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EFFECTS OF REDUCED WORKPLACE PRESENCE ON COVID-19 DEATHS: AN INSTRUMENTAL-VARIABLES APPROACH.

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ABSTRACT

Numerous government policies have attempted to keep workers out of the workplace, on the assumption that this will lower transmission of COVID-19. We test that assumption, measuring the effect of aggregate workplace absence on US COVID deaths at the county level through August. Instrumenting with an index of how many local workers pre-pandemic can work from home, based on differences in county occupational mix, we find no effect of workplace absence until mid-May, then a sharply rising effect. By August, moving 10 percent of a county's workers from the workplace would lower deaths there by three quarters one month later.

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1 Introduction.

Workers working from home and businesses limiting the number of staff on site are among the most important tools that have been used to slow the spread of the the SARS-CoV-2 virus. This has taken the form both of individual actions, as workers choose to stay away from the workplace and businesses reduce on-site operations; and government mandates to close down particular types of business or lockdowns or shelter-in-place orders.

This study is an attempt to measure how effective staying away from work, in the aggregate, has been in reducing COVID-19 mortality in the US up to the end of August. This is difficult because of the endogeneity problem: A change in aggregate attendance at workplaces may have an effect on COVID-19 spread and subsequent mortality, implying causation from workplace presence to disease; but a rise in COVID-19 spread or a rise even in the *expectation* of a COVID-19 spike can also cause workers to stay home and cause a government shutdown order, implying causation in the opposite direction. In fact, we will show that over the period under study aggregate absence from work is positively correlated with COVID-19 deaths, implying that the second direction of causation is dominant. To overcome this problem, we propose an instrumental-variables (IV) strategy based on an index of ability to work from home developed by Dingel and Neiman (2020).

We find no effect of absence from the workplace up to mid-May and a very strong effect of reducing death rates after that.

2 Related work.

This work complements a number of other studies. Studies that look at the county-level determinants of COVID severity include Desmet and Wacziarg (2020), Wu et al. (2020), and McLaren (2020); less commonly, zip code-level data have been used (Benitez et al. (2020)). Welsch (2020) focuses on the effect of mask wearing, using an IV strategy based on voting patterns to correct for the endogeneity problem. Bick, Blandin, and Mertens (2020) use special survey data to document workers' switching to working at home during the pandemic. Marinoccio et al. (2020) review research on the role of workplace transmission. Spiegel and Tookes (2020) compile county-level data on a wide range of government restrictions to curb COVID, and find a strong effect of stay-at-home orders and certain business closings such as bars and restaurants. They do not use measures of the extent to which workers actually stay away from their workplace, and do not use IV's to deal with the endogeneity of these policies. Ilin et al. (2020) use intra-national and national data from several countries including the US to measure the effect of government mandates on workplace absence (and other aspects of mobility) and the effect of workplace absence on COVID spread. They do not acknowledge the endogeneity problem. Fang, Wang, and Yang (forthcoming) explore the quasi-experimental setup of the lockdown in Wuhan and use the intercity migration data to quantify the lockdown's effect on the nation-wide spread of COVID-19 in China. They control for the endogeneity problem by using the previous-year data of Wuhan and a group of seven comparable cities. They do not measure workplace absence or examine its effect, and they focused only on the confirmed cases instead of deaths. In a closely related paper, Yilmazkuday (2020) studies the effect of workplace absence and other Google mobility measures on COVID case and fatality rates across countries, at the national level. The study is richer than ours in its variety of mobility types and its international coverage, but does not address endogeneity or allow for time-varying effects.

Our study focuses on quantifying the effect of a given degree of workplace absence on mortality, and how that has changed over time. Previous studies either studied changes in worker mobility without the consequences for pandemic spread; or focused on the effect of policy changes, rather than the effect of changes in actual presence at the workplace; or examined the effect of changes in presence at the workplace on the pandemic spread, but without using IV's to correct for endogeneity. This study does so. The only related study we have found that uses an IV approach is Welsch (2020), and that study is on mask use, not presence at the workplace. In addition, this study evaluates changes in the effect over time, which is not done in the other studies.

