays monthly counts, in millions, of excess health-related absences and Covid-19 reported cases in the CPS reference week. Excess health-related absences are computed by subtracting the seasonal (monthly) trend number of health-related absences, estimated from January 2010 through February 2020, from actual health-related absences. We do not adjust the Covid-19 case series for seasonality. The gray vertical line marks February 2020. In Panel B, this figure displays binned scatterplots of the state-level relationships between Covid-19 reported case rates (per thousand people) or Covid-19 reported death rates (per million people) and the rate of health-related absences (per thousand employed workers). We use the semiparametric approach of Cattaneo et al. (2019) to remove state and month effects.

We next examine the relationship at the state–month level between Covid-19 reported cases or deaths and health-related absences. In Panel B of Figure 1, we plot the rate of health-related absences per 1,000 workers against the rate of Covid-19 cases per 1,000 people and Covid-19 deaths per million people after removing state and month fixed effects, following Cattaneo et al. (2019), for the period from March 2020 through June 2022. When per-capita Covid-19 case or death rates are elevated in a state relative to other states in the same month, and relative to other months in the same state, health-related absence rates also tend to be elevated. The slope of the relationship between residualized cases and residualized health-related absences is about 1.2, consistent with the approximately one-for-one relationship in Figure 1. We find a slope of about 0.8 in a county-level version of this analysis (Appendix Figure A3). Taken together, the panels of Figure 1 imply that the increase in health-related absences can be attributed to Covid-19 illnesses and related absences.[8]

We then investigate whether health-related absence rates during the pandemic rose more for workers whose occupations put them at greater risk of contracting Covid-19. Appendix Figure A4 shows the relationship between health-related absences and occupation-level measures of Covid-19 exposure risk, both before and during the pandemic. The risk measures are from Mongey et al. (2021), who use O*NET data to classify occupations by their suitability to work-from-home (WFH) and by their level of physical proximity (PP) to other people entailed in typical work activities. Health-related absence rates rose more during the pandemic among workers in occupations with plausibly higher levels of exposure to Covid-19. These results bolster the claim that higher levels of health-related absences during the pandemic reflect Covid-19 illnesses and related absences.[9] Appendix Table A1 documents the robustness of this difference-in-differences result to controls for demographics, industry, and higher-level occupational categories.

Health-related absence rates before and during the pandemic vary markedly by age, sex, race, Hispanic ethnicity, and education (Appendix Table A2). Before the pandemic, health-related absences were strongly increasing in age, but the pandemic increase was sharply titled towards younger workers, making the health-related absence rate U-shaped with respect to age during the pandemic. Women experienced higher health-related absence rates than men, but there was little differential change by sex in the pandemic. Increases in health-related absences were far larger for Hispanic, Asian, and non-Hispanic Black workers than for non-Hispanic white workers. Finally, the change in the health-related absence rate was strongly related to schooling, with the increase in rate for high school graduates being twice that for bachelor's degree holders.

Increases in absence rates across different demographic groups correlate with reported Covid-

[8] By comparison, U.S. Covid-19 deaths account for most but not all excess mortality (Faust et al., 2021; Ackley et al., 2022), leaving unexplained between 12 and 32 percent of excess mortality.

[9] In related work, Song et al. (2021) show that business closure orders reduced Covid-19 cases among non-essential workers relative to essential workers, and Houštecká et al. (2021) show that similar risk measures predict pre-pandemic flu case rates among workers.

19 case rates. In Appendix Figure A5, we compare increases in absence rates with data from Centers for Disease Control and Prevention (CDC) on reported cases by age, sex, race and ethnicity. We find that groups with higher rates of reported Covid-19 cases saw significantly larger increases in health-related absence rates during the pandemic.[10]

3 Do Covid-19 Absences Reduce Labor Supply?

We next examine the effects of health-related absences on labor force participation and other outcomes using a worker-level event study. We then seek to isolate the specific effects of Covid-19 absences from those of other health shocks.

3.1 Event Study

We compare people with health-related absences to observably-similar people without such absences in a window of approximately one year around the absence. In particular, we estimate the following regression specification:

$$\mathrm{LF}_{i,t+k} = \beta_k \mathrm{HRA}_{i,t} + \mathbf{X}_{i,t}\mathbf{\Lambda}_k + \phi_{s,t,k} + u_{i,t+k}, \tag{1}$$

where $\mathrm{LF}_{i,t+k}$ indicates whether person i is in the labor force in month $t+k$. The variable $\mathrm{HRA}_{i,t}$ indicates whether i began a health-related absence spell in t, where t is a pandemic month (March 2020 onward). The terms $\phi_{s,t,k}$ are fixed effects for the interaction of the month and i's state of residence, and $\mathbf{X}_{i,t}$ contains controls. Depending on the specification, the controls may include fixed effects for demographic cells as well as indicator variables for worker i's labor market status in the month before the absence $(t-1)$ and for their detailed occupation group in the same month.[11] The specification is estimated separately for each time horizon k, which Dube et al. (2022) formalize as the "local-projection" approach to difference-in-differences.

To interpret the coefficient β_k in Equation 1 as the causal effect of a health-related absence on subsequent participation, a sufficient identifying assumption is that

$$\mathrm{E}[u_{i,t+k}|\mathrm{HRA}_{i,t} = 1, \mathbf{X}_{i,t}, \phi_{s,t,k}] - \mathrm{E}[u_{i,t+k}|\mathrm{HRA}_{i,t} = 0, \mathbf{X}_{i,t}, \phi_{s,t,k}] = 0 \tag{2}$$

[10] In the CDC data, about 35 percent of cases are of "unknown" race and ethnicity or are missing this information. We exclude non-Hispanic people who are not white, Black, or Asian from Appendix Figure A5, as we suspect their data are most compromised by these reporting issues. In addition, the comparison between absences and cases across demographic groups is difficult to interpret due to demographic differences in employment rates.

[11] By "labor market status," we mean indicators for nonparticipation, unemployment, and employment, further distinguishing by full-time or part-time employment status (both usual and actual full-time). We group workers into demographic cells according to their age (in years), sex, race/ethnicity (non-Hispanic white, non-Hispanic Black, Hispanic, Asian, American Indian, other), education (less than high school, high school graduate, some college, bachelor's degree, more than bachelor's), and the presence of a child at home.

for k months after the health-related absence. In words, Equation 2 means that, but for the health-related absence in month t, there would be no average difference in participation rates between individuals, conditional on their initial characteristics ($\mathbf{X}_{i,t}$) as well as the state and month (as captured in the fixed effects $\phi_{s,t,k}$).

The main threat to causal interpretation is that workers with absences may be less attached to the labor force than workers without absences. This could generate a spurious differential decline in post-absence participation rate among workers with absences, reflecting average differences in underlying health or other characteristics rather than the absence itself. We discuss this, other threats, and our efforts to address them in the next subsection.

We estimate Equation 1 with several sample restrictions. As workers must be employed to have an absence, we restrict the sample to those employed in the initial month t. Taking a "clean controls" approach (Cengiz et al., 2019), we further limit the sample to workers who either begin a single continuous health-related absence spell in month t or never have a health-related absence in any month we observe them. This restriction avoids contamination of the estimated effects from earlier or later absences. As health-related absences are relatively rare, the clean-controls restriction has minimal impact on our results (see Appendix B). Unless otherwise noted, our sample period is from November 2018 (fifteen months prior to the pandemic) through June 2022.[12]

3.2 Results of Event Study

Panel A of Figure 2 plots estimated coefficients from three specifications of Equation 1. In the blue line, we include only state–month fixed effects as controls. In the orange line, we augment this specification with demographic-cell fixed effects. In the black line, our baseline specification, we further add the controls for labor market status and occupation group in month $t-1$. The rotation-group structure of the CPS means we cannot estimate effects of health-related absences from four to eight months before or after the absence.

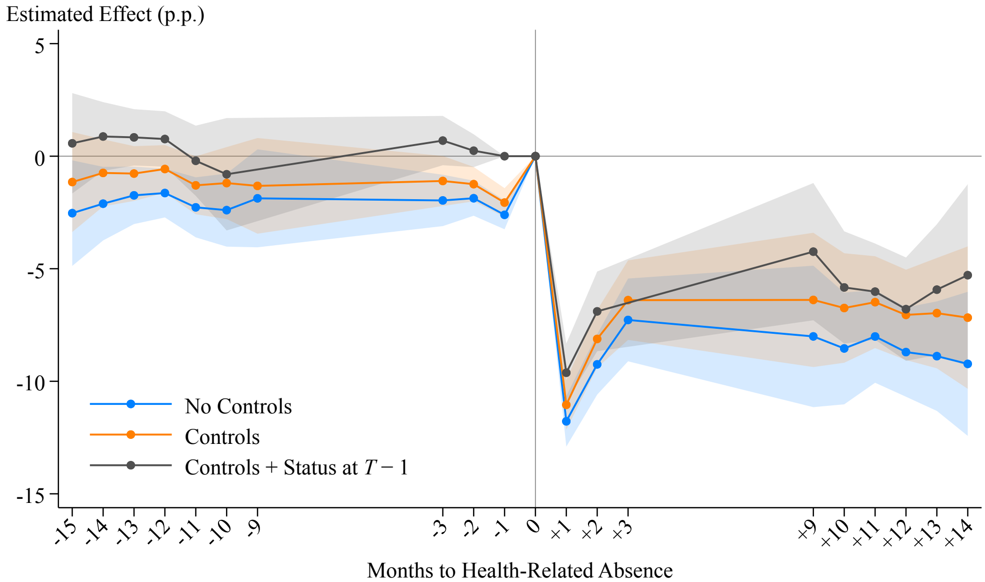
All three specifications show a sharp drop in the probability of labor force participation in the months following a health-related absence, which persists through the 14-month observation window. Our baseline estimate is that the probability of labor force participation falls about 7 percentage points after a health-related absence relative to similar workers without such absences. These effects are similar in magnitude to the effects of hospitalization on participation (García-Gómez et al., 2013; Dobkin et al., 2018), but they are approximately half as large as the estimates of the share of people with self-reported long Covid not working for health reasons by Davis et al. (2021), Evans et al. (2021), and Ziauddeen et al. (2022).[13]

[12] Appendix B also discusses survey nonresponse, particularly attrition over the multiple CPS waves.

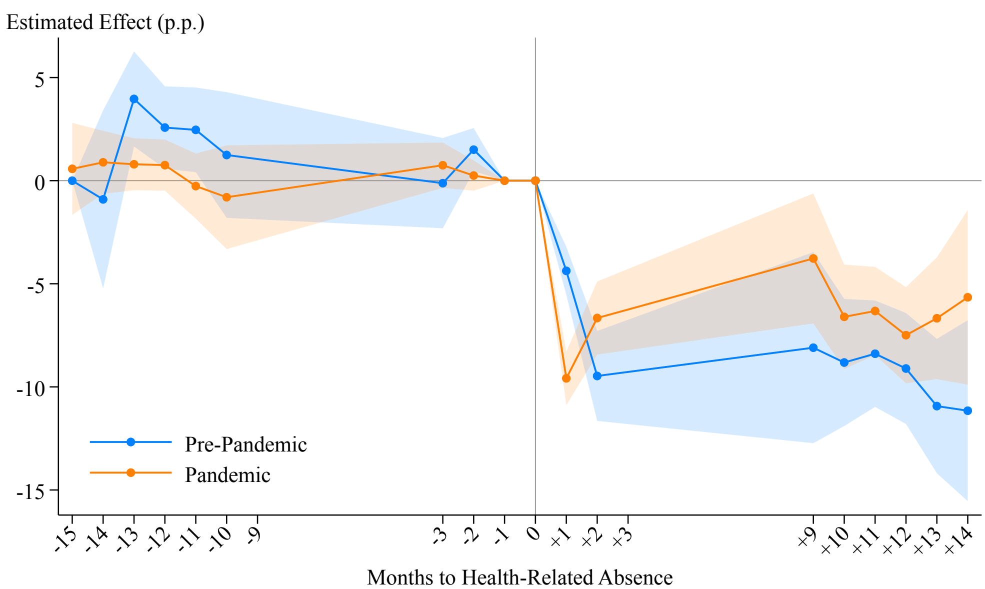
[13] The twelve-months-out nonparticipation rate of workers with health-related absences is comparable in magnitude to these survey estimates, suggesting that the absence of control groups in prior studies appears to explain discrepancies between our estimates and theirs.

Figure 2: Labor Force Participation Impacts of Health-Related Absences

Panel A: Pandemic Event Study



Panel B: Pre-Pandemic Versus Pandemic



Notes: In Panel A, this figure plots coefficient estimates β_k from Equation 1, which represents the effect of a health-related absence during the pandemic (March 2020–June 2022) on the probability of labor force participation k months before or after the absence. The blue, orange, and gray lines respectively plot estimates without demographic controls, with demographic controls, and with controls for demographics and labor market status. In Panel B, this figure plots coefficient estimates from a variation of Equation 1 that allows for heterogeneous effects of pre-pandemic and pandemic health-related absences. We include all controls in this specification, interacted with a pandemic indicator that equals one starting in March 2020. In both panels, gaps between months 3 and 9 are due to sample rotation. Color bands depict pointwise 95-percent confidence intervals. Standard errors are clustered at the worker level.

The successive sets of controls are intended to address the fact that workers with health-related absences are modestly less likely to participate in the labor force in the months before their absence. This finding is consistent with workers with a health-related absence having unobservable pre-existing health issues or other labor-market disadvantages that may reduce their subsequent labor force participation relative to the comparison group. The pre-period differences are somewhat attenuated when we compare demographically-similar workers in same occupation, and they are all but eliminated when we control for labor force participation in the period before the health-related absence. The stable pre-trends suggest that, comparing workers with similar demographics and recent labor market histories, workers without health-related absences appear to be an appropriate comparison group for workers with such absences.

Panel B of Figure 2 takes a first step towards identifying the specific effects of absences attributable to Covid-19. Here we estimate the participation effects of health-related absences before and during the pandemic (March 2020 onward). These estimates come from specifications with the full set of controls. Absence effects seem somewhat smaller in the pandemic than beforehand, though the confidence intervals overlap considerably. At face value, the panel suggests that Covid-19 absence effects might be smaller than those of the average non-Covid health-related cause of absence. However, it is also possible that the effects of non-Covid health-related absences shifted during the pandemic. We return below to estimating Covid-specific effects.

3.3 Unobservable Health as a Confounding Variable

We have pursued two further analyses to assess whether our results are plausibly explained by unobservable differences in health between workers with and without absences.

The first is to treat the participation effects of observable differences in health as an upper bound on the potential bias from unobservable differences in health in our absence event study. We construct this bound by adding into the sample workers with disabilities and those who, before their absence, ever report not being in the labor force or working fewer hours for health reasons. We then allow observable ill-health to have its own dynamic effects on participation. Our estimated effects of health-related absences in our main analysis are significantly larger than those estimated for observable ill-health, suggesting that unobservable health differences cannot explain the entire absence effect (Appendix Figure A9).

We also compare workers with absences to those who will have absences in the future. This approach therefore uses only differences in the timing of absences, plausibly improving balance on unobservable health. Estimates for the short-run effects of health-related absences are similar to our main results, although they are rather imprecise (Appendix Figure A10). Due to data limitations, we cannot use this approach to estimate long-run absence effects.

Overall, these analyses provide little support to the idea that the participation of absent workers would, without the absences, have deteriorated much relative to that of observably similar workers. Appendix B presents both analyses in greater detail.

We also consider our sample restrictions in Appendix B. Including workers in observable ill-health modestly raises the participation effects of health-related absences (a drop of 9 percentage points, rather than 7, after 12 months). These larger participation effects appear to reflect more-severe non-Covid health issues in this less-healthy population, rather than a larger effect of Covid absences. On balance, we conclude that our estimates are representative for U.S. workers, including those excluded from our main sample.

3.4 Isolating the Effects of Covid-19 Absences

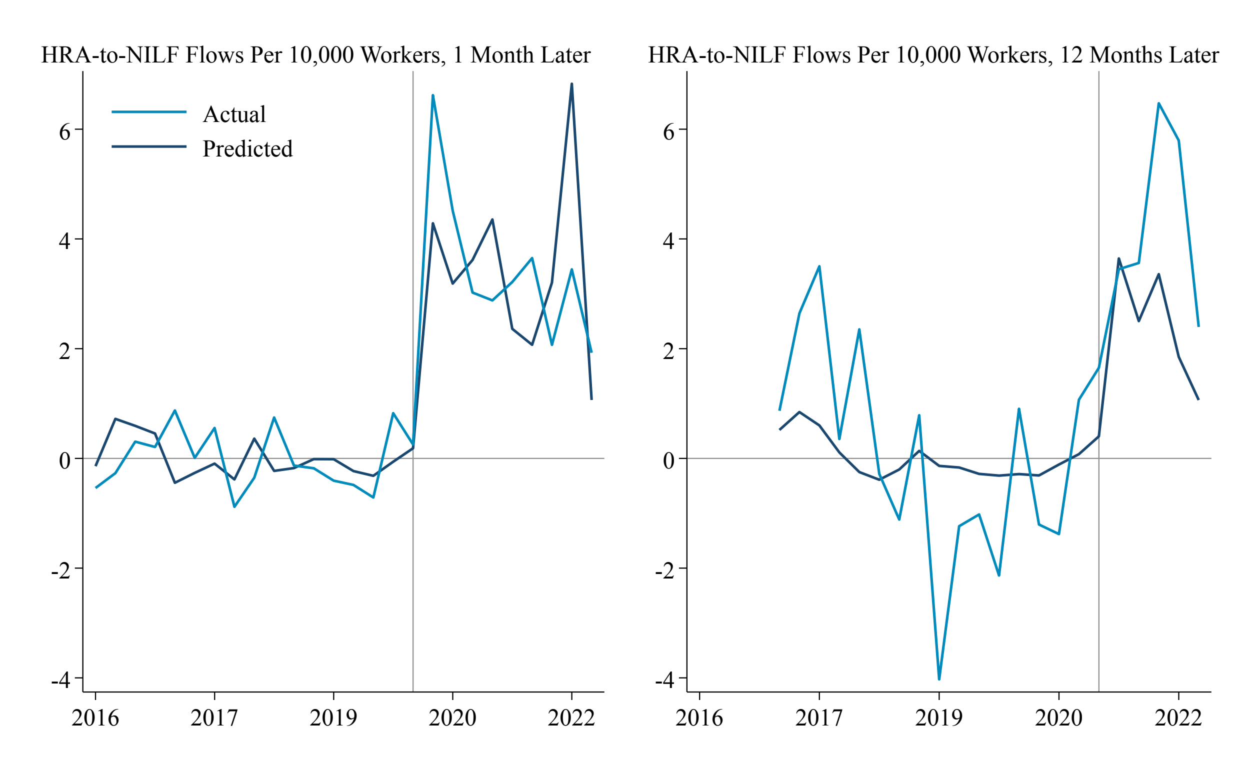
While many health-related absences during the pandemic are likely due to Covid-19 (see Section 2), many surely result from other health issues. We now seek to distinguish the specific effects of Covid-19 absences from those of other health issues. Taking three different approaches to this question, we conclude the effects of a Covid-19 absence appear to be near, if not somewhat smaller than, the average effects of health-related absences.

Aggregate Evidence. The pandemic increase in the health-related absence rate, with the effects of health-related absences on participation, implies there should be an increase in flows from health-related absences into nonparticipation. This increase, if visible in aggregates, provides a test of our event-study estimates. One feature of this analysis is its robustness to heterogeneous effects across Covid and non-Covid absences: If Covid-19 absence effects are small, the Covid-induced rise in absences will not raise total flows into nonparticipation by as much as we would predict from the average health-related absence.

Figure 3 shows actual and predicted flows from health-related absences to nonparticipation, expressed as a share of previously-employed workers, at one- and twelve-month horizons following the absence. To predict these flows, we multiply the national time series of health-related absences (Panel A of Figure 1) with the point estimates from the event study (Panel A of Figure 2) at the relevant time horizon. We find that, for every 10,000 workers, there are an additional four during the pandemic who flow from health-related absence to nonparticipation one month later. This rise is off a pre-pandemic monthly base rate of five per 10,000 workers. It thus represents a substantial increase in proportional terms. The rise in twelve-month flows from health-related absence to nonparticipation is similar. Our event-study estimates prove quite accurate in explaining the pandemic rise in flows, as the predicted series in this figure illustrates.

