THE IMPACTS OF COVID-19 ILLNESSES ON WORKERS

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ABSTRACT

We show that Covid-19 illnesses persistently reduce labor supply. Using an event study, we estimate that workers with week-long Covid-19 work 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 illnesses have reduced the U.S. labor force by approximately 500,000 people (0.2 percent of adults) and imply an average forgone earnings per Covid-19 absence of at least $9,000, about 90 percent of which reflects lost labor supply beyond the initial absence week.

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1 Introduction

There have been over 57 million reported cases and approximately 250,000 deaths from Covid-19 among working-age U.S. adults through July 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 Covid-19 illnesses 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. 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 are higher among occupations with higher Covid-19 exposure.

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 probable Covid-19 illnesses 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 illnesses 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 illnesses 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 illnesses.

We next use our results to assess the aggregate impacts of Covid-19 illnesses 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 illnesses 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 illnesses at the average rate of health-related absences in 2021. These losses fundamentally reflect an overall increase in

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[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.

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

Finally, we calculate the average cost of a Covid-19 absence in forgone labor earnings through fourteen months after the absence, including both extensive and intensive margins of adjustment. The average Covid-19 absence results in forgone earnings of at least $9,000, about 90 percent of which is due to persistent labor-supply reduction beyond the week-long absence itself. This economic cost can be viewed as a revealed-preference measure of the medium-term health impact of Covid-19 illnesses. 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), and 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 approximately 1.5 million workers (Bach, 2022; Domash and Summers, 2022; Cutler, 2022). The existing survey evidence has several limitations, including the 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.[2] 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.[3]

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 workers' own Covid-19 illnesses.

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 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.[4] 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).

Several features of this definition of work 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-

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

[3]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).

[4]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.

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 illnesses. In Section 3, we address whether the effects of Covid-19 illnesses appear to differ from other causes of health-related absences.

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

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.

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.[5]

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

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[5]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.

Figure 1: Health-Related Absences Versus Covid-19 Illnesses

Panel A: National Time Series
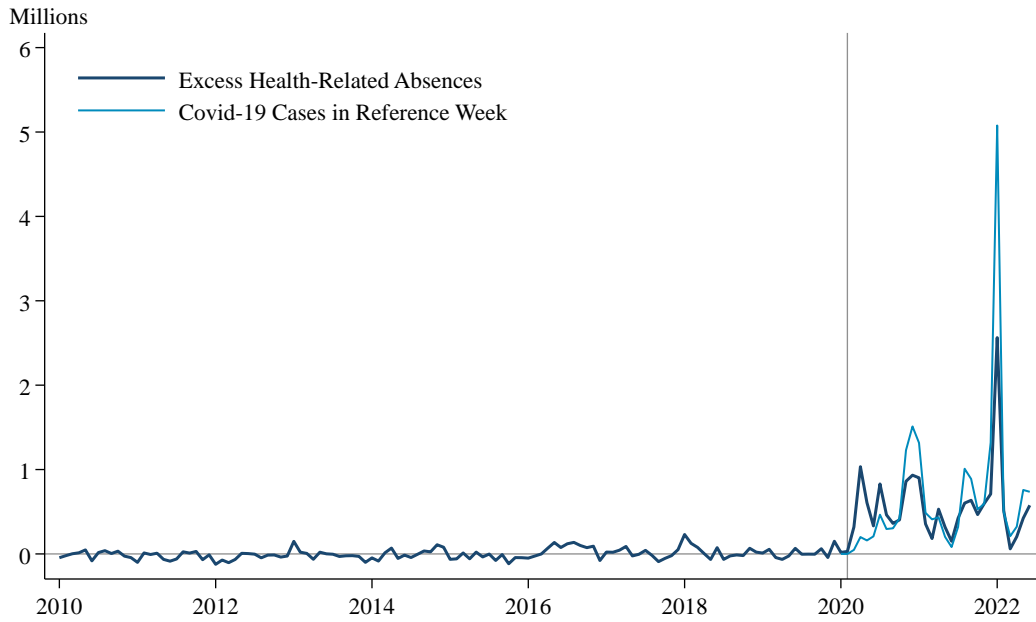
Panel B: State–Month Panel, Removing State and Month Fixed Effects
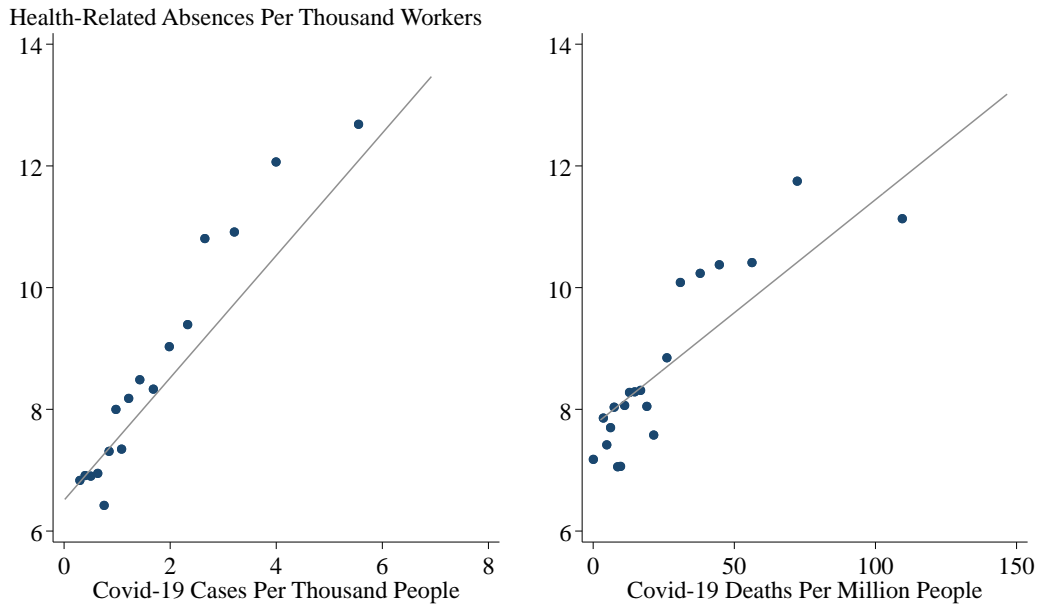
Notes: In Panel A, this figure displays monthly counts, in millions, of excess health-related absences and Covid-19 cases in the CPS reference week. Excess health-related absences are computed by subtracting the seasonal (monthly) trend number of health-related absences, estimated from January 2010 through 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, it displays binned scatterplots of the state-level relationships between Covid-19 case rates (per thousand people) or Covid-19 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.

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levels of health-related absences during the pandemic reflect Covid-19 illnesses.[6] 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 after 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-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.[7]

3 Do Covid-19 Illnesses Reduce Labor Supply?

We next examine the effects of health-related absences on labor force participation and other labor market outcomes using a worker-level event study. We then assess the degree to which these effects reflect Covid-19 illnesses.

3.1 Event Study

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

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

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[6]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.

[7]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.

where $LF_{i,t+k}$ indicates whether person i participates in the labor force in month $t+k$. The variable $HRA_{i,t}$ 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 worker 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.[8] The specification is estimated separately for each time horizon k, which Dube et al. (2022) define as the "local-projection" approach to difference-in-differences.

We estimate Equation 1 on the worker-level panel data 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 restrict 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. Unless otherwise noted, our sample period is from November 2018 (fifteen months prior to the pandemic) through June 2022.

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).[9]

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[8]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.

[9]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

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

Panel A: Event Study
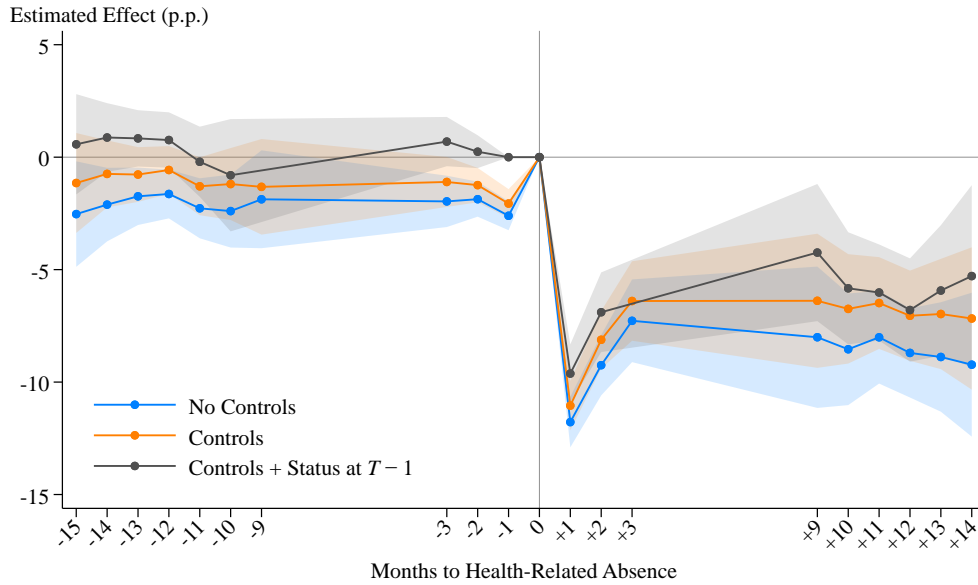
Panel B: Aggregate Flows from Health-Related Absence into Nonparticipation
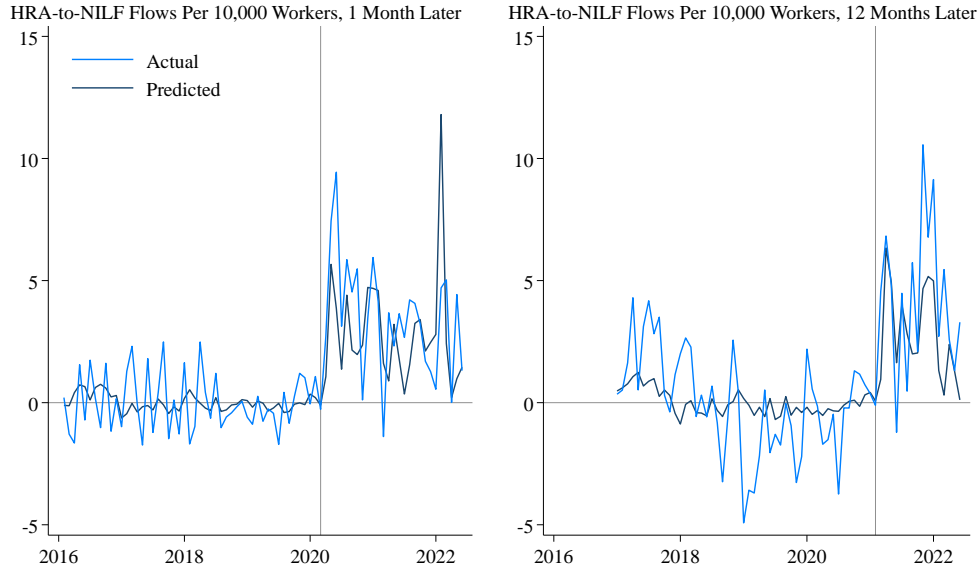
Notes: In Panel A, the figure plots coefficient estimates β_k from Equation 1, which represents the effect of a health-related absence during the pandemic on the probability of labor force participation k months before or after the absence. The blue, orange, and gray lines respectively plot estimates without demographic controls, with demographic controls, and with controls for demographics and labor market status. Gaps between months 3 and 9 are due to sample rotation. The color bands depict pointwise 95-percent confidence intervals. Standard errors are clustered at the worker level. In Panel B, 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. All lines are adjusted for seasonality using pre-pandemic month fixed effects. Vertical lines identify when flows could be affected by the pandemic.

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.

Appendix B discusses two threats to identification in our event study: (1) unobservable differences in health, as noted above, and (2) survey nonresponse, particularly attrition over the multiple CPS waves. Neither are likely to explain our results.

3.3 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 3 plots estimates of age-specific effects at two time horizons, one to two months after the absence and nine to fourteen months.[10] 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 much larger in the longer run. Among workers age 65 to 85 with a health-related absence, approximately one in five exit the labor force around a year later due to that absence. We do not find notable heterogeneity by worker sex, race, ethnicity, and education (see Appendix Figure A11).

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 3 reports the effect of health-related absences on these reason-specific rates of nonparticipation. The reason most responsive to health-related absences is "other." Rates of illness- and disability-related nonparticipation increase only modestly, and these increases

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between our estimates and theirs.

[10]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.

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 A14).

Other Extensive-Margin Responses. Panel C of Figure 3 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 adults themselves intend to be lasting.

