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Abstract

The COVID-19 pandemic did not simply move work from offices to homes. It revealed a divide in which jobs could adapt when physical presence and fixed workplace routines suddenly became costly. This paper asks whether workers in more digitally ready occupations experienced different prescription-based mental-health treatment trajectories during and after this reorganization of work. We link Danish administrative registers for 2015–2023 to online job advertisements and follow white-collar workers who were employed in 2019 and aged 29–60 that year. We assign each worker the digital intensity of her 2019 four-digit occupation and hold this exposure fixed through 2023. The estimand is an intention-to-treat effect of baseline occupational digital readiness, not the effect of remote work chosen *ex post*. Workers in more digital occupations experienced relative declines in prescription-based mental-health treatment after 2019, with the clearest reductions in depression- and sleep-related prescriptions among women. For women’s antidepressant use, these declines persist and strengthen in 2022–2023. Event studies show little evidence of differential pre-trends for the main prescription outcomes. Standard labor-market outcomes, especially earnings, display pre-existing gradients without a discrete break at the pandemic onset. In contrast, work from home, commuting distance, health-care composition, and family outcomes move more sharply around 2020: realized work from home jumps in 2020, while work-from-home offers in vacancies continue to rise into the post-COVID period. The consequences of remote-work exposure for treated mental-health outcomes therefore appear context-dependent.

Keywords: Work from home; digitalization; COVID-19; mental health; occupational exposure; fertility; commuting.

JEL classifications: J22; J24; I12; I18; M54.

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1 Introduction

The COVID-19 pandemic was not only a health shock. It was also a large and sudden shock to the organization of work. Seemingly similar workers entered the pandemic in jobs that differed sharply in whether tasks could be performed digitally, whether work could continue away from the workplace, and whether the costs of commuting, fixed schedules, and physical presence could be reduced (Dingel and Neiman, 2020; Mongey et al., 2021). In the aggregate, the resulting rise in remote and hybrid work proved durable rather than transitory, remaining well above pre-2020 levels after restrictions ended (Barrero et al., 2021; Bick et al., 2023). Much less is known about whether the pandemic-era reorganization of work translated into lasting differences in workers’ mental health. This paper asks whether pre-pandemic occupational digital readiness shaped treated mental-health trajectories during and after COVID-19. We estimate corresponding changes in work organization, commuting, health-care use, and family outcomes to shed light on why mental-health prescriptions changed differently for workers whose jobs were more or less digitally prepared before the pandemic.

The implications of digitally adaptable work for mental health and mental-health treatment are theoretically ambiguous. Such jobs may reduce mental-health strain, and therefore the need for pharmacological treatment by preserving work continuity, reducing commuting, increasing schedule control, and making paid work easier to combine with family responsibilities; workers value precisely these amenities Mas and Pallais (2017). But the same technologies may increase mental-health strain by increasing isolation, weakening workplace interaction, blurring work-home boundaries, and intensifying the demands placed on workers who remain continuously reachable. Recent U.S. evidence on remote-work exposure points to exactly these costs: using pre-pandemic occupational capacity for remote work, Emanuel et al. (2026) find that the rise of remote work increased isolation and worsened mental health where it removed workplace contact without offsetting social support. Existing studies of WFH and well-being are correspondingly mixed, with effects that depend on gender, family structure, and whether remote work was chosen or imposed (Bertoni et al., 2021; Gueguen and Senik, 2023; Bilgrami, 2023). This underscores the importance of understanding how the consequences of a common technological shock are mediated by institutions, household constraints, and the margins along which workers and firms adjust.

Denmark provides a useful setting for studying digital readiness in a different institutional context. The shock was large, but it occurred in a labor market with universal, tax-financed health care, comparatively strong income stabilization, and a job-retention scheme that preserved many worker–firm matches (Kreiner and Svarer, 2022; Borgensgaard, 2022), together with a relatively rapid, age-sequenced reopening of childcare and lower-primary schooling (Lundtofte, 2021; OECD, 2020). Critically, the institutional environment shaped how a given level of digital readiness translated into realized work arrangements. Non-

critical public-sector employees faced stronger formal work-from-home mandates during the first lockdown, while private-sector adoption depended more on employer discretion, task feasibility, and production constraints (Søsted and Videbæk Munkholm, 2020; Hale et al., 2021). The public-private split therefore provides reduced-form variation in how the digital-readiness gradient differed across two institutional and organizational environments.