3 Data.

Data are summarized in Table 1. All data in this note are at the county level, from 50 states. The ideal would be individual data but that is not publicly available, so the present goal is to work with the finest possible geographic units. After dropping counties with missing data, the data include 2,830 counties and cover a total population of 321,170,881.[1] Counties are extremely heterogeneous in size, so all descriptive statistics reported in Table 1 and all regressions are weighted by population.

3.1 Mortality Data.

This paper focuses on mortality data, which is less likely than case rates to be distorted by local variations in testing policy. Johns Hopkins University Coronavirus Resource center publishes the daily time series of cumulative COVID-19 deaths in each county.[2] We aggregate the mortality cases by week; the first confirmed COVID-19 death in the US occurred on 2/28, so we obtain 27 weekly mortality rates by taking the first difference of the cumulative death tolls on the 28 consecutive Sundays from 2/23 to 8/30. Each week therefore starts on Monday morning and ends on Sunday midnight. All mortality figures are then scaled as deaths per million residents to isolate population influences on the mortality

[1] 309 counties are dropped because they do not have the Google mobility data, and another 3 counties are dropped due to missing population data.

[2] The time series data, along with other COVID-19 data, is available at the GitHub Data Repository: https://github.com/CSSEGISandData/COVID-19. The data used December 2018 county definitions, before the division of Valdez-Cordova Census Area in Alaska on January 2, 2019.

counts.

Figure 1a shows that population-weighted weekly average mortality increased significantly after week 4, and peaked around week 8 (mid-April) with about 42 deaths per million. It then decreased gradually to a temporary low-point of about 15 per million by week 18, while increasing slightly to over 20 per million in more recent data. Population-weighted median weekly average mortality exhibit similar trends, but it remained relatively constant, compared to the mean, after week 8, fluctuating around 10 deaths per million. The large difference in mean and median suggests that the distribution of average mortality is highly skewed to the right, especially from week 6 to 14 (April and May). Indeed, the grey-shaded region of interquartile values (all values between the 25^{th} to 75^{th} percentile) suggest that from week 6 to week 9, the population-weighted mean was higher than the 75^{th} percentile in the weighted data. The large width of the interquartile band over time also shows that there are significant variations in mortality rates across the country each week. On average, the population-weighted standard deviation of county-level mortality rates for each week is about 36.6977.

3.2 Mobility Data.

Google has compiled data on its users who allow their accounts to track their location, and has now made this data available to the public during the pandemic.[3] We use the average percentage change in the number of Google users who were physically at their workplace in each week, compared to a baseline measured over January 3 to February 6. This is our main variable of interest, denoted below as $atwork_{ct}$ for county c in week t. We define the weeks using the same method as we do with the mortality data: the weeks start on Monday and end on Sunday.

Figure 1b shows the population-weighted mean and median of the percentage change in the number of people staying at their workplaces compared to the base period, with the interquartile values in grey. The median and mean are almost the same throughout the studied period. Their figures drop sharply after the beginning of our period of interest, to a low point of -50% in week 7 (early April)—about 50% fewer people were physically at their workplace by that time compared to the base period. The rate gradually recovered to about -30% by week 17 (mid-June) and remained relatively steady afterward. The width of the interquartile band indicates that the variation of workplace absence across counties in each week increased initially but remained quite constant after week 4. On average, the population-weighted standard deviation of county-level workplace absence rates for each week is about 7.4699.

3.3 Demographic and Economic Controls.

We use the data from McLaren (2020) for additional controls. These demographic and economic variables come from the American Community Survey (ACS) of the US Census Bureau. The most recent

[3] The data is at https://www.google.com/covid19/mobility/.

round of the ACS with complete publicly-available information required for this exercise is the 2018 survey; the five-year data are used here, meaning that all data are an average from the five years of the ACS ending in 2018.

(i) Racial and ethnic variables. For each county we have the fraction of population accounted for by each of the four minority groups, African-American, Hispanic/Latino, Asian, and First Nations. Table 1 shows that the African-American and Hispanic/Latino populations shares are much larger than the other two, and there is substantial variation in all four across counties.

(ii) Income, Education, and Health Insurance. Controls include: median household income in each county; the fraction of adults 25 years of age or older without high-school diploma; with high-school diploma but no further education; with some college but no four-year degree; and with a four-year college degree; and the poverty rate in each county, summarized in rows 5-11 of Table 1. The ACS also asks respondents if they have any healthcare insurance, and the fraction who respond in the negative for each county is included in the data here; the population weighted average nationwide stands at 9.36%.