Intensive-Margin Responses. Panel D of Figure 3 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.[11]

Workers work fewer hours per week and shift on net into lower-wage jobs 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.[12] Effects on workers' usual weekly hours follow the opposite pattern, growing over the longer term. 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 following 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.[13] Adjustment on this margin occurs slowly. We see little change in rates of multiple job-holding at either time horizon.
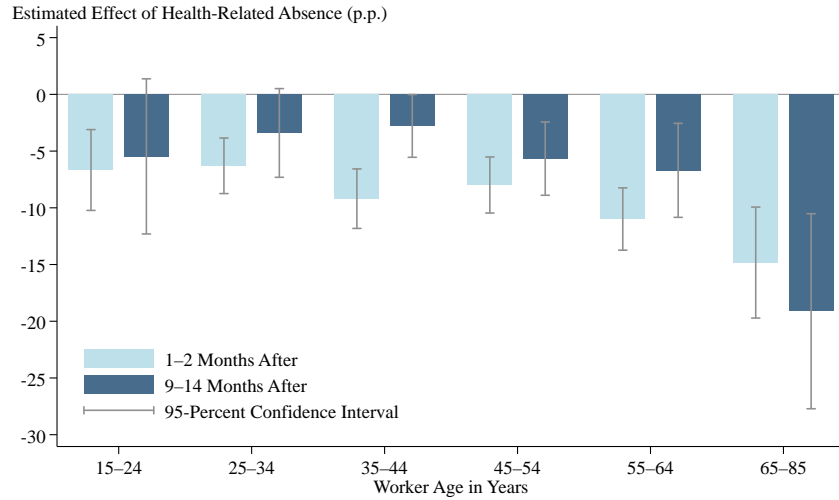
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[11]We focus on workers' main jobs because we lack key data about other jobs. Relatively few workers in our sample (5 percent) hold multiple jobs. The minimal effect on multiple job-holding further suggests this focus on primary jobs is immaterial to our forgone-earnings calculation.

[12]Appendix Figure A19 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 per worker are likely smaller than Figure 3 suggests.

[13]Specifically, we use a Poisson regression: $E[w|n,o] = \exp(\alpha_n + \alpha_o)$, 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 3: Effects of Health-Related Absences on Workers

Panel A: Effects by Age Group

Estimated Effect of Health-Related Absence (p.p.)

- 1–2 Months After
- 9–14 Months After
- 95-Percent Confidence Interval

Worker Age in Years

Panel B: Effects by Reason for Nonparticipation

Estimated Effect of Health-Related Absence (p.p.)

- 1–2 Months After
- 9–14 Months After
- 95-Percent Confidence Interval
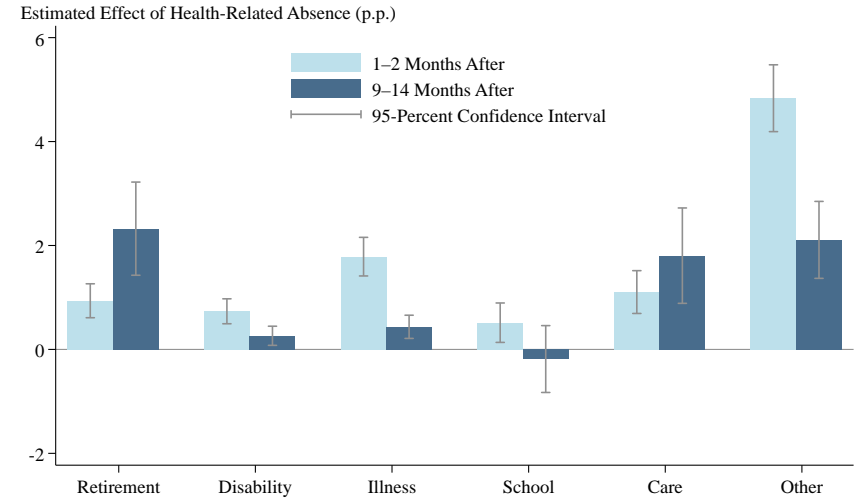
Panel C: Effects by Extensive-Margin Concept

Estimated Effect of Health-Related Absence (p.p.)

- 1–2 Months After
- 9–14 Months After
- 95-Percent Confidence Interval
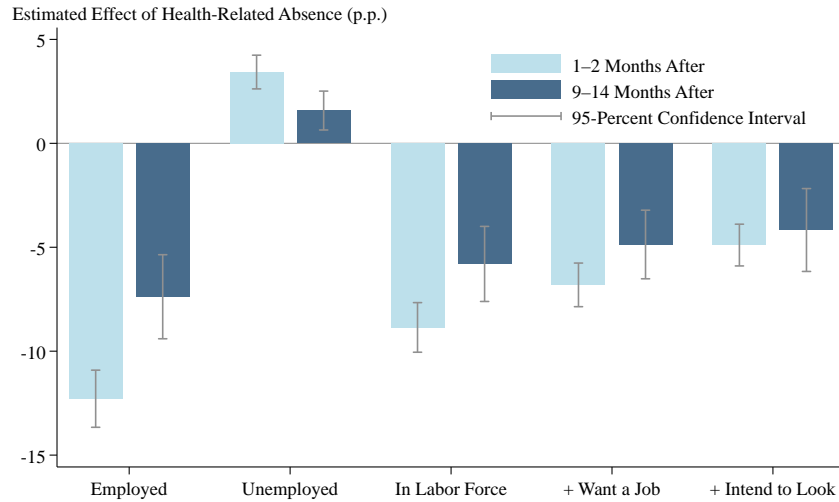
Panel D: Intensive-Margin Impacts

Estimated Effects of Health-Related Absence (p.p. or percent)

- 1–2 Months After
- 9–14 Months After
- 95-Percent Confidence Interval
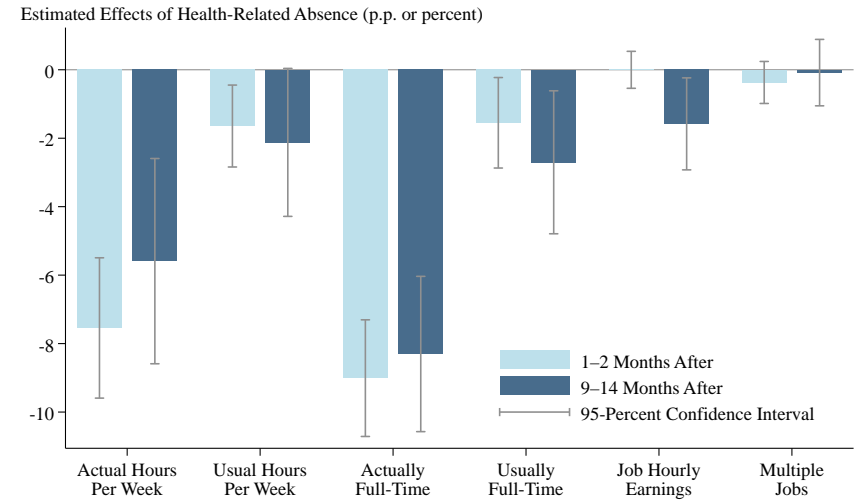
Notes: This figure displays effects of health-related absences at one and twelve months after the absence. All panels estimate the event-study specification (Equation 1) with our full set of controls. Confidence intervals reflect standard errors clustered by worker. See Appendix A for event-study figures and tests for heterogeneous effects of likely Covid-19 absences.

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3.4 Do the Effects of Health-Related Absences Represent Covid-19 Illnesses?

While many health-related absences during the pandemic are likely Covid-19 illnesses (see Section 2), many surely result from health problems other than Covid-19. We now assess whether the effects of Covid-19 illness are likely to resemble the effects of health-related absences on average. We find much support for this approximation.

Aggregate Evidence. The pandemic increase in the health-related absence rate, taken with the average effect of health-related absences on participation, implies an increase in total flows from health-related absences into nonparticipation. Such an increase should be visible in pure summary statistics and can thus validate the event-study estimates.

Panel B of Figure 2 shows actual and predicted flows from health-related absence to nonparticipation, expressed as a share of previously-employed workers, at one- and twelve-month horizons following the absence. For every 10,000 workers employed twelve months ago, there are about an additional five workers who had a health-related absence exactly one year ago and are out of the labor force on average after the pandemic. Combining the actual increase in health-related absences with our event-study estimates, we find that our estimates can accurately explain this increase in flows from health-related absence into nonparticipation. This suggests our event-study estimates are representative for excess health-related absences, not only the average absence.

Panel Variation in Illness Effects. We now turn to state-level variation in Covid-19 exposure to identify the specific effect of Covid-19 illness on participation. In particular, we estimate a variant of our event-study specification that allows for heterogeneous effects of absences according to some interaction variable $Z_{i,t}$:

$$\mathrm{LF}_{i,t+k} = \beta_k \mathrm{HRA}_{i,t} + \gamma_k \left(\mathrm{HRA}_{i,t} \times Z_{i,t}\right) + \mathbf{X}_{i,t}\mathbf{\Lambda}_k + \phi_{s,t,k} + u_{i,t+k}. \tag{2}$$

We consider two interaction variables: the state-level Covid-19 case rate one week after the CPS reference week and the state-level Covid-19 death rate two weeks after the reference week. Appendix Figure A8 shows that cases one week after the reference week, and deaths two weeks after, have maximal predictive power for absence rates relative to other time windows, which supports their use in testing for heterogeneous effects of Covid-19 illnesses. We demean both by the month-specific national average Covid-19 case and death rates, to fully separate this analysis from effect heterogeneity over time, which we analyze next.

We find in Table 1 that, at both the 1-month and 12-month post-absence horizons, health-related absence effects do not change significantly when state-level Covid case and death rates are elevated relative to the national average. When the state Covid-19 case rate is one standard deviation above the contemporaneous national mean, the effect of health-related absences on participation one

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Table 1: Health-Related Absences Most Likely to Be Covid-19 Illnesses Cause Labor Force Exit

	1 Month		12 Months	
	(1)	(2)	(3)	(4)
Health-Related Absence	-0.065***	-0.064***	-0.084***	-0.084***
	(0.005)	(0.006)	(0.014)	(0.014)
× Standardized Case Rate	0.001		0.005	
	(0.007)		(0.017)	
× Standardized Death Rate		0.000		0.006
		(0.007)		(0.017)
People	201,014	194,392	58,287	58,287
Illnesses	3,753	3,585	1,157	1,157

Notes: This table reports estimates of Equation 2, 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. Standard errors are clustered by person. * = $p < 0.10$, ** = $p < 0.05$, *** = $p < 0.01$.

month later is statistically unlikely to be more than 1.4 percentage points ($= 0.001 + 1.96 \times 0.007$) smaller in absolute magnitude. The difference is somewhat less precisely estimated at the twelve-month time horizon. Covid-19 absences appear about as harmful to participation as health-related absences on average.

The appendix contains two robustness checks to this conclusion. Appendix Figure A9 plots all event-study coefficient estimates on the interaction terms. We find little differential response of participation to health-related absences when these absences are relatively more or less likely to reflect Covid-19 illnesses. Appendix Table A3 further shows that using county-level, instead of state-level, case and death rates does not alter our conclusions.

Time Variation in Illness Effects. If the effects of Covid-19 illnesses on participation differ markedly from the average of other causes of health-related absences, then the average participation effect of health-related absences should change over time, as likely Covid-19 illnesses go from zero to a significant share of health-related absences. To fix ideas, let β_k^q, $\beta_k^{\text{Covid},q}$, and $\beta_k^{\text{NotCovid},q}$ denote the average, Covid-specific, and non-Covid-specific effect of a health-related absence on participation occurring in quarter q on participation k months later, respectively. We can express the average effect as a weighted average of the Covid- and non-Covid-specific components:

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

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

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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. An implication of Equation 3 is that

$$\beta_k^{\text{Covid},q} = 0 \iff \beta_k^q = (1 - s_q)\beta_k^{\text{NotCovid},q}.$$

That is, zero Covid-specific effects would imply total effects should move inversely with the Covid-19 share of absences.

We estimate a version of Equation 1 that allows for heterogeneous effects of health-related absences by quarter.[14] Appendix Figure A10 shows the results at 1-month and 12-month post-absence horizons. There is some indication that average effects of health-related absences have weakened since the start of the pandemic. This suggests that Covid-19 illnesses 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 thus far see no evident change over time in the longer-run participation effect of health-related absence.

4 The Labor Supply Lost to Covid-19 Illnesses

This section considers two measures of the economic cost of the pandemic's harm to health: (1) the aggregate impact of Covid-19 illnesses on U.S. labor force participation and (2) the 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 Illnesses 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 illnesses 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

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

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 illnesses have likely become, over the last two years, an important contributor to the net change in the participation rate. By June 2022, Covid-19 illnesses reduced the participation rate by 0.18 percentage points, with a range of 0.13 to 0.22 percentage points, as we show in Appendix Figure A21. 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 illnesses, 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.[15] By this reasoning, an additional 140,000 people would be in the labor force but for prior Covid-19 illness.

The second consideration is Covid-19 illnesses 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.[16]

Table 2: Average Earnings Losses of Covid-19 Absences

Margin	(1)	(2)	(3)	(4)	(5)	(6)
	Estimated Effect		Average Forgone Earnings (at $903/week)			
	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 cost of a Covid-19 work absence. 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.

4.2 Forgone Earnings from Covid-19 Absences

Table 2 shows the average cost of a Covid-19 absence in terms of forgone 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 3. We then monetize these responses using workers' average weekly earnings before health-related absences, $903. Column 3 displays forgone earnings during the absence. Column 4 then uses the estimates in Column 1 to compute total forgone earnings from one to three months after the absence for each margin of labor supply. Column 5 uses the estimates in Column 2 to compute total forgone earnings from four to fourteen months after the absence. Column 6 adds Columns 3–5 to compute total forgone earnings for each margin of labor supply.

