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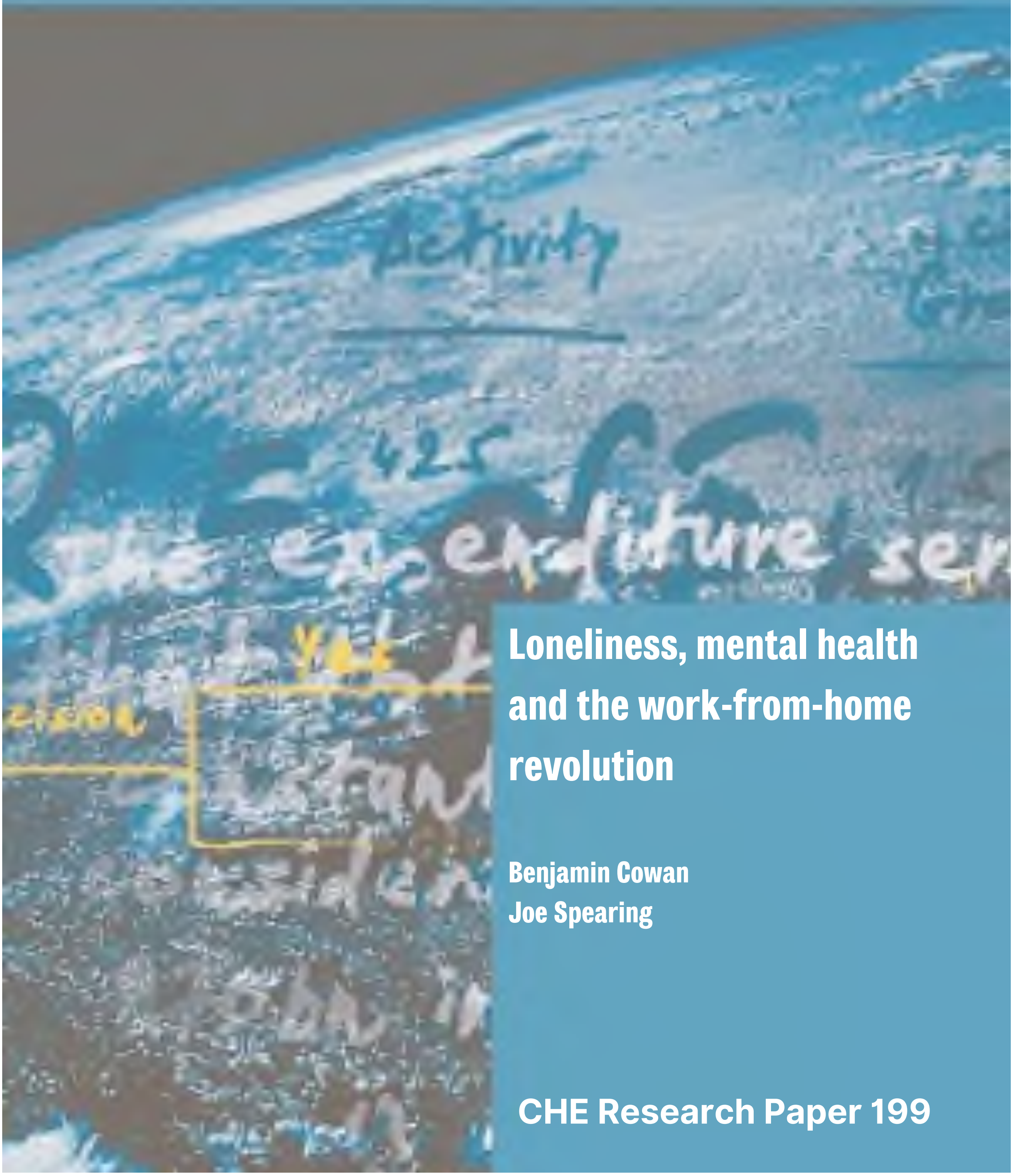

Loneliness, mental health and the work-from-home revolution

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CHE Research Paper 199

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The research was carried out under a UK data service special license agreement.

The code which generates the results in this paper is available at https: //github.com/Joe-spearing/Remote_work_replication.

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Abstract

We examine the effect of the large post-COVID increase in remote work on loneliness and mental health, using Understanding Society data from the United Kingdom. We use differences-in-differences estimators that flexibly control for a rich set of co-variates to compare changes in key variables amongst two groups: those who worked in teleworkable occupations in 2019, and those who worked in non-teleworkable occupations in 2019. We find that relative to those who worked in non-teleworkable occupations, workers in teleworkable occupations significantly increased their propensity to remote work from 2020 onwards. They also experienced higher levels of self-reported loneliness, particularly amongst women, and worse mental health. By contrast, we find no evidence of changes in job satisfaction and any improvement in work-related autonomy is limited to men. Our results suggest that the rise of remote work may contribute to increased loneliness and worsening population health, albeit at modest levels.

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1. Introduction

A long-running trend in the U.S. and other western countries is that people spend less and less time interacting with others in person. Atalay (2024) finds that time spent alone has increased, while time with individuals from other households has decreased, since the early 2000's. Sharkey (2024) finds a corresponding upward trend in time spent at home over the same period that was exacerbated by the COVID-19 pandemic. Though perceptions of loneliness are subjective and may change over time, Buecker et al. (2021) find that loneliness steadily increased among young adults over the last several decades. Social isolation and loneliness have been linked to poor mental health and other adverse outcomes (Leigh-Hunt et al. 2017). These and related trends have led the U.S. Surgeon General's office to declare an epidemic of loneliness and isolation (Surgeon General et al. 2023).[1]

The objective of this paper is to examine whether the sharp increase in remote work that occurred in conjunction with the COVID-19 pandemic has contributed to an increase in feelings of loneliness among workers. Though remote-work rates have fallen from their peak at the height of the pandemic (2020-2021), the share of workdays worked from home has stabilized at around 25-30% in the U.S over the past few years, roughly four times higher than it was prior to 2020.[2] Many countries around the world have also seen sustained increases in remote work relative to pre-pandemic levels, with rates that are especially high in English-speaking countries such as the U.K. (Zarate et al. 2024).

Because part- or full-time remote work likely reduces in-person interactions with colleagues in many jobs, we hypothesize that it could lead to an increase in loneliness. Social support from colleagues appears to be an important determinant of job satisfaction (Surgeon General 2022), with teleworkers often experiencing a drop in such support (Vander Elst et al. 2017). 53% of remote workers say that working from home hurts their ability to feel connected to co-workers (Parker 2023). We note that such an increase in loneliness may occur both among workers who work from home as well as those who continue to work on site in as much as their opportunities for social interaction are diminished when their colleagues work remotely.[3]

In this paper, we use workers' pre-pandemic occupational characteristics and panel data to compare the outcomes of those who were in "teleworkable" occupations prior to 2020 (our treatment group) to those who were not in such occupations (our control group). From each group's perspective, the rapid increase in the propensity to work remotely in 2020 and beyond is an unexpected, exogenous hock, but it likely affected those already working in teleworkable occupations more than those who were working in non-teleworkable occupations (which we later confirm in our analysis). In addition,

[1] Coinciding with the decrease in in-person interactions over the past several years has been the rise in virtual ones. For example, average daily time spent on social media increased world- wide by roughly one hour between 2012 and 2023 (https://www.statista.com/statistics/433871/%20daily-social-media-usage-worldwide/) But virtual interactions appear to be a poor substitute for in- person ones in helping individuals to feel less lonely and isolated (Rouxel and Chandola 2024; Thompson 2025).

[2] https://wfhresearch.com/wp-content/uploads/2025/02/WFHResearch_updates_ February2025.pdf

[3] For example, Yang et al. (2022) find a decrease in synchronous communication and less collaboration overall between colleagues when many workers in a firm begin teleworking.

we use data (U.K. Understanding Society) that run through 2022, allowing us to examine outcomes after the acute phase of the pandemic.

Few studies attempt to estimate causal effects of remote work arrangements on loneliness or other mental health outcomes, as we detail in the next section. Because characteristics of individuals in teleworkable occupations are significantly different from those in non-teleworkable occupations, our setting is one in which an unconditional parallel trends assumption is unlikely to be plausible.

Rather, we rely on a conditional parallel trends assumption in which we control for a rich set of predetermined characteristics. We do so in a flexible way that allows for heterogeneity in the effects of time and treatment by our included covariates via the Doubly Robust (DR) estimator of Callaway and Sant'Anna (2021). We also examine the robustness of our results to the synthetic difference-in-differences (SDD) estimator of Arkhangelsky et al. (2021). To our knowledge, the DR and SDD estimators, which have several advantages relative to standard two-way fixed effect (TWFE) estimators, are new to this literature.

We find that relative to workers in non-teleworkable occupations, workers in teleworkable jobs experienced a sustained increase in loneliness after the onset of the pandemic that is concentrated among women. This increase can explain at least 10-15% of the increase in loneliness over the years in which it is measured in Understanding Society (2017-2022).[4]

Our main focus in this paper is loneliness given the trends toward social isolation discussed above. However, because loneliness has been linked to poorer mental health, we also examine the effect of the remote-work revolution on workers' mental health itself using an extensive battery of questions available in Understanding Society. We note that increases in remote-work prevalence are likely to affect mental health through channels other than loneliness: for example, some studies point to increased autonomy and flexibility as reasons why workers often prefer to work from home at least some of the time (Bloom et al. 2015; Aksoy et al. 2022; Choudhury et al. 2024). Corresponding with the rise in loneliness, we find some evidence of a decline in women's mental health in teleworkable jobs relative to non-teleworkable ones, though these effects are sometimes imprecisely estimated and also present for men. We do not observe positive effects of the remote-work revolution on women's self-reported autonomy or overall job satisfaction, suggesting that at least some women who choose to work remotely might trade off some aspects of their wellbeing for other benefits (such as flexibility in blending work and home life).

[4] We calculate this by taking our baseline estimates of 0.023 and 0.039 and applying these to the 15% of the population who worked in teleworkable occupations in 2019.

2. Related literature

There is a burgeoning literature that examines the relationship between remote work and aspects of workers' (mental) health and job satisfaction.[5] A number of these papers use cross-sectional variation in telework and/or occupational "teleworkability" to estimate partial correlations with worker well-being (see, for example, Galanti et al., 2021; Islam, Baun, and Racette, 2023; Song and Gao, 2020; Oreškovic´ et al., 2023; Giménez-Nadal and Velilla, 2024; Hennecke and Knabe, 2025; Miyake et al., 2022). Another set of papers exploit longitudinal variation in remote work situations using individual panel data (Agnoletto 2024; Senik et al. 2024; Gueguen and Senik 2023; Esposito et al. 2024; Bilgrami 2023).

Papers using panel data in this literature generally include individual fixed effects to account for time-invariant unobserved heterogeneity that may be correlated with both working arrangements and worker wellbeing; nevertheless, decisions to switch into/out of remote working are treated as exogenous. This assumption will fail if factors that vary over time, such as changes in family circumstances or occupation, influence both remote-work and mental-health outcomes. In addition, mental health circumstances may cause individuals to change their work setting (reverse causality). In our analysis, we utilize workers' pre-determined occupational characteristics–which affect the likelihood they transitioned to remote work when it became more common during and following the COVID pandemic–as variation that is more plausibly unrelated to changes in mental health over time.

Finally, another group of papers utilize plausibly exogenous variation in remote work arrangements to identify causal effects of remote work on health (Goux and Maurin, 2025; Nguyen, 2022; Costi et al., 2024; Bertoni et al., 2021). We make several contributions to this literature: first, we use an individual panel dataset that spans before, during, and after the pandemic. Our dataset is representative of the working-age population in the U.K. with a rich array of questions on health, life satisfaction, and working conditions. Second, our identification strategy does not rely on the exogeneity of instruments (such as distance to one's workplace) that may fail the exclusion restriction. Rather, it relies on predetermined differences in occupational characteristics that make workers more or less likely to work remotely following the onset of the pandemic. This strategy is similar to the one employed by Bertoni et al. (2021), but that paper focused only on outcomes during the height of the pandemic (June-July 2020) for a sample of older workers. Along with other papers in this literature, the authors assume that their instrument (in their case, pre-pandemic occupational teleworkability interacted with time) affects mental health only via an individual's own remote-work behaviour. As we later argue, we believe that there are other channels by which this instrument affects mental health, such as the remote-work decisions of one's co-workers. Lastly, we are one of the first papers in this literature to examine the causal effect of the remote-work revolution on loneliness after the acute pandemic period.[6]

[5] Job satisfaction is a major correlate of self-reported mental health (Faragher, Cass, and Cooper, 2005; Belloni, Carrino, and Meschi, 2022)

[6] Bertoni et al. (2021) examine loneliness at the height of the pandemic, when lockdowns were still prevalent, and Costi et al. (2024) examine loneliness after the pandemic for Italian public-sector workers who were mandated to return to the office in September 2021. However, their sample has no pre-pandemic observations and is relatively small (382 individuals).

3. Data

We link data from the Understanding Society data set, a representative UK panel data set, with occupational teleworkability scores as defined by Dingel and Neiman (2020).

3.1. UKHLS

Understanding Society is a large, representative UK panel data set. We use data from 2011 to 2022. Each wave of the survey is roughly annual, but waves can overlap and individuals may be interviewed in different months of the year due to their availability. As a result, respondents interviewed in subsequent waves may be interviewed in consecutive years, twice in the same year, or with one year's gap. On average, around 10% of people interviewed in one year are not interviewed in the subsequent year. Although the survey endeavours to follow individuals in every subsequent wave, sample attrition and re-entry is not trivial (Lynn et al. 2012). Understanding Society provides detailed information about demographics, labour-market behaviour, and health outcomes at an individual level.

3.1.1.Remote work outcomes

In even waves of the survey, respondents who work are asked about their right to use a remote work option and whether they indeed work remotely. Specifically, the survey asks respondents:

"I would like to ask about working arrangements at the place where you work. Which of the following arrangements are available at your workplace?"

One of the options is: "To work from home on a regular basis".

Additionally, the survey asks respondents who reply that some flexible arrangements are avail- able, "Do you currently work in any of these ways?"

We define a person as working remotely if they report using the right to work remotely (and not working remotely if they do not). We also code respondents who do not work, or report that the right to work remotely is not offered at their workplace, as not working remotely. In our analysis, we do not make extensive use of the variable about being offered remote work, because the wording of this question refers specifically to the respondents' workplace, i.e., an affirmative response does not formally imply that the worker themselves has the right to work remotely.

3.1.2. Loneliness

Loneliness may be an important mechanism through which remote work affects poor mental health, either through being lonely if someone works at home, or being lonely if a person still works in an office but there are fewer other people in the office. In Understanding Society, loneliness is reported (from wave 9 onwards) from one of three options: “hardly ever or never”, “some of the time”, and “often”.

We construct two variables to study this phenomenon. Firstly, we define someone as lonely if they report being lonely “some of the time” or “often”. This variable equals one if a person “is lonely” and zero otherwise. We construct a variable “is often lonely” which is equal to one if a person reports that they are lonely “often” and zero otherwise.