We combine Danish population and labor-market registers with individual-level prescription-dispensing records from the Danish Health Data Authority and records of health-care utilization. We link these data to the near-universe of online job advertisements and to the Danish Labour Force Survey. The linked register data follow workers annually from 2015 to 2023 and record employment, earnings, occupation, employer, family structure, dispensed prescriptions, health-care utilization, and commuting distances. The estimation sample consists of workers employed in 2019 in white-collar occupations (managers, professionals, technicians, and clerical workers) who were ages 29–60 in 2019, keeping the analysis away from the main schooling and retirement margins while focusing on occupations for which both online vacancy data and work-from-home arrangements are substantively relevant.

The vacancy data let us measure the digital intensity of each four-digit occupation before the pandemic, defined as the 2019 share of postings requiring digital skills, which we classify from posting text using a large-language-model framework Zhu and Hardt (2025) following the digital-skill taxonomy of Nania et al. (2019). This complements task- and vacancy-based measures of remote-work potential (Dingel and Neiman, 2020; Bamieh and Ziegler, 2022). We assign each worker the digital intensity of her 2019 occupation, standardize it across occupations, and hold this exposure fixed thereafter. The design compares within-worker changes among individuals who entered the pandemic in more versus less digitally intensive occupations, separately for the acute COVID period, 2020–2021, and the post-COVID period, 2022–2023.

Our identification strategy uses a continuous-exposure difference-in-differences design that interacts pre-pandemic occupational digital readiness with the pandemic shock. It shares the occupational-exposure logic of Emanuel et al. (2026), who compare changes in outcomes between workers in remotable and nonremotable occupations rather than between workers who choose different work arrangements. We instead compare within-worker changes across levels of digital intensity in workers’ 2019 occupations, holding exposure fixed thereafter. Identification requires that, absent the pandemic and the associated reorganization of work, workers in more and less digitally intensive baseline occupations would have followed parallel outcome trends after accounting for worker and year fixed effects. Under this assumption, the estimates have an intention-to-treat interpretation: they capture the differential consequences of the pandemic associated with baseline occupational digital readiness, rather than the causal effect of an individual’s adoption of remote work. Event studies pro-

vide the central diagnostic for assessing pre-pandemic trends and tracing the timing and persistence of subsequent changes.

Our main outcomes are prescription records for depression, sleep, anxiety, and substance-abuse medications. These capture contact with the health-care system and pharmacological treatment, not necessarily latent mental health. A decline in prescriptions could reflect lower underlying need, but it could also reflect changes in care-seeking, diagnosis, prescribing, or substitution toward non-pharmaceutical care. We therefore study prescriptions together with health-care utilization—general practitioner (GP) visits, online GP visits, GP-based talk therapy, mental-health screening, and psychologist and psychiatrist visits.

The first result is that workers in more digitally intensive baseline occupations experienced relative reductions in the use of several prescription-based mental-health treatments after 2019, with the clearest and most persistent effects among women. Event studies show little evidence of systematic differential pre-trends for depression, sleep, and substance-abuse prescriptions, lending credibility to the common-trends assumption, followed by relative declines after the onset of COVID. In the pooled-sector sample, the estimated effects of a one-standard-deviation increase in baseline digital intensity are reductions in women’s antidepressant use of 0.102 percentage points during 2020–2021 and 0.250 percentage points during 2022–2023; the latter equals 4.3 percent of the pre-2020 mean. The corresponding reductions in sleep-medication use are 0.080 and 0.124 percentage points. These are relative reductions against a background of rising antidepressant and sleep-medication use across levels of occupational digital readiness. The larger estimated antidepressant reduction in 2022–2023 shows that the relative difference was not confined to the acute pandemic period. Men show weaker and more outcome-specific effects, although public-sector men experience statistically significant relative declines in sleep-medication use in both periods. Relative reductions are observed in both sectors: public-sector effects during COVID span more outcomes and both sexes, while private-sector reductions are concentrated among women. For private-sector women, reductions in antidepressant and sleep-medication use are clearer after 2021, whereas substance-abuse prescriptions decline relatively in both periods.