We find an average cost per Covid-19 absence of about $9,000, which amounts to about 18 per-

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[15]This calculation imposes several further assumptions: that Covid-19 week-long illness 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.

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

cent of these workers' counterfactual earnings over the fourteen-month observation period. Over half of the earnings loss reflects a reduction in employment, with the 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 illnesses 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 illnesses 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 illnesses 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 earnings by about $9,000, or 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 illness has significant implications for individual well-being and aggregate labor supply.

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Appendices for Online Publication
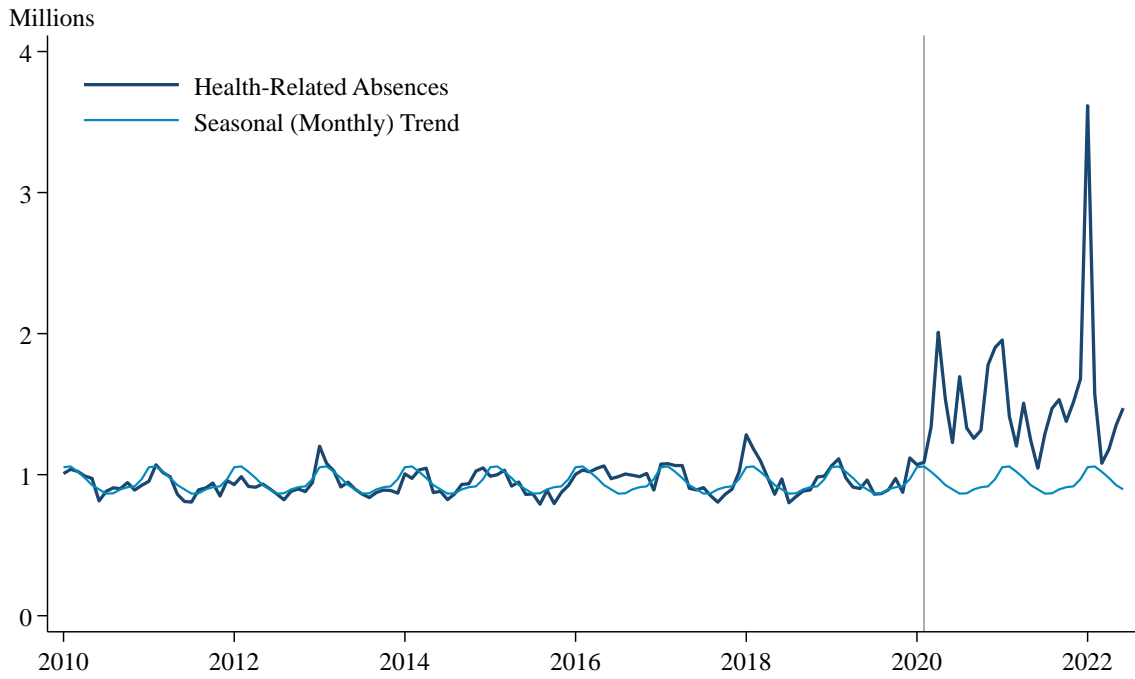
A Additional Tables and Figures

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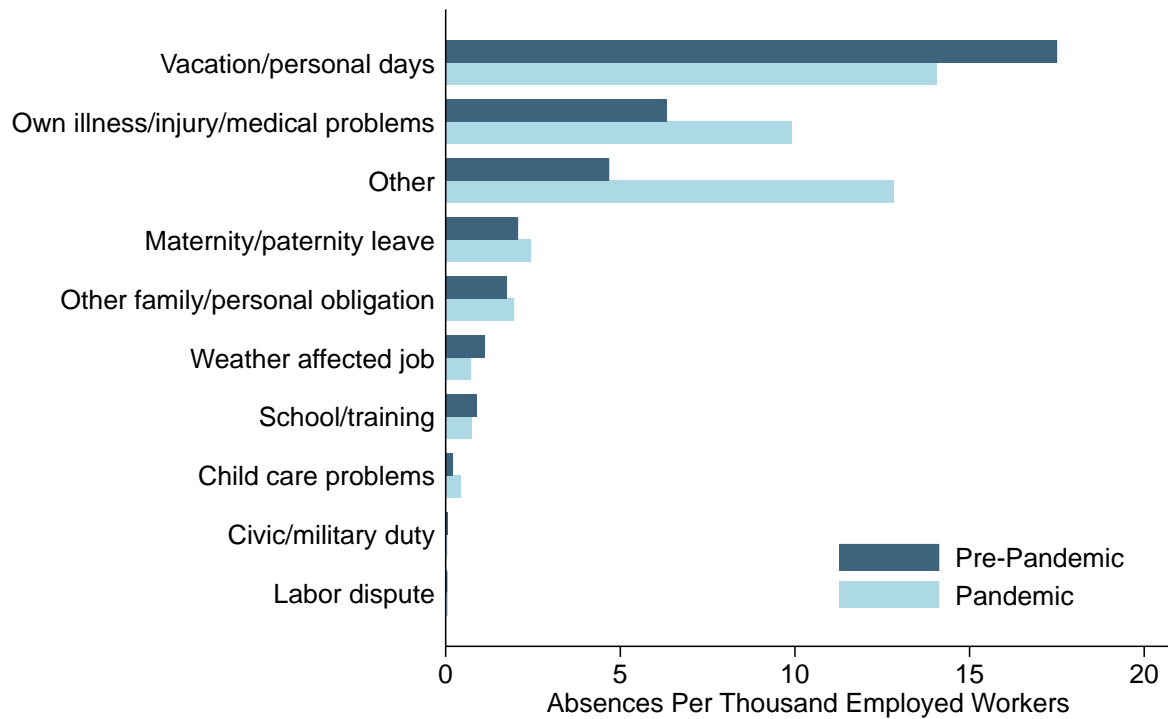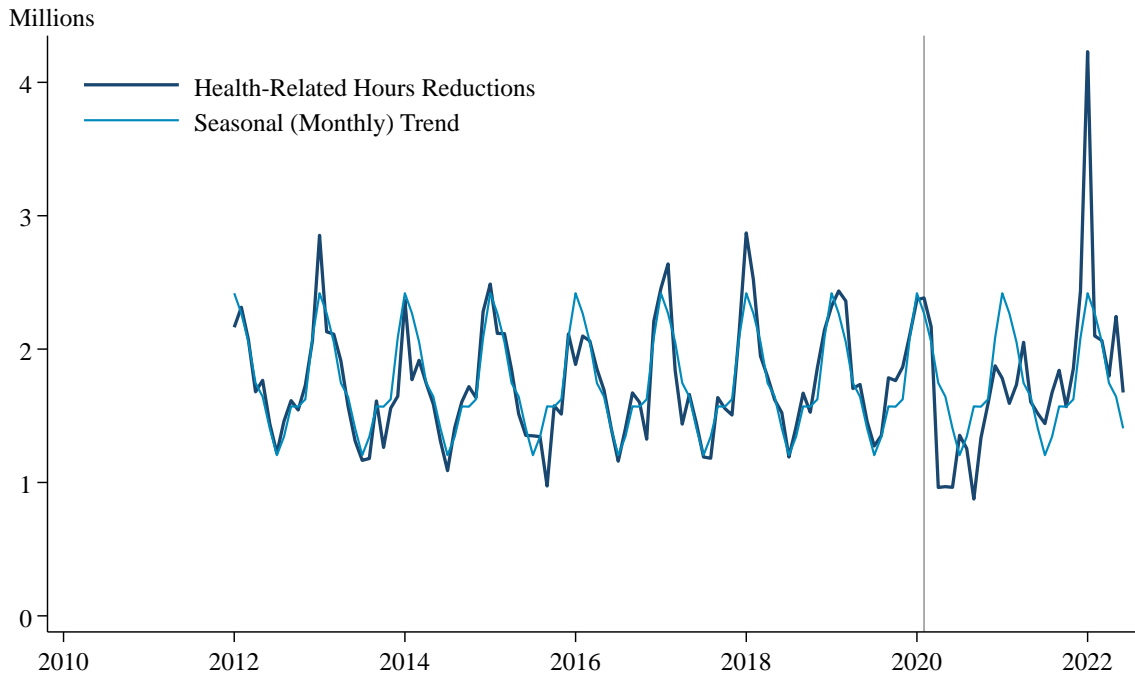

Notes: This figure displays the number of absences per thousand employed workers before and during the pandemic among workers in our sample.

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

Health-Related Absences Per Thousand Workers
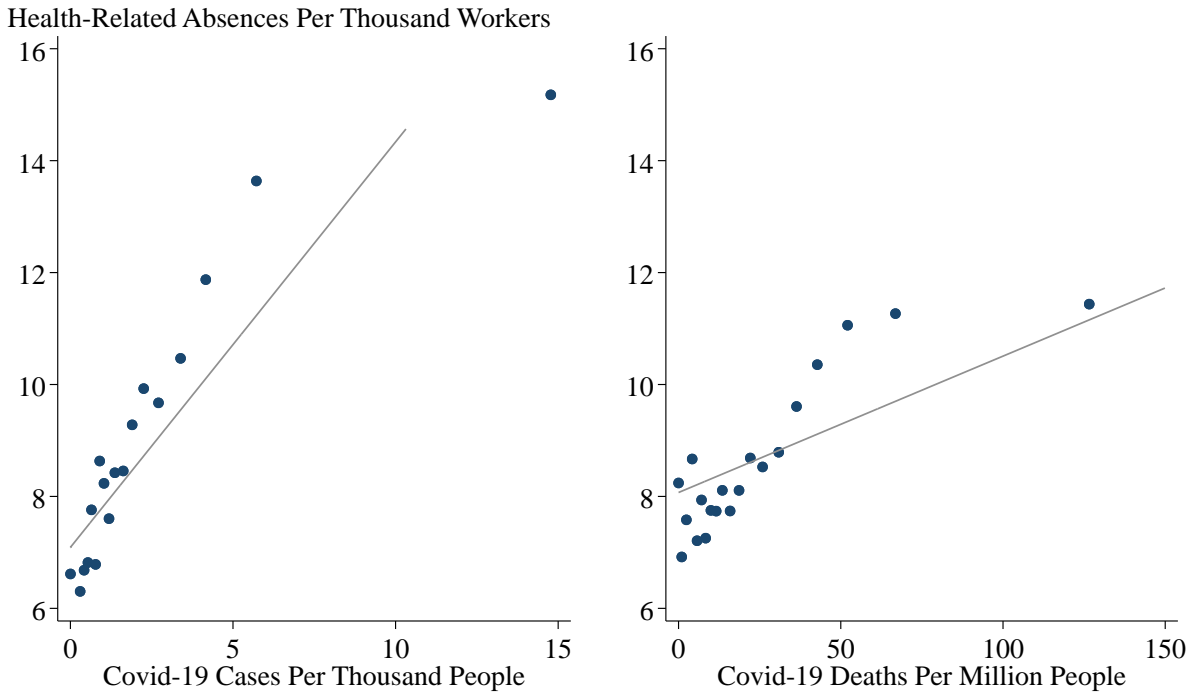
Notes: This figure displays binned scatterplots of the county-level relationships between Covid-19 case and death rates (per million people) versus the rate of health-related absences (per million workers). We use the semiparametric approach of Cattaneo et al. (2019) to remove county and month effects 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
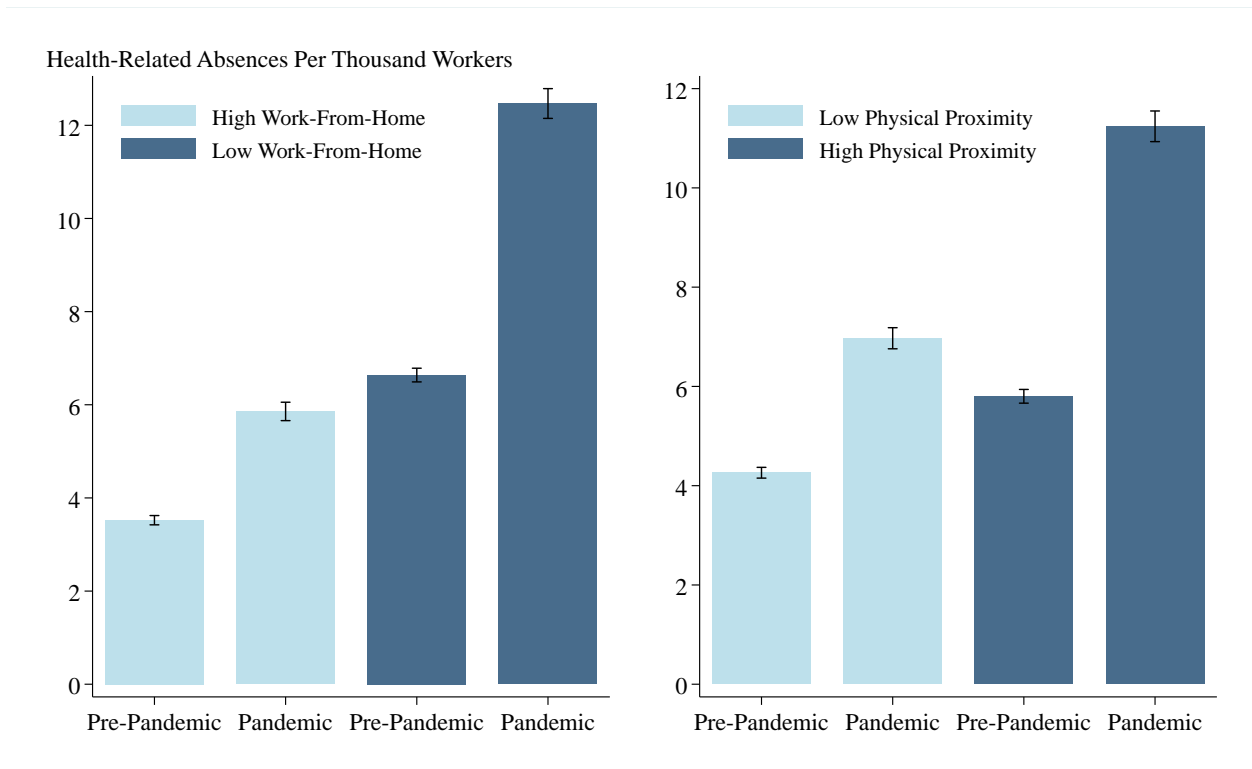