3.1.3. Mental health variables

We measure mental health using the GHQ12 survey and the SF12 indices. The GHQ12 survey is a battery of 12 questions that asks individuals about the severity of 12 mental health symptoms. Individuals select from four ordered responses, normalized so that higher numbers reflect worse mental health.

We construct the GHQ12 caseness score as the sum of symptoms of mental illness in which a respondent scores 3 or 4. The caseness therefore varies between 0 (no symptoms) and 12 (the maximum number of symptoms) and can be understood as a measure of the number of adverse mental health symptoms a person experiences. Additionally, we construct the sub-indices as the sum of the respondent’s scores on symptoms of each type: Anxiety and Depression; Loss of Confidence; and Social Dysfunction. Both the GHQ12 caseness and its subindices have been shown to be useful screening tools for mental illness (McCabe et al. 1996, Graetz 1991, Anjara et al. 2020), and have been used in economics research (e.g., Gathergood, 2013, Belloni, Carrino and Meschi, 2022, Spearing 2024).

The SF12 measure is derived from the SF12 survey, a series of 12 questions to which respondents give one of an ordered list of responses. These responses are then weighted and aggregated to provide a summary index of mental health, ranging from 100 (best) to 0 (worst) (Jenkinson and Layte, 1997). The SF12 index has been used effectively as a screening measure for mental illness (Kontodimopoulos et al. 2007, Tibubos and Kröger 2020), and as a summary measure of mental health in economic research (e.g., Davalos and French 2011, Wallace, Nazroo, and Becares 2016, Jolivet and Postel-Vinay 2024).

3.1.4. Other dependent variables

We consider a number of additional dependent variables which may inform factors which offset or compound effects on loneliness.

Job satisfaction may drive mental health, since work is a key component of a person’s life. Individuals may be willing to trade off loneliness against higher job satisfaction. In Understanding Society, it is reported on a scale from 1 (completely dissatisfied) to 7 (completely satisfied). We treat this job satisfaction variable both as a likert scale - a cardinal variable ranging between 1 and 7- and converted into a binary variable for “high job satisfaction”. We consider a person to have high job satisfaction if they score higher than the median person for job satisfaction.

Secondly, we consider working conditions as drivers of mental health. Previous research (e.g., Spearing, 2025) has shown that work-related autonomy plausibly drives mental health. The option to remote work may increase work-related autonomy due to choice about the manner and place in which one works, but employers may also react by increasing surveillance and restricting other types of autonomy. In Understanding Society, employees report autonomy over five aspects of work- tasks, task order, task pace, task manner, and task hours- scored from 1- “a lot of this type of autonomy”- to 4- “none of this type of autonomy”. Following Spearing (2025), we convert these variables to binary autonomy variables, where a person has low autonomy if they score 3 or 4 on this scale, and does not have low autonomy if they score 1 or 2 on this scale. We also define an overall low autonomy score, which is the number of dimensions of autonomy on which a person has low autonomy.

Finally, Understanding Society asks respondents if they care for someone they live with. We hypothesize that people who have the option to remote work might be more likely to face pressures to use their workplace flexibility to adopt caring responsibilities, which may introduce new psychological costs. We therefore include a dummy variable for whether someone is a carer.

3.1.5.Covariates

We use a number of other variables as controls and to examine heterogeneity. Education is measured as the highest qualification a person has achieved: no qualification, other (non- academic) qualifications, GCSEs or A-levels (high school qualifications), a college degree, and a higher degree. We can also observe if a person has biological children, and if a person lives in a home which is owned by the person who lived there. The survey asks a series of questions about a person’s work life, including monthly income, occupation, working hours, and average length of commute. There are also key demographic variables including ethnicity and marital status.

In the third wave, respondents are also scored on the “big 5” personality traits: agreeableness, conscientiousness, extraversion, neuroticism, and openness. We assume that a person’s score on these variables is consistent over time, i.e., their score in all subsequent waves is assumed to be their score in the third wave.

3.2. Occupation teleworkability

We measure occupational “teleworkability” according to Dingel and Neiman’s (2020) index. Dingel and Neiman define occupational teleworkability based on O*NET variables, which summarise information about occupations in the United States: they assume that each occupation can be done remotely unless some information from the O*NET data suggests otherwise. For example, if the average person within an occupation reports that dealing with the public face-to face is a requirement of the job, they assign a teleworkability score of 0 to this occupation. This process categorises all occupations within the O*NET data set as teleworkable or not teleworkable.

We access a special license version of the Understanding Society dataset which contains information on occupation at the ISCO88 4-digit level, and link respondents’ occupations to the SOC codes used in the O*NET data set using crosswalks provided by Hardy (2016). This linkage allows us to assign each ISCO88 occupation a teleworkability score of 1 or 0. In some cases, an ISCO88 occupation can be linked to multiple SOC occupations, only some of which are teleworkable. In those cases, we assign a value of 1 to the ISCO88 occupation if a majority of the SOC occupations it is linked to are teleworkable.[7]

The Understanding Society data set is a UK panel data set, and the O*NET data is a US data set. We therefore implicitly rely on similarity between UK and US occupations. We are not the first to apply O*NET data to a European context (e.g., Goos, Manning, and Salomons 2014, Hardy, Keister, and Lewandowski 2018, Lewandowski 2020), including the UK (Jolivet and Postel-Vinay 2024, Spearing, 2024). Where researchers have assessed the similarity of occupations measured in Europe and in the O*NET data set, results have generally pointed to a high correlation (CEDEFOP 2013; Spearing 2025). Furthermore, in our context, we are able to test to what extent the teleworkability scores from the US occupations predict remote working in the UK: a regression of remote working on the teleworkability of a respondent's occupation among those who work yields a coefficient of 0.112, with a p-value of 0.000. Overall, we are confident that the teleworkability of US occupations measured by Dingel and Neiman (2020) is informative about the propensity of UK workers in those occupations to work remotely.

3.3. Sample Selection and panel construction

Our sample is composed of all unique individuals who report working in 2019. We define two groups: one group of people who were working in 2019 in occupations measured as "teleworkable", and one group of people who were working in 2019 in occupations not measured as "teleworkable". Though the work-from-home revolution potentially affects all within the economy, we can think of the former of these groups as facing higher treatment intensity than the latter.

UKHLS survey waves span multiple, overlapping years. To give a concrete example, wave 6 spans the years 2014 to 2016 with 23,439 observations in 2014, 19,640 observations in 2015, and 2,110 observations in 2016. Wave 7 spans the years 2015 to 2017, with 22,163 observations in 2015, 18,471 observations in 2016, and 1,528 observations in 2017. As a consequence, since the final wave contains the years 2022, 2023, and 2024, there are observations of people interviewed in 2023 and 2024 which are not reported in the most recent wave (but will be in the subsequent waves). Within the currently published data there is very high attrition in these years (see Appendix A for the full distribution of time periods in the data set). Because many of our estimates require a balanced panel and to keep the time period roughly consistent across our estimates, we use data up through 2022 for our main estimates (though for summary statistics that don't utilize the panel component of the data, we are able to report estimates from more recent years).[8] The alternative would be to drop all observations whose responses have not been reported as of 2023, which would significantly reduce the sample size.

[7] Due to the incompleteness of the crosswalk, there are some occupations we must match by hand. The full crosswalk is available at https://github.com/Joe-spearing/Remote_work_replication.

[8] Note the implication that when we report the year pair 2022-2023 in our event studies, the observations which have this label are all from 2022.

3.4. Descriptive statistics

Figure 1 shows the percentage of workers who say they use the option to work remotely over time. This percentage trends up gradually over a period of 9 years prior to the onset of COVID in early 2020 and then nearly doubles after that. The percentage working remotely then remains roughly constant through the end of 2023. This finding is consistent with Barrero, Bloom, and Davis' (2023) finding that the incidence of working remotely has not returned to pre-pandemic levels even after vaccinations.

Table 1 shows overall summary statistics on key variables used in the analysis. Of note, our sample is 47% composed of workers who were working in a teleworkable occupation in 2019, but over the entire period of study (2011 to 2022), they actually work remotely only 7.2% of the time. Mean values for the GHQ12 caseness, Anxiety and Depression, Loss of Confidence and Social Dysfunction are 1.7, 7.5, 3.1 and 12.4 respectively. The SF12 measure of health ranges between 0 and 100 and has an average score of around 50.

Table 2 compares characteristics of workers in teleworkable and non-teleworkable occupations in 2019. Workers in teleworkable jobs are slightly older and more likely to be female, white, married, have children, and own their own home. Their commute times to their traditional workplace are longer, and they work a little over one additional hour per week, on average.

Unsurprisingly, since teleworkable jobs tend to require the use of computers, workers in such jobs tend to have higher degrees, be in jobs at the management/professional level, and earn substantially more than those in non-teleworkable jobs. However, average differences in mental health measures by occupational teleworkability are small.

3.5. Sample attrition

As we reference above, sample attrition can be substantial. In principle, this attrition could bias results if attrition is correlated with dependent variables. To explore this possibility, we investigate how weighting observations by the inverse of the propensity score for being observed affects average outcomes in the treatment and control group.[9] Details of the propensity score model and results of this exercise are presented in Appendix C. Overall, these findings suggest that the effect of attrition on our results is likely to be small.

[9] Other researchers have inverse propensity-weighted estimators to correct for attrition bias (see, e.g., Wooldridge 2002). Since our preferred estimators already include a weighting of observations (see Section 4.2) we cannot also apply inverse-propensity score weighting.

4. Empirical Strategy

4.1. Identification strategy

Our empirical strategy compares how outcomes of interest evolve for people in two groups. One group of people were working in teleworkable occupations in 2019. We argue that this group found it easier to take advantage of changing norms around remote working to increase the share of work performed remotely in 2020 and beyond. Our comparison group is a group of people who were working in non-teleworkable occupations in 2019. This group found it harder to increase their remote work, since it is less likely to be feasible without changing occupation. Our argument is illustrated in Figure 2. We plot the share working remotely for our treatment group, that is, people who worked in a teleworkable occupation in 2019, and our control group, people who worked in non-teleworkable occupations in 2019. Before 2020, the share of people working remotely is already higher in the teleworkable group. However, they experience a larger increase in 2020, which is sustained in the following years. The treatment group increases their propensity to remote work by 10 percentage points, while the control group increases their propensity to remote work by around 3 percentage points. Note that this measure only accounts for remote working at the extensive margin, which does not account for the possibility that those in the treatment group who already worked remotely some of the time increased the share of hours they worked remotely (relative to the control group).

Identification depends on standard differences-in-differences assumptions: firstly, the change in working conditions for those in the treatment group relative to the control group is exogenous. This assumption rules out the possibility that people select into their occupations in anticipation of changing working conditions in those occupations. Secondly, we make a parallel trends assumption: the evolution of the dependent variable in the control group is a good counterfactual for the evolution of the dependent variable in the treatment group if they had not been treated.

Since the characteristics of those in teleworkable and non-teleworkable occupations are quite different along several dimensions (see Table 2), we use a double-robust estimator (see below) to relax this assumption to a conditional parallel trends estimator (i.e., trends are parallel conditional on a set of pre-treatment characteristics).

4.2. Estimation

4.2.1.Doubly-robust estimator

We use differences-in-differences estimators to compare the evolution of dependent variables in the treatment and control group. Our preferred specification is the doubly-robust estimator (Callaway and Sant'Anna, 2021). The effect of the treatment in time t is given by:

$$ATT(t) = \frac{1}{N}\Sigma_i[(\frac{D_i}{\bar{D}_i} - \frac{\frac{\hat{p_D}(X_i)(1-D_i)}{1-\hat{p_D}(X_i)}}{\overline{\frac{\hat{p_D}(X_i)(1-D_i)}{1-\hat{p_D}(X_i)}}})(Y_{it} - Y_{i0} - \hat{m}_{D,t}(X_i))] \qquad (1)$$

$p\hat{}D\ (Xi)$ is a model which predicts, the probability that a person is in the treatment group based on observables X_i, $\hat{m}_D,t\,(X_i)$ is the predicted value of the dependent variable, Y_{it}, for an untreated person from a linear model of observables X_i, and D_i is a dummy variable which takes the value 1 if a person is treated and 0 if untreated. Y_{i0} is the value of the dependent variable for person i in the period immediately before treatment (2019). The linear model is flexible, allowing controls to have different associations with the outcomes depending on the year.[10]

Event studies are composed of these average treatment effects (ATT) over time. In addition, we aggregate these to summary treatment effects weighting treatment periods by the share of observations in each period.

This estimator has the advantage of being “doubly-robust” (Sant’Anna and Zhao, 2020). The estimator is consistent for the causal effect provided either the propensity score or the linear model is correctly specified.

4.2.2.Control variables

We use a number of observable “control variables” to account for differential probabilities of being treated, and differential evolution of potential outcomes. The control variables we include are education defined by highest qualification achieved in 2019, a dummy for whether the person has children in 2019, a dummy for whether the person owns the home they live in in 2019, and a dummy for whether the person was married in 2019. Including pre-treatment (COVID) values of these controls ensures that their values are not determined in part by treatment.

4.2.3.Time variable

Our identification strategy depends on the correct notion of calendar time. For this reason, we categorize the time period by the date of interview. Since people are roughly annually surveyed, our time variable is the calendar year when we are estimating the effect on variables which are present in every wave. For variables which are observed in every other wave (such as whether a person works remotely) we use year-doubles as our time variable, e.g., a dummy variable for whether an observation was from the years 2020-2021 (these year-doubles do not overlap with the pre- and post-treatment periods, the latter of which begins with 2020).

[10] The propensity score is a logit function of the covariates. The model for the predicted values of the dependent variable is a flexible linear model of the covariates.

4.2.4. Synthetic difference-in-differences estimator

For robustness tests, we use the synthetic differences-in-differences (SDD) estimator (Arkhangelsky et al., 2021). This is an alternative way of dealing with non-parallel trends: rather than control flexibly for pre-determined characteristics and rely on a conditional parallel trends assumption as in the DR estimator, the SDD estimator weights control observations in the pre-treatment period to approximate parallel trends in outcomes between treatment and control observations. The SDD estimator is estimated as a weighted two-way fixed effect estimator:

$$Y_{it}^{resid} = \beta D_i \times 1(t \geq 2020) + \mu_i + \tau_t + \epsilon_{it} \qquad (2)$$

where Y_{it}^{resid} is the residual from a regression of the dependent variable on controls, μ_i are person fixed effects and τ_t are year or year-double fixed effects. The SDD estimator weights the TWFE estimator by unit weights ω, which weight individuals in the control group in order to limit pre-trends, and time weights λ, which minimize differences in the average of the dependent variable amongst control units before and after treatment. Appendix B details how unit and time weights are calculated.

4.2.5. Complementarity of estimators

We use two different types of estimators in order to show robustness to different sets of econometric assumptions. These estimators have different strengths and weaknesses.