The second result is that the timing of the mechanism evidence is consistent with a role for the reorganization of work. Wages and several labor-market outcomes differ by baseline digital intensity, but many of these differences predate COVID and evolve smoothly through 2020, providing limited support for these outcomes as explanations for the pandemic-timed prescription changes. Higher baseline occupational digital intensity is associated with a sharp relative increase in realized high-intensity WFH during 2020–2021 that partially recedes afterward, while the relative increase in the share of job postings mentioning WFH continues into 2022–2023. These distinct patterns suggest an immediate shift in work location alongside a more persistent change in advertised work arrangements. Among employed workers,

higher baseline occupational digital intensity is also associated with a relative increase after 2019 in the probability of having a workplace more than 10 km from home, with limited evidence of differential pre-trends. This is consistent with daily physical presence becoming less binding, although it does not establish reduced commuting frequency or residential relocation. The interpretation is consistent with the residential reorganization discussed by Ramani et al. (2024), but our distance measure cannot distinguish residential moves from changes in workplace location.

The third result is that the relative prescription declines are accompanied by changes in the composition of care and in family outcomes. The evidence for a pandemic-timed response differs across outcomes and subgroups. Raw GP visits, GP-based talk therapy, screening, and psychiatrist visits rise or remain elevated after 2019 in both the lower- and higher-readiness categories used in the descriptive figures (see also Grønkjær et al. (2025)). Against that background, higher baseline occupational digital intensity is associated with relative declines in total GP contact and some specialist mental-health services, alongside relative increases in online GP visits among women and in GP-based talk therapy concentrated in the private sector. This pattern is consistent with changes in the composition of care rather than a uniform decline across recorded services, but does not establish whether lower prescription use reflects lower underlying need or changes in access, care-seeking, or prescribing. Among women, higher baseline occupational digital intensity is associated with relative declines in divorce after 2019 and, in the public sector, relative increases in births. However, GP-based talk therapy among private-sector women, divorce among public-sector women, and marriage in the private sector display differential pre-trends, limiting a pandemic-onset interpretation of these estimates. The family results are consistent with changes in household time, family timing, and work-family compatibility, the margins emphasized in recent work linking work from home to higher fertility Aksoy et al. (2026), but these channels are not directly identified. We do not treat lower divorce or higher births as independent proof that latent mental health improved; pregnancy, childbirth, and breastfeeding can themselves alter prescribing.

The paper contributes to three literatures. First, it extends the digital-divide literature from access to technology toward access to digitally adaptable jobs: the inequality that mattered during COVID was not whether workers had internet access, but whether their jobs could be reorganized when physical presence became costly (van Dijk, 2020; Scheerder et al., 2017). Second, it contributes to work on remote work, job amenities, and well-being. Existing evidence shows that workers value flexibility and that WFH changes commuting, time use, and job design (Mas and Pallais, 2017; Pabilonia and Vernon, 2022), but its mental-health consequences remain contested (Emanuel et al., 2026; Bertoni et al., 2021); we show that predetermined occupational digital readiness reduced several prescription-based mental-health treatment outcomes in Denmark, especially for women. Third, it contributes to work

on gender, family constraints, and the post-pandemic revaluation of work (Alon et al., 2020; Michaels, 2024; Aksoy et al., 2026). The subgroup estimates are generally negative for professionals, workers with longer commutes, workers outside the largest cities, and married workers, while the estimates for workers living alone are mixed, with relative increases in some prescription outcomes among private-sector men.

More broadly, our findings suggest that access to digitally adaptable jobs may matter not only for how work is performed, but also for how workers organize their lives around it. The work-location and family results are consistent with a relaxation of spatial and household constraints, although these channels are not separately identified. The persistence of several prescription differences beyond the acute pandemic period suggests that the relevance of digital readiness extends beyond short-run work continuity. Unequal access to adaptable jobs may therefore have consequences for workers’ lives and mental-health treatment that are not captured by employment and earnings alone.

The remainder of the paper proceeds as follows. Section 2 describes the Danish institutional setting and discusses how pandemic policies, sector-specific work arrangements, and the organization of health care shaped the environment in which occupational digital readiness translated into realized work arrangements. Section 3 introduces the linked administrative, vacancy, and survey data and describes the construction of the occupational digital-readiness measure. Section 4 presents the empirical strategy. Section 5 reports the main evidence on treated mental-health outcomes and health-care utilization, including heterogeneity by baseline sector. Section 6 examines prescription initiation and cessation, followed by evidence on possible mechanisms related to labor-market adjustment, mobility, remote work, commuting distance, and family outcomes. Section 7 studies heterogeneity by baseline characteristics. Section 8 presents robustness analyses and additional health outcomes, and Section 9 concludes.