Notes: This figure graphs health-related absence rates (per thousand employed workers), comparing the pre-pandemic (Jan. 2015 – Feb. 2020) and pandemic (March 2020 – June 2022) periods, and splitting workers by their Covid-19 exposure risk. We use two occupation-level measures of exposure risk, both 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


Change in Health-Related Absences Per Thousand Workers
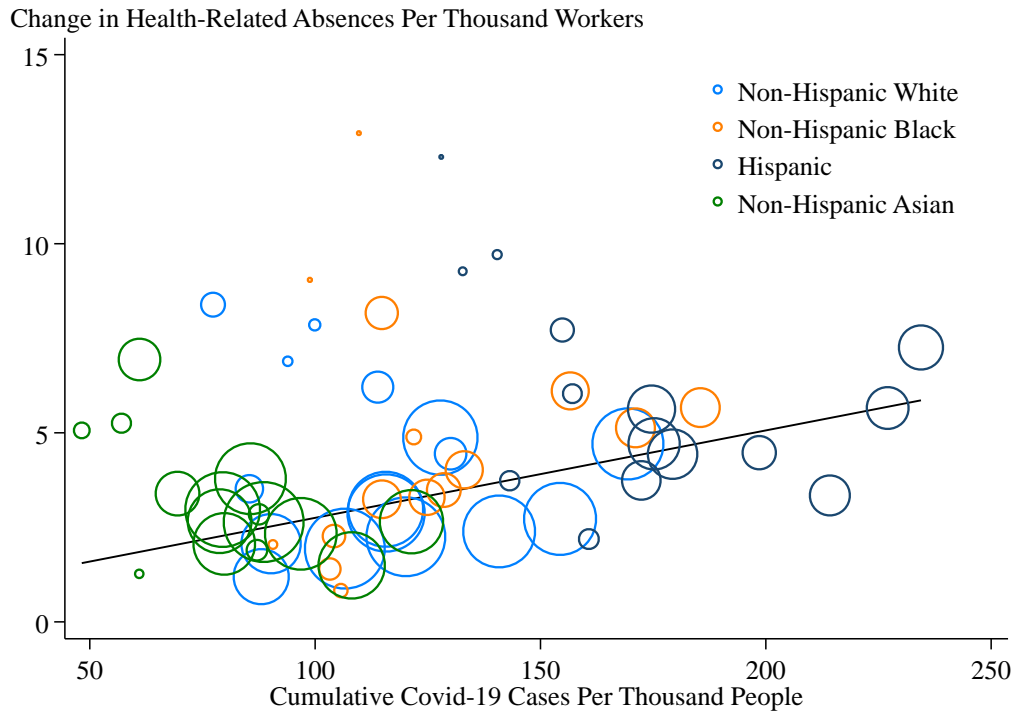
Notes: This figure displays the relationship between the pandemic change in the rate of health-related absences and the Covid-19 case rate across demographic groups. The change in the health-related absence rate is measured as the difference in the average number of absences per thousand employed workers 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: Effects of Health-Related Absences Exceed Potential Bias from Ill-Health
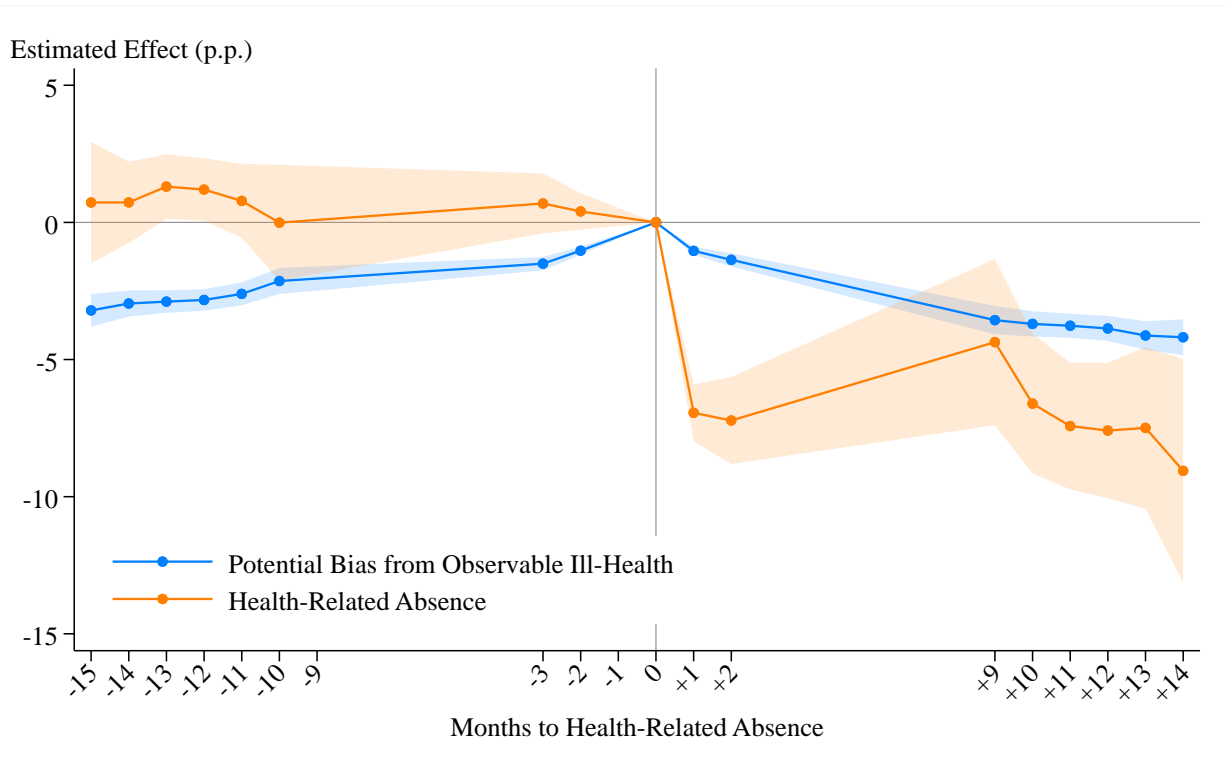
Notes: This figure plots, in blue, the estimated effect of being observably unhealthy on labor force participation *k* months later. For comparison, we plot in orange the baseline event study from Figure 2. The sample for the blue line is of all civilian adults. The color bands depict pointwise 95-percent confidence intervals. Standard errors are clustered at the worker level.