Time Variation in Absence Effects. Suppose a health-related absence occurring in quarter q has an effect on participation of $\beta_k^{\text{Covid},q}$ k months later if it is Covid-related and an effect of $\beta_k^{\text{NotCovid},q}$ if it is not Covid-related. We can express the average effect β_k^q as a weighted average of the Covid

Figure 3: Aggregate Flows from Health-Related Absence into Nonparticipation



Notes: The figure displays actual and predicted rates of nonparticipation following health-related absences per 10,000 people working one month ago or 12 months ago ("HRA-to-NILF" flows). Predicted rates of health-related nonparticipation are calculated using the rate of health-related absences and our event-study estimates of the effect of a health-related absence at the relevant post-absence time horizon. The estimates are adjusted for seasonality using pre-pandemic month fixed effects and then aggregated to quarterly frequency. Vertical lines identify when flows could first be affected by the pandemic.

and non-Covid absence effects:

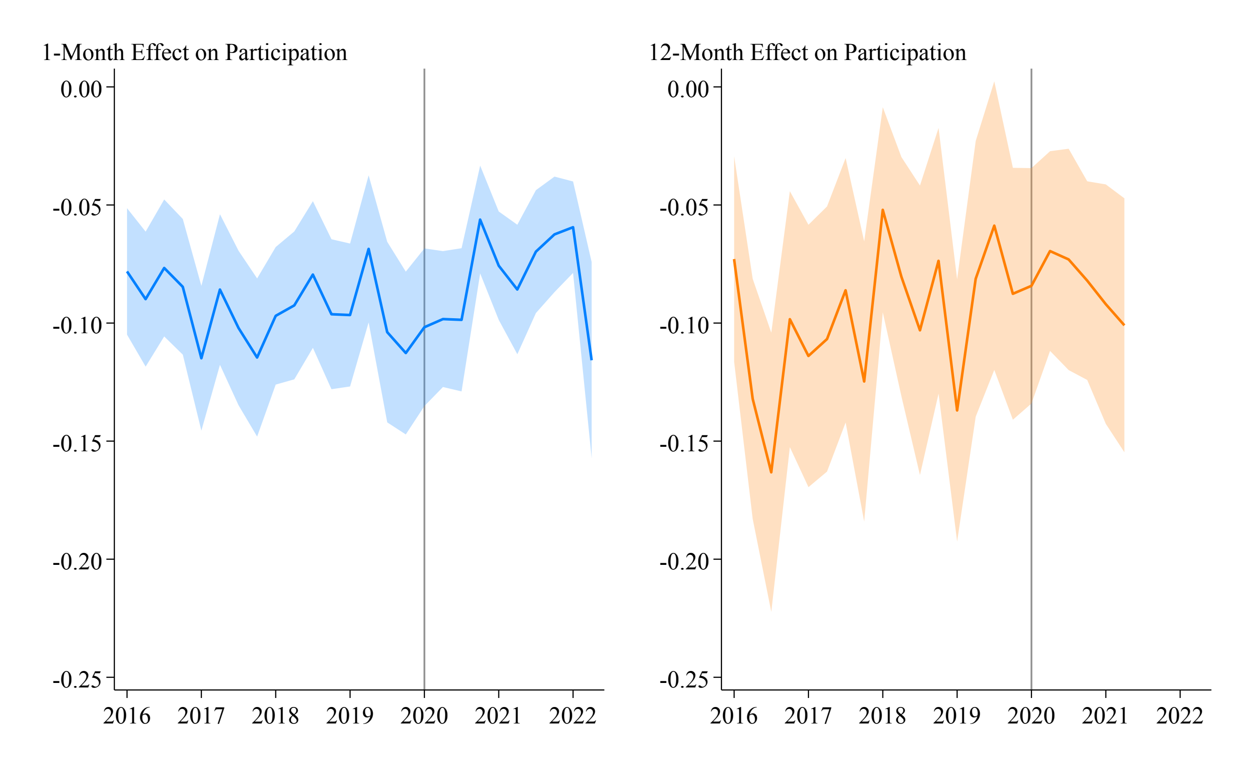
$$\beta_k^q = s_q \beta_k^{\text{Covid},q} + (1 - s_q)\beta_k^{\text{NotCovid},q}, \tag{3}$$

where s_q is the Covid-19 share of health-related absences in quarter q. If, for instance, $\beta_k^{\text{Covid},q}$ is much smaller than $\beta_k^{\text{NotCovid},q}$, so that Covid-19 has milder impacts on participation than the average non-Covid health-related absence, then the average effect β_k^q would decline in the Covid-19 share s_q of health-related absences.

We estimate a version of Equation 1 that allows for heterogeneous effects of health-related absences by quarter.[14] Figure 4 shows the results at one-month and twelve-month horizons. Consistent with Panel B of Figure 2, there is some indication that average effects of health-related absences have weakened since the start of the pandemic. This suggests that Covid-19 absences

[14]The specification also includes quarter-specific coefficients on controls.

Figure 4: Effects of Health-Related Absences by Quarter-of-Absence Cohort



Notes: This figure plots the effects of health-related absences on labor force participation one and twelve months after the absence. We estimate effects for quarterly cohorts of absences. The color bands depict pointwise 95-percent confidence intervals. Standard errors are clustered at the worker level.

may be or have become somewhat less damaging to participation in the short run than other causes of health-related absences, though non-Covid absence effects may have also declined. We see no clear change over time in the longer-run participation effect of health-related absence.

One could in principle manipulate Equation 3 to obtain Covid-specific effects: $\beta_k^{\text{Covid},q} = \beta_k^{\text{NotCovid},q} + (\beta_k^q - \beta_k^{\text{NotCovid},q})/s_q$, where one approximates the non-Covid health-related absence effect using pre-pandemic health-related absences. In practice this approach yields noisy, unstable estimates of Covid-specific absence effects. This instability arises because it extrapolates Covid-specific effects from an imprecisely estimated difference in coefficients, and this noise is magnified because s_q is less than one. It also relies on the stability over time of non-Covid absence effects. We therefore turn to a more-robust approach that uses health-related absences occurring in contexts where s_q is plausibly close to one, letting us directly interpret absence effects as Covid-specific.

Panel Variation in Absence Effects. To estimate the specific effects of Covid-19 absences, we now use state-level variation in Covid-19 case and death rates over time. In particular, we estimate a variant of our event-study specification that allows for heterogeneous effects of absences according to some interaction variable $Z_{i,t}$:

$$\text{LF}_{i,t+k} = \beta_k \text{HRA}_{i,t} + \gamma_k \left(\text{HRA}_{i,t} \times Z_{i,t}\right) + \mathbf{X}_{i,t} \mathbf{\Lambda}_k + \phi_{s,t,k} + u_{i,t+k}. \tag{4}$$

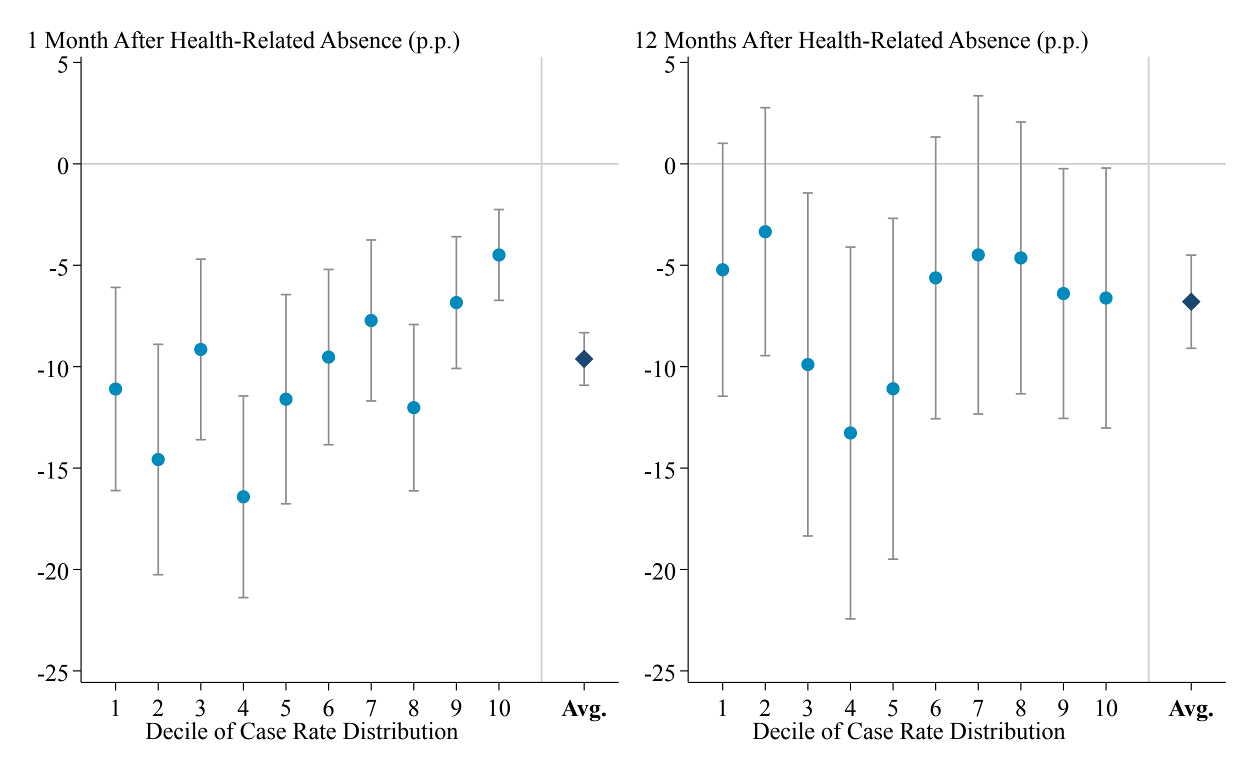
We use decile bins of the state-level Covid-19 case rate one week after the CPS reference week as our main interaction variables $Z_{i,t}$.[15] The average Covid-19 case rate ranges from 1.3 per 10,000 state residents in the bottom decile bin to 95 per 10,000 state residents in the top decile bin. This compares to a range of health-related absence rates of 68 per 10,000 workers in the bottom decile bin to 160 per 10,000 workers in the top decile. Calculating the Covid share of absences is not possible without additional assumptions, though these statistics suggest the fraction is quite high. To assess whether the top-decile health-related absence effect is likely to provide an unbiased estimate of Covid-specific effects, we also consider the slope of estimated absence effects with respect to the case rate.

Figure 5 plots estimates of the effects of health-related absences during the pandemic by the decile of the Covid-19 case rate in the state and month of absence for health-related absences. During the pandemic, health-related absences appear to have somewhat smaller effects on participation in the subsequent month when the absence is more likely to be attributable to Covid-19. In the top case-rate decile, we find a one-month reduction in the participation rate of approximately 5 percentage points, as compared to our baseline estimate of 10 percentage points. The trend towards smaller absence effects at higher case rates is less apparent in long-run absence effects, though those estimates are less precise. We find 12-months-out participation effects of around 5 percentage points, for both all pandemic health-related absences and those occurring the top decile of Covid case rates.

The appendix contains additional relevant analysis. Appendix Figure A13 plots the full event study for health-related absences in the top decile of Covid-19 cases and deaths. The effects health-related absences look broadly similar when these absences are relatively more or less likely related to Covid-19. Appendix Figure A14 also provides results using decile bins of the state-level Covid-19 death rate. Appendix Figure A15 further shows that using county-level, instead of state-level, case and death rates does not alter our conclusions. Overall, our results indicate that there is not a meaningful difference between the effects of health-related absences on labor force participation that occur before the pandemic and those related to Covid-19.

[15] These are deciles from March 2020 onward. Appendix Figure A12 shows that cases one week after the reference week have maximal predictive power for absence rates relative to other time windows, which supports their use in estimating the heterogeneous effects of Covid-19 absences.

Figure 5: Effects of Health-Related Absences by Covid-19 Case Rate



Notes: This figure plots the effects of health-related absences on labor force participation one and twelve months after the absence for health-related absences occurring in March 2020 onward. We estimate effects by decile of the Covid-19 case rate in the state and month of the absence. The color bands depict pointwise 95-percent confidence intervals. Standard errors are clustered at the worker level.

3.5 Why Do Health-Related Absences Reduce Labor Supply?

We now investigate why health-related absences reduce labor supply by examining demographic variation in the effects of such absences, the specific reasons offered for not participating in the labor force, and the impacts on other measures and margins of labor supply.

Heterogeneous Effects by Demographics. We modify Equation 1 to allow for demographic heterogeneity in the effects of health-related absences during the pandemic. Panel A of Figure 6 plots estimates of age-specific effects at two time horizons, one to two months after the absence and nine to fourteen months.[16] Among younger workers (less than 65 years old), participation effects are present but relatively small at both the short- and longer-term horizons. For such workers, health-related absences reduce participation by less than 5 percentage points in the longer run. For older workers, however, effects are somewhat larger in the short run and are much larger in the longer run. The post-absence decline in the participation rate among workers age 65 to 85 is

[16]The two periods reflect the rotation-group structure of the CPS. Here and in several following analyses, we pool our data in this way, rather than focus on effects at specific months after absences, to modestly boost power.

approximately 20 percentage points around a year later. We do not find notable heterogeneity by worker sex, race, ethnicity, and education (see Appendix Figure A16).

Nonparticipation by Reason. The CPS asks adults not in the labor force what "best describes [their] situation at this time," and allows respondents to report retirement, disability, illness, school, care or "other." Panel B of Figure 6 reports the effect of health-related absences on these reason-specific rates of nonparticipation. The reason most responsive to health-related absences in the short run is "other." Rates of illness- and disability-related nonparticipation increase only modestly, and these increases dissipate rapidly. By contrast, effects on retirement and "taking care of house or family" increase over the year after the absence. Older workers drive the response of retirement-related nonparticipation to health-related absences (see Appendix Figure A19).

Other Extensive-Margin Responses. Panel C of Figure 6 reports event-study estimates on probabilities of employment, unemployment, and two broader definitions of the labor force. Initial effects of health-related absences on employment are larger than on participation, reflecting a transient increase in unemployment. Health-related absences cause an additional 12 percent of workers to not be employed in the months immediately after their absence. The final two sets of bars expand the labor force concept, first to include those who say they want a job but are not currently looking, and then to add those who do not want a job and are not currently looking but who intend to look within a year. The results show that most of the effect of health-related absence on participation reflects disengagement from work that the people themselves intend to be lasting.

Intensive-Margin Responses. Panel D of Figure 6 considers other margins on which employed workers may adjust their labor supply following a health-related absence: actual and usual hours in their main job, industrial and occupational choice, and multiple job-holding.[17]

Workers work fewer hours per week following health-related absences. Among employed workers, actual hours worked per week in the primary job fall 8 percent on average during the two months after the absence. These hours reductions appear to abate over time.[18] Effects on workers' usual weekly hours follow the opposite pattern, growing over the longer term, though the difference is statistically insignificant. The subsequent two sets of bars show that reductions in average weekly hours reflect workers switching from full-time work to part-time or no work.

We also find evidence that workers switch into lower-wage industries and occupations fol-

[17] We focus on workers' main jobs because we lack key data about other jobs. Relatively few workers in our sample (5 percent) hold multiple jobs. The minimal effect on multiple job-holding further suggests this focus on primary jobs is immaterial to our forgone-earnings calculation.

[18] Appendix Figure A24 shows there is a pre-trend in the event study for actual hours. Our controls appear inadequate here in achieving balance on unobservable worker health. Controlling for additional lags of participation is mostly unable to resolve the pre-trend. Due to this bias, the effects of health-related absences on actual hours are likely smaller than Figure 6 suggests. To reduce the potential impact of this imbalance on the rest of our analysis, our controls for pre-absence labor market status include actual and usual full- and part-time status (see Footnote 11).

lowing a health-related absence on average. We predict workers' hourly earnings from workers' industries and occupations, and we examine the effects of health-related absences on these predicted hourly earnings.[19] Adjustment on this margin occurs slowly. We see little change in rates of multiple job-holding at either time horizon. We also do not find significant effects on gross transitions between industries or between occupations, employer-to-employer transitions, transitions in work activities, or on net in employment toward occupations by work-from-home suitability or physical proximity to other people (see Appendix Figure A26).

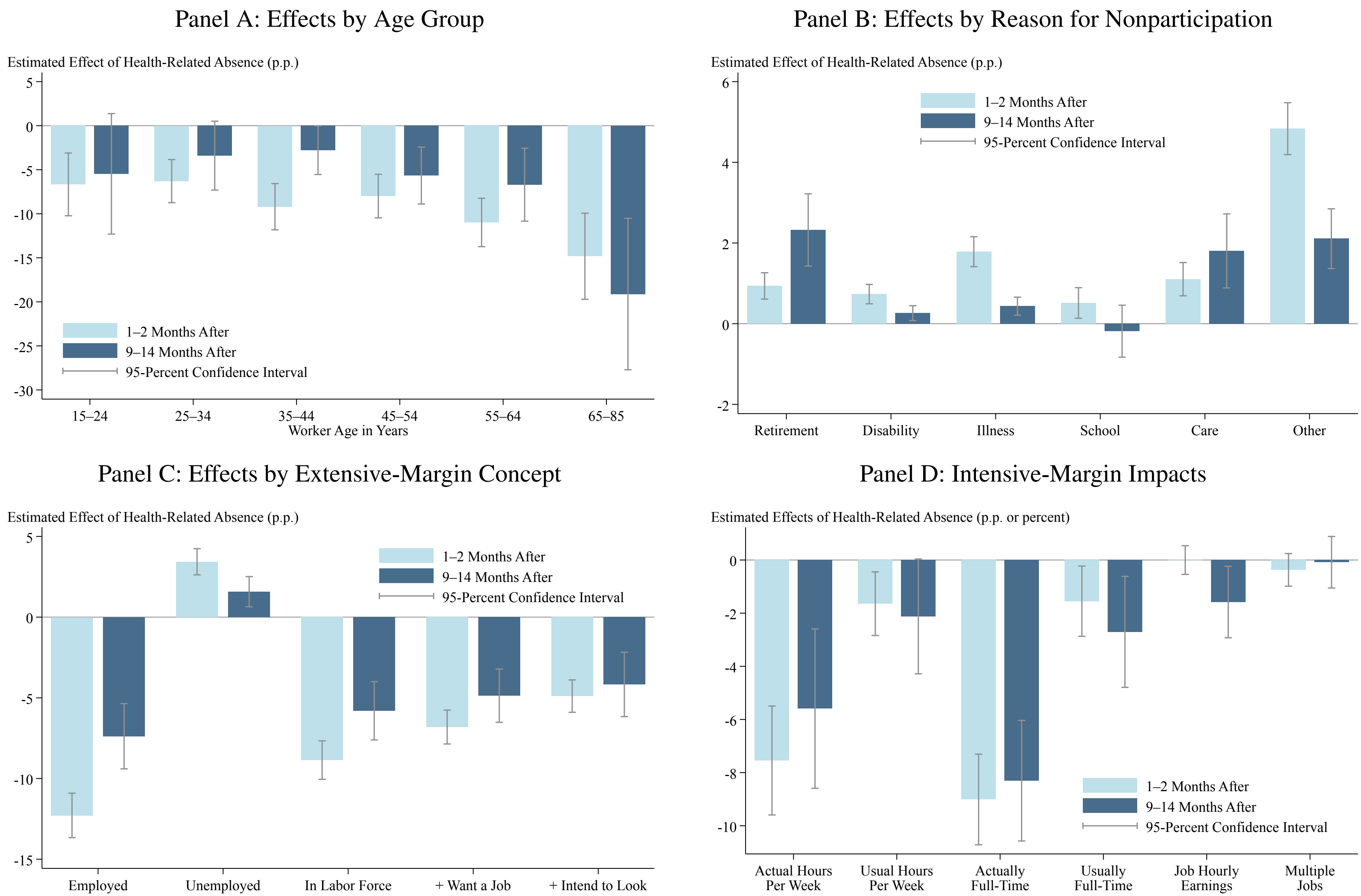
Additional Results. We have also examined the mediating role of labor market conditions. The average health-related absence in our pandemic sample occurred at a state unemployment rate of 6.3 percent, but the rapid pandemic recovery means there is substantial variation in labor-market conditions in our data (interquartile range of 3.4 p.p.). These results can shed light on how our results might translate to other contexts.