(iii) Commuting. The ACS reports detailed information on how workers get to work, from which we extract the fraction of workers who share a vehicle to get to work, and the fraction who use public transit (not including taxis) to do so. Both means of transport involve being inside a vehicle with other people for the length of the ride and could therefore potentially help spread the virus. The public-transit fraction is dramatically skewed. Even the 75th percentile county (weighted, as always, by population) has only a 3.93% share who use public transit, significantly below the mean of 4.81%. The highest share is held by Kings County, NY, at 61.36%.

3.4 Instrumental variable: The ability to work from home.

A crucial variable is our instrumental variable to correct for the endogeneity of presence at the workplace. Dingel and Neiman (2020) have attempted to quantify the likelihood that a given US worker is able to work from home, using the O*NET surveys conducted by the US Department of Labor, which codify differences across US occupations. They develop a systematic criterion for coding each occupation as either possible or impossible to do from home, based on the O*NET survey responses. For example, if a majority of respondents within an occupation report that their job requires the use of heavy machinery, the occupation is coded as impossible to do from home. These values are then aggregated up to the 22 broad Bureau of Labor Statistics occupational categories, which allows us to average this index across occupations by county, using the 2018 five-year occupational shares. The last row of Table 1 shows that by this measure about 39% of workers can work from home, and – importantly – this figure varies substantially across counties.

4 Empirical method.

In order to have the cleanest identification possible, to the extent possible we use controls that are all either predetermined or interactions of predetermined variables with time dummies in order to isolate the endogeneity problem to our variable of interest, $atwork_{ct}$. This imposes some constraints on our method, to which we will return shortly.

The most straightforward way of measuring the effect of workplace absence would be a linear regression:

$$mortpm_{ct} = \boldsymbol{\beta}\boldsymbol{X}_{ct} + \gamma atwork_{ct-4} + \epsilon_{ct}, \tag{1}$$

where $mortpm_{ct}$ is the number of deaths per million in county c in week t, $\boldsymbol{X}_{ct}$ is a vector of controls including week dummies and interactions between week dummies and other controls; $atwork_{ct}$ is the percent of workers in county c who were in their workplaces in week t relative to the benchmark weeks in January and February; and ϵ_{ct} is an error term with mean zero. The variable of interest, $atwork_{ct}$, is lagged four weeks to account for lags in transmission and the progress of the disease; trial and error indicated that this lag length seemed to have the strongest explanatory power in our data.[4] Our first estimation uses OLS to estimate (1), but concerns about the endogeneity of $atwork_{ct}$ lead us to use an IV, which takes the form of Two-Stage Least Squares, in which the first stage has all of the regressors in (1) except for $atwork_{ct-4}$, and includes in its place the Dingel-Neiman index of the fraction of workers in each county who can work at home, on its own and interacted with week dummies. These should satisfy the orthogonality condition because they are all predetermined prior to the pandemic; the test of overidentifying restrictions can then be interpreted as a test of the validity of the overall specification of the model.

There are other candidates for IV's. Barrios et al. (2020) show that pre-pandemic voting turnout and other measures of 'social capital' have predictive power for social distancing, and Welsch (2020) shows that voting patterns in the 2016 presidential elections have predictive power for mask wearing. Perhaps these variables would be correlated with workplace absence as well. However, we do not want to conflate the effects of workplace absence with these other behaviors, and the Dingel-Neiman index has the advantage that it is uniquely relevant to presence at the workplace. One could use data on state-level mandates such as shelter-in-place rules, as codified in Ilin et al. (2020) and in Spiegel and Tookes (2020), for example. However, these are very likely to respond themselves to outbreaks and forecasts of imminent outbreaks, and thus be endogenous themselves. It turns out that our IV strategy has all of the power we need, generating a very strong first-stage regression as will be seen below, so we do not need to expand the IV list.