Figure A7: Differential Attrition in the Health-Related Absence Event Study
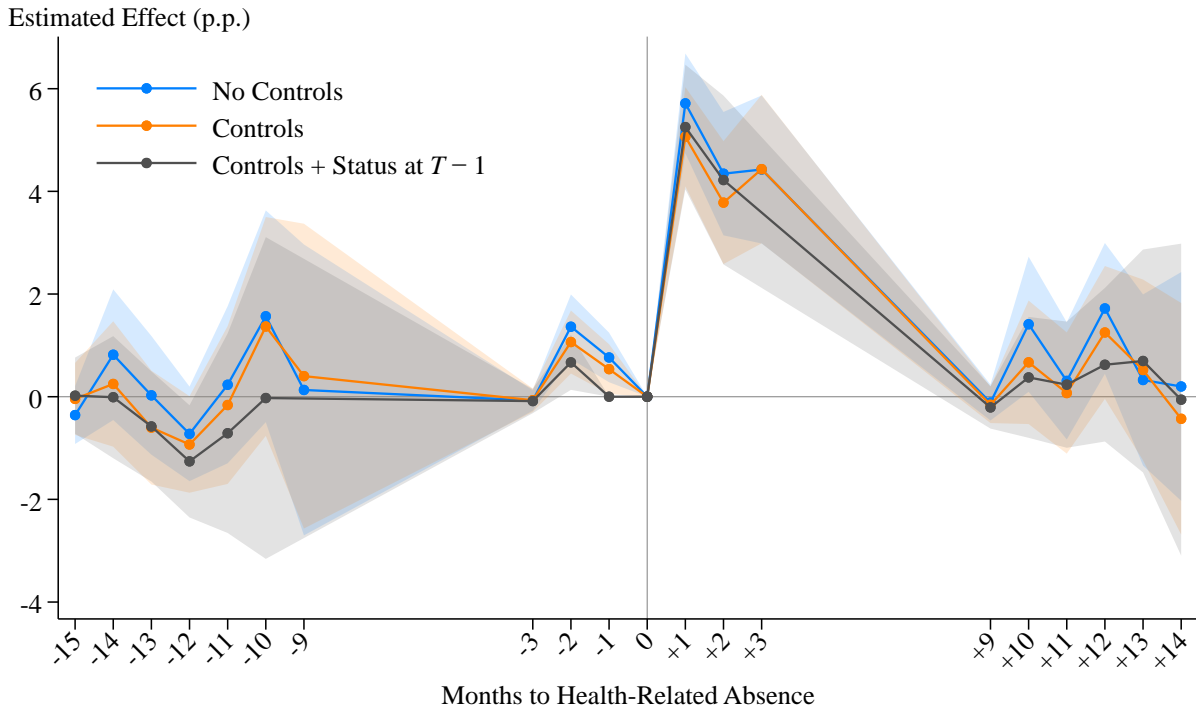
Notes: This figure plots estimates of coefficients β_k from a version of Equation 1 where the outcome is an indicator for survey nonresponse. The coefficients represent the effect of a health-related absence on the probability of nonresponse k months before or after the absence. Positive coefficients 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 A8: 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
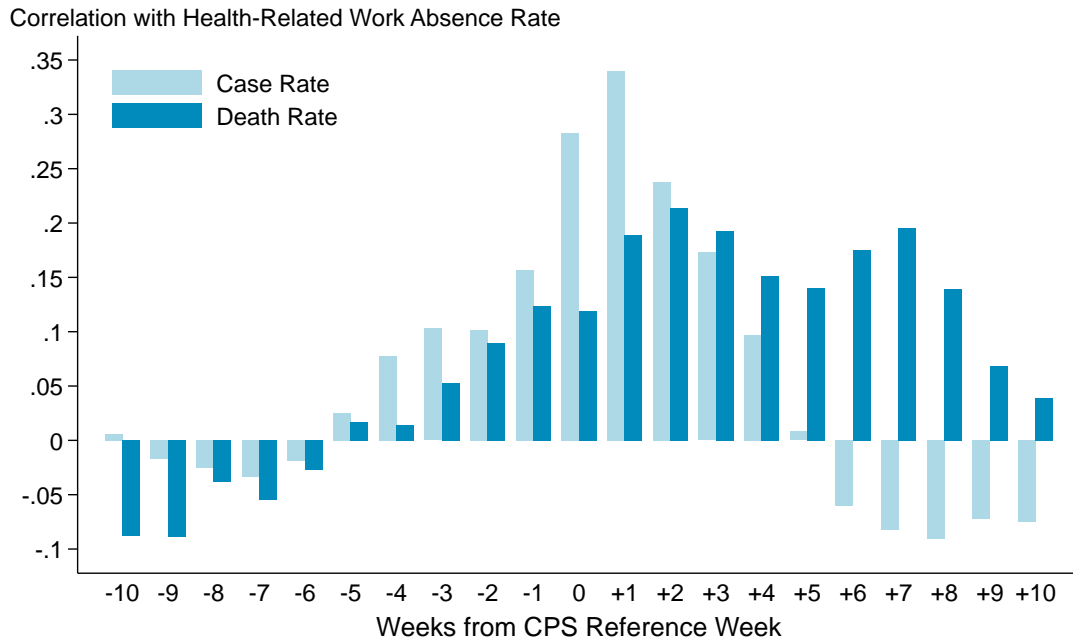
Panel B: Week-Specific Partial Correlation, after Removing State and Month Fixed Effects
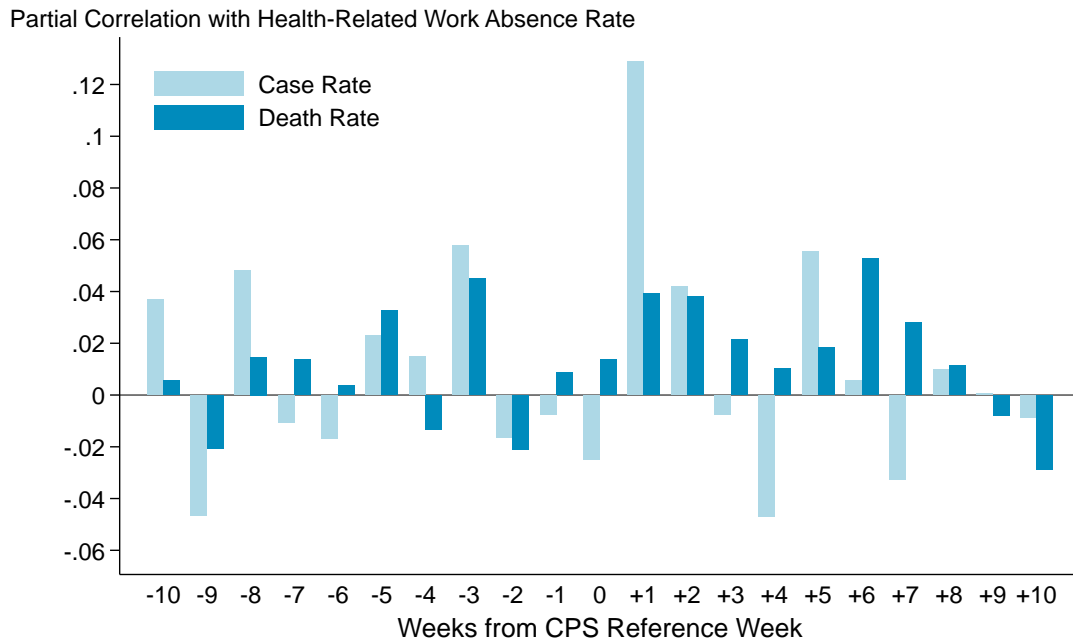
Notes: This figure examines the relationship between the state–month rate of health-related absences among the employed to the per-capita Covid-19 case and death rate in the same state in a given week, after removing state and month fixed effects. We count weeks relative to the month's CPS reference 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 A9: Interaction-Term Coefficients in the Health-Related Absence Event Study
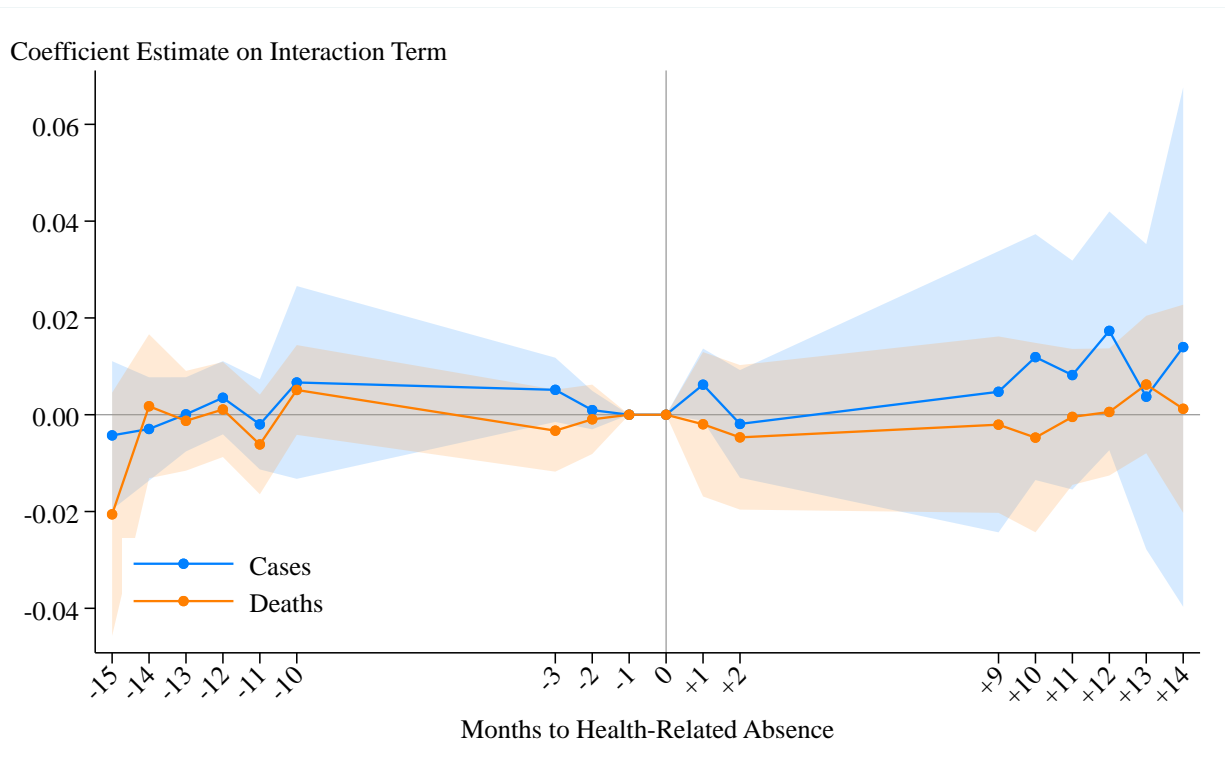
Notes: This figure displays event-study estimates of the coefficient on the interaction term in Equation 2. Gaps between months −10 and −3, and between +2 and +9, are due to sample rotation. The blue, orange, and gray lines plot respectively the estimates using the state–month Covid-19 case rate, the 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.
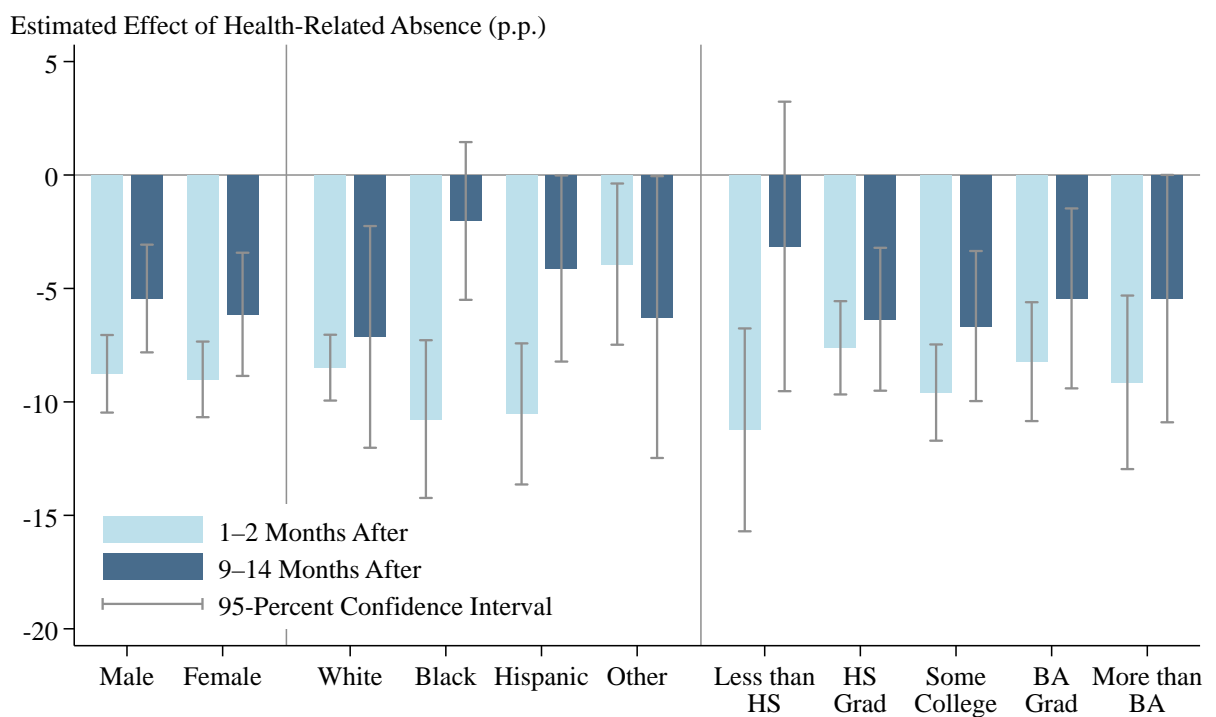
Figure A10: Effects of Health-Related Absences by Quarter-of-Absence Cohort
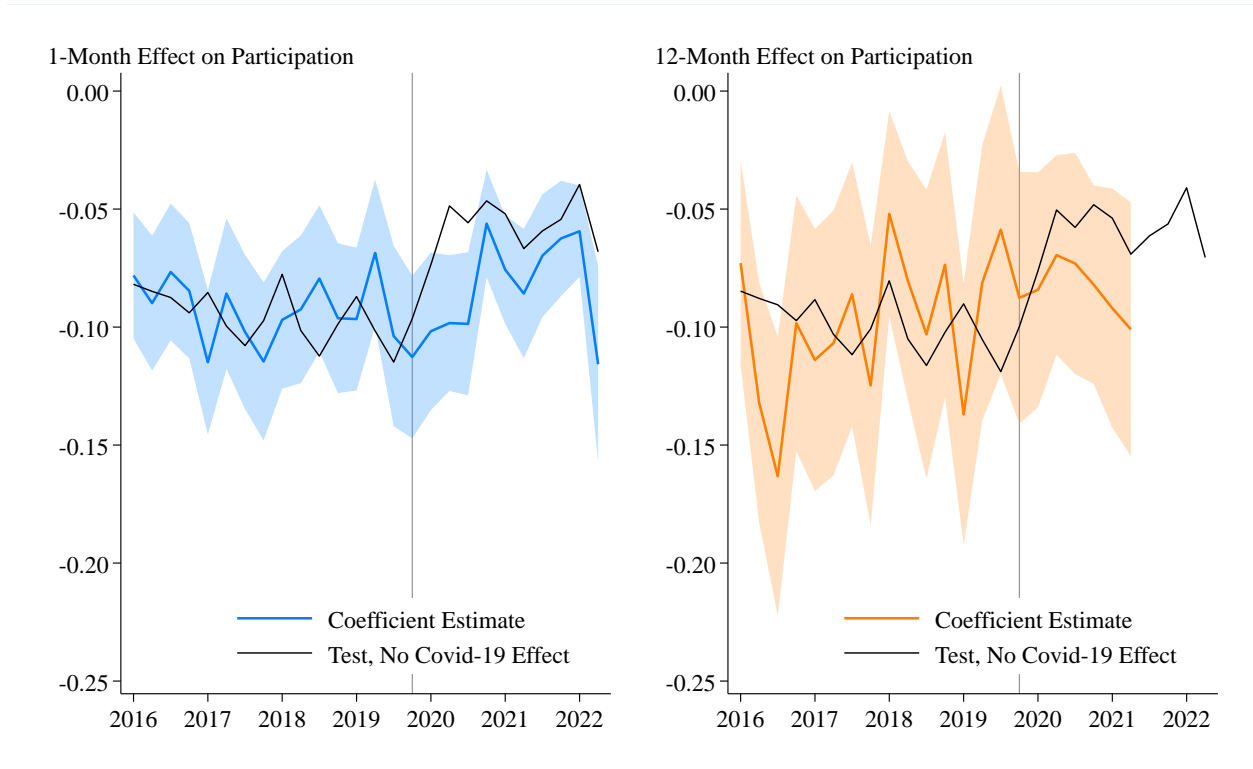
Notes: This figure plots the effects of health-related absences on labor force participation one and twelve months after the absence. We estimate effects for quarterly cohorts of absences. The color bands depict pointwise 95-percent confidence intervals. Standard errors are clustered at the worker level.

The black lines indicate the coefficient estimate that would be consistent with a zero effect of Covid-19 absences on participation and a stable participation effect of non-Covid-19 health-related absences after approximating approximating the quarter-specific Covid-19 share of health-related absences, s_q, by excess absences as a share of total health-related absences. This reasoning provides a null hypothesis of no Covid-specific effect. For the 1-month effect on participation, we can reject the null for several quarters, which suggests that it is unlikely that Covid-19 absences had no effect on participation one month after the absence. However, this approach does not allow us to rule out zero Covid-specific effects on participation after one year, as the black line is within the confidence interval.

Figure A11: Effects of Health-Related Absences by Sex, Race/Ethnicity, and Schooling

Estimated Effect of Health-Related Absence (p.p.)

Legend:
- 1–2 Months After
- 9–14 Months After
- 95-Percent Confidence Interval

X-axis categories: Male, Female, White, Black, Hispanic, Other, Less than HS, HS Grad, Some College, BA Grad, More than BA

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 A12: Participation Impacts of Health-Related Work Abences, by Age Group

Participation, Age 15–24 (p.p.)

Participation, Age 25–34 (p.p.)

Participation, Age 35–44 (p.p.)

Participation, Age 45–54 (p.p.)

Participation, Age 55–64 (p.p.)