The doubly robust estimator has the benefit of being doubly robust, in the sense that it is consistent for the causal effect if either the linear outcome model or the propensity score is correctly specified. Its major weakness is its inability to account for unobserved time varying confounders: if there are determinants of the dependent variable which vary across time and do not appear in the data set, then both the linear model and the propensity score will be misspecified. We would expect in these cases to detect statistically significant pre-trends, although this is not guaranteed.

The SDD estimator imposes parallel pre-trends. In principle, it therefore selects a weighting of control units which upweights control units whose relevant unobservable characteristics are most similar to the treatment group's. We are not aware of a method of testing this assumption, however. The SDD estimator requires a balanced panel. For this reason, when computing SDD estimators we drop all observations of people who do not appear in each year from 2014 to 2022, or each year pair from 2014-2015 to 2020-2021.[11] There is therefore a theoretical risk that, if attrition is endogenous to the treatment, SDD estimators exhibit collider bias. In Appendix C, we investigate the possibility of attrition bias by plotting dependent variables in the treatment and control group, both unweighted and weighted by the inverse of the propensity scores for being observed. For the most part, we find there is little evidence of substantial attrition bias.

We will also see that our SDD results are qualitatively similar to the doubly-robust results, suggesting that we can be confident that this possibility does not affect our estimates in practice.

[11] Including the year pair 2022-2023 would reduce our sample size significantly because of the 2023 observations which are not yet published (see Section 3.3 for a more detailed discussion of sample size issues).

4.2.6. Interpreting the estimates

Under the differences-in-differences assumptions, our estimates can be interpreted as the causal effect of being in a teleworkable occupation during the period of expansion of remote work on the outcomes we analyse. Note that this object is an effect of the treatment on the treated, i.e., it is the effect of expanding remote work on those in teleworkable occupations. We cannot extrapolate from our results to the effect on those people in non-teleworkable occupations. As we have seen, people in teleworkable occupations are different to those in non-teleworkable occupations, and we might therefore expect them to have a different response to changing working conditions.

Secondly, this estimate is an intent-to-treat estimate, where the treatment is an increase in remote working by oneself or one's colleagues. While the intensity of this treatment increases faster in the treatment group than in the control group, some people in the treatment group do not work remotely, and some people in the control group do work remotely. The estimate is analogous to the reduced form in a two-stage least squares estimator. We cannot calculate the corresponding instrumental variable estimator for two reasons: firstly, we only observe remote work at the extensive margin, whereas in reality there was also likely variation in the intensity of remote work. Secondly, interpreting an instrumental variable requires an additional exclusion restriction: that changes in the working conditions which occurred from 2020 only affected loneliness and mental health via their effect on the share of one's own work performed remotely.

In reality, there are a bundle of effects which might run via changed opportunities to work remotely (even if these are not taken up) and the effect of coworkers working remotely (even if the respondent does not).

5. Results

5.1. Effect on propensity to remote work

Table 3 presents our differences-in-differences estimates of the causal effect of the pandemic shock on the propensity to remote work for those in teleworkable occupations, using both the doubly robust (DR) and synthetic difference-in-differences (SDD) estimators. Corresponding event-study figures are shown in Figure 3. The DR estimates suggest that the expansion of remote work in 2020 and beyond caused a three and a half percentage point increase in the share of people working remotely in teleworkable occupations relative to those in non-teleworkable occupations. However, the effect is somewhat larger for women than men. While women in teleworkable occupations increased their propensity to work by four percentage points as a result of the shift in working conditions, the shift in working conditions has around half the effect on men. By contrast, the SDD estimates suggests a larger effect which is more consistent across sexes, with a 5-6 ppt increase.

Overall, we do not interpret these results as suggesting that the effect on remote work was small. Firstly, while we are only able to measure the effect on remote work at the extensive margin, an increase on the extensive margin indicates that there may also have been an increase in remote work on the intensive margin, i.e., those in teleworkable jobs who were previously using the opportunity to remote work likely increased the share of their work that was performed remotely.

5.2. Loneliness

Table 4 presents our estimates of the effect of the remote-work shock on being lonely and being often lonely (corresponding event-study figures are in Figure 4). Combining sexes, we find a significant adverse effect on loneliness as a result of remote working. The point estimates are larger for the SDD estimates, but we cannot reject the hypothesis that the SDD estimate is the same as the DR estimate.[12]

Disaggregating by sex, men experience no statistically significant effect on our loneliness measures. The point estimate of the effect on being lonely using the SDD is the one case in which the effect is economically meaningful, but it is imprecisely estimated. The effect on women is larger than it is for men across both dependent variables and both estimators, with effects on being lonely that range from 7-12% of pre-treatment means and are statistically significant at the 5% level or better.

Overall, our results strongly suggest that loneliness increased amongst women in teleworkable occupations as a result of the work-from-home revolution.

5.3. Mental health

The effect of working from home on loneliness suggests that there may be a broader effect on population mental health. In Table 5, we examine the effect of the pandemic remote-work shock on various measures of mental health using the DR estimator (event study graphs are contained in Figure 5). In the full sample, we find an adverse effect of the expansion of remote work on the GHQ12 caseness score and the probability of having a caseness score higher than 8, albeit these effects are relatively modest in size and the latter is only statistically significant at the 10% level. We detect no significant effects on the individual components that make up the overall casenes score, but all effects are positive (pointing to lower mental health). Breaking down the effect on the caseness by sex, women experience a larger effect on the overall caseness, but men experience a larger effect on the probability of the caseness being above 8. Due to the attenuated sample size, in neither of these cases are the point estimates for each sex outside the estimate for the other sex's 95% confidence interval.

In Table 6, we repeat the same exercise using the SDD estimator (with event-study graphs in Figure 6). Notably, the estimated effect sizes for all sexes on the GHQ12 caseness and the probability of having high caseness are similar. The SDD estimator returns a higher and statistically significant effect on symptoms of social dysfunction, though we cannot reject the hypothesis that the point estimate is consistent across estimators. Finally, when disaggregating by sex, the results are inconsistent across the estimators: while the DR estimator suggests a larger effect for women, the

[12] For example, the point estimate for the effect on loneliness is 0.039 with a standard error of 0.014. The DR estimate of 0.023 is therefore within the 95% confidence interval of the SDD estimate.

SDD estimator suggests a larger effect for men. Again, we emphasize that the effects are often not statistically significantly different across estimators. When disaggregating by sex, the reduction in sample size causes a loss of precision.

We do not estimate any statistically significant effects on the SF12 measure of mental health using either the DR or SDD estimator. This result likely derives from the fact that while the SF12 measure and the GHQ12 caseness are correlated (with a coefficient of 0.67), there are variables used in constructing the GHQ12 caseness that are not used in the SF12, including those relating to concentration, sleep, feeling useful, feeling under strain, feeling able to make decisions, confidence, and feeling worthless. The SF12 is therefore a complementary measure of overall mental health to the GHQ12 caseness, with some non-overlapping elements.

Overall, our results are consistent with the work-from-home revolution causing a small degradation in the mental health of people working in teleworkable occupations relative to non- teleworkable occupations, at least as measured by the GHQ12 caseness. This effect seems to be reasonably consistent across sexes.

5.4. Job quality

We additionally estimate whether the remote-work shock led to an improvement in self-rated job quality, as measured by job satisfaction and work-related autonomy. As we have noted, several studies have investigated the relationship between working from home and job satisfaction.

Additionally, other research has shown that work-related autonomy is positive for mental health and speculated that this might be a mechanism through which remote work *improves* mental health (Spearing, 2025).

We present our estimates using the DR estimator in Table 7, and the SDD estimator in Table 8 (corresponding event-study graphs are in Figures 7 and 8). While in all of our estimates there is no detectable effect on job satisfaction, the estimators derive divergent conclusions about the effect of remote working on work-related autonomy: using the DR estimator, we get relatively precise zeros for the effect on most measures of work-related autonomy, although there is a small reduction in the incidence of low autonomy over working hours. By contrast, the SDD estimator strongly suggests significant and fairly sizable decreases in the prevalence of low work-related autonomy, especially for men.

To reconcile these results, we note that the DR estimator exhibits statistically significant pre- trends for several of the work-related autonomy variables (see Figure 7), with low autonomy along several dimensions becoming relatively more common for workers in teleworkable jobs even prior to the onset of COVID. This suggests that there are other unobserved variables that drive these results but which are accounted for by the unit weights in the SDD estimator.

We conclude that there is likely an effect of changes in remote-work practices in improving work-related autonomy among men, although this result is not robust to all of our specifications.

5.5. Additional analyses

In addition to our primary outcome variables, we investigate a number of potential mechanisms that might explain our results. These mechanisms include the possibility that workers who are able to work from home might increase their propensity to offer care, or that people in different occupations might experience differential exposure to labour market outcomes such as unemployment and retirement. We report these results in Appendices D and E. Overall we conclude that such effects are small and unlikely to account for our results.

An important finding is that the work-from-home revolution increased loneliness for those in teleworkable occupations. We might think that this effect would be especially pronounced for unmarried individuals, since they might have lower quality relationships outside of work or be more likely to live alone. In order to test this possibility, we re-estimate our main results for loneliness for those who were unmarried in 2019. We report our results in Appendix F. Overall, there is evidence from the DR estimator that unmarried women in teleworkable jobs may have experienced a larger adverse effect on loneliness, but the SDD estimates become imprecise on this smaller sample.

Finally, we investigate heterogeneity of our results by scores on the big 5 personality types. We report results in Appendix G. We find that effects on loneliness are strikingly consistent across personality types. On the other hand, the adverse effect on mental health appears more pronounced for individuals high in neuroticism and agreeableness. In addition, our null results about job satisfaction obscure a beneficial effect for those who are low in conscientiousness and an adverse effect for those high in conscientiousness.

6. Discussion and conclusion

The aftermath of the COVID-19 pandemic saw a cross-national, persistent shift in working conditions as opportunities for remote work increased. In this paper we explore the consequences and mechanisms for remote work and workers' loneliness and mental health.

Our identification strategy exploits the fact that while all workers within the economy likely experienced weakly higher opportunities for remote work as a result of the aftermath of the pandemic, the experience was more intense for those who were in teleworkable occupations before 2020. We compare the evolution of key variables- loneliness, mental health, and job quality- in 2020 and beyond for those working in teleworkable occupations in 2019 and those working in non-teleworkable occupations in 2019 in a differences-in-differences design. Our paper advances beyond previous research by using a design which does not require strong exclusion restrictions, and allows for the effect of shifting working conditions to operate via the extensive margin of remote work, intensive margin, and the effect of reduced interactions with colleagues amongst those who continue to work onsite.

Our key findings are threefold. Firstly, we find that working from home increased the incidence of loneliness amongst women in teleworkable occupations relative to women in non-teleworkable occupations. Secondly, we confirm previous literature which showed an overall adverse effect of the change in working conditions on mental health. Thirdly, we find a beneficial effect on men's experience of work-related autonomy in some specifications.

Turning to policy implications, our results do not suggest a case for government intervention to promote lower use of remote work. Firstly, even though we provide evidence of an adverse effect on loneliness and mental health, we also show evidence of some beneficial aspects of remote work that may compensate for these losses. We also show evidence of heterogeneity (see Appendix G) which implies, firstly, that a reduction in remote working would not be a Pareto improvement and secondly, workers may adjust to the shock of changing working conditions by sorting into jobs which match their preferences. However, there may be a case for governments promoting policies to reduce loneliness in an economy which has permanently higher rates of remote working.

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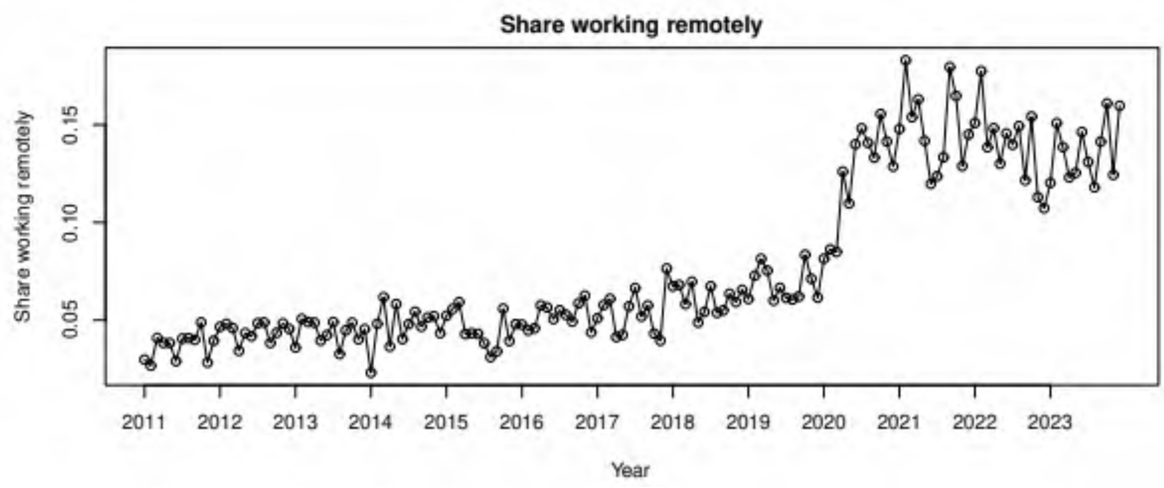
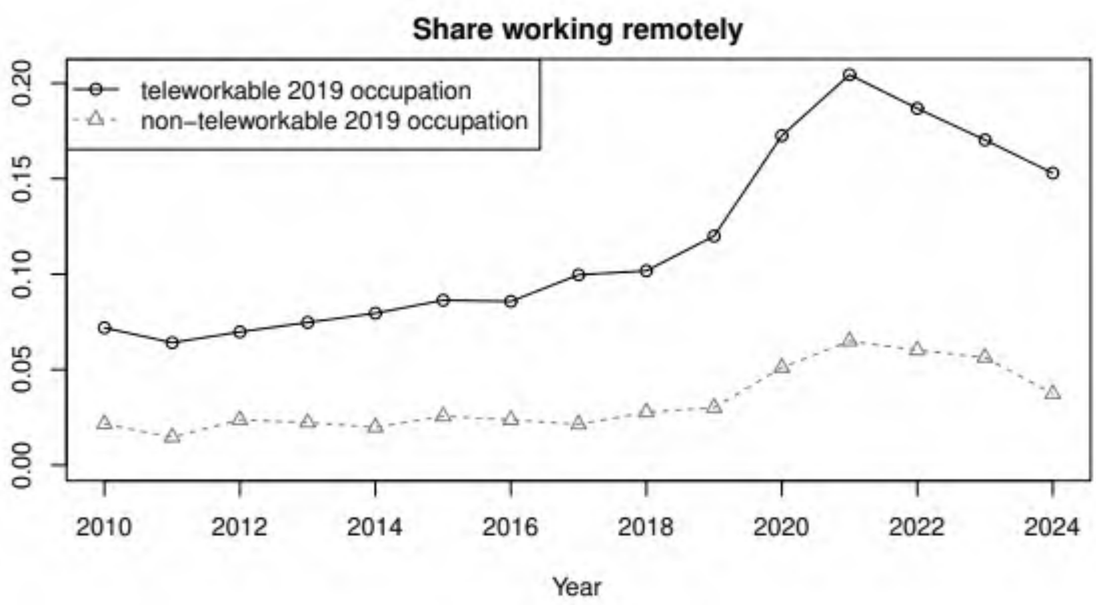
8. Appendix – Figures and Tables