A major problem with the linear specifications is that they imply the same effect of a given change

[4] It is also consistent with Spiegel and Tookes (2020), who find that shelter-in-place orders have effects on the growth of COVID deaths 4-6 weeks after they are implemented.

in workplace absence for all counties and dates. Taken literally, this is absurd, since in a county that currently has no outbreak, the effect of an increase in workplace absence must logically be zero. The standard approach to estimation with count data, Poisson Pseudo-Maximum Likelihood (PPML), addresses this by allowing for effects to scale with the size of the pandemic (see Santos Silva and Tenreyro (2006) and Cameron and Trivedi (2013) for detailed accounts). We estimate the model using the Generalized Method of Moments (GMM). To implement PPML, we assume that the death rate is governed by:

$$mortpm_{ct} = \exp(\beta \boldsymbol{X}_{ct} + \gamma atwork_{ct-4}) + \epsilon_{ct}, \tag{2}$$

where ϵ_{ct} is an error term with mean zero. Given the potential endogeneity of $atwork_{ct-4}$, we use as IV's both the Dingel-Neiman index on its own and its interactions with week dummies. Letting $\mathbf{z}_{ct}$ be the vector of controls other than $atwork_{ct}$ together with these IV's, we impose the orthogonality assumption that:

$$E[\mathbf{z}_{ct}\epsilon_{ct}] = 0. \tag{3}$$

The PPML-GMM method chooses β and γ to minimize a weighted inner product of sample means of the $\mathbf{z}_{ct}u_{ct}$ terms, where u_{ct} is the residual from (2). We use the standard robust weighting matrix. The Hanson J-test is used to test (3), which is a test of the validity of the instruments but can also be read as a test of the validity of the overall specification of the model. Once again, since our IV's are predetermined pre-pandemic variables plus interactions with week dummies, a rejection of this test is best interpreted as a rejection of the specification rather than exogeneity of the instruments. Our last specification allows the effect of workplace absence to vary over time as the pandemic evolves; this is the same as (2) but the $\gamma atwork_{ct-4}$ term is replaced by $\gamma(t) atwork_{ct-4} \equiv (\gamma_0 + \gamma_1 t + \gamma_2 t^2 + \gamma_3 t^3) atwork_{ct-4}$.

As mentioned above, our strategy is to use controls that are all either predetermined or interactions of predetermined variables with time dummies in order to isolate the endogeneity problem to our variable of interest, $atwork_{ct}$. However, it is important to control for state-level shocks as well. We do this in two ways. The first way is to control for contemporaneous mortality per million in the rest of the state. This controls both for the severity of the outbreak in the rest of the state, which will likely cause contagion across county lines, and for state-level policy shocks.[5] The disadvantage is that if COVID fluctuations in county c spill over to the rest of the state, rest-of-state mortality could be endogenous to county-c shocks. The second way is to cluster standard errors at the level of the state.[6] These two approaches give qualitatively similar results. Therefore, in the next section, we will only report outcomes from the model omitting rest-of-state mortality controls.

[5] Spiegel and Tookes (2020) code up all US state and county level COVID restrictions in their data period. We could use those as controls, but they would be subject to the same sort of endogeneity problem. Our $atwork_{ct}$ variable absorbs the effect of those policy rules and voluntary absences together, which is appropriate to our needs since we are trying to measure the effect of workplace absence whether mandatory or voluntary.

[6] We could also cluster at the level of each state-week combination. Clustering at the state level is more conservative and will result in larger standard errors.

5 Results.

Table 2 shows the results of the four specifications. Throughout, the dependent variable is county deaths per million. The estimated coefficients of the many controls, week dummies, and interactions are not reported. All regressions are weighted by population.

Columns 1 and 2 show the importance of the IV approach. The OLS estimation of (1) shows a strong negative coefficient for $atwork_{ct-4}$, indicating that serially-correlated bad outbreaks induce absence from workplaces – the 'reverse causation' problem. Correcting for this endogeneity with the IV switches the sign, to a weak, insignificant positive coefficient.[7] The first stage is very strong, as the first-stage F-statistic takes a value of 3,635.9, with a p-value of 0.000. However, the overidentifying restrictions are rejected, which we can interpret as a rejection of the linear specification.

Column 3 shows the PPML results with the coefficient on $atwork_{ct-4}$ constrained to be constant, and column 4 shows the case where it is allowed to vary over time. In these estimations, we do not include interactions between the controls and week dummies because the numerical optimization algorithm unfortunately does not converge when they are included.