We first confirm the short-run unemployment increases we find in Panel A of Figure 6 are driven by state–months with relatively high unemployment rates. For every one-percentage-point higher state unemployment rate during a worker's absence, their probability of being unemployed in the subsequent month rises by one percentage point (see Appendix Table A3). This sensitivity of absence effects to the state unemployment rate diminishes quickly following the absence. At the same time, the participation effects are essentially insensitive to the state unemployment rate during the absence.

Taken together, these findings suggest that health-related absences are more likely to cause involuntary job loss in a slacker local labor market, but only briefly. In addition, participation losses appear to be true declines in labor supply, rather than "disguised" involuntary job loss. Relative to the U.S., the European response to the pandemic may have reduced the short-run employment impact of health-related absences, by preventing involuntary separations, but these interventions may have little enduring effect on employment or participation.

[19] Specifically, we use a Poisson regression: $\mathrm{E}[w|n,o] = \exp(\alpha_n + \alpha_o)$, where w is hourly earnings, α_n is an industry fixed effect and α_o is an occupation fixed effect. We restrict the sample to 2016–2019 and calculate hourly-equivalent earnings for non-hourly workers. We take this approach because the CPS measures earnings twice, at the ends of each four-month period in which workers are in the sample. We thus cannot study long-run earnings impacts directly, as we do not observe earnings both pre-absence and more than three months after an absence.

Figure 6: Effects of Health-Related Absences on Workers



Notes: This figure displays effects of health-related absences at one and twelve months after the absence. All panels estimate the event-study specification (Equation 1) with our full set of controls. Confidence intervals reflect standard errors clustered by worker. See Appendix A for event-study figures and tests for heterogeneous effects of likely Covid-19 absences.

4 The Labor Supply Lost to Covid-19 Absences

This section considers two measures of the economic cost of the pandemic's harm to health: (1) the aggregate impact of Covid-19 absences on U.S. labor force participation and (2) the labor supply cost in terms of forgone labor earnings, both on average per Covid-19 absence and in aggregate. These calculations combine the volume of excess health-related absences with our estimates of health-related absence effects.

4.1 Covid-19 Absences and the Labor Force Participation Rate

We convert our event-study estimates into aggregate impacts using the time series of excess health-related absences from Figure 1. We calculate the cumulative participation impact of Covid-19 absences from January 2020 ($t = 0$) to month t as

$$\sum_k \widehat{\beta}_k \left(\text{AbsenceRate}_{t-k} - \text{AbsenceRate}_{\text{pre},t}\right),$$

where $\widehat{\beta}_k$ is the event-study coefficient at k months after the health-related absence, AbsenceRate_t is the probability of a health-related absence in month t, and $\text{AbsenceRate}_{\text{pre},t}$ is the seasonally adjusted probability of a health-related absence before the pandemic.

An issue to address is what happens to workers after we can no longer observe them. Do the participation effects dissipate at some distant time horizon? We present a baseline estimate as well as a range defined by two extreme cases. Our baseline assumption is that the effects decay linearly beyond fourteen months after the absence at the average pace of decay from months 1 to 14. Our two extreme cases for these effects are: (1) they persist indefinitely at $\widehat{\beta}_{14}$, given the maximum panel length of fourteen months, or (2) they vanish immediately and entirely at fifteen months after the absence.

We find that Covid-19 absences have likely become, over the last two years, an important contributor to the net change in the participation rate. By June 2022, Covid-19 absences reduced the participation rate by 0.18 percentage points, with a range of 0.13 to 0.22 percentage points from our extreme cases, as we show in Appendix Figure A27. These reductions in the participation rate imply that approximately 500,000 adults are neither working nor actively looking for work due to the persistent effects of Covid-19 absences, with a range of 340,000 to 590,000 adults. This point-in-time participation-rate loss is near the steady-state loss associated with the 2021-average rate of health-related absences. That is, if the health-related absence rate remains near its 2021 level, and if the impacts of Covid-19 absences are unchanged, then our estimates suggest the participation rate will be persistently reduced by approximately 0.2 percentage points.

Two further considerations push towards larger estimates of Covid-19's effect on the participation rate. First, our calculation excludes any impact on people who, when ill, are unemployed or out of the labor force, as these people are inherently not absent from a job. Some would have become employed in subsequent months if not for their Covid-19 illnesses. To account for Covid-19 illnesses among this population, we multiply our event-study estimates by (1) the excess health-related absence rate and (2) the sample probability that someone currently not employed is employed twelve months later.[20] By this reasoning, an additional 50,000 people would be in the labor force but for prior Covid-19 illness.

The second consideration is Covid-19 absences which occur outside the CPS reference week. If all week-long absences last for only one week before a change in status occurs (e.g., return to work or exit from employment), then the CPS counts only one in four health-related absences, and our estimates should be multiplied by four. To address this concern, we estimate the average duration of health-related absences using month-to-month persistence: A health-related absence in month t raises the probability of health-related absence in month $t+1$ by about 23 percentage points. Assuming a constant weekly hazard rate of escape, this persistence implies an average duration of about 3.3 weeks, which suggests our results are understated by about 22 percent, or an additional 110,000 people out of the labor force.[21]

4.2 Earnings-Equivalent Losses from Covid-19 Absences

Table 1 shows the average labor supply loss of a Covid-19 absence in terms of earnings, combining the event study results on both extensive and intensive margins of labor supply. Columns 1 and 2 restate event-study estimates as presented in Figures 2 and 6. We then monetize these responses using workers' average weekly earnings before health-related absences, \$903. Column 3 displays the earnings equivalent during the absence.[22] Column 4 then uses the estimates in Column 1 to compute the total earnings-equivalent loss from one to three months after the absence for each margin of labor supply. Column 5 uses the estimates in Column 2 to compute the total earnings-equivalent loss from four to fourteen months after the absence. Column 6 adds Columns 3–5 to compute the total earnings-equivalent loss for each margin of labor supply.

We find an average loss per Covid-19 absence equivalent to about \$9,000 in earnings, which amounts to about 18 percent of these workers' counterfactual earnings over the fourteen-month observation period. Over half of the earnings loss reflects a reduction in employment, with the

[20] We make several further assumptions: that Covid-19 week-long absence rate is the same in the employed and non-employed populations, that the average effect on participation is the same, and that non-employed people who get Covid-19 have the same transition probabilities as the non-employed on average. The calculation is $0.07 \times (106{,}000{,}000) \times (.13) \times (.01 - .0063) \times 14$. A note of thanks to Lea Rendell, who found an error in our original calculation.

[21] These calculations are $1/(1-0.234^{1/4}) = 3.28$ and $4/3.28 - 1 = 0.22$.

[22] These are labor supply losses measured in earnings units but are not necessarily income losses for the workers, insofar as some are covered by sick leave.

Table 1: Average Earnings-Equivalent Losses of Covid-19 Absences

	(1)	(2)	(3)	(4)	(5)	(6)
	Estimated Effect		Earnings Equivalent (at $903/week)			
Margin	1–3 Months After	9–14 Months After	Absence	1–3 Months After	4–14 Months After	Total
Employment	-12.3 p.p.	-7.4 p.p.	903	1,331	2,932	5,165
	(0.7)	(1.0)	(28)	(86)	(420)	(534)
Hours	-7.5%	-5.6%	0	817	2,221	3,038
	(1.0)	(1.5)	(0)	(116)	(612)	(728)
Job Earnings	-0.0%	-1.6%	0	0	628	628
	(0.3)	(0.7)	(0)	(30)	(273)	(303)
Total			903	2,148	5,781	8,832
			(28)	(232)	(1,305)	(1,593)

Notes: This table estimates the average labor supply loss of a Covid-19 work absence in terms of foregone earnings. Columns 1–2 report event-study estimates from Equation 1. Columns 3–6 monetize these impacts using the average weekly earnings of workers before their health-related absences. Standard errors, reported in parentheses, are clustered by worker. We use the average effects of health-related absences in months 9 to 14 to project impacts for months 4 to 8. Standard errors in Columns 3–6 account for both sample variability in average earnings and in effects of health-related absences by assuming independent errors. Appendix Table A4 shows that absence effects are similar for higher- and lower-wage workers, which allows us to approximate average earnings losses without accounting for the covariance of absence effects and pre-absence earnings levels.

remainder from declines in hours worked and in predicted hourly earnings according to workers' industrial and occupational choices. Around 90 percent of the average cost of a Covid-19 absence reflects the indirect costs from reduced labor supply over the fourteen months after the absence. That is, only about 10 percent of the earnings loss induced by week-long Covid-19 absences occur in the absence week itself, pointing to the importance of long-term labor-supply responses.

5 Conclusion

This paper studies the impacts of Covid-19 absences on labor supply in the United States. Using an event study, we find that workers who miss a full week of work due to probable Covid-19 absences become about 7 percentage points less likely to be in the labor force one year later compared to similar workers who do not miss work for health reasons. This labor-supply impact suggests that Covid-19 absences have reduced the U.S. labor force participation rate by approximately 0.2 percentage points, or 500,000 people.

We further find significant adjustments on other margins of labor supply, including hours and choice of industry and occupation. In total over these margins, we estimate that Covid-19 absences reduce total labor supply by about $9,000 in equivalent earnings, or by 18 percent, over the fourteen months following a health-related absence. About 90 percent of the forgone labor earnings reflects long-term reductions in labor supply beyond the absence itself. These results show Covid-19 has reduced aggregate labor supply through the channels of health and health risk.

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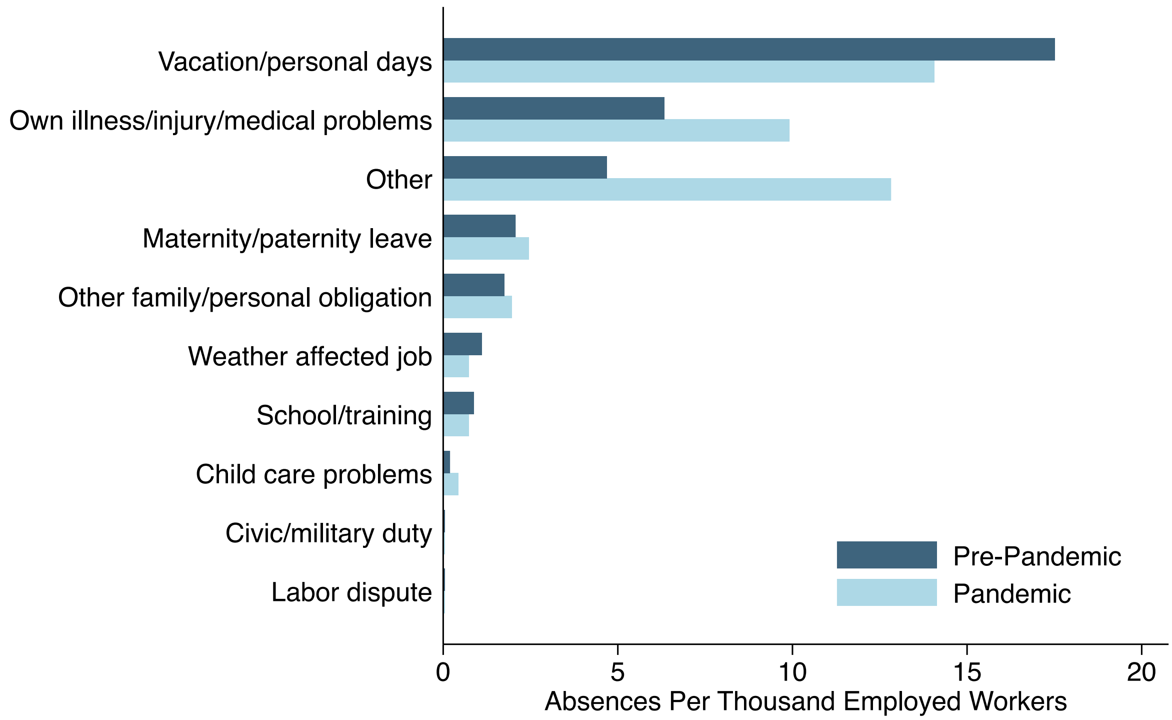
Appendices for Online Publication

A Additional Tables and Figures

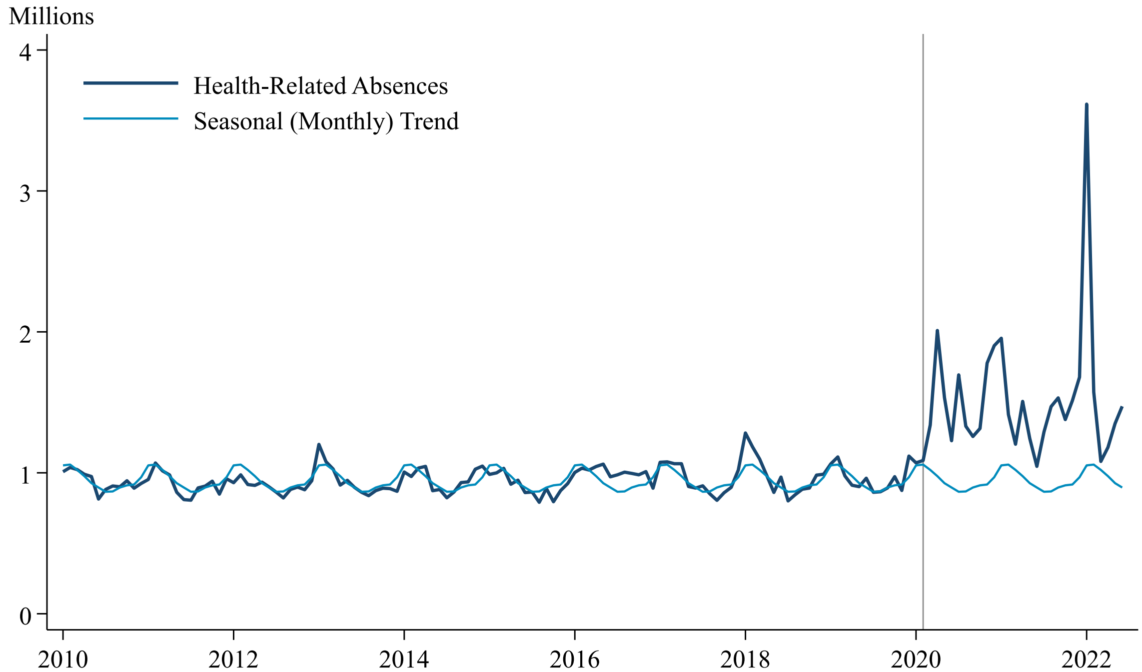
Figure A1: Absence Rates by Reason, Pre-Pandemic and Pandemic Averages



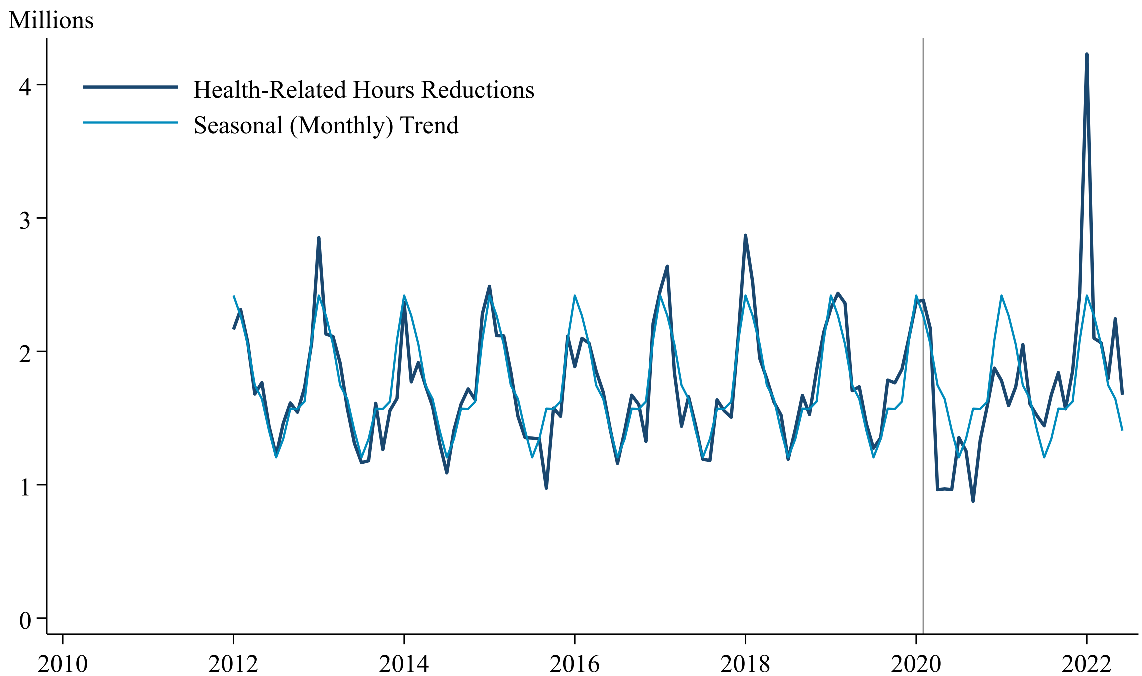
Notes: This figure displays the number of absences per thousand employed workers before and during the pandemic among workers in our sample.

Figure A2: Health-Related Absences and Hours Reductions versus Seasonal Trends

Panel A: Absences

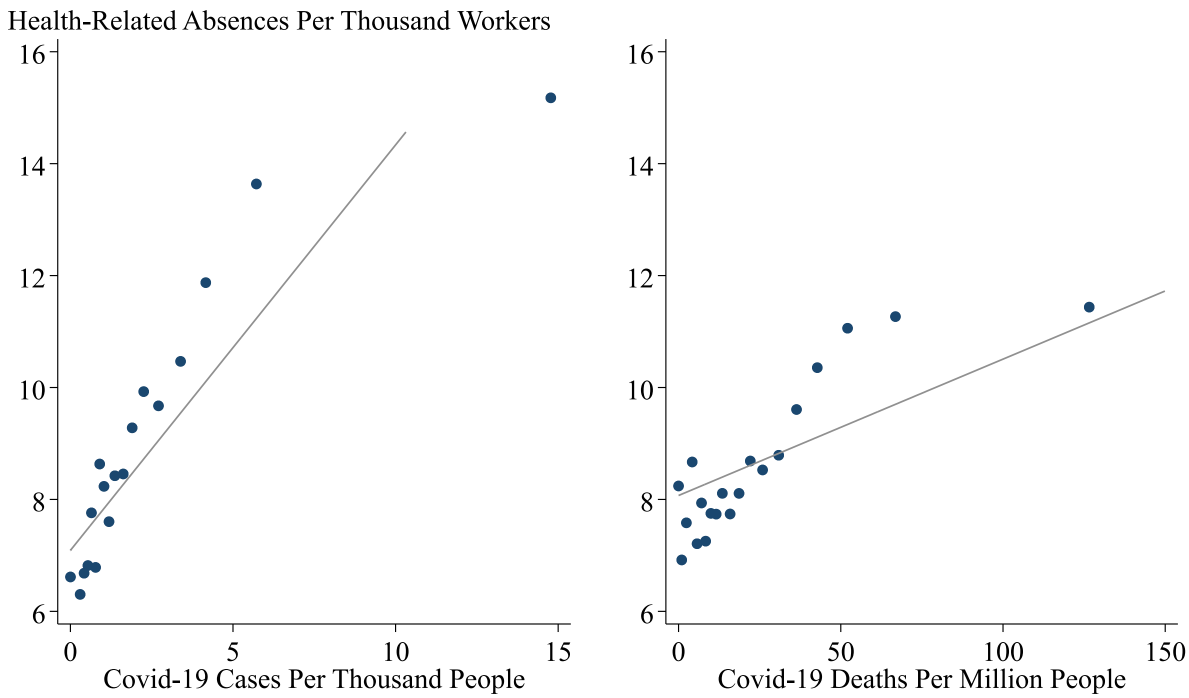


Panel B: Hours Reductions



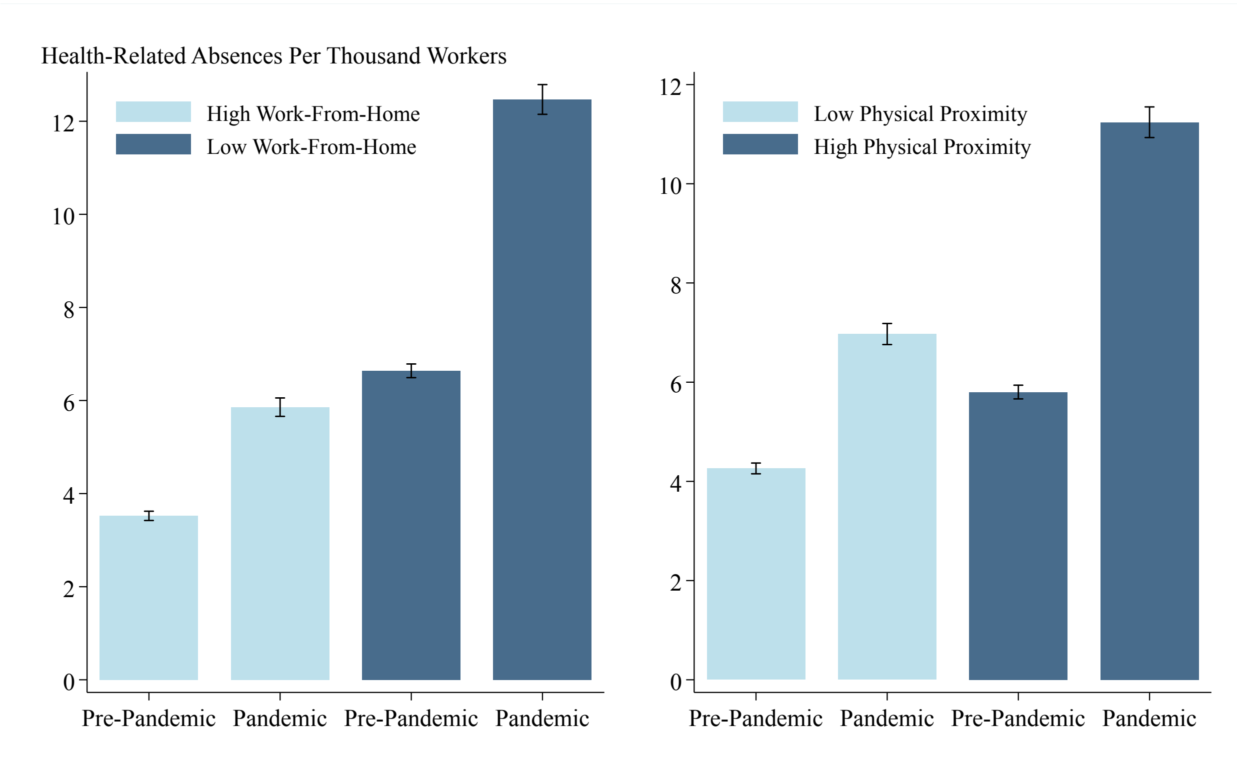
Notes: This figure displays, in Panel A, the number of workers who missed the entire week of work for a health-related reason during the CPS reference week. In Panel B, it displays the number of workers who usually work full-time but worked part-time for a health-related reason during the CPS reference week.