Figure 1: The share of workers who work remotely over time



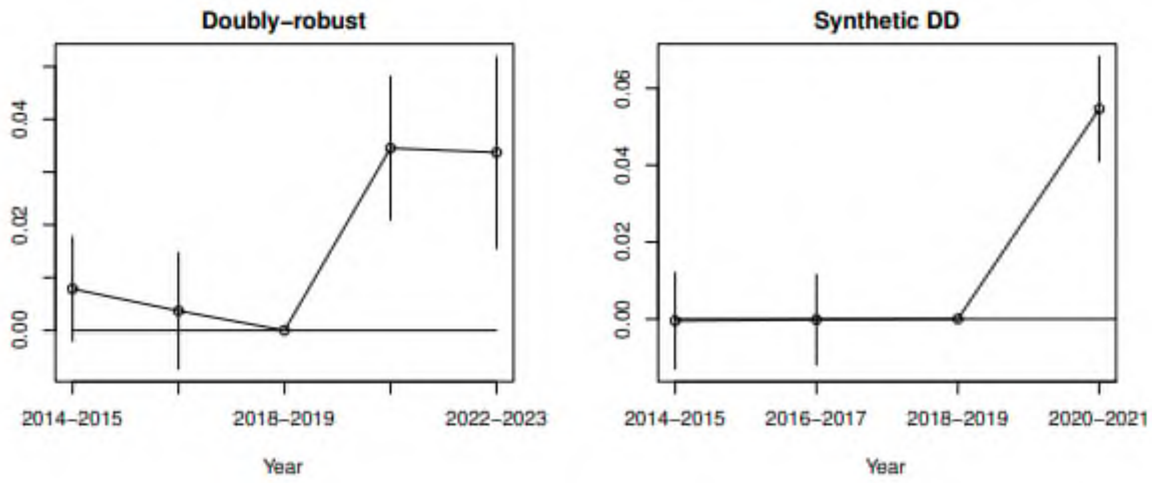
Notes: the Figure shows the percentage of respondents interviewed in each month who report working who use the option to work remotely. Data are from Understanding Society.

Figure 2: Share of people working by occupation group



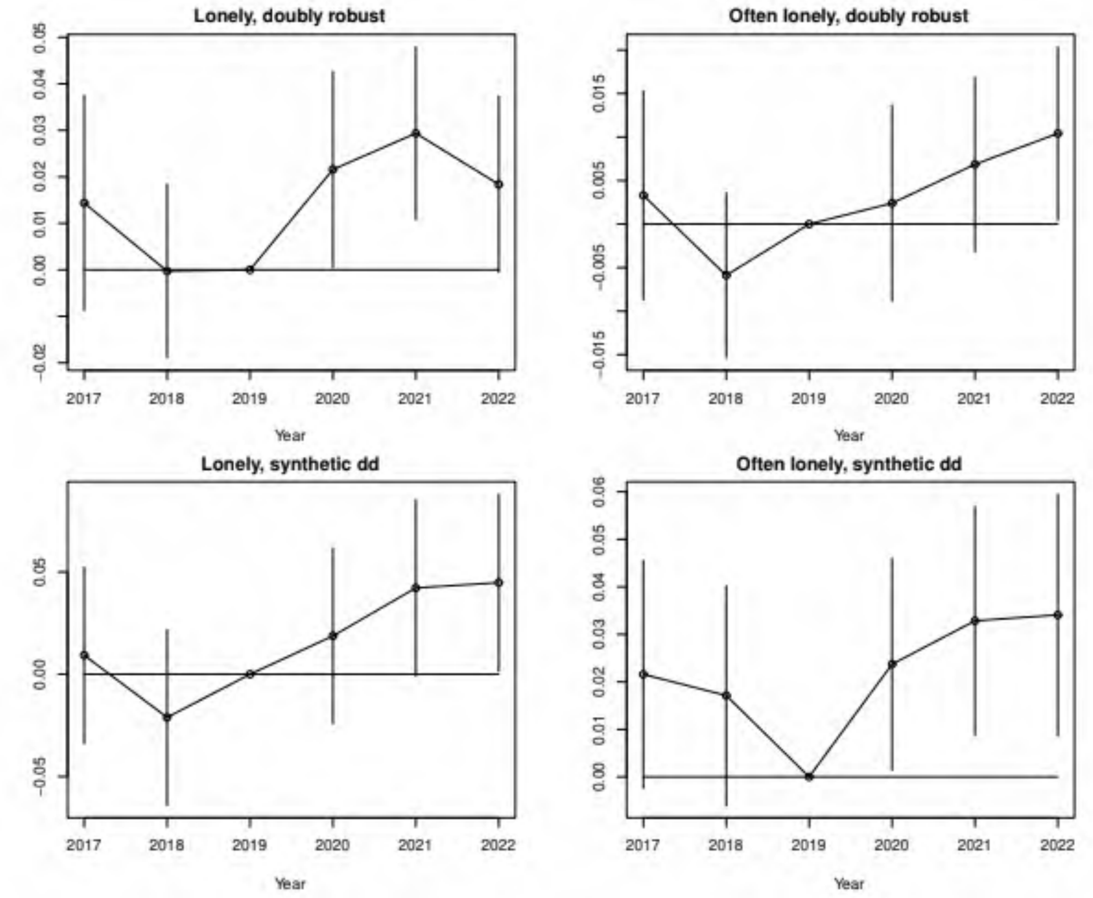
Notes: the figure shows the share of people who worked in teleworkable occupations in 2019 and who worked in non-teleworkable occupations in 2019 using the option to remote work in each year. We define Teleworkability using Dingel and Neiman’s (2020) measure.

Figure 3: Event studies for remote work



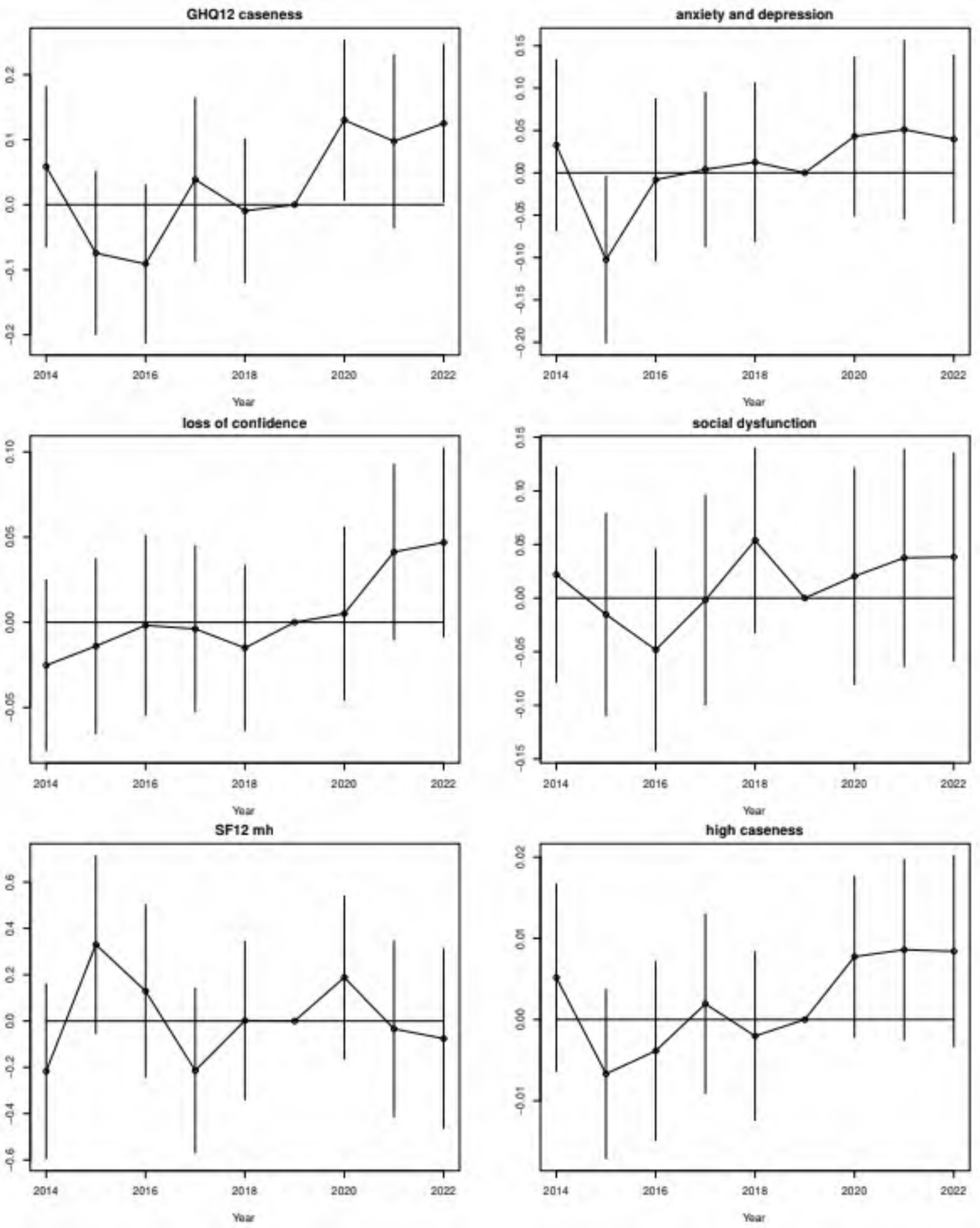
Notes: the Figure shows event studies for working remotely. For the doubly-robust estimator, we use the Callaway and Sant'Anna (2021). Propensity scores and the linear model for the dependent variable are functions of education, home ownership, having children, and being married in 2019. In the synthetic Differences-in-Differences (Arkhangelsky et al. 2021) estimator, we control for the same variables (details are provided in Appendix B).

Figure 4: Event studies for loneliness



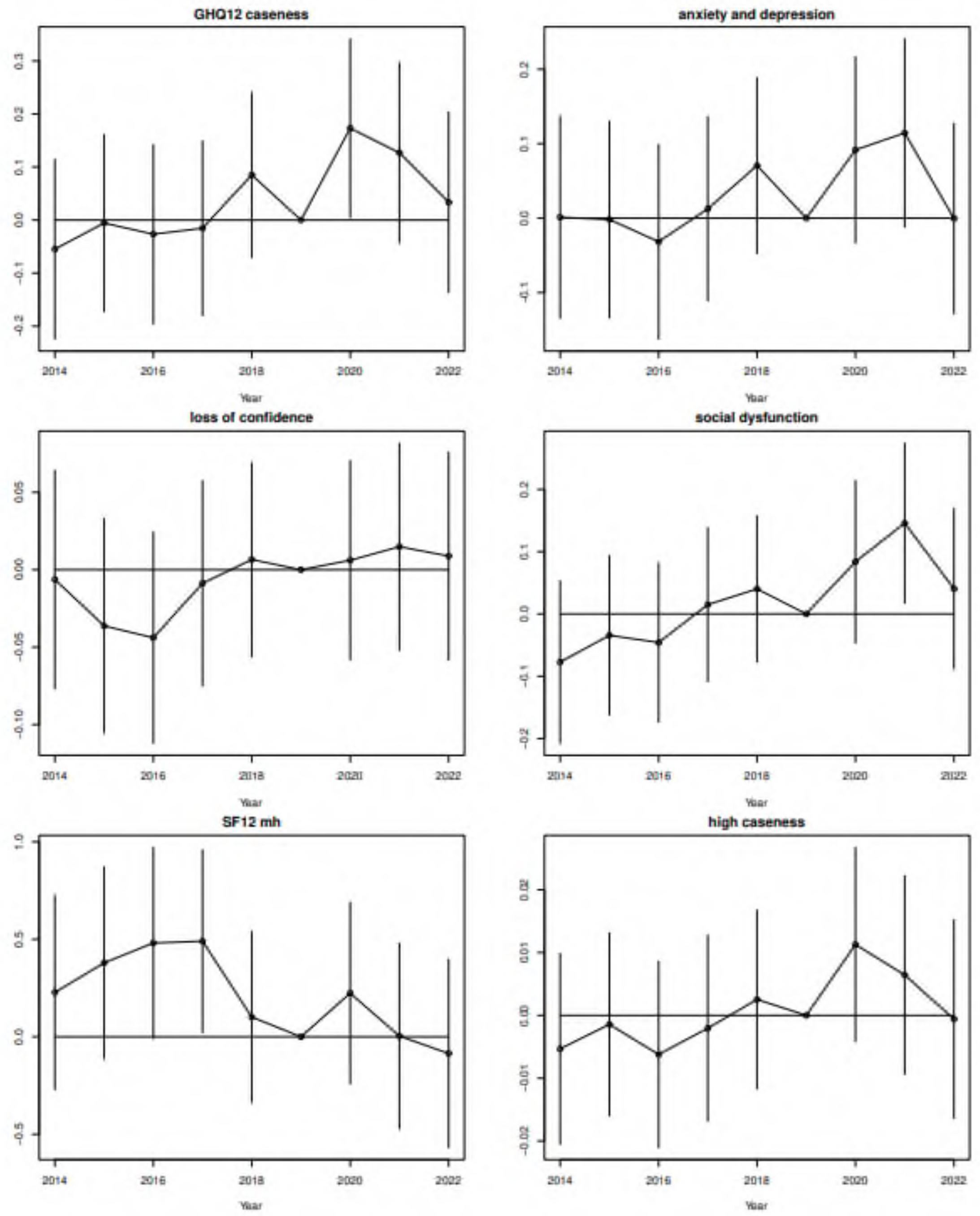
Notes: the Figure shows event studies loneliness. A person is lonely if they report being lonely "sometimes" or "often". They are "often lonely" if they report being lonely "often". The note to Figure 3 provides details about estimation.