Participation, Age 65-85 (p.p.)
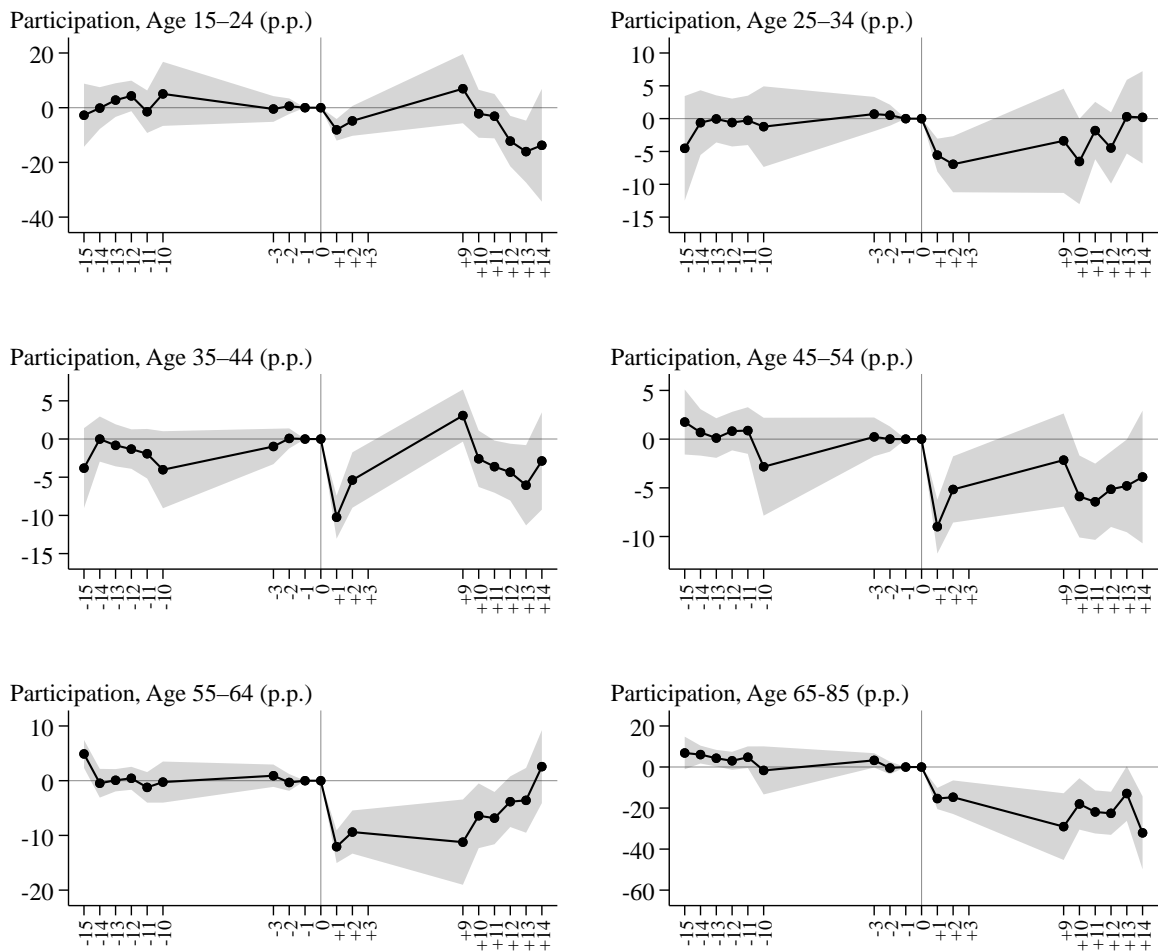
Notes: This figure plots estimates of coefficients β_k from Equation 1, limiting the sample by age in each panel. The coefficients represent the effect of a health-related absence on these outcomes k months before or after the absence. All specifications include our full set of controls. Gaps between 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 A13: Heterogeneous Effects of Health-Related Absences on Participation by Age Group
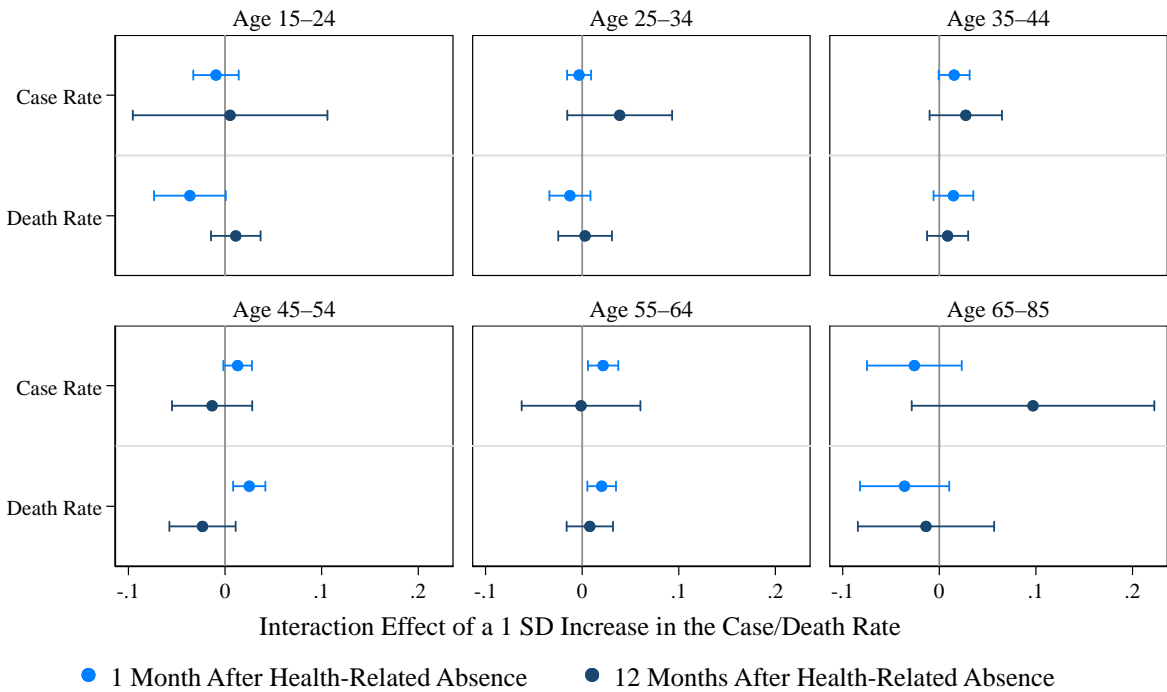
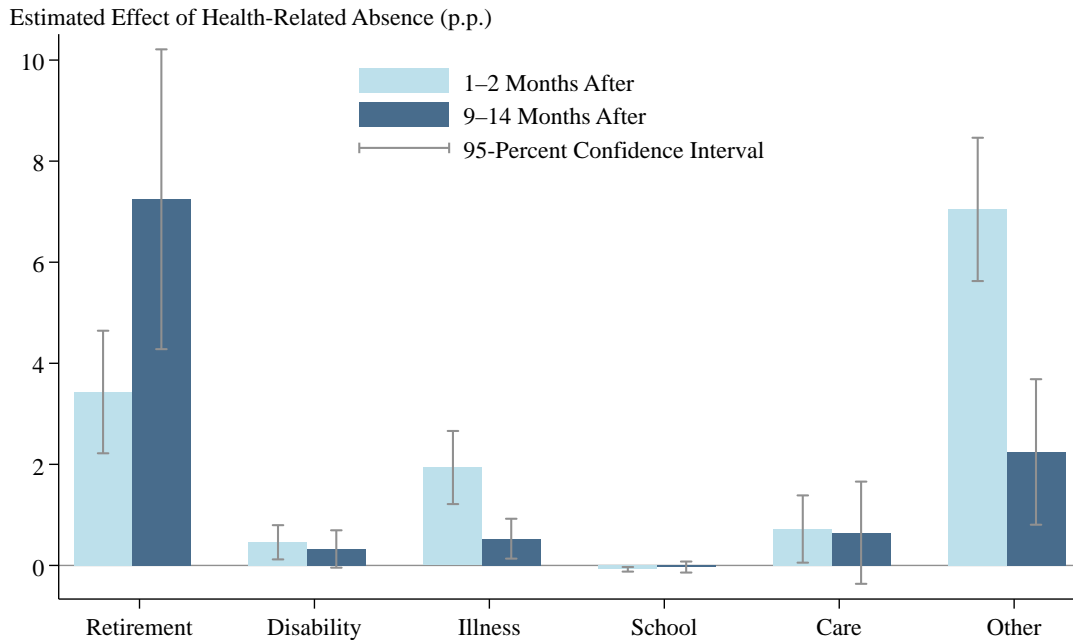*Notes:* This figure displays estimates of coefficients on the interaction term in the Equation 2, which captures the heterogeneous effects of health-related absences on participation when absences are relatively more or less likely to be due to Covid-19 illness. We display these interaction-term coefficients at one and twelve months after the health-related absence, dividing the worker sample into six age groups.

Figure A14: Nonparticipation Responses to Health-Related Absence, by Reason and Age

Panel A: Workers Age 15 to 54
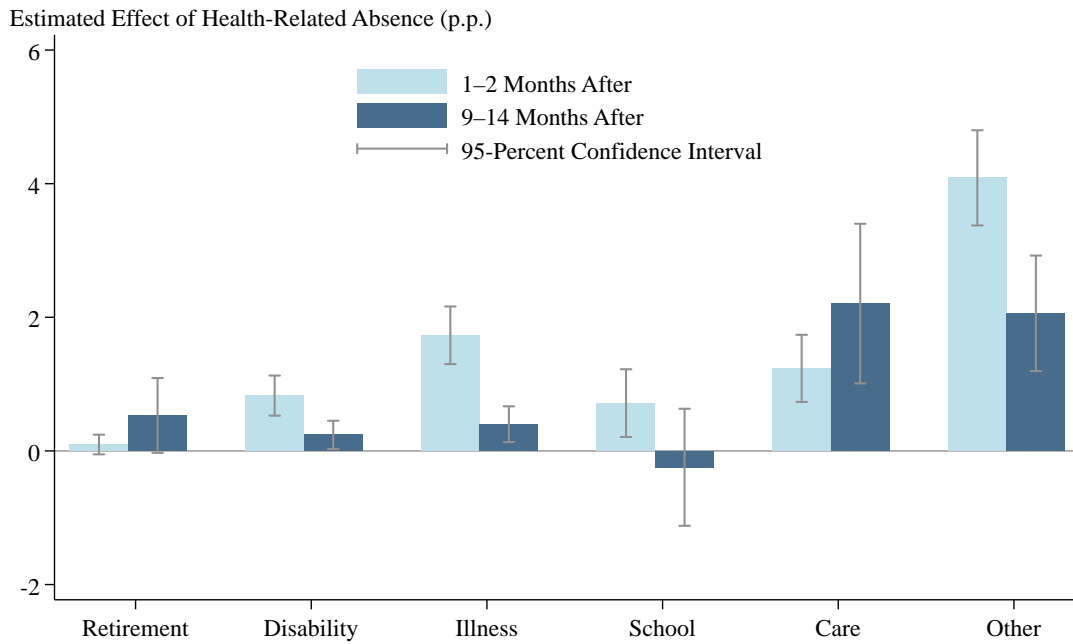
Panel B: Workers Age 55 and Over