Figure A3: County Health-Related Absences Covary with Covid-19 Cases and Deaths



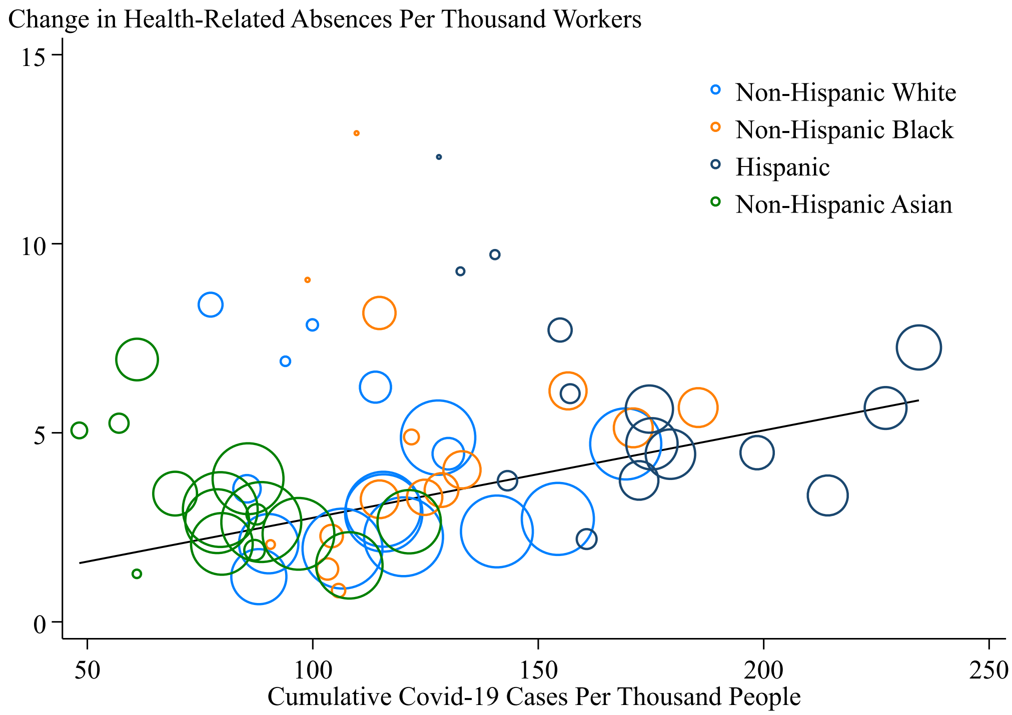
Notes: This figure displays binned scatterplots of the county-level relationships between Covid-19 case and death rates (per million people) versus the rate of health-related absences (per million workers). We use the semiparametric approach of Cattaneo et al. (2019) to remove county and month effects and to add back national means. The sample period is March 2020 through June 2022. Covid-19 case rates are as of the CPS reference week, and Covid-19 death rates are as of two weeks after the reference week. CPS data allow us to match workers to their counties of residence for approximately 47 percent of observations. For the remaining share of workers, we measure state–month case and death rates excluding cases and deaths in the identified subset of counties.

Figure A4: Health-Related Absence Rates Rose More in Pandemic for More-Exposed Workers



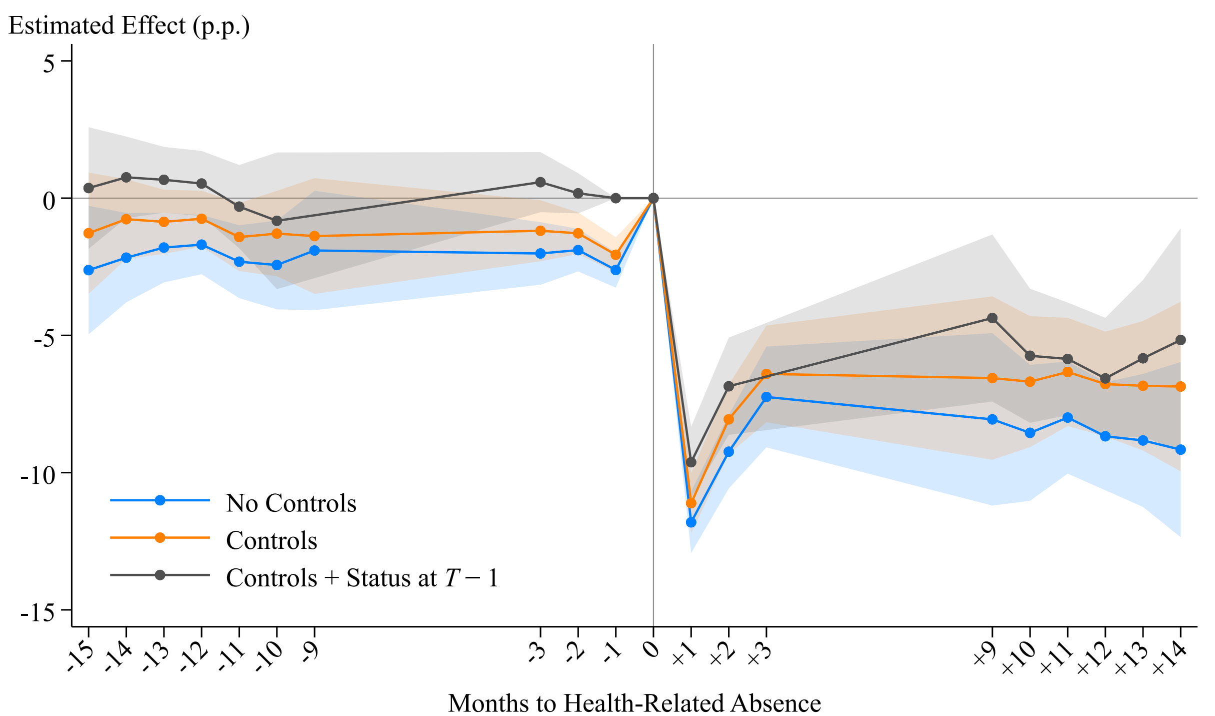
Notes: This figure graphs health-related absence rates (per thousand employed workers), comparing the pre-pandemic (Jan. 2015 – Feb. 2020) and pandemic (March 2020 – June 2022) periods, and splitting workers by their Covid-19 exposure risk. We use two occupation-level measures of exposure risk, both from Mongey et al. (2021): the extent to which work-from-home is possible in the occupation, and the extent to which work in the occupation involves physical proximity to other people. Error bars indicate 95-percent confidence intervals.

Figure A5: Health-Related Absence Rate Versus Case Rate Across Demographic Groups



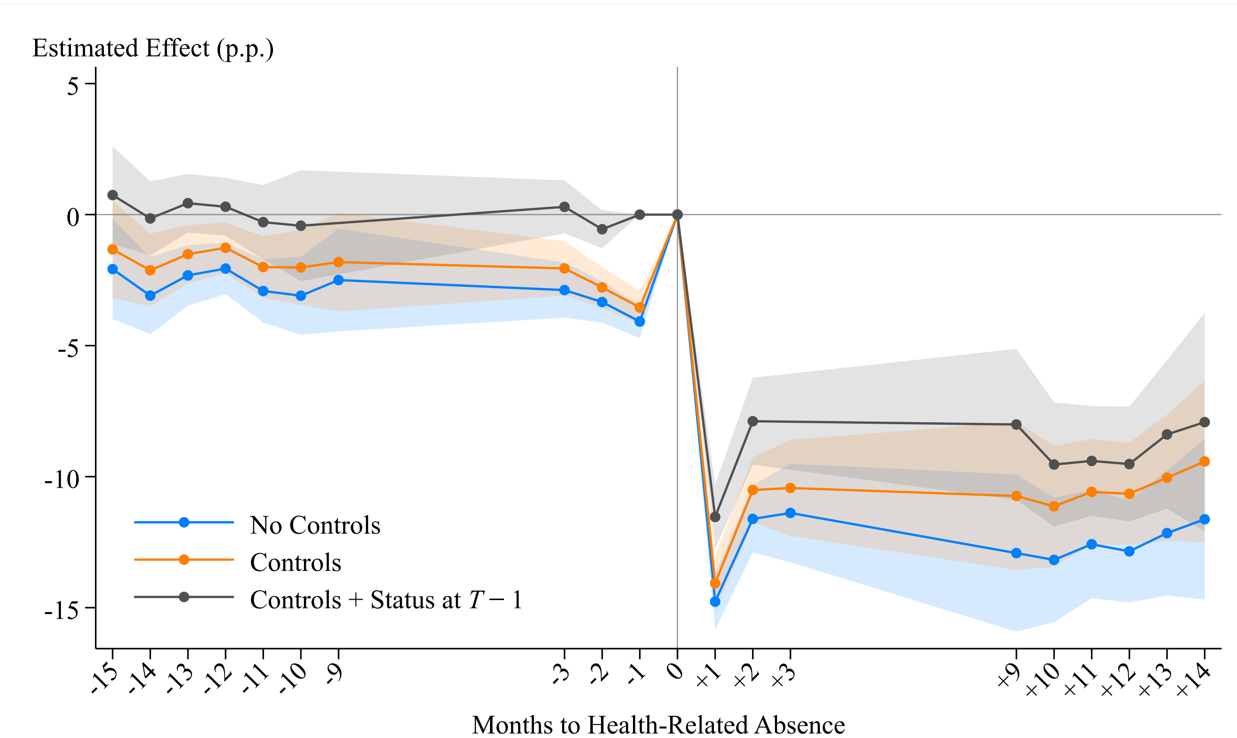
Notes: This figure displays the relationship between the pandemic change in the rate of health-related absences and the Covid-19 case rate across demographic groups. The change in the health-related absence rate is measured as the difference in the average number of absences per thousand employed workers between the pre-pandemic period (January 2016 – February 2020) to the pandemic period (March 2020 – June 2022). The cumulative case rate is measured as the number of Covid-19 cases per thousand people since the beginning of the pandemic. Each point reflects a demographic group, defined as a sex, racial/ethnic group (non-Hispanic white, non-Hispanic Black, Hispanic, and non-Hispanic Asian), and ten-year age bin (10–19, . . . 70–79, 80 and over). Other non-Hispanic people are excluded from the analysis due to data quality issues discussed in the main text. Each point is scaled to the number of employed workers in the demographic group. To improve visualization, we exclude three small demographic cells with outlier declines in health-related absences.

Figure A6: Event-Study Estimates, Without Clean-Controls Restriction



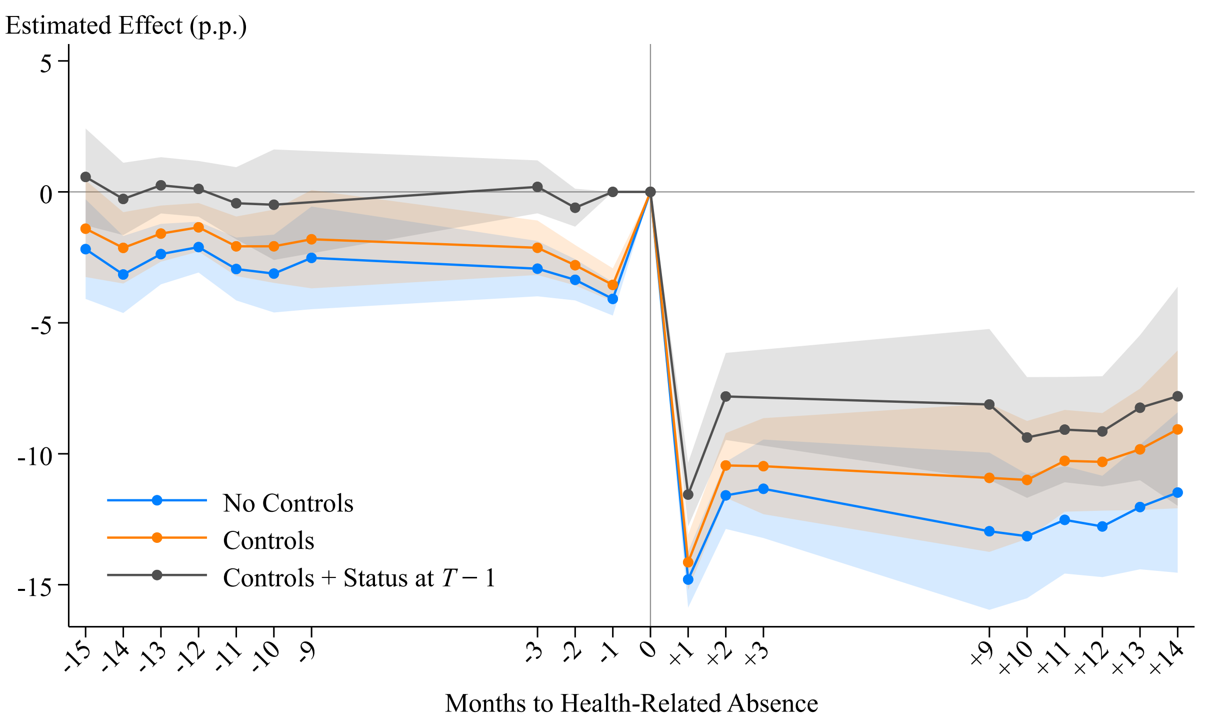
Notes: This figure plots estimates of coefficients β_k from Equation 1. The sample is of civilian adults who never report having a physical disability or other health issues before their absence, but we remove the "clean-controls" sample restriction. The coefficients represent the effect of a health-related absence on these outcomes k months before or after the absence. All specifications include our full set of controls. Gaps between months -10 and -3, as well as $+2$ and $+9$, are due to sample rotation. The color bands depict pointwise 95-percent confidence intervals. Standard errors are clustered at the worker level.

Figure A7: Event-Study Estimates, Full Sample, With Clean-Controls Restriction



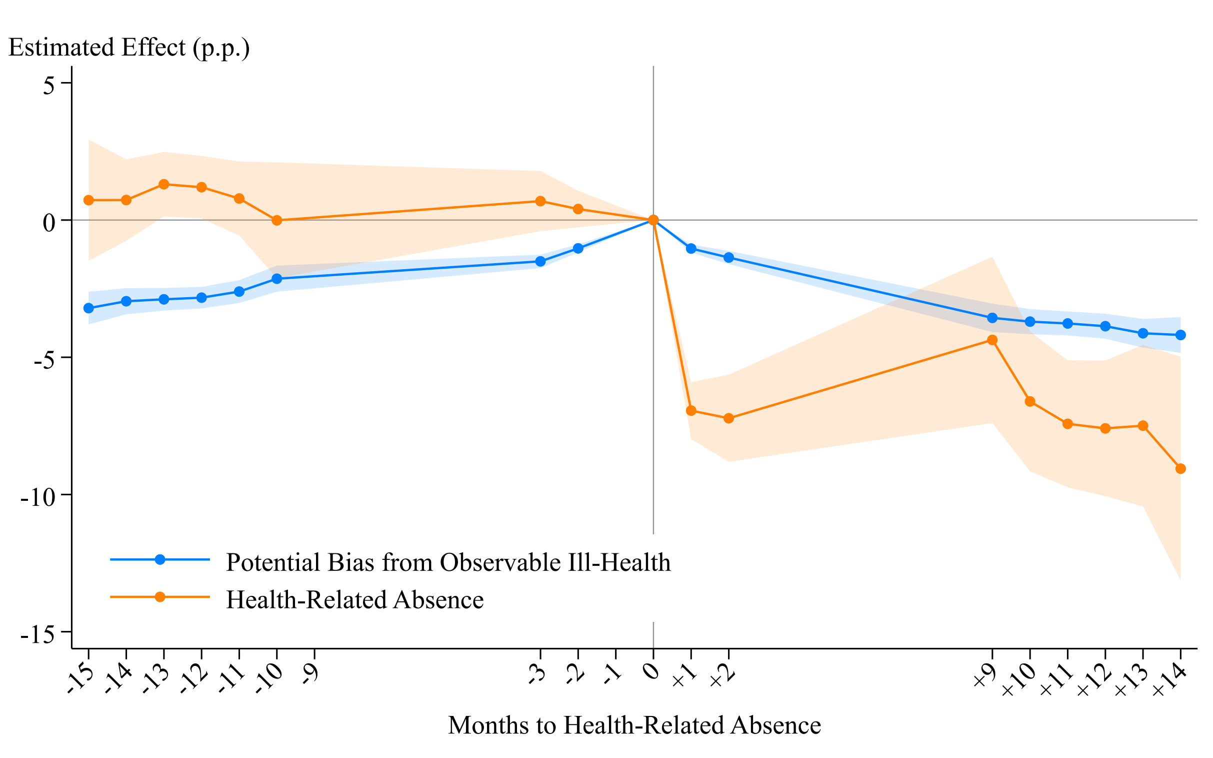
Notes: This figure plots estimates of coefficients β_k from Equation 1. The sample is of civilian adults, including both workers with pre-existing health issues, and we impose the "clean-controls" sample restriction. The coefficients represent the effect of a health-related absence on these outcomes k months before or after the absence. All specifications include our full set of controls. Gaps between months -10 and -3, as well as $+2$ and $+9$, are due to sample rotation. The color bands depict pointwise 95-percent confidence intervals. Standard errors are clustered at the worker level.