Figure 5: Event studies for mental health outcomes, doubly robust



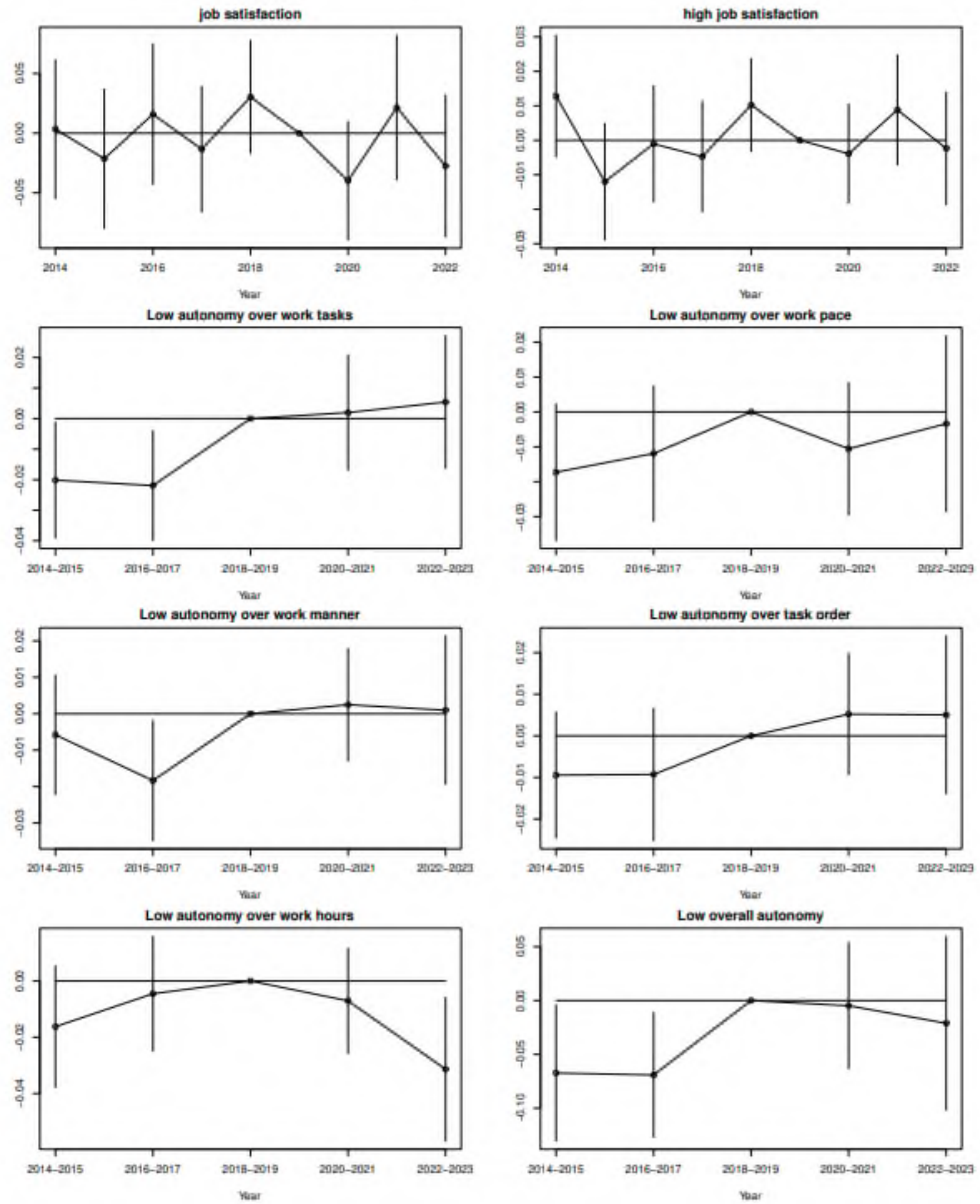
Notes: the Figure shows event studies for mental health outcomes. The GHQ12 caseness is the total number of adverse mental health symptoms a person has, from a possible 12. Anxiety and depression, loss of confidence and social dysfunction are subindices of the GHQ12 and measure the severity of symptoms of that kind. The SF12 is a measure of overall mental health, with higher numbers indicating better mental health. A person has a "high" caseness if their GHQ12 caseness score is higher than 8. The note to Figure 3 provides details about estimation.

Figure 6: Event studies for mental health outcomes, synthetic dd



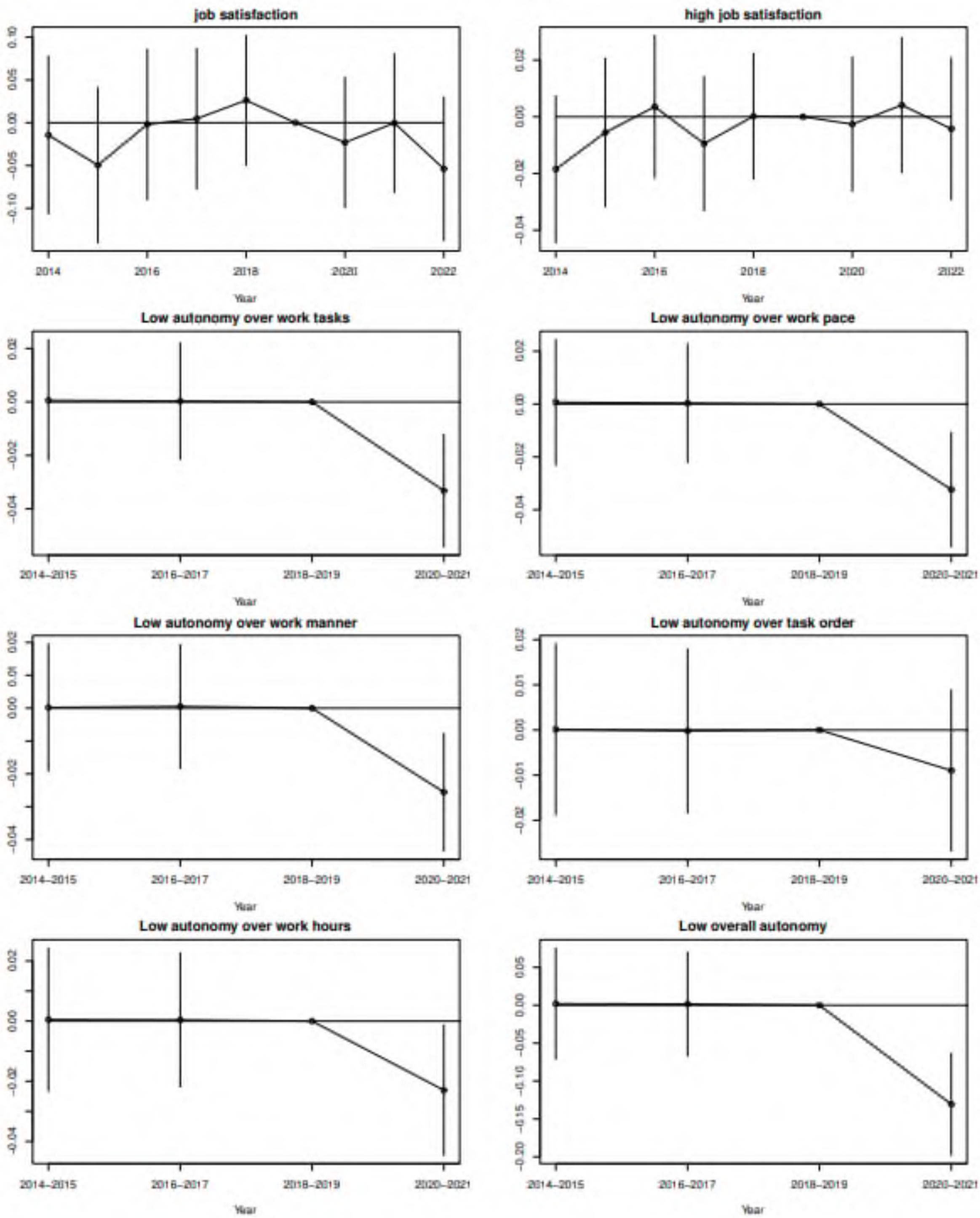
Notes: the Figure shows event studies for mental health outcomes. The Note to Figure 5 details the variable definitions. The note to Figure 3 provides details about estimation.

Figure 7: Event studies for job quality indicators



Notes: the Figure shows event studies for mental health outcomes. Job satisfaction is scored from 1 ("completely dissatisfied") to 7 ("completely satisfied"). A person has high job satisfaction if they have more than the median level of job satisfaction. A person has low autonomy over an aspect of work if they report having a little or no autonomy over that aspect of work. Overall low autonomy is the number of aspects of work-related autonomy a person scores low for, out of a possible 5. The note to Figure 3 provides details about estimation.

Figure 8: Event studies for job quality indicators, synthetic dd



Notes: the Figure shows event studies for mental health outcomes. The Note to Figure 7 details the variable definitions. The note to Figure 3 provides details about estimation.

Table 1: Sample summary statistics

Statistic	N	Mean	St. Dev	Min	Max
Age	181,536	42.728	13.118	15	92
Sex	181,541	0.532	0.499	0	1
Does paid work	181,142	0.853	0.354	0	1
Offered remote work	92,386	0.143	0.350	0	1
Use remote work	92,372	0.072	0.258	0	1
Teleworkable occupation in 2019	181,544	0.470	0.499	0	1
GHQ12 caseness	169,884	1.668	2.889	0	12
Anxiety and depression	170,307	7.477	2.464	4	16
Loss of confidence	170,468	3.121	1.327	2	8
Social dysfunction	170,130	12.412	2.129	6	24
High caseness	169,884	0.057	0.232	0	1
SF12 mental health	169,233	48.792	9.590	0.000	77.090
Job satisfaction	157,039	5.382	1.333	1	7
High job satisfaction	157,039	0.167	0.373	0	7
Is lonely	82,212	0.420	0.493	0	1
Is often lonely	82,212	0.059	0.237	0	1
Low autonomy over tasks	79,526	0.247	0.431	0	1
Low autonomy over work pace	79,491	0.229	0.420	0	1
Low autonomy over work manner	79,500	0.154	0.361	0	1
Low autonomy over task order	79,495	0.158	0.365	0	1
Low autonomy over work hours	79,502	0.469	0.499	0	1

Notes: the Table shows summary statistics for our sample. Our sample is of respondents who report working in 2019 in occupation to which teleworkability can be assigned. Paid work is a dummy variable for if a person does paid work. “Use remote work” is a dummy variable which is equal to 1 if a person uses the opportunity to work remotely. We assume that if a person does not work in a workplace which offers remote working they do not work. Teleworkable occupation in 2019 is a dummy variable which takes the value 1 if a person’s occupation in 2019 was teleworkable as measured by Dingel and Neiman (2020). The GHQ12 caseness is the number of symptoms of mental illness from a possible 12 that a person experiences. Anxiety and Depression, Loss of Confidence and Social Dysfunction measure the severity of symptoms of these types. SF12 indices are constructed from weighted responses to 12 health-related questions. They are scored from 100 (best) to 0 (worst). A person has high caseness if they have a GHQ12 caseness score above 8. Job satisfaction is a variable which ranges from 1 (completely dissatisfied) to 7 (completely satisfied). A person has high job satisfaction if they are completely satisfied (that is, they have more job satisfaction than the median person). A person is lonely if they report being lonely often or some of the time. A person is often lonely if they report being often lonely. A person has low autonomy over tasks, work pace, work manner, task order, or work hours if they report having “a little” or “none” of this kind of autonomy.

Table 2: Characteristics of workers in teleworkable and non-teleworkable occupations in 2019

	Non-teleworkable	Teleworkable
Loneliness and Mental Health		
Is lonely	0.410	0.380
Is often lonely	0.064	0.057
GHQ12 caseness	1.572	1.687
Anxiety and depression	7.421	7.582
Loss of confidence	3.179	3.188
Social dysfunction	12.369	12.436
High caseness	0.053	0.056
SF12 mental health	48.285	47.742
Covariates		
Age	43.262	44.227
Sex	0.470	0.595
Commute time	17.921	23.582
Has kids	0.151	0.211
Working hours	32.149	33.358
Monthly labour income	1, 999.051	2,760.420
Married	0.533	0.598
White	0.808	0.830
Owns home	0.710	0.820
Job quality measures		
Job satisfaction	5.399	5.442
High job satisfaction	0.187	1.165
Low autonomy over tasks	0.293	0.193
Low autonomy over work pace	0.269	0.187
Low autonomy over work manner	0.196	0.115
Low autonomy over task order	0.208	0.108
Low autonomy over work hours	0.533	0.379
Job level		
Management and professional	0.264	0.653
Intermediate	0.212	0.257
Routine	0.524	0.087
Education		
Degree or equivalent	0.374	0.636
A-level or GCSE	0.488	0.312
Other/No qualification	0.118	0.043

Notes: the Table shows the average values of key variables in 2019 for those working in teleworkable occupations and occupations which are not teleworkable. See note to Table 1 for a description of mental health variables and how we define occupation teleworkability. Commute time is in minutes. Working hours is the usual number of hours worked per week. Monthly labour income is expressed in 2015 GBP. Education variables are the highest qualification obtained. Job level is defined at the occupation level. “Has kids” is a binary variable if the person has biological children.

Table 3: Effect of expansion of remote work on propensity to remote work for those in teleworkable occupations

Use remote work		
	All	
Estimate	0.034***	0.055***
Standard error	(0.006)	(0.007)
Obs	120,756	39,696
P value pre-trend	0.119	0.997
Pre-treatment mean	0.059	0.066
	Men	
Estimate	0.023**	0.059**
Standard error	(0.012)	(0.010)
Obs	53,229	17,984
P value pre-trend	0.045	0.958
Pre-treatment mean	0.067	0.075
	Women	
Estimate	0.042***	0.051***
Standard error	(0.008)	(0.008)
Obs	67,527	21,692
P value pre-trend	0.810	1.000
Pre-treatment mean	0.052	0.058
Doubly Robust	Y	N
Synthetic dd	N	Y

Notes: the Table shows differences-in-differences estimates. We compare changes in propensity to remote work in years after 2020 for those in teleworkable occupations in 2019 to changes in propensity to remote work in the years after 2020 for those in occupations which are not teleworkable in 2019. We use the Callaway and Sant'Anna (2021) doubly-robust differences-in-differences estimator, where propensity scores and the linear model for the dependent variable are functions of education, home ownership, having children, and being married in 2019, and the Synthetic Differences-in-Differences (Arkhangelsky et al. 2021) estimator where we control for these same covariates (see Appendix B for details). ∗p<0.1; ∗∗p<0.05; ∗∗∗p<0.01

Table 4: Effect of expansion of remote work on loneliness for those in teleworkable occupations

	Lonely	Often lonely	Lonely	Often lonely
		All		
Estimate	0.023***	0.007*	0.039***	0.017**
Standard Error	(0.008)	(0.004)	(0.014)	(0.008)
Obs	132,914	132,914	11,100	11,100
P value pre-trend	0.434	0.460	0.342	0.169
Pre-treatment mean	0.371	0.075	0.353	0.057
		Men		
Estimate	0.002	0.003	0.028	0.003
Standard Error	(0.012)	(0.006)	(0.021)	(0.010)
Obs	58,519	58,519	5,184	5,184
P value pre-trend	0.210	0.900	0.206	0.449
Pre-treatment mean	0.316	0.061	0.300	0.042
		Women		
Estimate	0.031***	0.007	0.047**	0.026**
Standard Error	(0.011)	(0.006)	(0.020)	(0.013)
Obs	74,395	74,395	5,916	5,916
P value pre-trend	0.910	0.495	0.890	0.400
Pre-treatment mean	0.415	0.087	0.400	0.069
Doubly Robust	Y	Y	N	N
Synthetic dd	N	N	Y	Y