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 A15: Event Studies for Nonparticipation by Reason
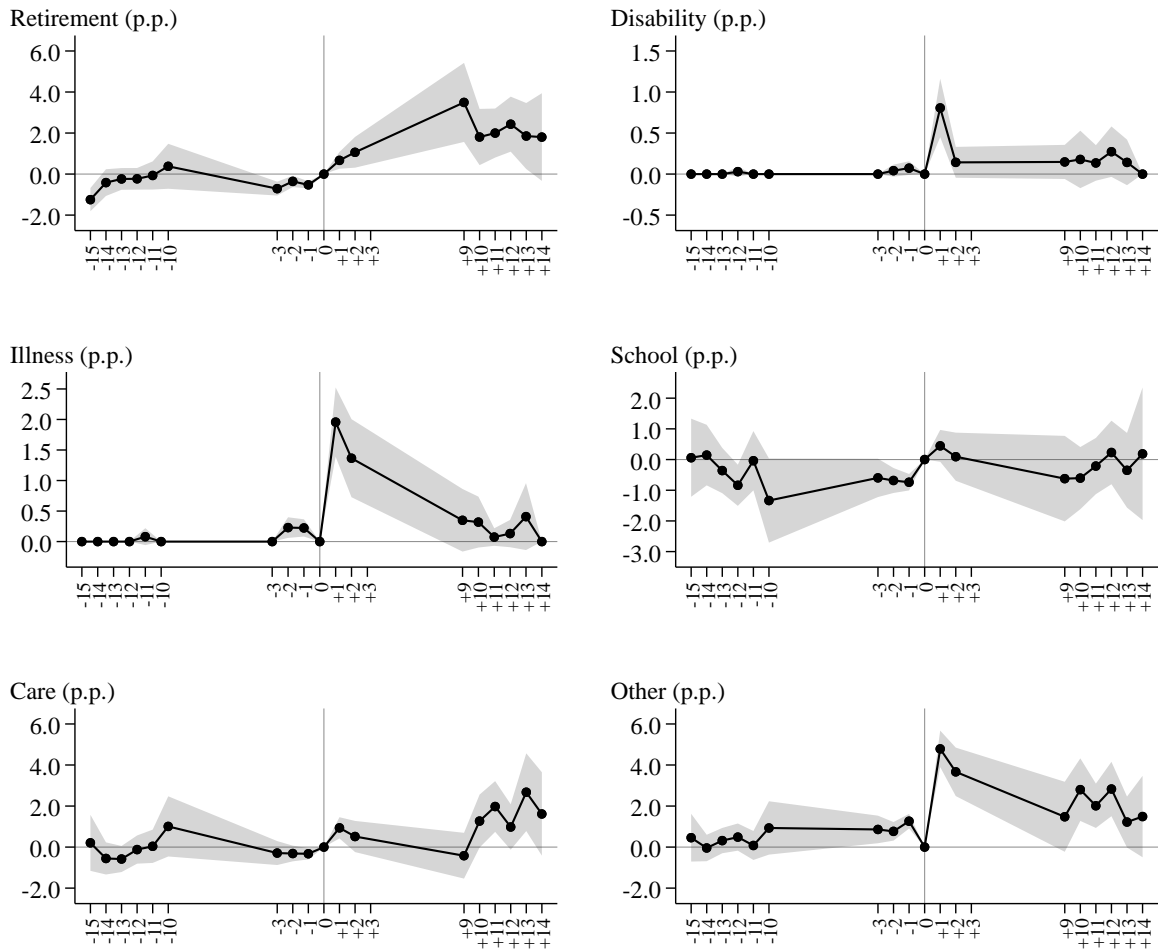
Notes: plots estimates of coefficients β_k from Equation 1, using different measures of intensive-margin labor supply as outcomes. The coefficients represent the effect of a health-related absence on these outcomes k months before or after the absence. All specifications include our full set of controls. 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 A16: Heterogeneous Effects of Health-Related Absences on Nonparticipation by Reason
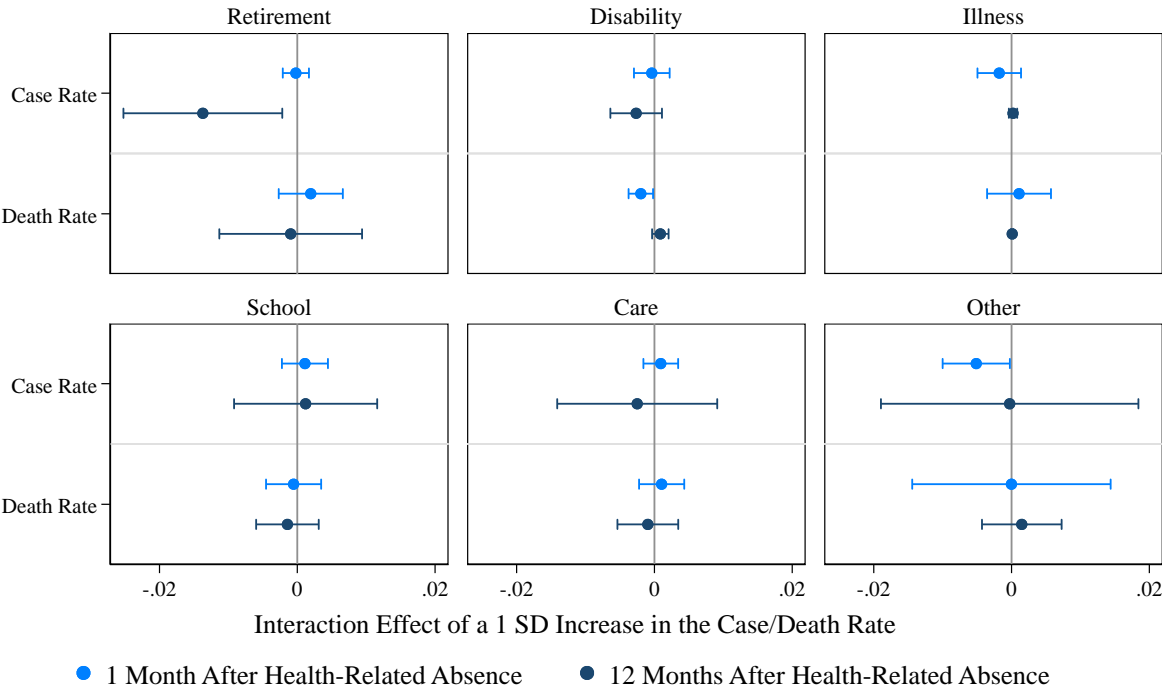
Notes: This figure displays estimates of coefficients on the interaction term in the Equation 2, which captures the heterogeneous effects of health-related absences on labor-market outcomes when absences are relatively more or less likely to be due to Covid-19 illness. We display these heterogeneous effects at one and twelve months after the health-related absence for each reason-specific rate of nonparticipation as an outcome.

Figure A17: Event Studies for Other Measures of Extensive-Margin Labor Supply
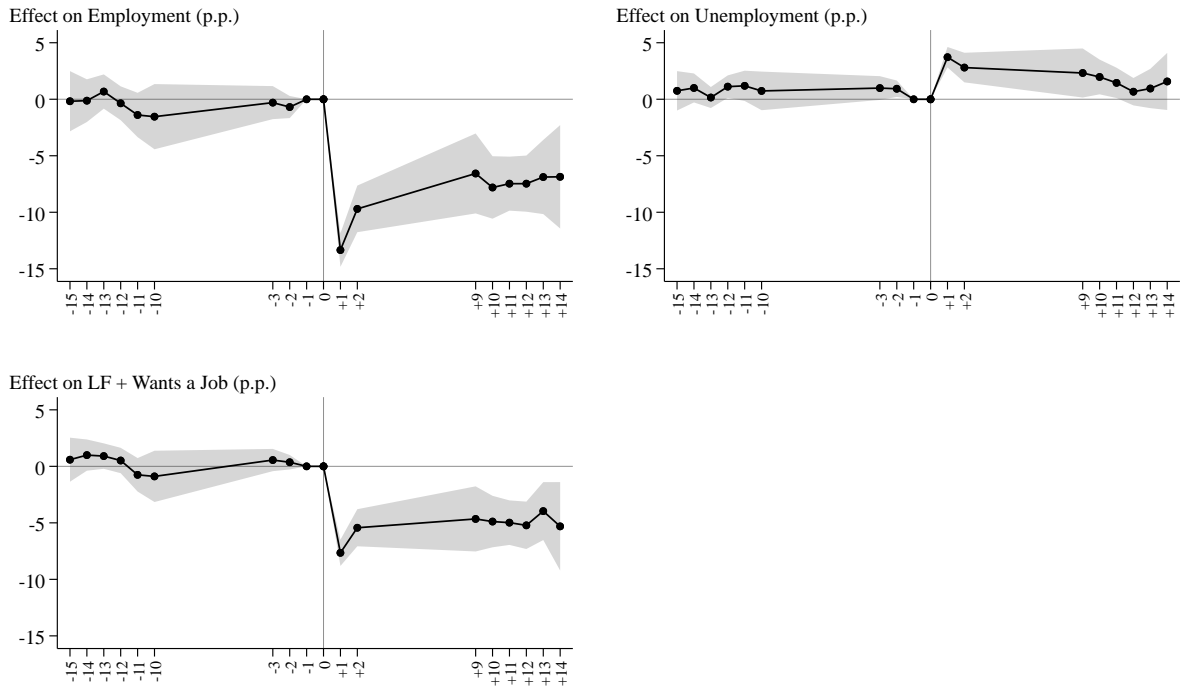

Notes: This figure plots estimates of coefficients β_k from Equation 1, using different measures of extensive-margin labor supply as outcomes. The coefficients represent the effect of a health-related absence on these outcomes k months before or after the absence. The CPS question regarding workers' 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 A18: Heterogeneous Effects of Health-Related Absences on Other Measures of Extensive-Margin Labor Supply
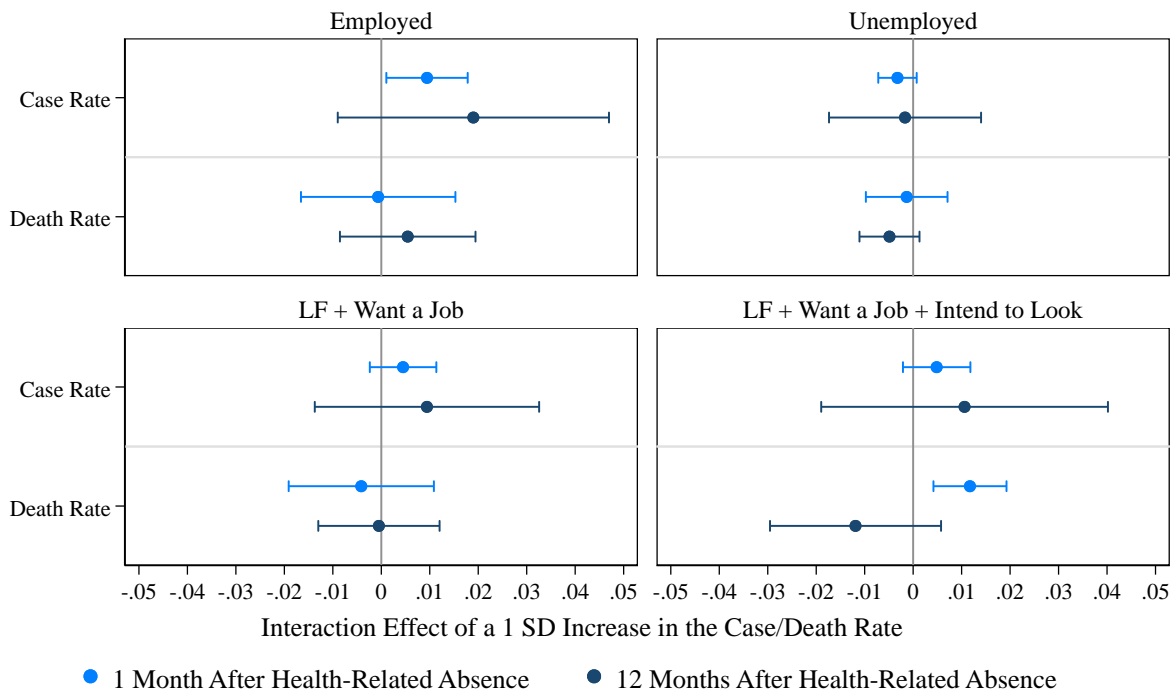
Notes: This figure displays estimates of coefficients on the interaction term in the Equation 2, which captures the heterogeneous effects of health-related absences on labor-market outcomes when absences are relatively more or less likely to be due to Covid-19 illness. We display these heterogeneous effects at one and twelve months after the health-related absence for 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).

Figure A19: Event Studies for Measures of Intensive-Margin Labor Supply

Effect on Actual Weekly Hours (p.p.)

Effect on Usual Hours (p.p.)

Effect on Actual Full-Time Status (p.p.)

Effect on Usual Full-Time Status (p.p.)

Effect on Job Average Hourly Earnings (p.p.)
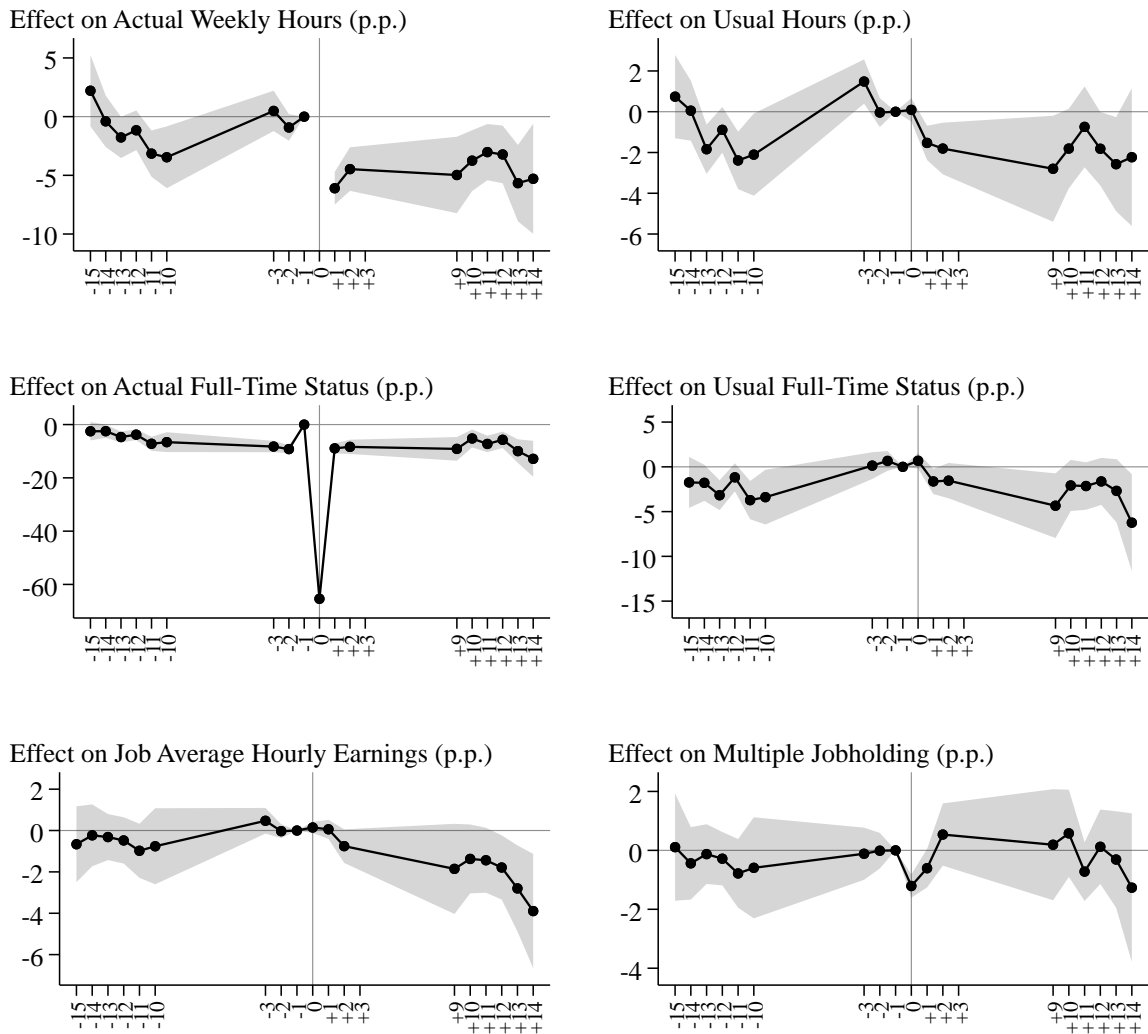
Effect on Multiple Jobholding (p.p.)