Figure A8: Event-Study Estimates, Full Sample, Without Clean-Controls Restriction



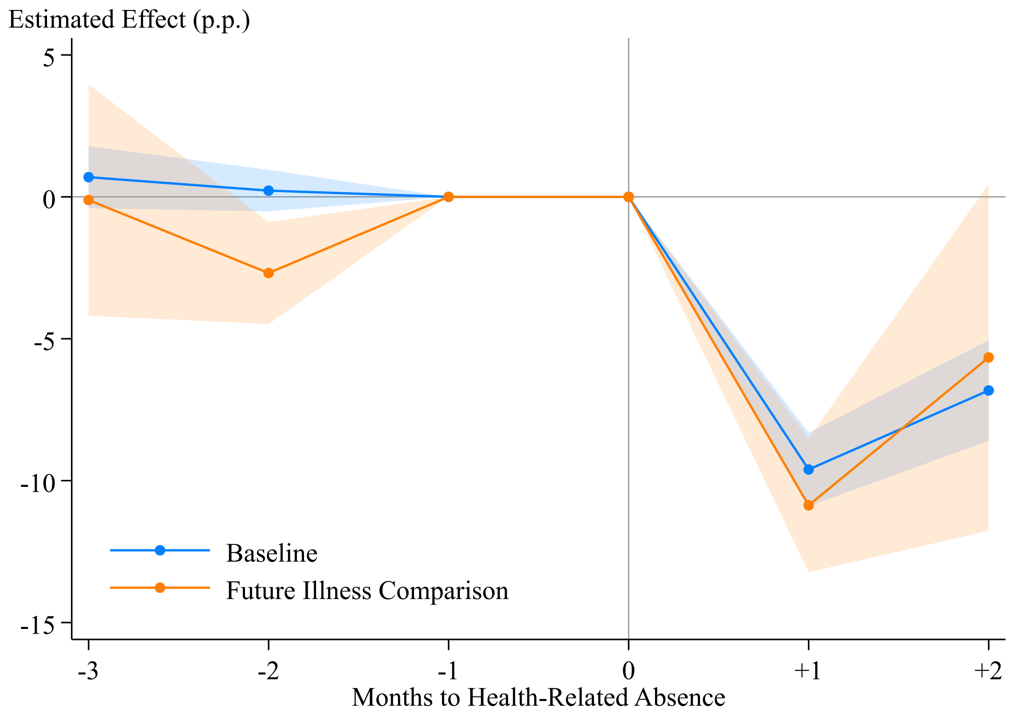
Notes: This figure plots estimates of coefficients β_k from Equation 1. The sample is of civilian adults, including both workers with pre-existing health issues, and we do not impose the "clean-controls" sample restriction. The coefficients represent the effect of a health-related absence on these outcomes k months before or after the absence. All specifications include our full set of controls. Gaps between months -10 and -3, as well as $+2$ and $+9$, are due to sample rotation. The color bands depict pointwise 95-percent confidence intervals. Standard errors are clustered at the worker level.

Figure A9: Effects of Health-Related Absences Exceed Potential Bias from Ill-Health



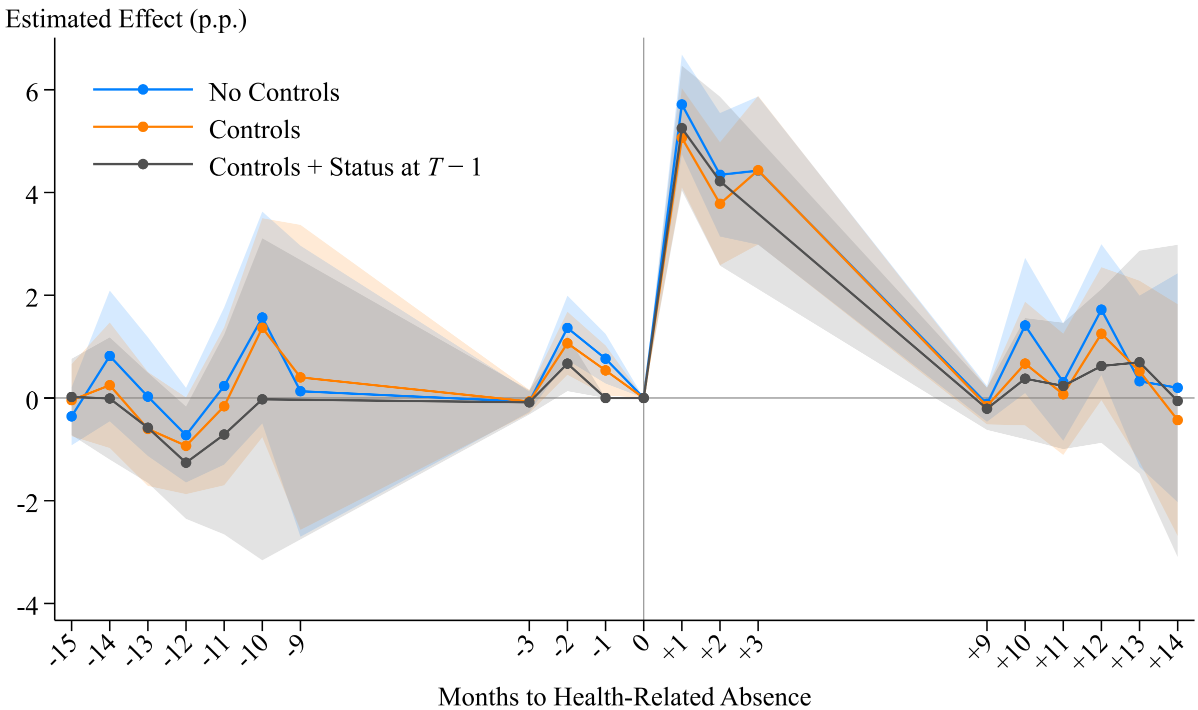
Notes: This figure plots, in blue, the estimated effect of being observably unhealthy on labor force participation k months later. For comparison, we plot in orange the baseline event study from Figure 2. The sample for the blue line is of all civilian adults. The color bands depict pointwise 95-percent confidence intervals. Standard errors are clustered at the worker level.

Figure A10: Event-Study Estimates, Workers with Later Health-Related Absences As the Comparison Group



Notes: This figure plots estimates of coefficients β_k from Equation 1. Estimates in blue are for our baseline specification. Estimates in orange are from a restricted sample of civilian adults who ever have a health-related absence while in the Current Population Survey. Both sets of coefficients represent the effect of a health-related absence on these outcomes k months before or after the absence. Both specifications include our full set of controls. The color bands depict pointwise 95-percent confidence intervals. Standard errors are clustered at the worker level.

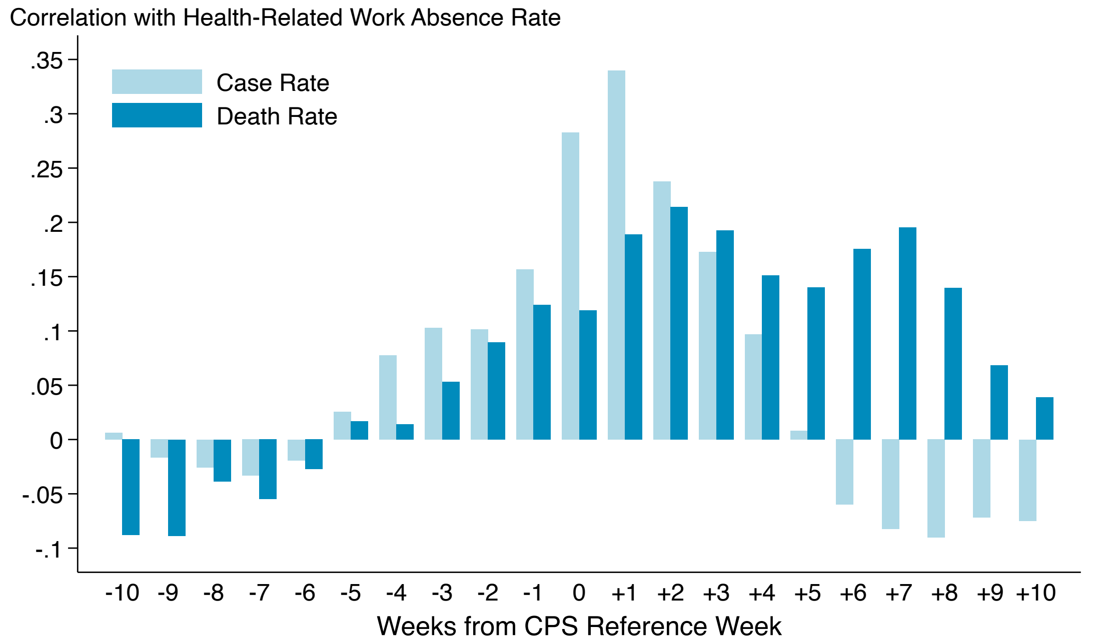
Figure A11: Differential Attrition in the Health-Related Absence Event Study



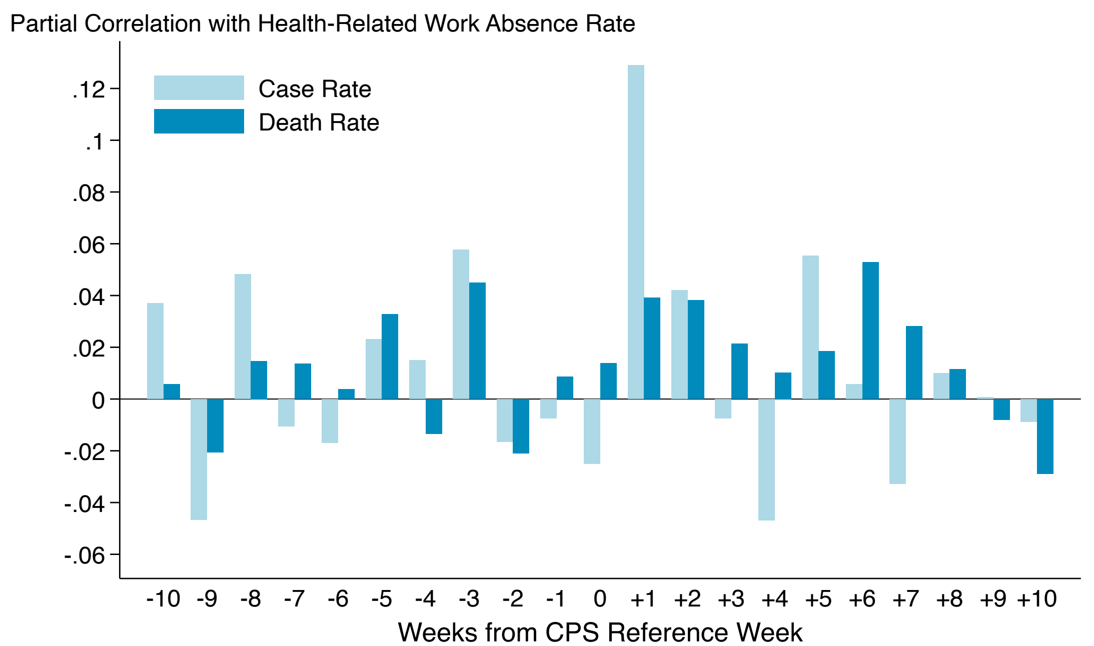
Notes: This figure plots estimates of coefficients β_k from a version of Equation 1 where the outcome is an indicator for survey nonresponse. The coefficients represent the effect of a health-related absence on the probability of nonresponse k months before or after the absence. Positive coefficients indicate differential attrition towards workers with health-related absences. The blue, orange, and gray lines respectively plot estimates without demographic controls, with demographic controls, and with demographic controls and labor force status one period before. Gaps between months 3 and 9 are due to sample rotation. The sample is of civilian adults who never report having a physical disability or other health issues before their absence. The color bands depict pointwise 95-percent confidence intervals. Standard errors are clustered at the worker level.

Figure A12: State-Level Association of Health-Related Absences, Covid-19 Case Rate, and Covid-19 Death Rate

Panel A: Correlation after Removing State and Month Fixed Effects

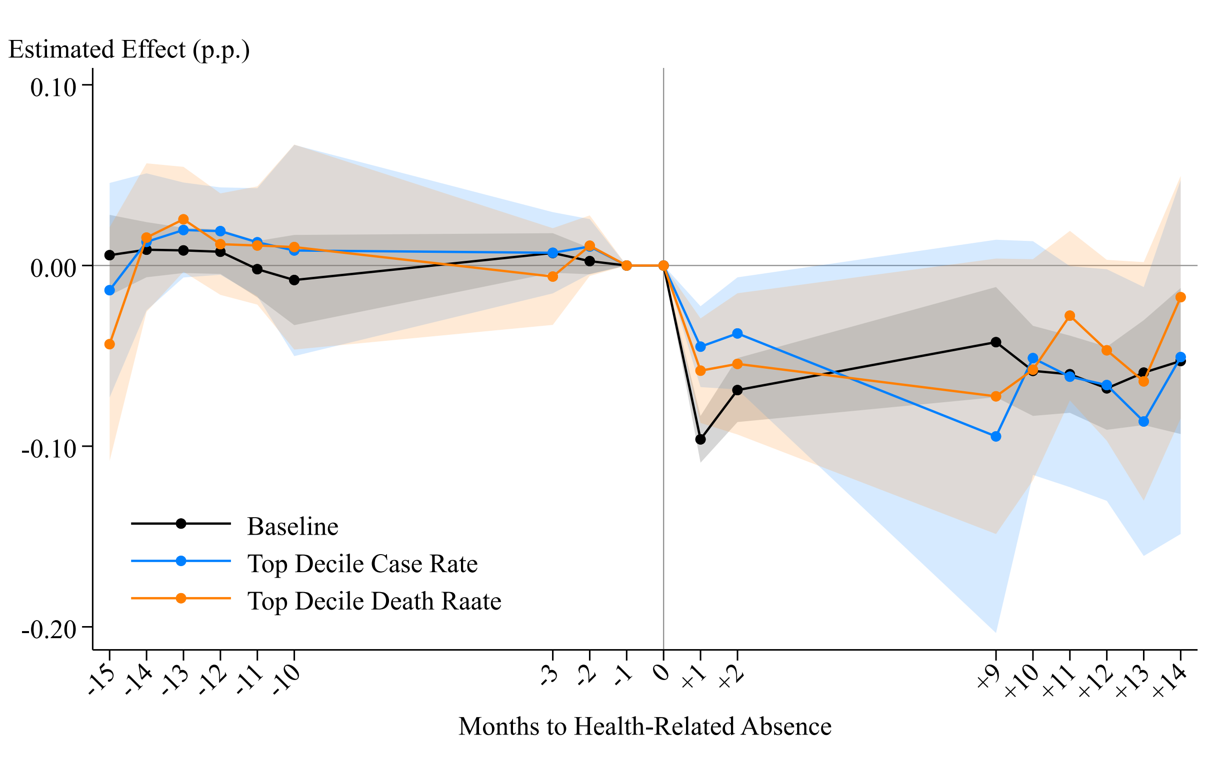


Panel B: Week-Specific Partial Correlation, after Removing State and Month Fixed Effects



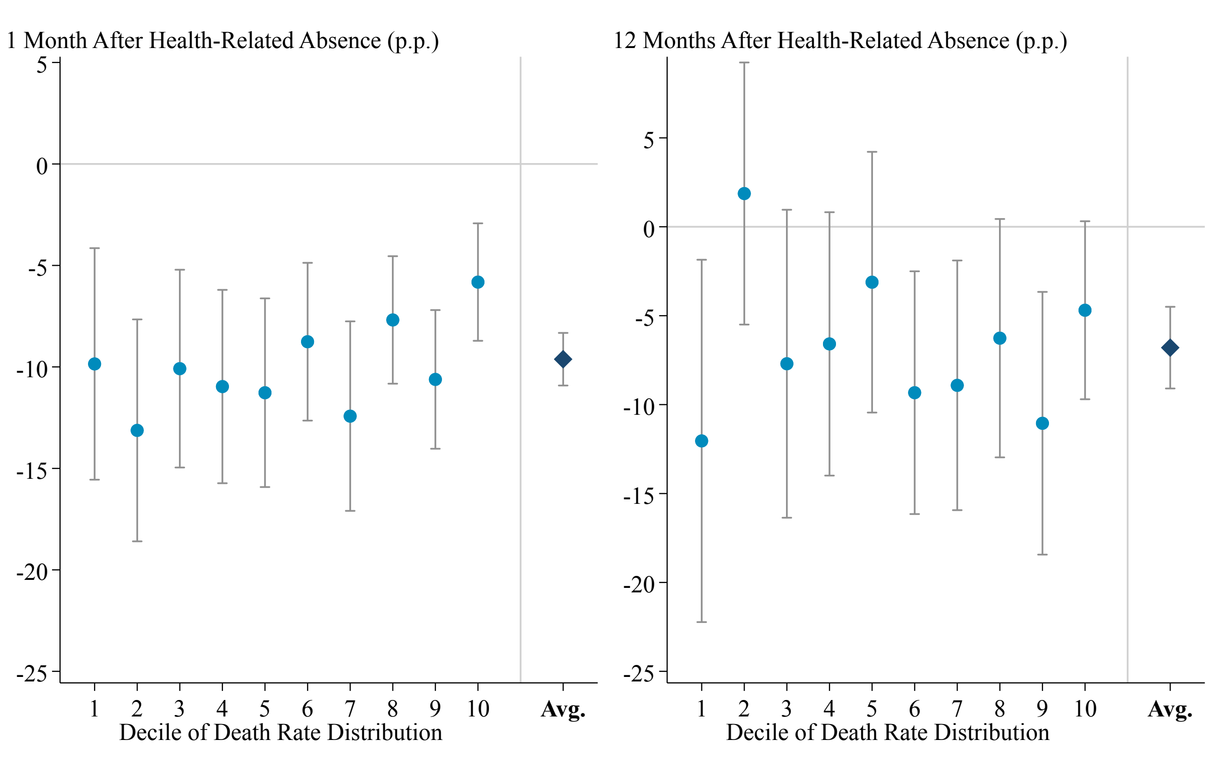
Notes: This figure examines the relationship between the state–month rate of health-related absences among the employed to the per-capita Covid-19 case and death rate in the same state in a given week, after removing state and month fixed effects. We count weeks relative to the month's CPS reference week. Panel A presents the correlation coefficients for each week. Panel B presents the partial correlation coefficient for each week after further removing variation from other weeks.

Figure A13: Event Study for Heterogeneous Effects of Health-Related Absences



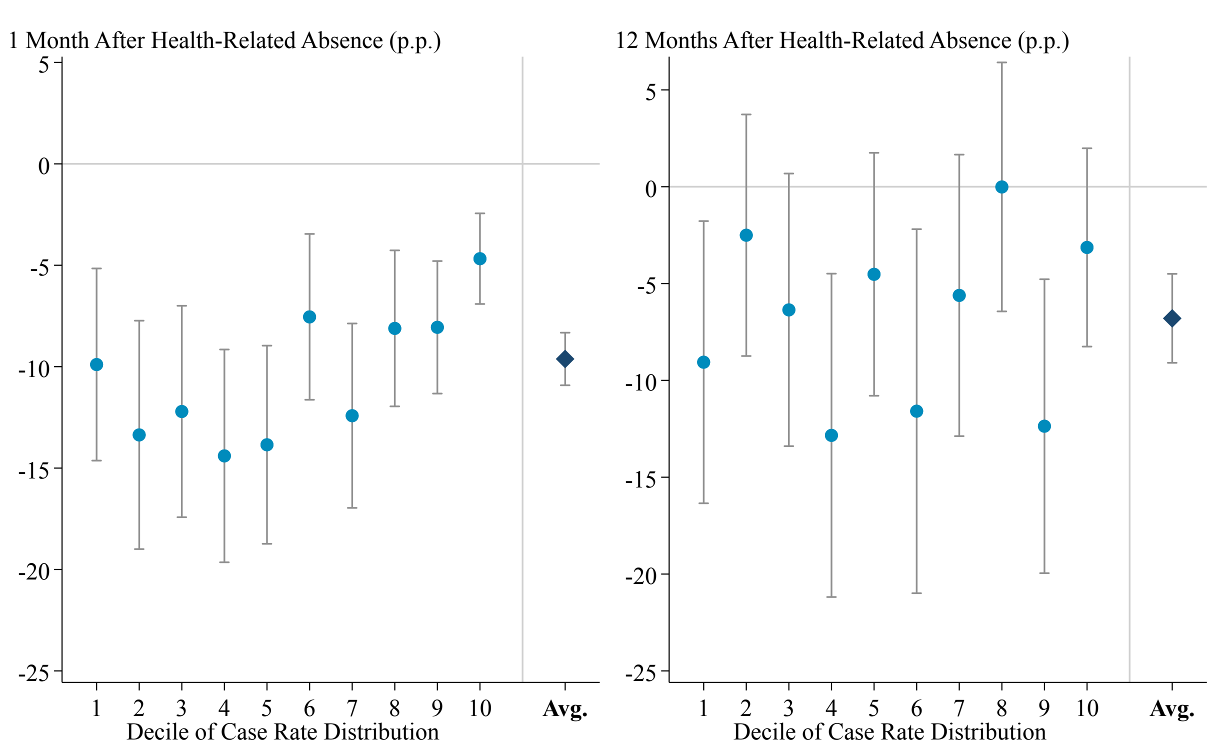
Notes: This figure displays event-study estimates of the coefficient on the interaction term in Equation 4. Gaps between months -10 and -3, and between $+2$ and $+9$, are due to sample rotation. The blue, orange, and gray lines plot respectively the estimates using the state–month Covid-19 case rate, the state–month Covid-19 death rate, and the worker-level health-related absence risk as the interaction variable. The sample is of civilian adults who never report having a physical disability or other health issues before their absence. The color bands depict pointwise 95-percent confidence intervals. Standard errors are clustered at the worker level.