Notes: the Table shows differences-in-differences estimates. Estimators are described in the note to Table 3. A person is lonely if they report being lonely often or some of the time, and often lonely if they report being often lonely. ∗p<0.1; ∗∗p<0.05; ∗∗∗p<0.01

Table 5: Effect of expansion of remote work on mental health for those in teleworkable occupations, doubly-robust estimator

	GHQ12 caseness	High caseness	Anxiety and depression	Loss of confidence	Social dysfunction	SF12
	All					
Estimate	0.118**	0.008*	0.045	0.031	0.032	0.026
Standard Error	(0.055)	(0.005)	(0.041)	(0.021)	(0.042)	(0.147)
Obs	225,672	225,672	226,563	226,817	226,144	223,343
p-value pre-trend	0.165	0.411	0.240	0.463	0.663	0.324
Pre-treatment mean	1.717	0.064	7.391	3.148	12.537	48.922
	Men					
Estimate	0.061	0.012**	-0.007	0.038	-0.017	0.139
Standard Error	(0.071)	(0.006)	(0.058)	(0.033)	(0.059)	(0.217)
Obs	99, 073	99, 073	99, 443	99, 532	99, 242	98, 040
p-value pre-trend	0.171	0.175	0.197	0.012	0.138	0.033
Pre-treatment mean	1.388	0.048	7.050	2.980	12.374	50.166
	Women					
Estimate	0.130*	0.003	0.073	0.027	0.044	-0.034
Standard Error	(0.074)	(0.007)	(0.052)	(0.030)	(0.056)	(0.219)
Obs	126,599	126,599	127,120	127,285	126,902	125,303
p-value pre-trend	0.066	0.307	0.147	0.800	0.458	0.093
Pre-treatment mean	1.974	0.076	7.658	3.281	12.665	47.946

Notes: the Table shows differences-in-differences estimates. Estimators are described in the note to Table 3. Mental health variables are defined in the note to Figure 5. ∗p<0.1; ∗∗p<0.05; ∗∗∗p<0.01

Table 6: Effect of expansion of remote work on mental health for those in teleworkable occupations, synthetic differences-in-differences

	GHQ12 caseness	High caseness	Anxiety and depression	Loss of confidence	Social dysfunction	SF12
	All					
Estimate	0.114**	0.008*	0.060	0.025	0.107***	-0.233
Standard Error	(0.054)	(0.005)	(0.041)	(0.021)	(0.040)	(0.157)
Obs	52, 308	52,308	52,983	53,145	52,650	51,282
p-value pre-trend	0.670	0,857	0,712	0,621	0,527	0,256
Pre-treatment mean	1,529	0,053	7,365	3,058	12,353	49,251
	Men					
Estimate	0,138*	0.016**	0.080	0.032	0.199***	-0.236
Standard Error	(0.074)	(0.006)	(0.059)	(0.030)	(0.057)	(0.255)
Obs	22,725	22,725	23,022	23,094	22,797	22,266
p-value pre-trend	0.704	0.228	0.543	0.287	0.601	0.029
Pre-treatment mean	1.276	0.040	7.116	2.909	12.221	50.290
	Women					
Estimate	0.045	-0.001	0.008	0.009	0.015	-0.109
Standard Error	(0.077)	(0.007)	(0.057)	(0.030)	(0.057)	(0.221)
Obs	29,547	29,547	29,925	30,015	29,817	28,989
p-value pre-trend	0.924	0.802	0.861	0.715	0.770	0.750
Pre-treatment mean	1.722	0.062	7.558	3.172	12.453	48.456

Notes: the Table shows differences-in-differences estimates. Estimators are described in the note to Table 3. Mental health variables are defined in the note to Figure 5. ∗p<0.1; ∗∗p<0.05; ∗∗∗p<0.01

Table 7: Effect on job quality characteristics, doubly-robust estimator

	Job satisfaction	High Job satisfaction	Tasks	Work Pace	Low Autonomy Work Manner	Low Autonomy Task Order	Work Hours	Overall
				All				
Estimate	-0.015	0.001	0.004	-0.007	0.002	0.005	-0.019*	-0.013
SE	(0.022)	(0.006)	(0.006)	(0.010)	(0.008)	(0.008)	(0.011)	(0.032)
Obs	131,712	131,712	66,959	66,911	66,923	66,920	66,921	66,764
P value pre-trend	0.843	0.409	0.0001	0.021	0.011	0.084	0.147	0.0002
Pre-treat mean	5.376	0.177	0.245	0.236	0.155	0.162	0.476	1.273
				Men				
Estimate	-0.026	0.002	-0.006	-0.010	-0.004	-0.007	-0.018	-0.040
SE	(0.036)	(0.010)	(0.013)	(0.014)	(0.011)	(0.010)	(0.014)	(0.043)
Obs	60,797	60,797	30,929	30,910	30,912	30,905	30,900	30,843
P value pre-trend	0.824	0.808	0.019	0.092	0.024	0.082	0.146	0.003
Pre-treat mean	5.343	0.167	0.220	0.209	0.137	0.151	0.419	1.134
				Women				
Estimate	-0.013	0.001	0.015	-0.002	0.009	0.015	-0.020	0.019
SE	(0.031)	(0.009)	(0.012)	(0.012)	(0.011)	(0.010)	(0.014)	(0.041)
Obs	70,915	70,915	36,030	36,001	36,011	36,015	36,021	35,921
P value pre-trend	0.361	0.583	0.005	0.053	0.042	0.033	0.456	0.003
Pre-treat mean	5.404	0.186	0.267	0.258	0.171	0.172	0.526	1.393

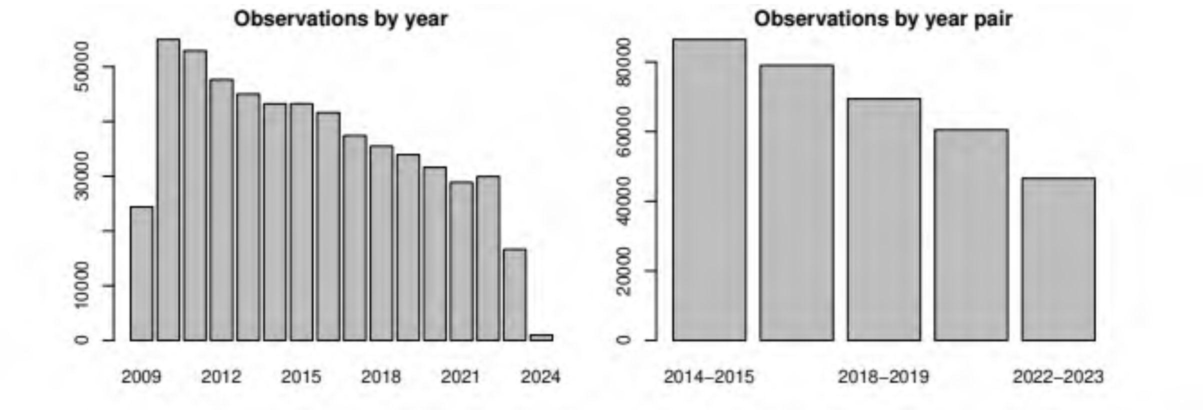
Notes: the Table shows differences-in-differences estimates. Estimators are described in the note to Table 3. Job quality variables are defined in the note to Figure 7. ∗p<0.1; ∗∗p<0.05; ∗∗∗p<0.01

Table 8: Effect on job quality characteristics, synthetic differences-in-differences

	Job satisfaction	High Job satisfaction	Tasks	Work Pace	Low Autonomy Work Manner	Low Autonomy Task Order	Work Hours	Overall
All								
Estimate	-0.020	0.004	-0.033***	-0.033***	-0.026***	-0.009	-0.023**	-0.131***
SE	(0.027)	(0.007)	(0.009)	(0.009)	(0.008)	(0.008)	(0.010)	(0.030)
Obs	37,134	37,134	29,124	29,060	29,068	29,028	29,052	28,864
P value pre-trend	0.709	0.523	0.999	0.998	0.999	0.999	0.999	0.998
Pre-treat mean	5.392	0.148	0.233	0.227	0.138	0.143	0.466	1.203
Men								
Estimate	-0.064	0.008	-0.055***	-0.049***	-0.048***	-0.023**	-0.053***	-0.236***
SE	(0.040)	(0.011)	(0.012)	(0.013)	(0.010)	(0.011)	(0.013)	(0.041)
Obs	16,461	16,461	13,324	13,320	13,316	13,284	13,292	13,232
P value pre-trend	0.572	0.604	0.997	0.981	1.000	0.980	0.992	0.088
Pre-treat mean	5.352	0.133	0.204	0.199	0.115	0.129	0.400	1.043
Women								
Estimate	0.010	0.002	-0.013	-0.019	-0.005	0.004	-0.001	-0.033
SE	(0.037)	(0.010)	(0.013)	(0.013)	(0.011)	(0.011)	(0.014)	(0.043)
Obs	20,637	20,637	15,784	15,724	15,740	15,732	15,748	15,620
P value pre-trend	0.953	0.654	0.998	0.969	0.978	0.987	0.999	0.978
Pre-treat mean	5.423	0.159	0.257	0.252	0.157	0.155	0.522	1.340

Notes: the Table shows differences-in-differences estimates. Estimators are described in the note to Table 3. Job quality variables are defined in the note to Figure 7. ∗p<0.1; ∗∗p<0.05; ∗∗∗p<0.01

Figure A.1: Distribution of time period variables within the Understanding Society data set



Notes: we show the count of observations by year and year pairs, within the UKHLS data set.

A. Distribution of time period variables within the data

In Figure A.1, we plot the number of observations in each time period in the Understanding Society data set, where the time period is defined both in terms of years and year pairs. The number of observations drops off significantly in 2023, and more significantly in 2024. There is also a reduction in observations in 2022-2023, although this fall is of a smaller magnitude. As we explain in the text, the sharp reduction in the later years is due to the fact that some interviews in the years 2023 and 2024 will be published in wave 15 (which is not currently available).

B. Synthetic Differences-in-Difference calculation

We follow Arkhangelsky et al. (2021), calculating unit weights as:

$$\min_{\omega} \Sigma_{t<2020}(\omega_0 + \Sigma_{i\in\Theta_c}\omega_i Y_{it}^{resid} - \frac{1}{N_t}\Sigma_{i\in\Theta_t} Y_{it}^{resid})^2 + \zeta^2 T_p||\omega||_2^2 \tag{B.1}$$

Here, ω is a vector of control unit weights indexed by i. Θ_c is the set of control units and Θ_t the set of treatment units. T_p is the number of periods before the treatment. $\zeta = (N_t T_t)^{1/4}\hat{\sigma}$, where T_t is the number of periods after the treatment and $\hat{\sigma}$ is the standard deviation of the residual of dependent variable in the pre-treatment period. Time weights are calculated as:

$$\min_{\lambda} \Sigma_{i\in\Theta_c}(\lambda_0 + \Sigma_{t<2020}\lambda_t Y_{it}^{resid} - \frac{1}{T_t}\Sigma_{t\geq 2020} Y_{it}^{resid})^2 \tag{B.2}$$

We can understand the unit weights as weighting the control units to minimize the sum of squared deviations from pre-trends and a penalty term which penalizes deviations from equal weights. The cost imposed on deviations from equal weights ensures the uniqueness of the solution and prevents overfitting. The time weights weight pre-treatment time periods to increase the weight on periods where the control units are most similar to their post-treatment behaviour.

We follow Kranz (2022) in residualizing the dependent variables using untreated observations. Specifically, we regress Y_{it} on control variables X_{it}, with time and person fixed effects, amongst only observations of people who are never treated or not yet treated. We then obtain the coefficient $\gamma\hat{}$, which we use to residualize all observations:

$$Y_{it}^{resid} = Y_{it} - X_{it}'\hat{\gamma} \tag{B.3}$$

The control variables we use are education in 2019, whether someone is married in 2019, whether someone has children in 2019, and whether someone owns their own home in 2019, interacted with a full set of time variables.

C. Propensity score weighting to explore attrition bias

Propensity-score weighting can correct for attrition bias if any association between treatment and attrition is explained by the observables included in the propensity score for being observed (Wooldridge 2002). We cannot strictly apply propensity score weighting to our estimators, because they are themselves weighted. However, we show the effect of weighting by the propensity score for being observed, in order to investigate whether there is selective attrition.

We specify our model for being observed in the post-treatment periods as below:

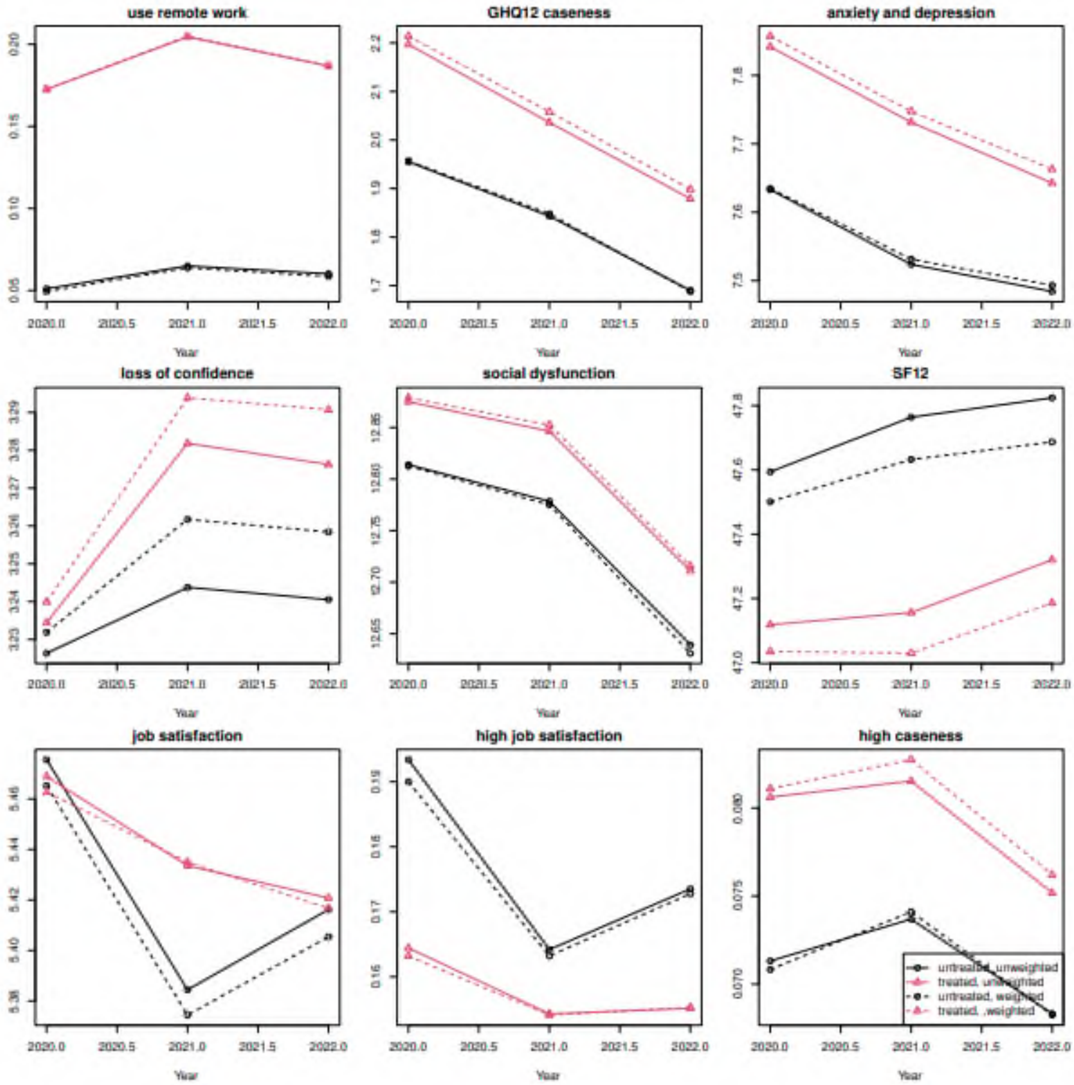
$$\Pr(o_{it} = 1 | X_{it}, t) = \Lambda(\Sigma_\tau 1(t = \tau) \times X_{it}'\alpha_\tau) \tag{C.1}$$

o_{it} is a variable which takes the value of one if person i is observed in period t and zero otherwise, and Λ is the logit function. X_{it} is a vector of predictors, including age in 2019, sex, marital status in 2019, education in 2019, whether a person has children in 2019, the “level” of their job in 2019 (management, intermediate, routine), their one-digit occupation in 2019, whether they are in the treatment or control group, and whether they own a home in 2019.