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 A20: Heterogeneous Effects of Health-Related Absences on Other Labor Market Outcomes
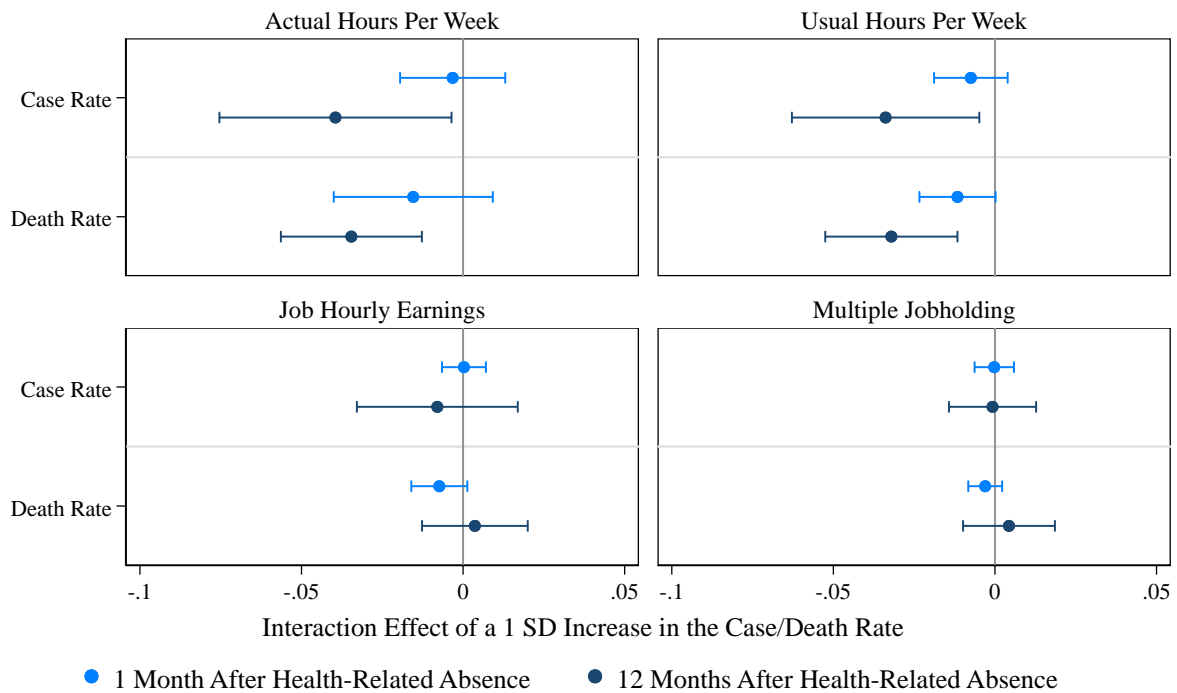
Notes: This figure displays estimates of coefficients on the interaction term in the Equation 2, which captures the heterogeneous effects of health-related absences on labor-market outcomes when absences are relatively more or less likely to be due to Covid-19 illness. We display these heterogeneous effects at one and twelve months after the health-related absence for four outcomes: 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. See Table 1 for results for labor force participation.

Figure A21: Estimated Effects of Covid-19 Illnesses on the U.S. Labor Force Participation Rate

Percent of Civilian Population, Age 16 and Older
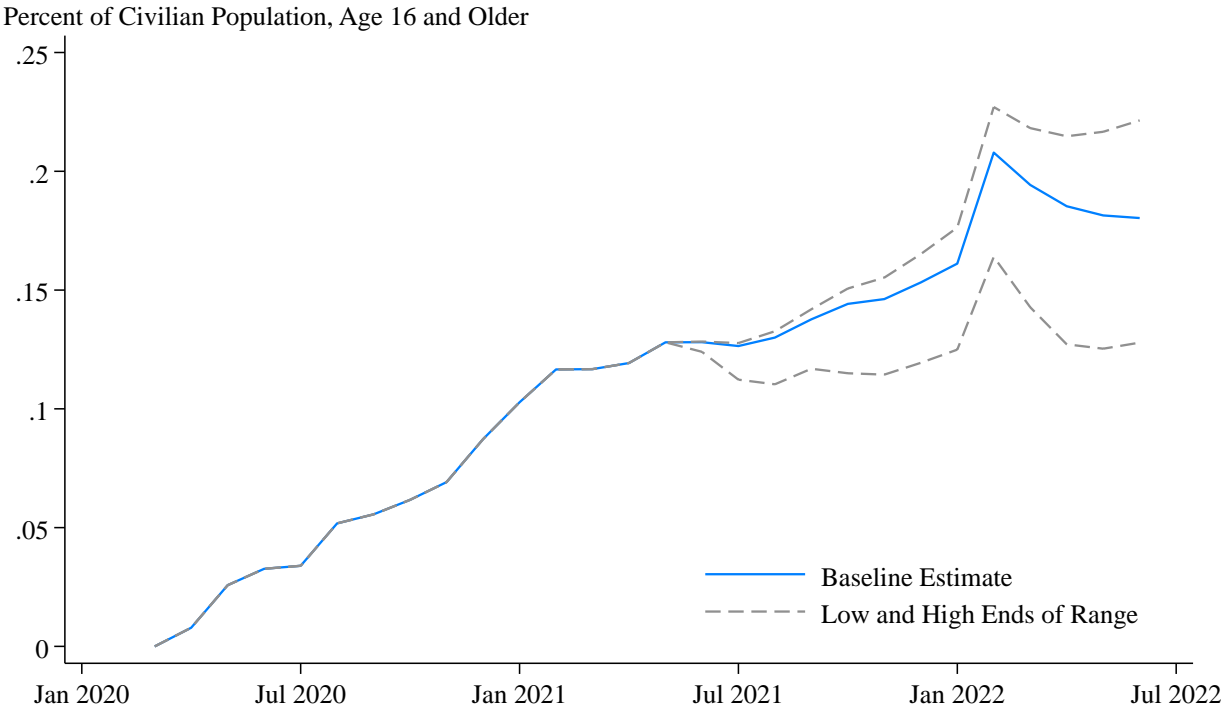
Notes: This figure presents our estimates of the aggregate labor force loss from Covid-19 illnesses, expressed as a share of the civilian adult population at each point in time. See the text for an explanation of our calculations for these aggregate losses.

Figure A22: Participation Effects of Health-Related Absences of Other Household Members

Estimated Effect on Labor Force Participation (p.p.)

Months to Health-Related Absence
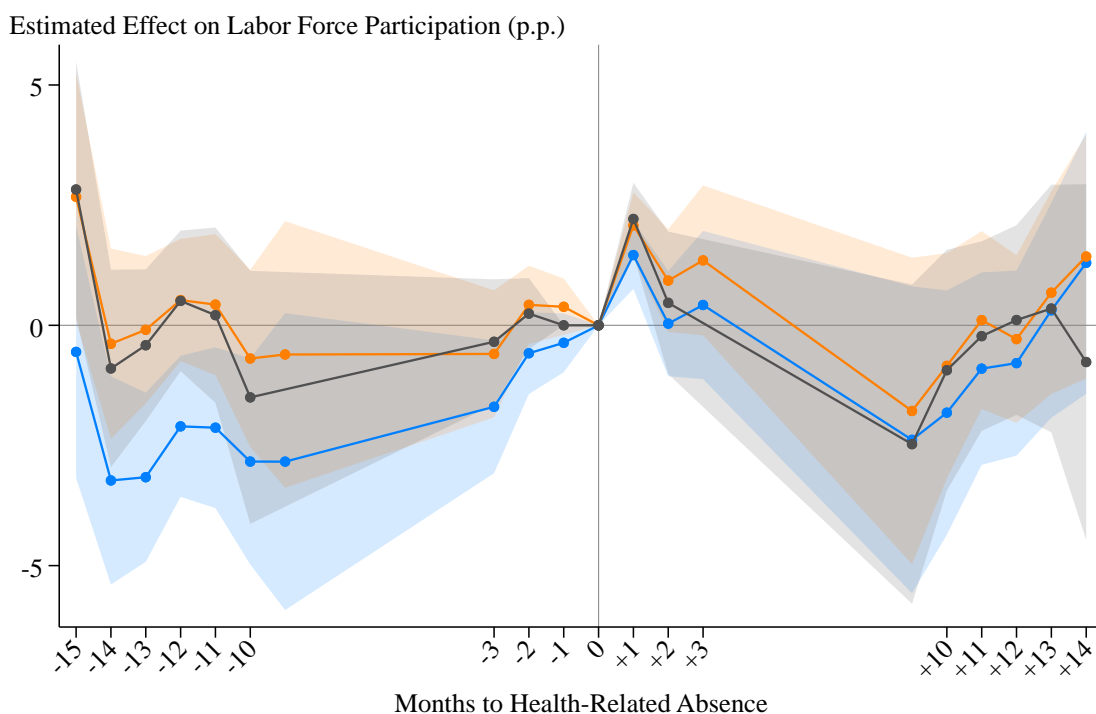
Notes: This figure plots coefficient estimates β_k from a modified version of Equation 1, expanded to include an indicator for whether other household members are currently absent for health reasons during the pandemic (February 2020 to June 2022). Gaps between months -10 and -3, and between $+2$ and $+9$ are due to sample rotation. The color bands depict pointwise 95-percent confidence intervals. Standard errors are clustered at the worker level.

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: Health-Related Absences Most Likely to Be Covid-19 Illnesses Cause Labor Force Exit: County-Level Analysis

	1 Month		12 Months	
	(1)	(2)	(3)	(4)
Health-Related Absence	-0.067***	-0.066***	-0.086***	-0.086***
	(0.005)	(0.005)	(0.014)	(0.014)
× Case Rate	0.006		0.001	
	(0.005)		(0.011)	
× Death Rate		-0.002		0.000
		(0.006)		(0.010)
People	200995	200995	58239	58239
Illnesses	3751	3751	1156	1156

Notes: This table reports estimates of Equation 2, 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 county–month reported Covid-19 case rate one week after the CPS reference week and the county–month Covid-19 death rate two weeks after the CPS reference week. Absence rates are computed for each sample. As not all counties are individually identified in the CPS, we treat the unidentified remainder of each state as if it were a single county. 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 2, 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. * = $p < 0.10$, ** = $p < 0.05$, *** = $p < 0.01$.

B   Supplementary Results

This appendix reports results of supplementary analyses. First, we consider unobservable differences in health between workers with and without health-related absences as a threat to inference in our event study. Second, we check for differential attrition in our event study. Third, we investigate whether health-related absences have spillover effects on other household members.

Unobservable Differences in Health. 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:

$$\text{LF}_{i,t+k} = \beta_{0,k}\text{HRA}_{i,t} + \beta_{1,k}(\text{HRA}_{i,t} \times \text{Unhealthy}_i) + \gamma_k\text{Unhealthy}_i + \mathbf{X}_{i,t}\mathbf{\Lambda}_k + \phi_{s,t,k} + u_{i,t+k}, \quad (4)$$

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 A6 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 A6 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 A6. In other words, our controls appear to achieve balance on unobservable health status before the absence.

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 A7, 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.[17] 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.

Within-Household Spillovers. Appendix Figure A22 estimates the effects of health-related absences of other household members on a worker's own labor force participation, controlling for their own health-related absence status. We find a small and short-lived increase in participation when other household members are absent for health reasons.

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.

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[17]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.