Figure A14: Effects of Health-Related Absences by Covid-19 Death Rate



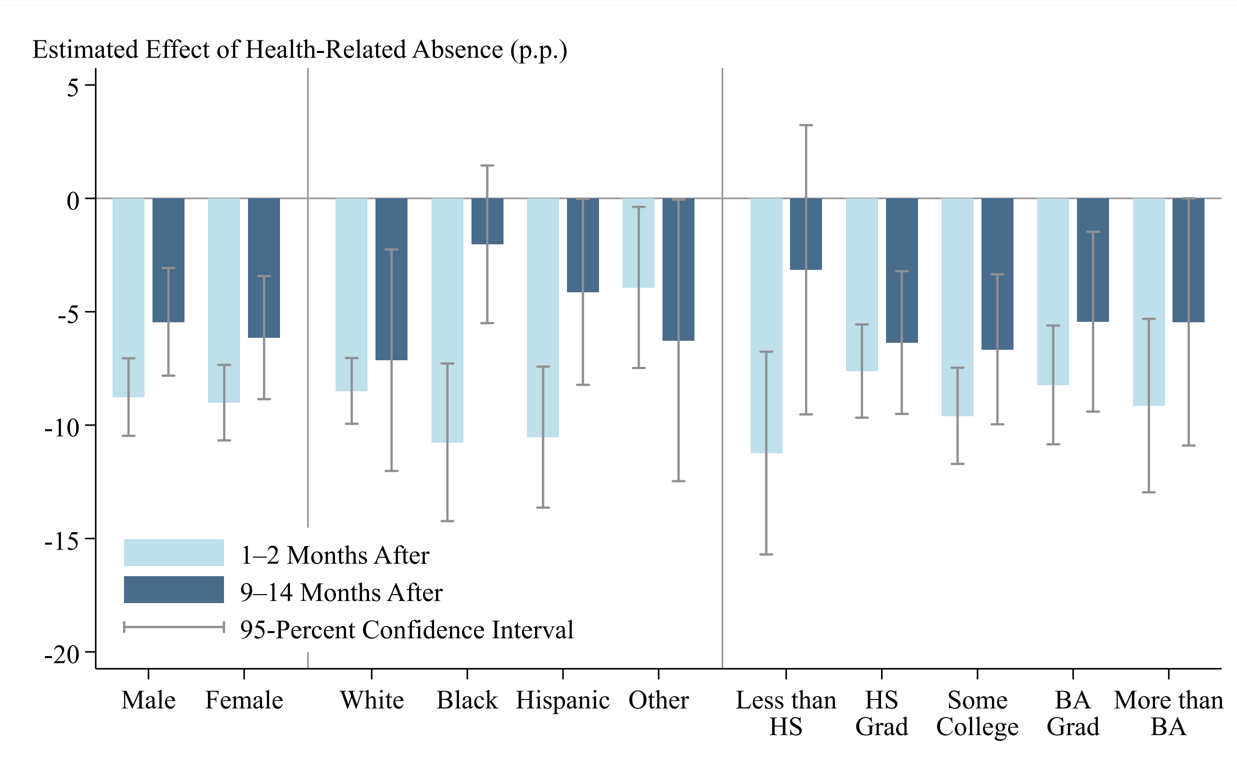
Notes: This figure plots the effects of health-related absences on labor force participation one and twelve months after the absence for health-related absences occurring in March 2020 onward. We estimate effects by decile of the Covid-19 case rate in the state and month of the absence. The color bands depict pointwise 95-percent confidence intervals. Standard errors are clustered at the worker level.

Figure A15: Effects of Health-Related Absences by Covid-19 Case Rate (County Level)



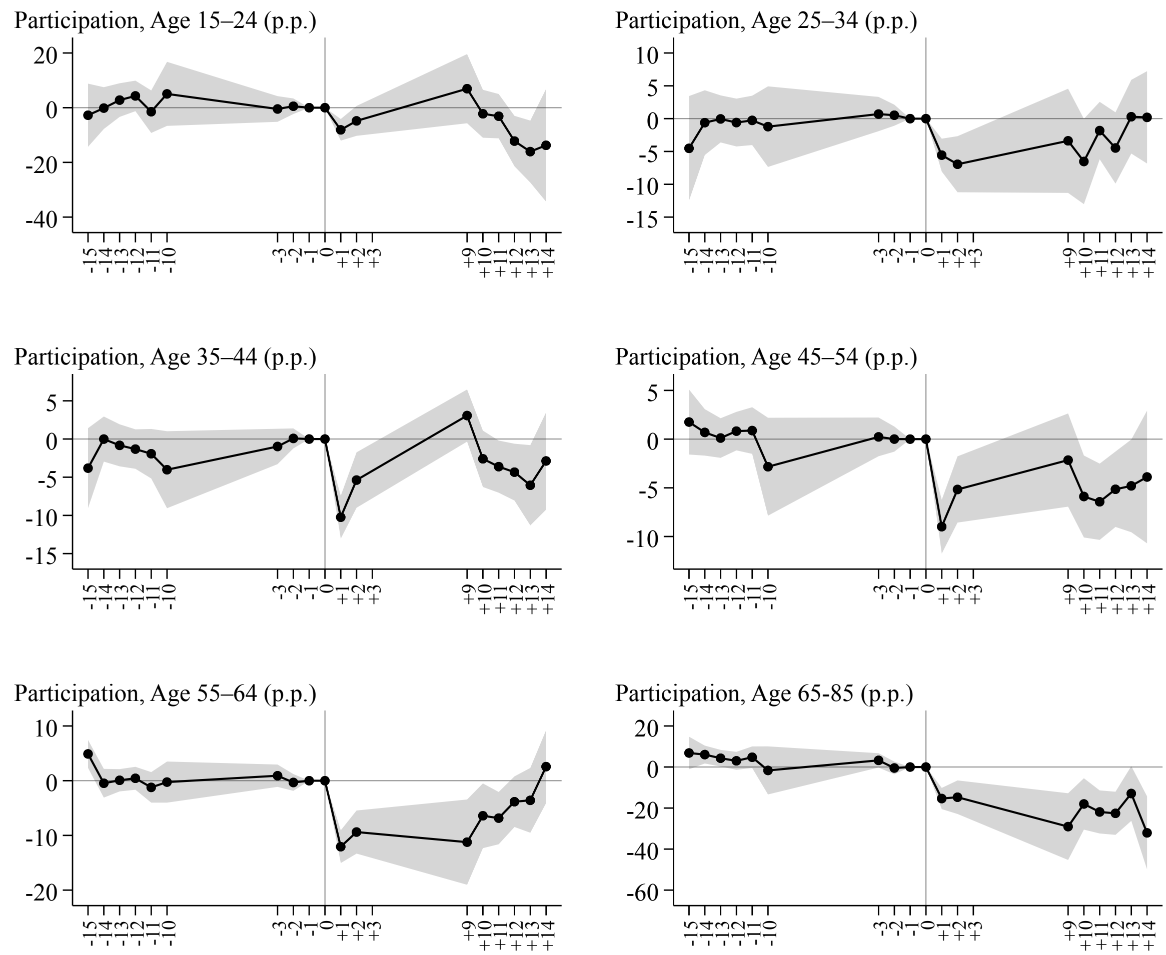
Notes: This figure plots the effects of health-related absences on labor force participation one and twelve months after the absence for health-related absences occurring in March 2020 onward. We estimate effects by decile of the Covid-19 case rate in the county and month of the absence. The color bands depict pointwise 95-percent confidence intervals. Standard errors are clustered at the worker level.

Figure A16: Effects of Health-Related Absences by Sex, Race/Ethnicity, and Schooling



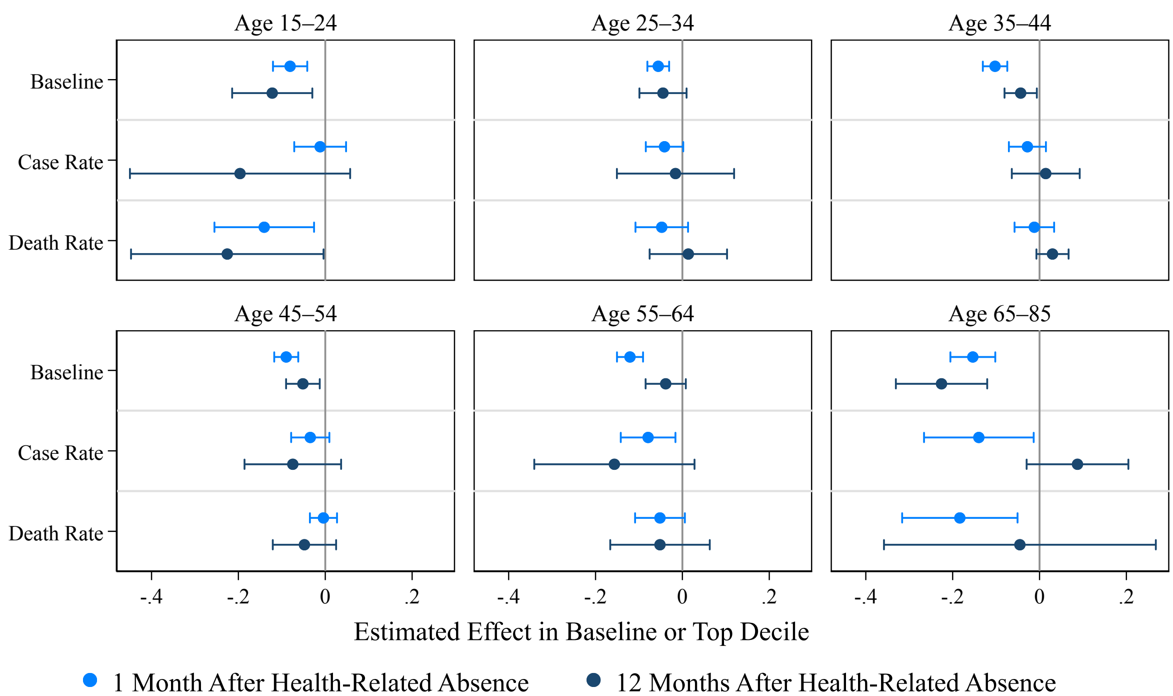
Notes: This figure displays demographic-specific effects of health-related absences on labor force participation at one and twelve months after the absence. We estimate our event-study specification (Equation 1), with our full set of controls, on subsamples of workers by sex, race/ethnicity, and schooling. Confidence intervals reflect standard errors clustered by worker.

Figure A17: Participation Impacts of Health-Related Work Abences, by Age Group



Notes: This figure plots estimates of coefficients β_k from Equation 1, limiting the sample by age in each panel. The coefficients represent the effect of a health-related absence on these outcomes k months before or after the absence. All specifications include our full set of controls. Gaps between months -10 and -3, and between $+2$ and $+9$, are due to sample rotation. For each specified age group, sample is of civilian adults who never report having a physical disability or other health issues before their absence. The color bands depict pointwise 95-percent confidence intervals. Standard errors are clustered at the worker level.

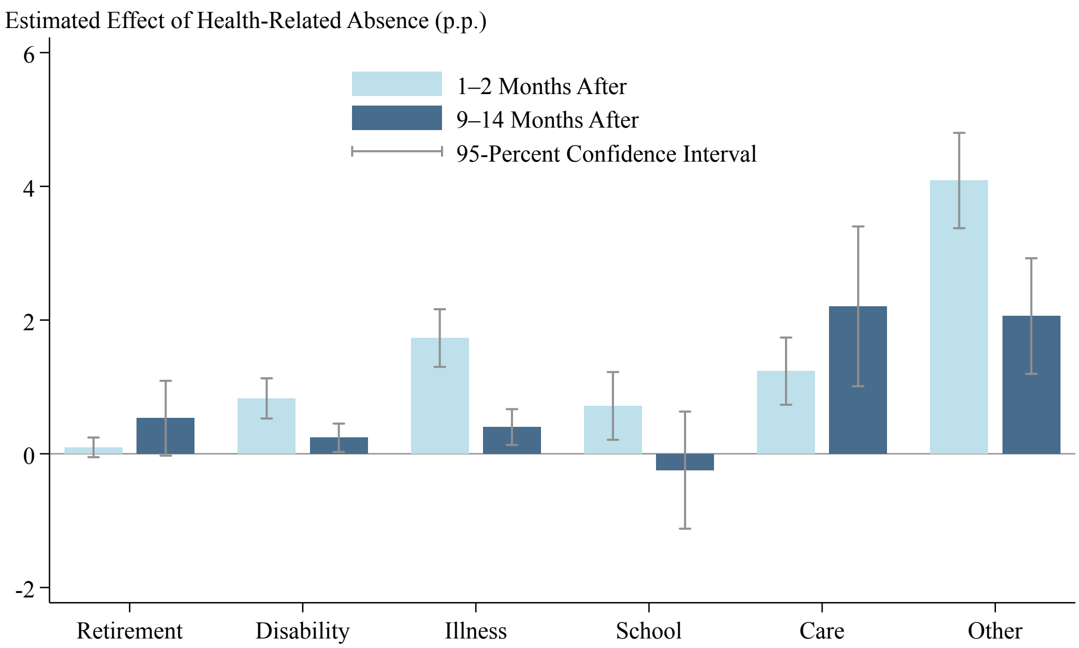
Figure A18: Heterogeneous Effects of Health-Related Absences on Participation by Age Group



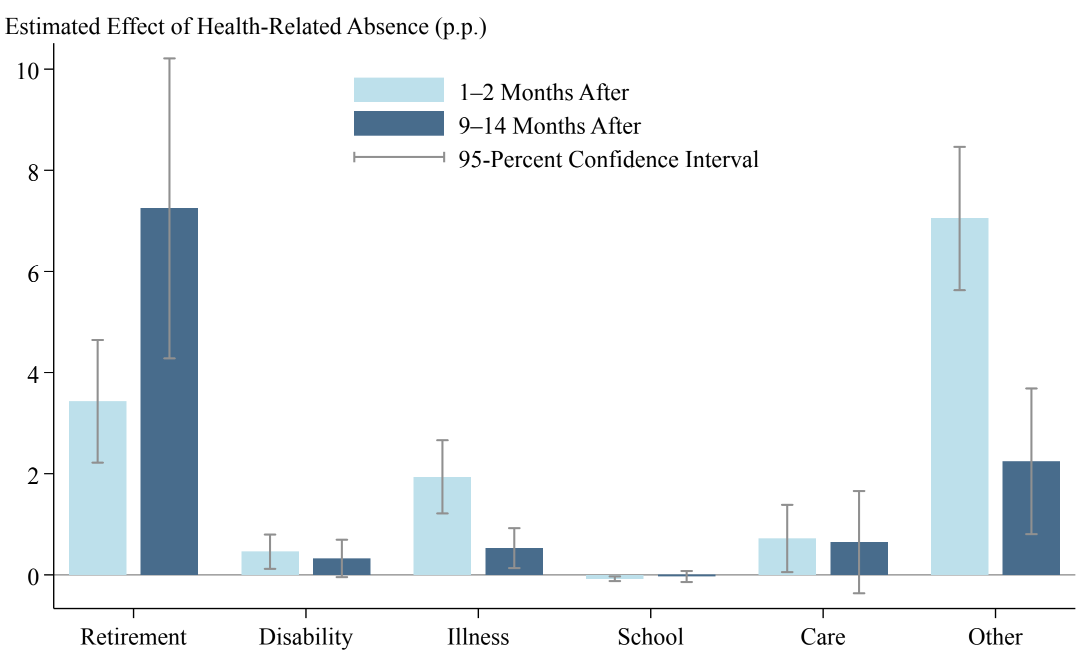
Notes: This figure displays estimated effects from Equation 4 of health-related absences on participation for absences occurring in the top decile of Covid-19 case or death rates. Deciles are computed for pandemic state–months. We display these coefficients at one and twelve months after the health-related absence, dividing the worker sample into six age groups, along with our baseline estimates from Panel A of Figure 6. Confidence intervals reflect standard errors clustered by worker.

Figure A19: Nonparticipation Responses to Health-Related Absence, by Reason and Age