We estimate the model using maximum likelihood. The resulting model has a pseudo R-squared of 0.379. The correlation of the model probabilities with the binary variable o_{it} is 0.439.

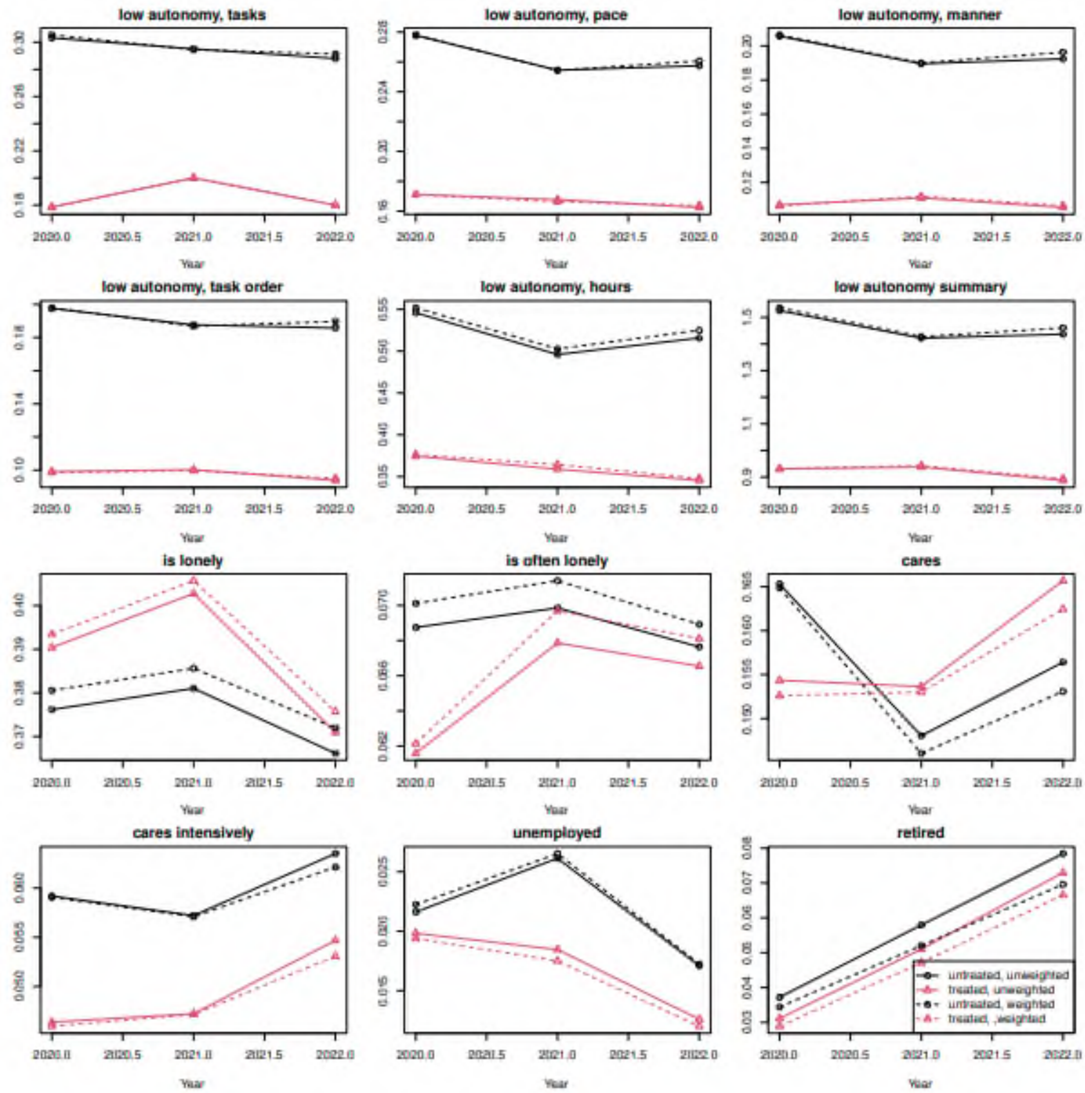
In Figures C.1 and C.2 we plot the average value of our dependent variables, in the treatment and control groups, unweighted and inversely weighted by their propensity scores for being observed i.e. we increase the weight on units that are statistically unlikely to be observed based on their observable characteristics. A stylized conclusion is that for the most part the propensity score weighting makes only a small difference to the average values, and tends to shift the treatment and control group by similar magnitudes. We interpret this to mean that it is unlikely that attrition bias drives our main results.

Figure C.1: Effect of inverse propensity score weighting on dependent variables in treatment and control group



Notes: we show the effect of weighting dependent variables by the inverse of the propensity score for being observed. We model the probability of being observed as a logit function of age, sex, marital status, education, having children, one digit occupation, job level and treatment status.

Figure C.2: Effect of inverse propensity score weighting on dependent variables in treatment and control group



Notes: we show the effect of weighting dependent variables by the inverse of the propensity score for being observed. We model the probability of being observed as a logit function of age, sex, marital status, education, having children, one digit occupation, job level and treatment status.

D. Effects on employment outcomes

The economic disruption of the pandemic may have caused changes in employment status which could confound our estimates. Since there are documented effects of unemployment (Gathergood 2013) and retirement (Spearing 2024) on mental health, we check whether there is evidence of an effect on these variables.

Results are shown in Table D.1. The notable result is that there appears to be a negative effect on the probability of women retiring in the SDD estimates. One possible interpretation is that women delayed retirement as a result of changing working conditions. However, we do not believe that this result drives our loneliness results, because the effect on retirement is notably smaller than the effect on loneliness, and because delayed retirement would be likely to decrease loneliness by preserving workplace relationships.

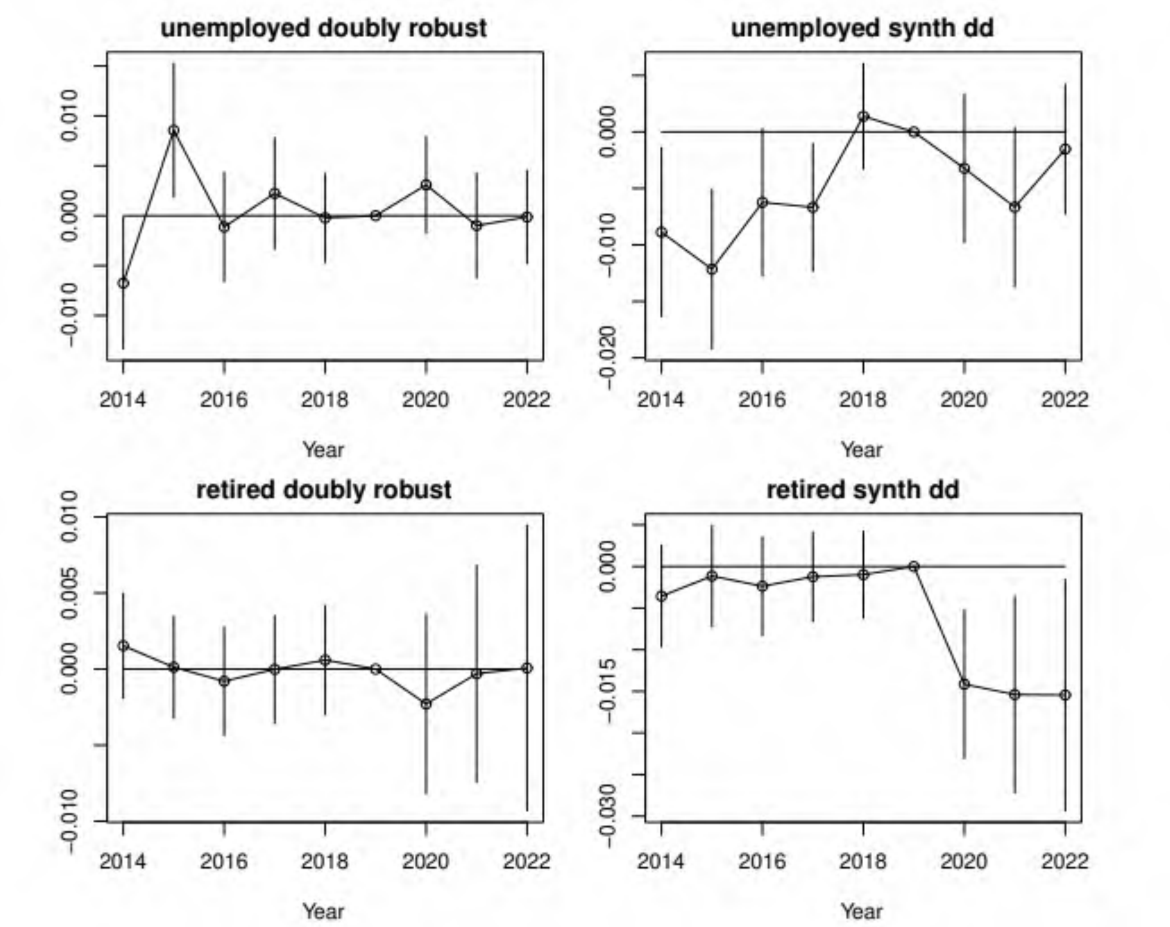
Many of the p-values for tests of significant pre-trends are significant. In order to assess whether these pre-trends drive our estimates, we examine the event studies in Figure D.1. In general, the significant pre-periods occur some time before 2019, suggesting that deviations from parallel trends do not drive the results.

Table D.1: Effect on labour market behaviour

	Unemployed	Retired	Unemployed	Retired
		All		
Estimate	0.001	-0.0001	0.002	-0.013***
Standard Error	(0.002)	(0.003)	(0.002)	(0.005)
Obs	236,225	236,225	58,995	58,995
p-value pre-trend	0.033	0.924	0.001	0.853
Pre-treatment mean	0.035	0.260	0.013	0.013
		Men		
Estimate	0.001	-0.003	-0.0005	-0.005
Standard Error	(0.003)	(0.006)	(0.003)	(0.008)
Obs	104,143	104,132	26,199	26,199
p-value pre-trend	0.168	0.121	0.058	0.470
Pre-treatment mean	0.038	0.261	0.014	0.016
		Women		
Estimate	-0.0005	-0.003	0.003	-0.020***
Standard Error	(0.002)	(0.004)	(0.003)	(0.007)
Obs	132,093	132,093	32,760	32,760
p-value pre-trend	0.037	0.528	0.009	0.967
Pre-treatment mean	0.032	0.259	0.012	0.012
Doubly robust	Y	Y	N	N
Synthetic dd	N	N	Y	Y

Notes: the Table shows differences-in-differences estimates. Estimators are described in the note to Table 3. ∗p<0.1; ∗∗p<0.05; ∗∗∗p<0.01

Figure D.1: Effect on labour market behaviour



Notes: event studies for the effect on unemployment and retirement. Estimators are described in the note to Table 3

E. Effect on caring propensity

In this section, we estimate the effect on caring. Caring might in principle be a driver of our baseline results, because those who have increased opportunities to work from home might use these opportunities to offer higher levels of informal care, which might have effects on loneliness or mental health. We define a person as caring if they report caring for someone they live with. We report them as caring intensively if they care for more than 10 hours a week.

Results are presented in Table E.1. All of our estimates are small and insignificantly different

Table E.1: Effect on caring variables

	Cares	Cares intensively	Cares	Cares intensively
All				
Estimate	0.003	0.006	0.002	-0.001
Standard Error	(0.006)	(0.004)	(0.007)	(0.004)
Obs	232,406	232,406	57,240	57,240
p-value pre-trend	0.468	0.308	0.699	0.844
Pre-treatment mean	0.178	0.073	0.157	0.046
Men				
Estimate	-0.0005	0.004	0.011	-0.002
Standard Error	(0.008)	(0.005)	(0.010)	(0.005)
Obs	101,954	101,954	25,047	25,047
p-value pre-trend	0.460	0.524	0.842	0.907
Pre-treatment mean	0.153	0.057	0.131	0.031
Women				
Estimate	0.005	0.007	-0.006	-0.001
Standard Error	(0.008)	(0.006)	(0.010)	(0.006)
Obs	130,452	130,452	32,148	32,148
p-value pre-trend	0.774	0.403	0.676	0.889
Pre-treatment mean	0.198	0.085	0.177	0.057
Doubly robust	Y	Y	N	N
Synthetic dd	N	N	Y	Y

Notes: the Table shows differences-in-differences estimates. Estimators are described in the note to Table 3. A person is a carer if they provide care for someone they live with. They care “intensively” if they care for someone more than 10 hours a week. ∗p<0.1; ∗∗p<0.05; ∗∗∗p<0.01

F. Effect on loneliness for unmarried individuals

We re-estimate our baseline results for loneliness amongst those who were unmarried in 2019. By necessity, we do not control for marital status in 2019 in this specification.

Results are presented in Table F.1. Consistent with our intuition, we find a larger effect on loneliness using the doubly robust estimator. The SDD estimator returns smaller and imprecise estimates, owing to the much smaller sample size. The 95% confidence interval for the SDD estimate of the effect on loneliness for all sexes runs up to 0.058. Overall, it is therefore likely that unmarried people were especially adversely affected by the expansion of remote work, but we cannot robustly estimate this effect with the smaller sample size and the SDD estimator.