The constant-coefficient version displays a negative and statistically significant estimate for the $atwork_{ct}$ coefficient, but the Hanson J statistic forcefully rejects the overidentifying restrictions with a p-value equal to zero for four decimal places. We can take this to be a rejection of the constant-coefficient model, implying that the effect of workplace absence has changed over the course of the pandemic. The time-varying-coefficient version shows a Hanson J statistic with a p-value above 0.8, so, at last, this specification is not rejected. It therefore appears to be important to allow for an effect that varies both with scale (by the PPML specification) and over time. We can regard this last version as the preferred specification.

Figure 2 shows the implied value of the $\gamma(t)$ coefficient week by week. The figures begin with week $t = 5$ because of the 4-week lag in the $atwork_{ct}$ variable. 95% error bands are indicated in the figure.[8] The coefficient is never statistically significant until week $t = 17$, but it becomes significant thereafter and quickly grows to a high value, finding a plateau at 0.1252. This tells a story of an initial period when absence from work made no discernible difference followed by a turning point after which absence from work had a strong and growing effect of reducing subsequent COVID deaths.

If we take the turning point of week 17, the lagged value of $atwork_{ct}$ that applies is from Week 13, which begins on May 18. This would imply that absence from work began to be important for death rates in the second half of May. The lagged at-work rates that matter for the last week of our data, Week 27, beginning on Monday, August 24, correspond to the last week of July. In that week, the population-weighted standard deviation of $atwork_{ct}$ was 7.13 percent. The population-weighted 25th-percentile

[7] This is very similar to the findings for masks in Welsch (2020), in which the sign changes once the IV is used.

[8] For each value of the week t, the value of $\gamma(t)$ is a linear combination of estimates in Table 2, and so the standard error of $\gamma(t)$ can readily be computed from the variance-covariance matrix of the estimators.

point value of $atwork_{ct}$ was -36.43% (observed in Los Angeles County, CA) and the 75th-percentile point value was -26.43% (observed in Lubbock County, TX), so the interquartile difference for that week was 10 percentage points. By the estimated values of the coefficient at the end of the period, namely $\gamma(27) = 0.1252$, a decrease of 10 percentage points in the value of $atwork_{ct}$ would multiply the subsequent death rate by $\exp(-10 \times 0.1252) = \exp(-1.252) = 0.2860$. In other words, by the end of the sample, a change in workplace presence as large as the interquartile range would cut subsequent death rates by almost three quarters.

6 Discussion.

We find strong support for the fourth specification in preference to the others, the PPML specification with time-varying coefficients for $atwork_{ct-4}$. The reason for the null results early in the sample are a matter of speculation. They may conceivably be due to a lack of understanding in early months of the importance of masks and distancing, so that even casual contact and grocery visits would lead to the spread of the virus and, as a result, workplace transmission was redundant. Once these measures became widely understood, virus exposure through casual contact became less common and the importance of exposure through the workplace became relatively more important. But this is merely conjecture. What is clear is that the data imply that finding ways to move people out of the physical workplace can save many lives.