Panel A: Workers Age 15 to 54

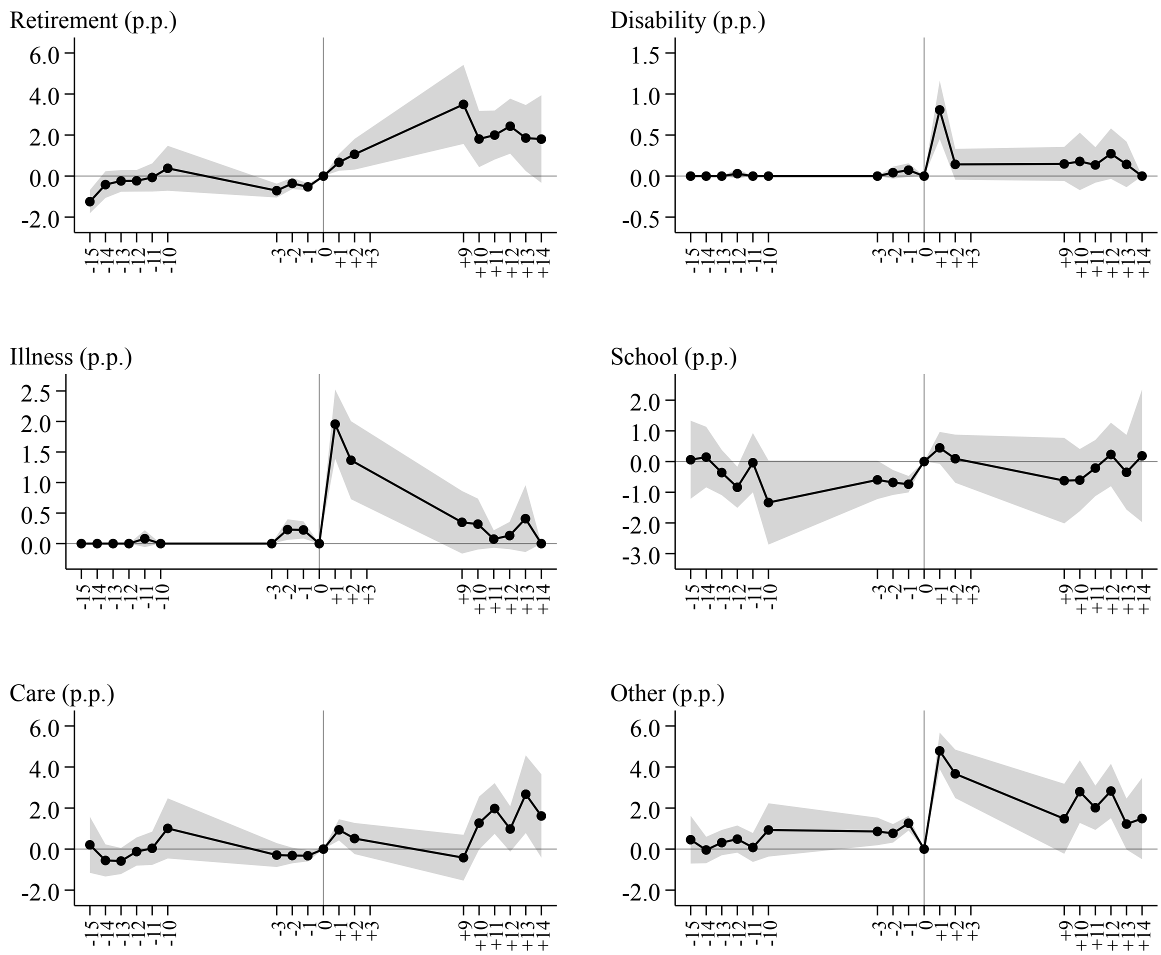


Panel B: Workers Age 55 and Over



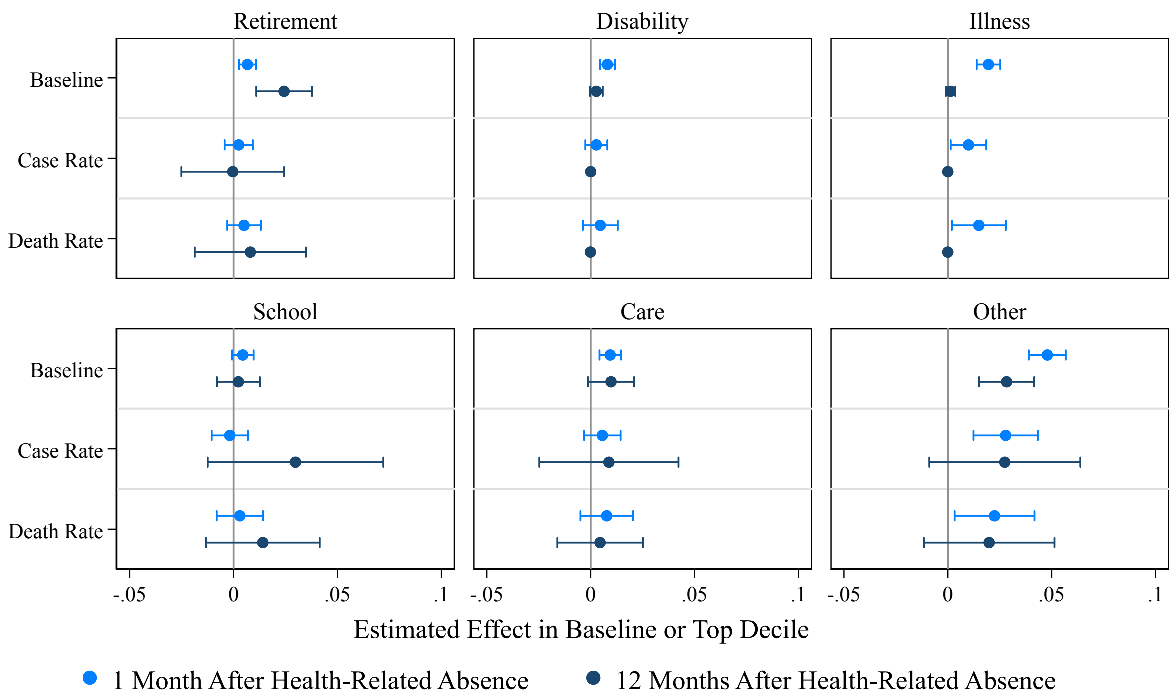
Notes: This figure displays effects of health-related absences on the probabilities of reasons for nonparticipation, as estimated by our event-study specification (Equation 1) using our full set of controls. For each reason, we report pooled effects at 1–2 months and 9–14 months after the health-related absence. Panel A uses the sample restricted to workers age 15 to 54, and Panel B uses the sample restricted to workers age 55 and over. Confidence intervals reflect standard errors clustered by worker.

Figure A20: Event Studies for Nonparticipation by Reason



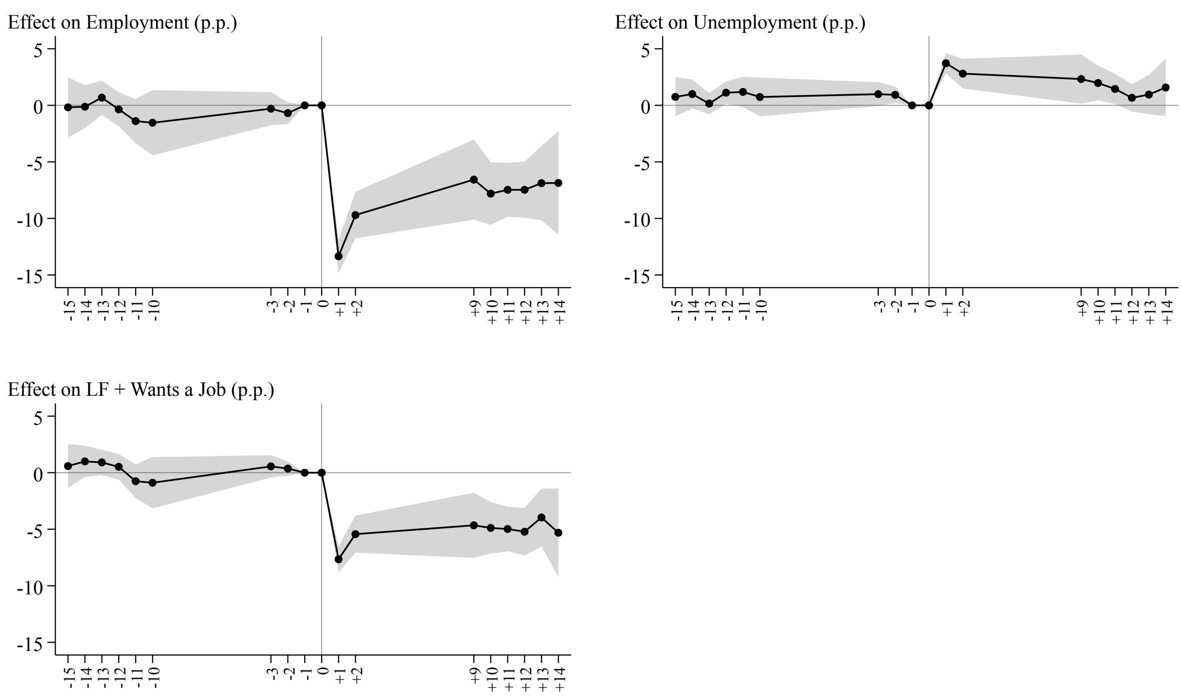
Notes: plots estimates of coefficients β_k from Equation 1, using different measures of intensive-margin labor supply as outcomes. The coefficients represent the effect of a health-related absence on these outcomes k months before or after the absence. All specifications include our full set of controls. Gaps between months -10 and -3, as well as $+2$ and $+9$, are due to sample rotation. The sample is of civilian adults who never report having a physical disability or other health issues before their absence. The color bands depict pointwise 95-percent confidence intervals. Standard errors are clustered at the worker level.

Figure A21: Heterogeneous Effects of Health-Related Absences on Nonparticipation by Reason



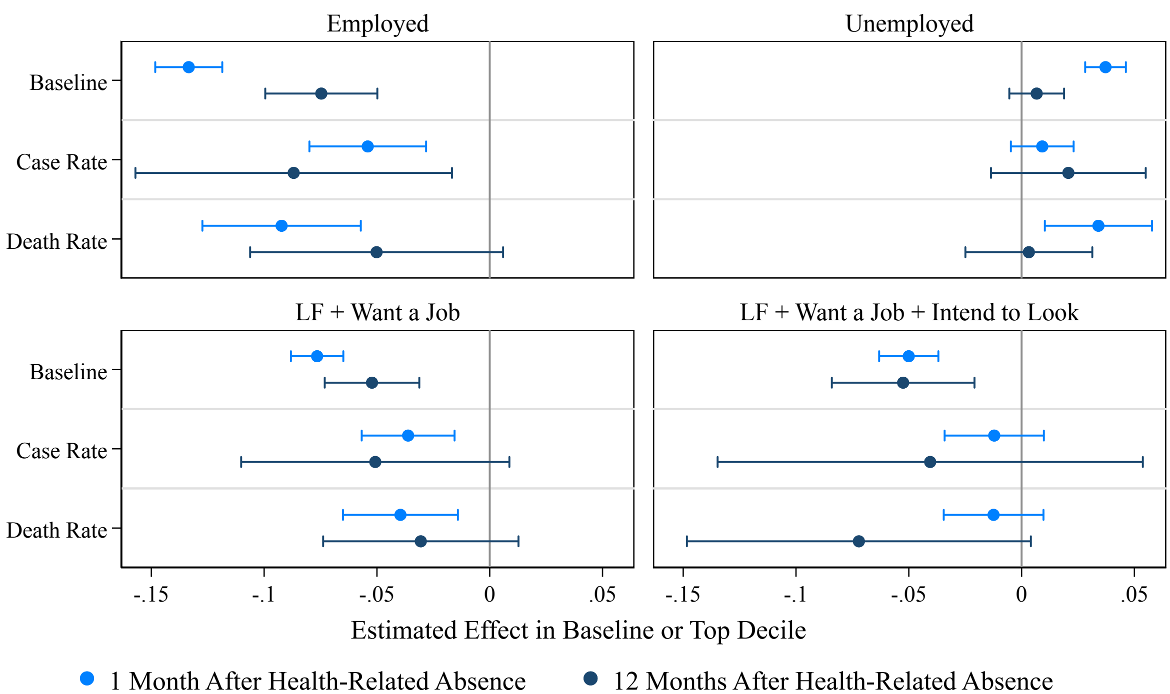
Notes: This figure displays estimated effects from Equation 4 of health-related absences on reason-specific rates of nonparticipation as outcomes, specifically for absences occurring in the top decile of Covid-19 case or death rates. Deciles are computed for pandemic state–months. We display these coefficients at one and twelve months after the health-related absence, along with our baseline estimates from Panel B of Figure 6. Confidence intervals reflect standard errors clustered by worker.

Figure A22: Event Studies for Other Measures of Extensive-Margin Labor Supply



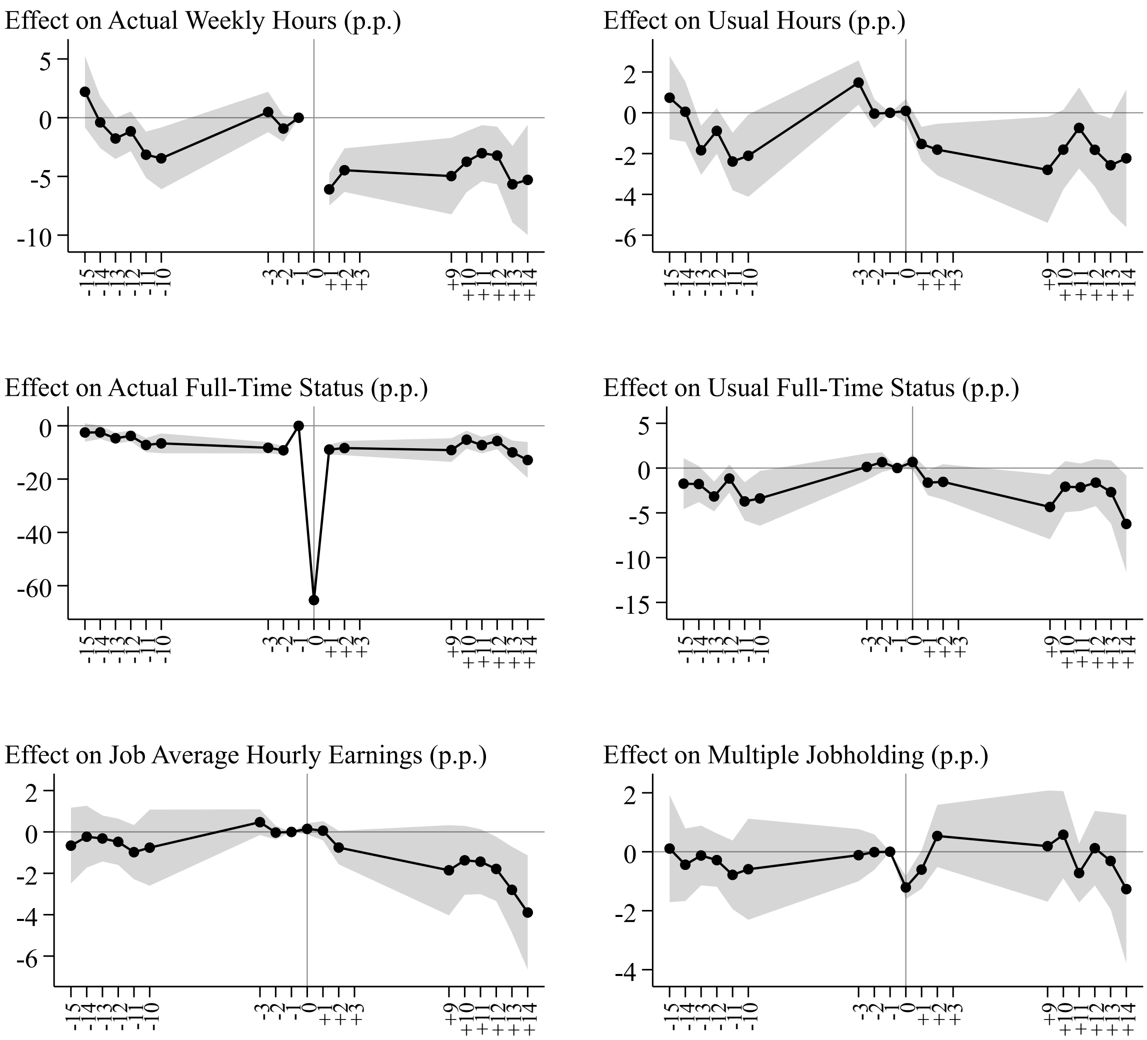
Notes: This figure plots estimates of coefficients β_k from Equation 1, using different measures of extensive-margin labor supply as outcomes. The coefficients represent the effect of a health-related absence on these outcomes k months before or after the absence. The CPS question regarding workers' intentions to search within twelve months is asked only among the outgoing rotation group, and thus we cannot produce an event study for this outcome. All specifications include our full set of controls. Gaps between months -10 and -3, as well as $+2$ and $+9$, are due to sample rotation. The sample is of civilian adults who never report having a physical disability or other health issues before their absence. The color bands depict pointwise 95-percent confidence intervals. Standard errors are clustered at the worker level.

Figure A23: Heterogeneous Effects of Health-Related Absences on Other Measures of Extensive-Margin Labor Supply



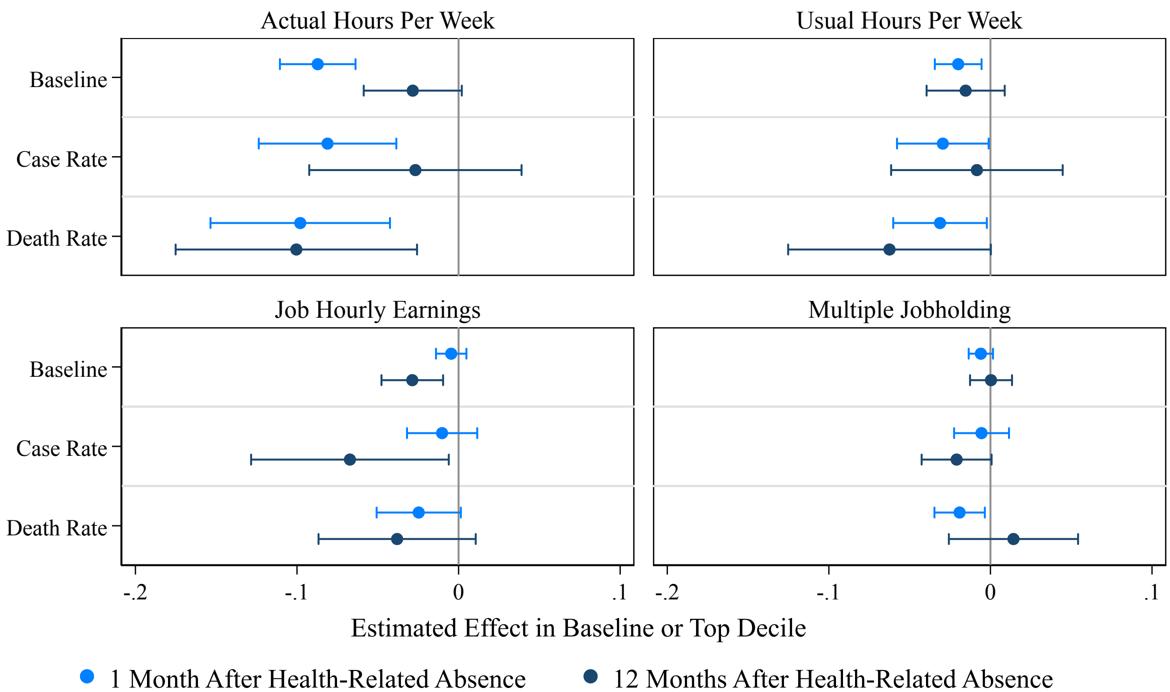
Notes: This figure displays estimated effects from Equation 4 of health-related absences on labor-market outcomes for absences occurring in the top decile of Covid-19 case or death rates. Deciles are computed for pandemic state–months. The outcomes of interest are four alternative measures of extensive-margin labor supply: employment; unemployment; in the labor force or want a job (but not looking); and in the labor force, want a job (but not looking), or intend to look for a job within 12 months (but do not want a job now and not looking now). We display these coefficients at one and twelve months after the health-related absence, along with our baseline estimates from Panel C of Figure 6. Confidence intervals reflect standard errors clustered by worker.

Figure A24: Event Studies for Measures of Intensive-Margin Labor Supply



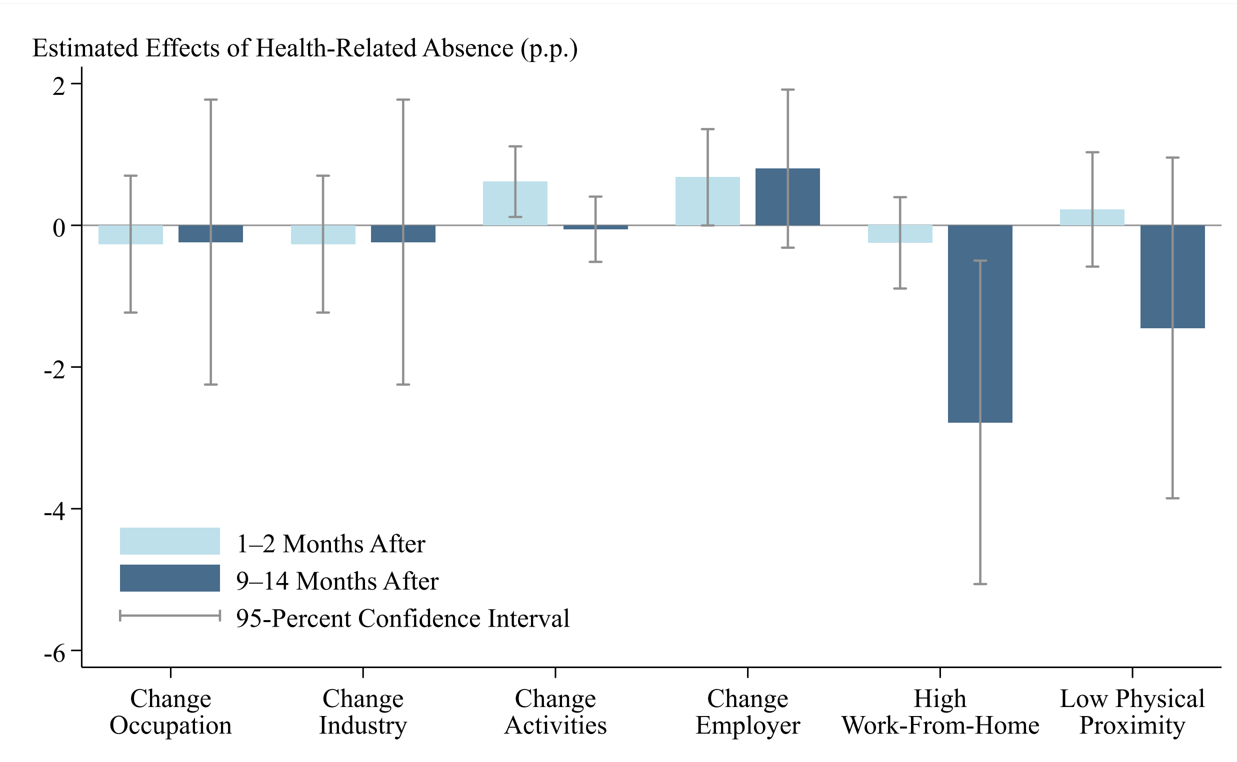
Notes: This figure plots estimates of coefficients β_k from Equation 1, using different measures of intensive-margin labor supply as outcomes. The coefficients represent the effect of a health-related absence on these outcomes k months before or after the absence. All specifications include our full set of controls. Gaps between months -10 and -3, and between $+2$ and $+9$, are due to sample rotation. The sample is of civilian adults who never report a physical disability. The color bands depict pointwise 95-percent confidence intervals. Standard errors are clustered at the worker level.