Table F.1: Effect of expansion of remote work on loneliness for those in teleworkable occupations amongst people who were unmarried in 2019

	Lonely	Often lonely	Lonely	Often lonely
		All		
Estimate	0.036***	0.009	0.007	0.020
Standard Error	(0.012)	(0.008)	(0.026)	(0.018)
Obs	58,238	58,238	4,068	4,068
p-value pre-trend	0.456	0.717	0.876	0.069
Pre-treatment mean	0.499	0.123	0.477	0.103
		Men		
Estimate	0.014	0.012	0.017	-0.002
Standard Error	(0.023)	(0.013)	(0.044)	(0.026)
Obs	22,783	22,783	1,662	1,662
p-value pre-trend	0.624	0.708	0.566	0.395
Pre-treatment mean	0.455	0.112	0.426	0.094
		Women		
Estimate	0.043***	0.005	-0.001	0.031
Standard Error	(0.017)	(0.010)	(0.033)	(0.025)
Obs	35,455	35,455	2,406	2,406
p-value pre-trend	0.603	0.895	0.983	0.206
Pre-treatment mean	0.528	0.130	0.513	0.110
Doubly robust	Y	Y	N	N
Synthetic dd	N	N	Y	Y

Notes: we repeat our main estimates of the effect on loneliness (see Table 4) amongst people who were unmarried in 2019. ∗p<0.1; ∗∗p<0.05; ∗∗∗p<0.01

G. Heterogeneity by personality types

Our baseline results suggest a picture where the increase in opportunities for remote work caused a small, adverse effect on loneliness and mental health for those working in teleworkable occupations versus those working in occupations which are not teleworkable.

Our overall finding of a small adverse effect may obscure a range of heterogeneous effects. Personality is an ideal scale on which to measure heterogeneity, because the relative importance of additional freedom and loneliness are likely to be mediated by personality type.

In order to investigate heterogeneity by personality, we re-estimate our main specification separately for each dependent variable on groups who score “high” and “low” on each of the “big 5” personality measures. We define a person as scoring “high” if they score higher than the median score on this personality measure. Because the demands on the sample size increase when performing this heterogeneity analysis, we perform this analysis only using the doubly robust estimator (which has larger sample sizes because it does not require a balanced panel).

In Figure G.1, we examine heterogeneity in propensity to remote work by personality. The results exhibit heterogeneity by agreeableness and openness. Those who are highly agreeable or low in openness are more likely to increase their probability of remote work as a result of greater opportunities for remote work.

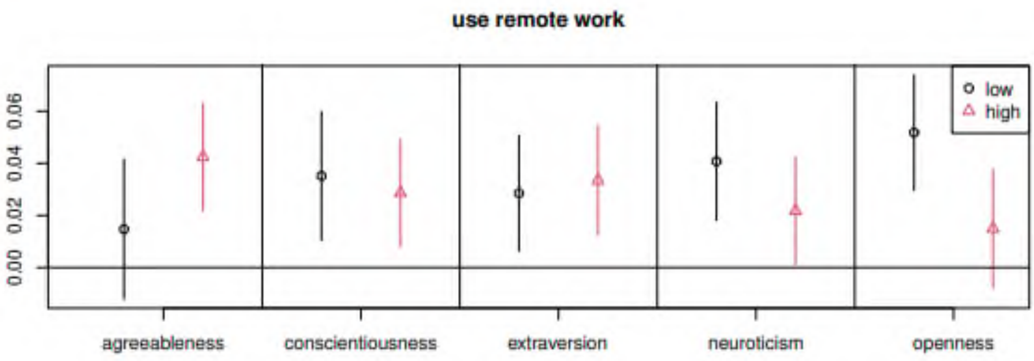
In Figures G.2 and G.3, we explore heterogeneity in the effect on loneliness. We note, first, a distinct lack of heterogeneity in the effect on ever being lonely. The magnitude of the effect is very similar for all personality types. The effect of increased opportunities for remote working increases loneliness for all personality types, either through being away from the office or by missing other colleagues who are working remotely. However, there is some evidence of heterogeneity by openness and neuroticism in the effect on being “often lonely”.

In Figures G.4 to G.8, we repeat this exercise for our main measures of mental health. Those who score high for agreeableness experience the greatest adverse effect, as measured by the GHQ12 caseness, symptoms of anxiety and depression, social dysfunction, and the SF12 measure. Interestingly, while our main results suggest no overall effect on the SF12 measure, disaggregated by personality suggests there may be an adverse effect for highly agreeable people and a beneficial effect for people with low agreeableness (although these effects are imprecisely estimated). We also see heterogeneity by neuroticism, with highly neurotic people seeing a larger effect on their caseness score, symptoms of loss of confidence, and symptoms of social dysfunction.

We show heterogeneous effects on job satisfaction in Figures G.9 and G.10. While our main results show no overall effect on job satisfaction, this finding may mask heterogeneous effects.

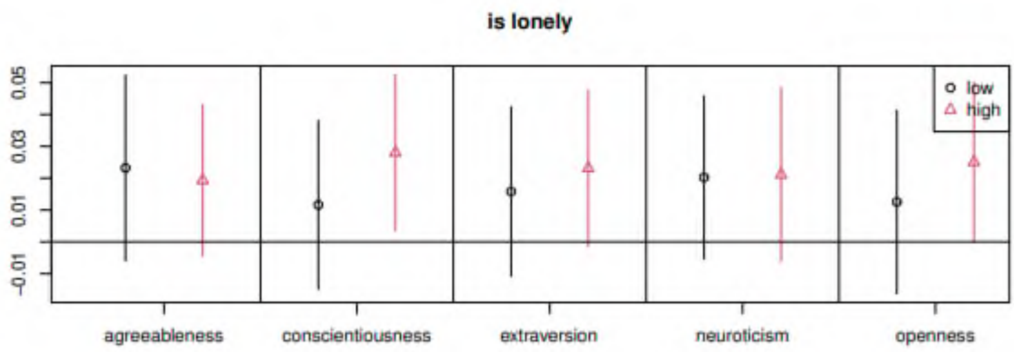
The probability of reporting high job satisfaction increases for those low in conscientiousness but increases for those high in conscientiousness. This result is consistent with the findings that workers have heterogeneous preferences over remote work (e.g., Appel-Meulenbroek et al. 2022; Bartik et al. 2024).

Figure G.1: Heterogeneity in the effect on using remote work



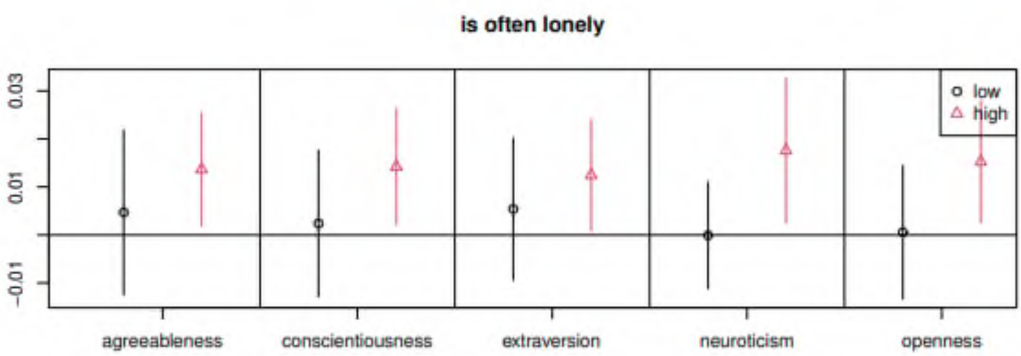
We re-run our baseline estimate with the doubly robust estimator, separately for those who score "high" and "low" on the big 5 personality traits. A person scores high on a trait if they score higher than the sample median.

Figure G.2: Heterogeneity in the effect on loneliness



We re-run our baseline estimate with the doubly robust estimator, separately for those who score "high" and "low" on the big 5 personality traits. A person scores high on a trait if they score higher than the sample median.

Figure G.3: Heterogeneity in the effect on being often lonely

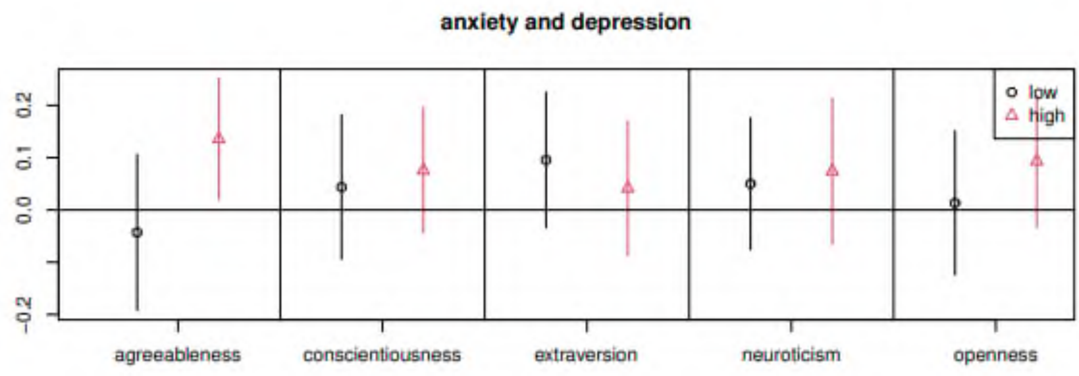


We re-run our baseline estimate with the doubly robust estimator, separately for those who score "high" and "low" on the big 5 personality traits. A person scores high on a trait if they score higher than the sample median.

Figure G.4: Heterogeneity in the effect on GHQ12 caseness

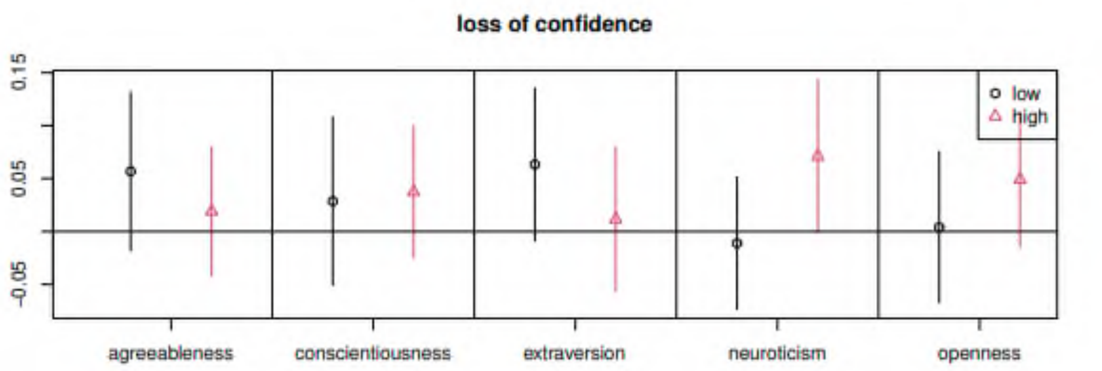
We re-run our baseline estimate with the doubly robust estimator, separately for those who score "high" and "low" on the big 5 personality traits. A person scores high on a trait if they score higher than the sample median.

Figure G.5: Heterogeneity in the effect on anxiety and depression



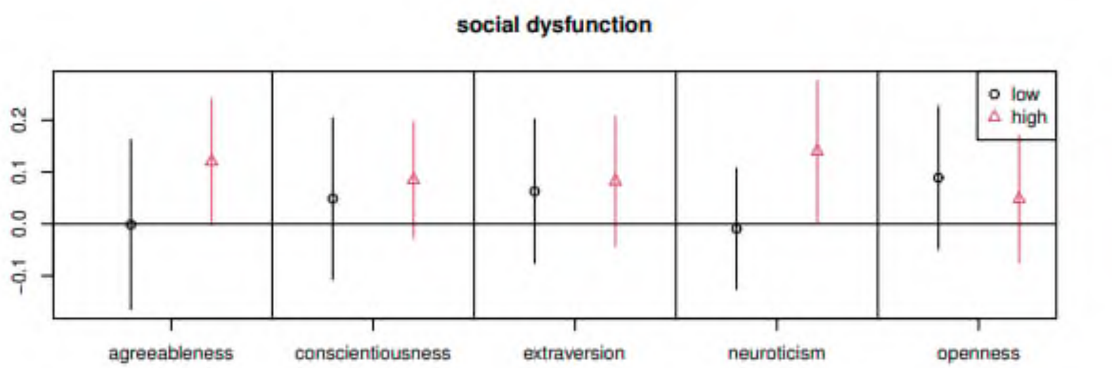
We re-run our baseline estimate with the doubly robust estimator, separately for those who score "high" and "low" on the big 5 personality traits. A person scores high on a trait if they score higher than the sample median.

Figure G.6: Heterogeneity in the effect on loss of confidence



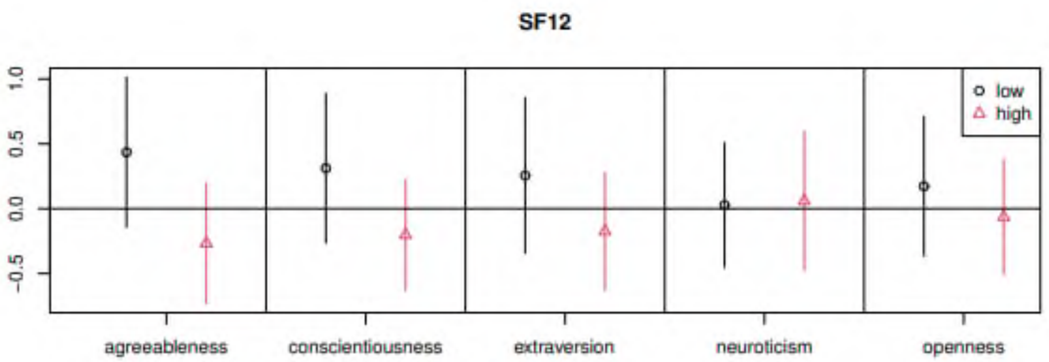
We re-run our baseline estimate with the doubly robust estimator, separately for those who score "high" and "low" on the big 5 personality traits. A person scores high on a trait if they score higher than the sample median.

Figure G.7: Heterogeneity in the effect on social dysfunction



We re-run our baseline estimate with the doubly robust estimator, separately for those who score "high" and "low" on the big 5 personality traits. A person scores high on a trait if they score higher than the sample median.

Figure G.8: Heterogeneity in the effect on SF12



We re-run our baseline estimate with the doubly robust estimator, separately for those who score “high” and “low” on the big 5 personality traits. A person scores high on a trait if they score higher than the sample median.

Figure G.9: Heterogeneity in the effect on job satisfaction

We re-run our baseline estimate with the doubly robust estimator, separately for those who score “high” and “low” on the big 5 personality traits. A person scores high on a trait if they score higher than the sample median.

Figure G.10: Heterogeneity in the effect on high job satisfaction

We re-run our baseline estimate with the doubly robust estimator, separately for those who score “high” and “low” on the big 5 personality traits. A person scores high on a trait if they score higher than the sample median.

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