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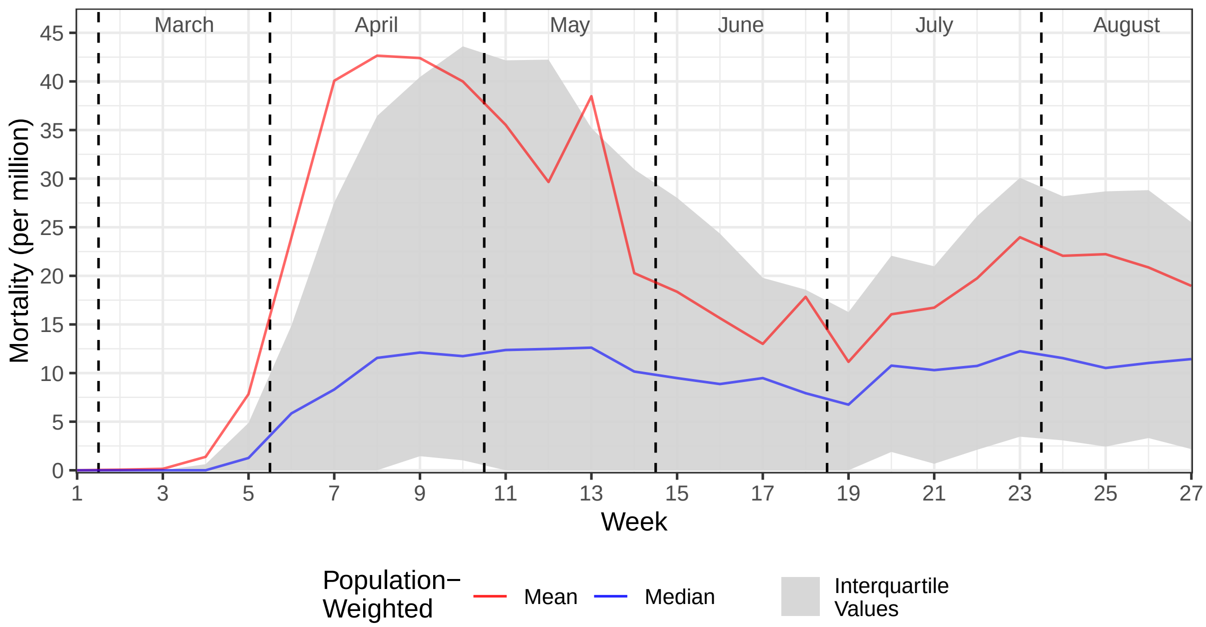
Table 1: Descriptive Statistics.

	Population-weighted mean.	Population-weighted standard deviation.	Population-weighted median.
African-American share.	12.62	12.66	8.16
Hispanic-Latino share.	17.85	17.06	11.2
Asian share.	5.46	6.41	3.39
First-nations share.	0.82	3.07	0.36
Median household income.	62,977.54	17,134.64	60,146
Poverty rate.	14.06	5.11	14.1
Share with less than high school.	12.41	5.40	11.5
High-school graduate share.	27.06	7.18	26.7
Share with some college.	29.07	4.82	29.3
Share with college degree.	31.47	11.02	31.4
No health insurance.	9.36	4.39	8.8
Fraction of workers who use a carpool to get to work.	9.15	2.00	9.05
Fraction who use public transit to get to work.	4.81	10.01	1.61
Fraction of workers who can work from home.	38.71	6.25	38.65