Figure A25: Heterogeneous Effects of Health-Related Absences on Other Labor Market Outcomes



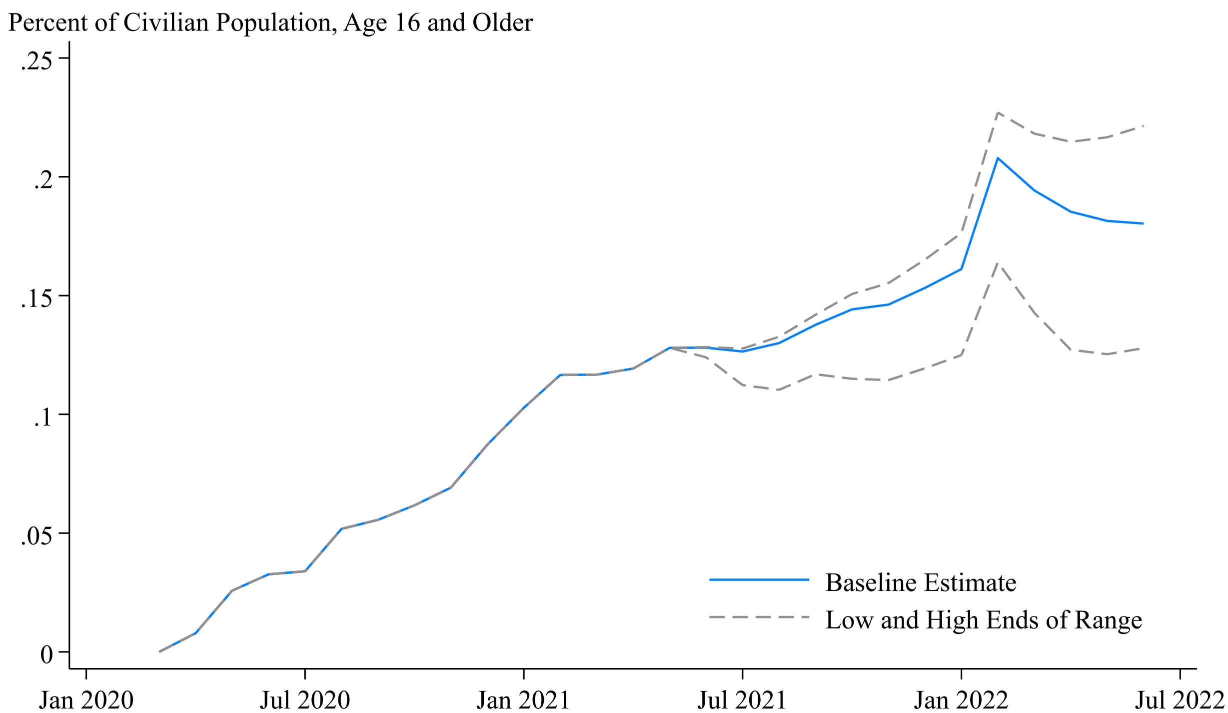
Notes: This figure displays estimated effects from Equation 4 of health-related absences on labor-market outcomes for absences occurring in the top decile of Covid-19 case or death rates. Deciles are computed for pandemic state–months. The outcomes of interest are log actual hours worked per week, log usual hours worked per week, log hourly earnings as predicted from the worker's job (industry–occupation intersection), and an indicator for multiple job-holding. We display these coefficients at one and twelve months after the health-related absence, along with our baseline estimates from Panel D of Figure 6. Confidence intervals reflect standard errors clustered by worker.

Figure A26: Impacts of Health-Related Absences on Work-Related Transitions



Notes: This figure displays effects of health-related absences on various outcomes related to transitions across occupations, industries, employers, and work activities, as estimated by our event-study specification (Equation 1) using our full set of controls. For each outcome, we report pooled effects at 1–2 months and 9–14 months after the health-related absence. Panel A uses the sample restricted to workers age 15 to 54, and Panel B uses the sample restricted to workers age 55 and over. Confidence intervals reflect standard errors clustered by worker.

Figure A27: Estimated Effects of Covid-19 Illnesses on the U.S. Labor Force Participation Rate



Notes: This figure presents our estimates of the aggregate labor force loss from Covid-19 illnesses, expressed as a share of the civilian adult population at each point in time. See the text for an explanation of our calculations for these aggregate losses.

Table A1: Occupation-Level Exposure Risk Measures Predict Health-Related Absences

	Dep. Var.: Health-Related Absences Per 1,000 Employed Workers							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Low WFH × Pandemic	3.495***		2.899***	2.822***	1.672***	1.863***	1.030**	1.142**
	(0.284)		(0.309)	(0.308)	(0.351)	(0.409)	(0.455)	(0.530)
High PP × Pandemic		2.730***	1.678***	1.675***	1.185***	0.619	0.975***	0.865*
		(0.283)	(0.307)	(0.305)	(0.314)	(0.391)	(0.362)	(0.443)
Observations	3,569,017	3,569,017	3,569,017	3,568,590	3,567,755	3,567,755	3,567,755	3,567,755
Clusters	803451	803451	803451	803314	803060	803060	803060	803060
State–Month FE				✓	✓	✓	✓	✓
Demographic FE				✓				
Demographic × Pandemic FE					✓	✓	✓	✓
Industry × Pandemic FE						✓		
Major Occ. Group × Pandemic FE							✓	
Detailed Occ. Group × Pandemic FE								✓

Notes: This table reports coefficient estimates from regressions of an indicator for health-related work absence on occupation-level measures of Covid-19 exposure risk. Columns 1–6 report estimates from the following specification:

$$\text{Absence}_{i,t} = \beta(\text{LowWFH}_i \times \text{Pandemic}_t) + \gamma(\text{HighPP}_i \times \text{Pandemic}_t) + \mathbf{X}_{i,t}\rho + u_{i,t},$$

where $\mathbf{X}_{i,t}$ is a vector of controls and fixed effects. Columns 1 and 2 estimate this specification without controls or fixed effects and only including one of the two risk measures. Column 3 includes both risk measures but no controls or fixed effects. Column 4 adds state–month and demographic-group fixed effects. Column 5 allows these demographic-group fixed effects to be pandemic-specific. Column 6 adds industry fixed effects interacted with a pandemic indicator. Columns 7 and 8, in lieu of industry fixed effects, add respectively major and detailed occupation-group fixed effects, therefore using only the within-occupation-group variation in exposure risk. Standard errors are clustered by state. $^{*} = p < 0.10$, $^{**} = p < 0.05$, $^{***} = p < 0.01$.

Table A2: Health-Related Absence Rates by Demographic Group

	Health-Related Absences Per 1,000 Employed Workers			Counts in the Pandemic	
	Rate, Pre-Pandemic	Rate, Pandemic	Change in Rate, Pandemic vs. Pre-Pandemic	Health-Related Absences	Unique People
Overall Sample	4.97 (0.06)	8.65 (0.12)	3.68 (0.13)	10311	327940
Age					
15–24	3.10 (0.12)	9.25 (0.33)	6.15 (0.35)	1214	48350
25–34	3.44 (0.11)	7.11 (0.23)	3.67 (0.25)	1779	145450
35–44	4.47 (0.13)	7.75 (0.23)	3.28 (0.26)	2026	73657
45–54	5.41 (0.14)	8.73 (0.26)	3.32 (0.29)	2102	65652
55–64	7.33 (0.18)	10.11 (0.31)	2.79 (0.36)	2148	56501
65 and Over	9.24 (0.34)	12.44 (0.54)	3.20 (0.64)	1042	25030
Sex					
Male	4.35 (0.08)	7.88 (0.15)	3.53 (0.17)	4952	171738
Female	5.69 (0.09)	9.54 (0.18)	3.85 (0.20)	5359	157808
Race and Ethnicity					
Non-Hispanic White	5.05 (0.08)	8.16 (0.14)	3.11 (0.16)	6686	218077
Non-Hispanic Black	6.61 (0.23)	11.16 (0.43)	4.55 (0.49)	1198	32606
Hispanic/Latino	4.36 (0.14)	9.59 (0.30)	5.23 (0.33)	1618	49266
Asian	4.35 (0.51)	11.98 (1.38)	7.64 (1.48)	155	3839
American Indian	2.84 (0.18)	5.75 (0.35)	2.91 (0.39)	482	21547
Other	6.28 (0.57)	9.86 (1.06)	3.58 (1.20)	172	4937
Education					
Less than High School	5.81 (0.22)	11.56 (0.47)	5.74 (0.51)	974	29256
High School Graduate	6.41 (0.14)	11.77 (0.27)	5.36 (0.30)	3545	92842
Some College	5.92 (0.13)	10.88 (0.26)	4.96 (0.28)	3397	92314
Bachelor's Degree	3.35 (0.10)	5.49 (0.18)	2.14 (0.20)	1679	85919
More than Bachelor's	2.82 (0.12)	3.92 (0.20)	1.11 (0.23)	716	49673

Notes: This table reports the rate of health-related work absences per thousand employed workers before (Jan. 2016–Feb. 2020) and during (Mar. 2020–Dec. 2021) the Covid-19 pandemic. We also report the raw counts of health-related absences and unique people during the pandemic. Standard errors are clustered by person.

Table A3: Heterogeneous Effects of Health-Related Absences by Labor Market Conditions

	1 Month Later		2 Months Later		12 Months Later	
	Unemployed (1)	In Labor Force (2)	Unemployed (3)	In Labor Force (4)	Unemployed (5)	In Labor Force (6)
Health-Related Absence	-0.034***	-0.077***	-0.006	-0.026	-0.005	-0.079***
	(0.013)	(0.015)	(0.016)	(0.021)	(0.014)	(0.029)
HRA × Unemp. Rate	1.139***	-0.311	0.537*	-0.675**	0.151	0.137
	(0.225)	(0.230)	(0.283)	(0.319)	(0.181)	(0.330)
Observations	764,655	764,655	351,585	351,585	334,852	334,852
Clusters	337253	337253	278258	278258	147112	147112

Notes: This table reports estimates of Equation 4, an event-study specification that allows for heterogeneous effects of health-related work absences. We report effects of health-related work absences at one, two twelve months after the absence and allow for effect heterogeneity with respect to the state–month unemployment rate associated with the health-related absence. We use official reported state unemployment rates from the Local Area Unemployment Statistics program of the Bureau of Labor Statistics. The scaling of these interaction variables is such that a coefficient of one implies that a one-percentage-point increase in the unemployment rate implies that a health-related absence increases the probability of unemployment after the absence by one percentage point. Standard errors are clustered by worker. $^{*} = p < 0.10$, $^{**} = p < 0.05$, $^{***} = p < 0.01$.

Table A4: Impacts of Health-Related Absences, by Job Earnings

	(1)	(2)	(3)	(4)	(5)	(6)
	Employment		Hours		Job Earnings	
	1–3 Months	9–14 Months	1–3 Months	9–14 Months	1–3 Months	9–14 Months
Health-Related Absence,	-0.101***	-0.067***	-0.066***	-0.045**	0.003	-0.002
Above-Median Earnings	(0.009)	(0.013)	(0.013)	(0.018)	(0.004)	(0.009)
Health-Related Absence,	-0.139***	-0.079***	-0.079***	-0.061**	-0.003	-0.028***
Below-Median Earnings	(0.011)	(0.016)	(0.017)	(0.024)	(0.004)	(0.010)
Test of Equality (*p*-value)	0.008	0.561	0.541	0.600	0.260	0.055
People	333336	155880	307343	136628	317143	138896
Illnesses	3,169	1,430	2,098	924	3,000	1,270

Notes: This table reports estimates of Equation 4, an event-study specification that allows for heterogeneous effects of health-related work absences. We report effects of health-related work absences at one and twelve months after the absence. We allow for effect heterogeneity with respect to whether the worker's pre-absence combination of occupation and industry places them above or below the median predicted hourly earnings. Standard errors are clustered by worker. $^{*} = p < 0.10$, $^{**} = p < 0.05$, $^{***} = p < 0.01$.

Table A5: Testing for Heterogeneous Effects of Covid and Non-Covid Health Absences, Expanded Sample

	1 Month		12 Months	
	(1)	(2)	(3)	(4)
Health-Related Absence	-0.150***	-0.148***	-0.131***	-0.129***
	(0.006)	(0.005)	(0.010)	(0.010)
× Case Rate	0.011***		0.028***	
	(0.003)		(0.011)	
× Death Rate		0.002		0.011*
		(0.006)		(0.006)
People	302457	302457	101107	101107
Illnesses	6667	6667	2180	2180

Notes: This table reports estimates of Equation 4, an event-study specification that allows for heterogeneous effects of health-related work absences. We report effects of health-related work absences at one and twelve months after the absence and allow for effect heterogeneity with respect to the state–month reported Covid-19 case rate one week after the CPS reference week and the state–month Covid-19 death rate two weeks after the CPS reference week. We report coefficients with respect to the standardized case/death rate, transforming these rates to have zero mean in each month nationally and unit standard deviation over the entire sample. Standard errors are clustered by worker. $^{*} = p < 0.10$, $^{**} = p < 0.05$, $^{***} = p < 0.01$.

B Supplementary Results

B.1 Sample Restrictions

Clean Controls. Appendix Figure A6 shows the results of our main event-study specification (Equation 1), but including workers who do not satisfy the "clean controls" sample restriction. This expanded control group contains some workers with health-related absences, and both treated and control groups can include workers with multiple such absences. A comparison of estimates between this figure and Panel A of Figure 2 shows them to be essentially undistinguishable. The clean-controls restriction has little impact on our results because health-related absences are relatively rare, and so nearly all observations serving as controls in the full sample are "clean" of prior health-related absences anyway.

No Pre-Absence Health Issues. Appendix Figure A7 shows the results of our main event-study specification (Equation 1), but including workers who have observable health issues before the health-related absence. In this expanded sample, we find somewhat larger effects of health-related absences on labor force participation: Health-related absences reduce the participation probability by 9 percentage points, rather than 7 percentage points, at 12 months after the absence. Appendix Figure A8 also eliminates the "clean controls" sample restriction.

The reason that we do not use this sample as our baseline is that it is more vulnerable to concerns about heterogeneous effects of health-related absences. In particular, it may be less plausible that the average health-related absence is representative of Covid-19 illnesses when we include these workers, some of whom likely have chronic health issues that cause absences. We indeed find some evidence of this concern when we redo our heterogeneity analyses (Section 3.4) in this expanded sample. Appendix Table A5 shows that health-related absences appear somewhat less severe in this sample when the standardized state Covid-19 case and death rates are relatively high. The estimate is of marginal statistical significance, but as explained in the main text, such heterogeneous effects of health-related absences are consistent with differential effects of Covid-related and non-Covid-related health absences. The larger participation effect in the expanded sample may thus reflect more severe non-Covid health absences, rather than more severe Covid-19 illnesses. Excluding workers with pre-absence health issues implies, if anything, that our baseline conclusions are somewhat conservative, though we think it is more likely that the expanded-sample estimates are not representative of the effects of Covid-19 illnesses.

B.2 Unobservable Differences in Health

Comparison to Observable Differences. Workers with health-related absences may be less healthy in general than workers without such absences. Our sample construction helps to reduce this threat by excluding those in observable ill-health, but unobservable differences in health may still be present. In particular, ill-health may undermine their labor force attachment in ways that are inappropriate to attribute to any single absence. For instance, known risk factors for severe Covid-19 cases include obesity, hypertension, and smoking (Mahamat-Saleh et al., 2021), which suggests that people who miss a week of work due to Covid-19 may be more likely to leave the labor force due to these other health issues, independent of their Covid-19 illnesses.

We assess this concern of unobservable ill-health by a comparison to the effects of observable ill-health. In particular, we modify our event study so as to include in the sample workers who were *observably* unhealthy before their health-related absence, who we have otherwise excluded. Our specification is:

$$\mathrm{LF}_{i,t+k} = \beta_{0,k}\mathrm{HRA}_{i,t} + \beta_{1,k}(\mathrm{HRA}_{i,t} \times \mathrm{Unhealthy}_i) + \gamma_k \mathrm{Unhealthy}_i + \mathbf{X}_{i,t}\mathbf{\Lambda}_k + \phi_{s,t,k} + u_{i,t+k}, \quad (5)$$

where, most importantly, the coefficients γ_k capture the "effect" of ever being in observable ill-health (but not absent for health reasons) on participation k months ahead. We view this as an upper bound on the bias from unobservable ill-health, insofar as observable ill-health is likely to be worse for participation than its unobservable counterpart. Appendix Figure A9 plots the estimates $\widehat{\gamma_k}$ in comparison to $\widehat{\beta_{0,k}}$, the effect of health-related absence for observably-healthy workers. Our estimated effects of health-related absence are considerably larger than this bound on the bias from unobservable differences in health.

Furthermore, Appendix Figure A9 shows that workers in observable ill-health have a significant positive pre-trend in their participation. This pre-trend is consistent with lower labor force attachment among such workers and is similar to the positive pre-trend in Figure 2 without controls, which vanished with controls. The ability of controls to eliminate this pre-trend may suggest that any remaining "post-trend" bias from unobservable health differences is considerably smaller than the bound in Appendix Figure A9. In other words, our controls appear to achieve balance on unobservable health status before the absence.

Future-Absent Worker Comparison Group. We now use only workers who will have a health-related absence in 9 to 15 months as a comparison group for workers who have such absences immediately. That is, here we exploit only differences in the timing of health-related absences. This approach thus partly addresses the issue of unobservable differences between workers with and without such absences. It requires only a sample restriction on our main specification (Equation 1).

Due to the CPS panel design, we can only estimate the effects of health-related absences in the short run, from three months before to two months after. The sample restriction to future-absent workers considerably reduces the size of the comparison group, reducing the precision of our estimates. For comparison, we also include in the figure our baseline results.

Appendix Figure A10 displays the event-study estimates. The confidence intervals are wide, but the estimates using only future-absent workers as comparison units are similar to our baseline results.

B.3 Attrition

We next investigate whether selective attrition from the sample gives rise to bias in our results. In particular, one might worry that, after a health-related absence, people who return to the labor force may be more likely to exit the sample than people who remain out, generating a spurious effect of health-related absences on participation. In Appendix Figure A11, we re-estimate our event-study specification (Equation 1), using as the outcome an indicator for survey nonresponse in a given month. This specification evaluates the extent of differential attrition between workers who have a health-related absence and observably-similar workers who do not. There is significant differential attrition of about 6 percentage points in favor of workers with absences in the months immediately following an absence, but differential attrition falls to nearly zero by 14 months after the absence.

Lee (2009) provides two assumptions under which measures of differential attrition yield bounds on causal effects. First, health-related absences must be as good as randomly assigned, conditional on controls, an assumption already inherent in causal interpretation of the event-study design. Second, health-related absences must have monotone effects on attrition—that is, Covid-19 illness may make all workers more or less likely to respond, but it cannot make some workers more likely and others less likely.[23] Under these assumptions, the attrition event study implies that the long-run participation effects of health-related absences are minimally sensitive to attrition. It is possible that the short-run participation effects may be materially understated, insofar as the attrited may be severely ill and out of the labor force.

References for Appendices

Lee, David S, "Training, Wages, and Sample Selection: Estimating Sharp Bounds on Treatment Effects," *The Review of Economic Studies*, 2009, *76* (3), 1071–1102.

Mahamat-Saleh, Yahya, Thibault Fiolet, Mathieu Edouard Rebeaud, Matthieu Mulot, Anthony Guihur, Douae El Fatouhi, Nasser Laouali, Nathan Peiffer-Smadja, Dagfinn Aune, and Gianluca Severi, "Diabetes, Hypertension, Body Mass Index, Smoking and COVID-19-Related Mortality: A Systematic Review and Meta-Analysis of Observational Studies," *BMJ Open*, 2021, *11* (10), e052777.

[23] In our context, the Lee (2009) assumption rules out the possibility that health-related absences decrease attrition among future labor force dropouts and increase attrition among those who continue to participate after their absence.