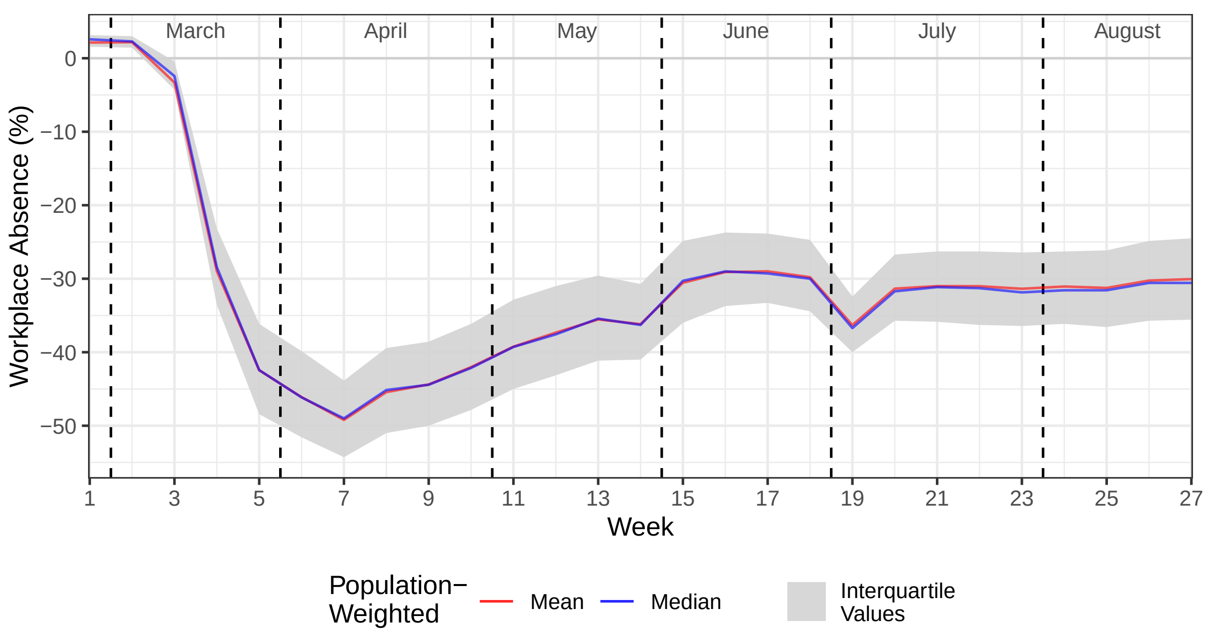
Table 2

Workplace Absence and Reduction in COVID-19 Mortality (per million), 2/24-8/30

	OLS	IV	PPML	PPML time trend
atwork	-0.9339^{**}	0.0870	-0.1192^{***}	0.1402^{*}
	(0.2856)	(0.6475)	(0.0347)	(0.0659)
atwork×*week*				-0.0342^{**}
				(0.0121)
$atwork \times week^2$				0.0022^{**}
				(0.0007)
$atwork \times week^3$				$3.69 \times 10^{-5**}$
				(1.4×10^{-5})
Instrument	No	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Weekly Interacted Controls	Yes	Yes	No	No
Summary Statistics and Tests				
Number of Observations	63,323	63,323	63,323	63,323
R^2	0.5612	0.5556	-	-
Overidentifying restriction p-value	-	0.0000	0.0000	0.8069



(a) Mortality per Million.



(b) Workplace Absence (*atwork*).

Figure 1: Trend of time-varying variables over time.

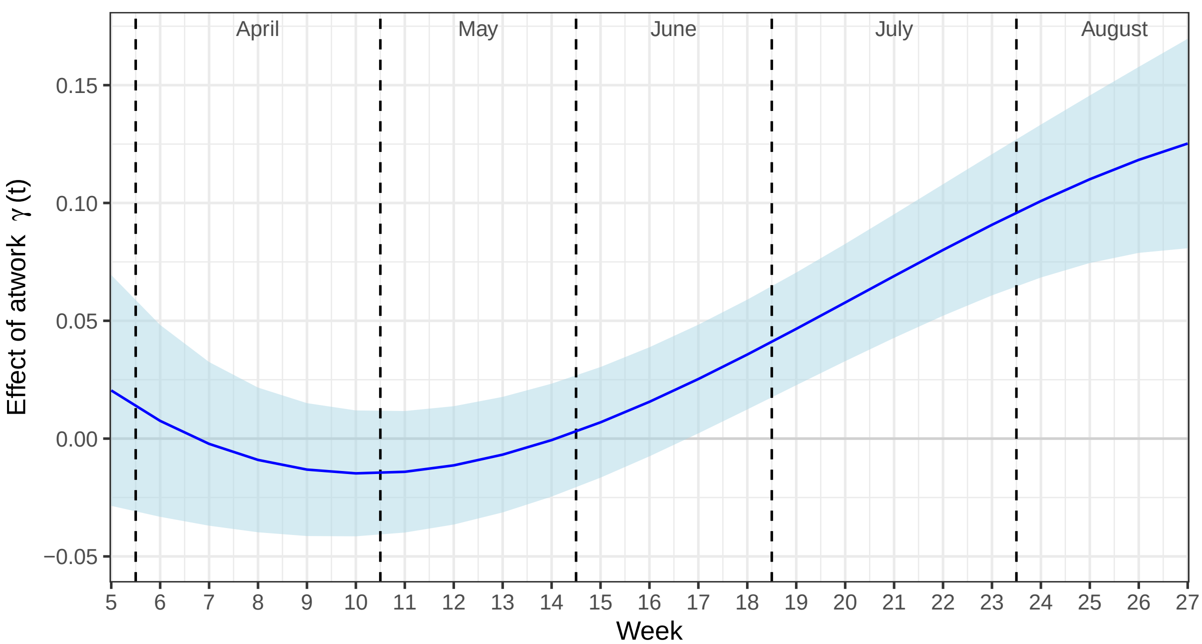


Figure 2: Time Trend of Workplace Absence Effect.