2 Institutional Background

2.1 COVID-19 policies and work arrangements

The COVID-19 pandemic generated a sharp but institutionally mediated shock to work, schooling, and household time allocation. Denmark combined early public-health restrictions with extensive income stabilization, a job-retention scheme designed to preserve worker–firm matches, and a comparatively rapid reopening of childcare and lower-primary schooling. These arrangements shaped how occupational digital readiness translated into realized work arrangements during the pandemic.

Denmark entered the pandemic with a flexicurity labor-market model: relatively flexible

employment protection in the OECD sense, combined with generous income insurance and active labor-market institutions (Kreiner and Svarer, 2022). During the pandemic, temporary job retention became a central policy instrument. The Danish wage-compensation scheme subsidized private firms that would otherwise have laid off at least 30 percent of their workforce, or more than 50 employees, provided that affected workers were retained on payroll. Compensation covered 75 percent of wages for salaried workers and 90 percent for hourly paid workers, subject to a monthly cap of DKK 30,000 per full-time employee. Furloughed workers remained on payroll but did not work during the compensation period, unlike workers who continued their tasks remotely. The scheme could therefore preserve employment relationships and limit earnings losses without preserving active work continuity. Borgensgaard (2022) estimates that the scheme prevented at least 10,400 exits from employment, though it also subsidized some matches that would not otherwise have been dissolved.

The United States relied on a different policy architecture. The CARES Act combined expanded unemployment insurance, direct household transfers, and the Paycheck Protection Program. The unemployment-insurance expansion included the Federal Pandemic Unemployment Compensation supplement of USD 600 per week; Ganong et al. (2020) show that this generated statutory replacement rates above 100 percent for a large share of eligible unemployed workers. Direct Economic Impact Payments provided up to USD 1,200 per eligible adult and USD 500 per qualifying child in the first round. The Paycheck Protection Program provided roughly USD 800 billion in forgivable loans to small businesses; Autor et al. (2022) estimate that it preserved roughly 2–3 million job-years, but at a cost of USD 169,000–258,000 per job-year retained. Thus, while the U.S. fiscal response was very large, it relied more heavily on unemployment insurance, direct transfers, and forgivable business loans, whereas Denmark’s primary labor-market instrument more directly preserved employment matches through wage compensation.

Non-pharmaceutical interventions also differed in ways that matter for remote work. Denmark introduced early national restrictions in March 2020. Non-critical public-sector employees were ordered to stay home during the first lockdown, while private-sector employers were strongly encouraged to facilitate work from home where feasible but generally retained greater discretion over implementation (Søsted and Videbæk Munkholm, 2020; Hale et al., 2021). This difference reduced employer discretion in the public sector, but does not by itself imply a weaker digital-readiness gradient in realized remote work: task feasibility could remain binding under either regime. The strength of that gradient is therefore an empirical question. Sector-specific estimates compare institutional and organizational environments alongside differences in worker and job composition; they do not isolate the effect of mandates.

School and childcare closures created a second source of heterogeneity. Denmark initially closed schools and childcare institutions as part of the first lockdown, but reopening was phased by age and grade. Daycare and schools for the youngest pupils reopened from mid-April 2020, beginning with early childhood education and care and primary grades up to grade 5, while older pupils remained in remote instruction for longer. The European Commission’s Joint Research Centre describes the Danish reopening as gradual and age-sequenced, starting with the youngest pupils and accompanied by distancing, handwashing, ventilation, and cleaning requirements (Lundtofte, 2021). OECD policy documentation similarly emphasizes the early reopening of daycare and primary schools (OECD, 2020).

This age-specific reopening matters for the interpretation of sex-specific estimates. School and childcare closures altered household time constraints, especially for parents of young children. At the same time, work from home had ambiguous implications: it could reduce commuting time and preserve work continuity, but it could also blur work-family boundaries and increase exposure to childcare demands during working hours.

Relative to the United States and many other countries, Denmark’s school closures were comparatively short for young children. In the United States, schools moved rapidly to remote instruction in spring 2020, but reopening decisions were decentralized across states and districts. National Center for Education Statistics (2022) reports that 77 percent of U.S. public schools moved classes to online distance-learning formats in spring 2020, and Fuchs-Schündeln (2022) documents that schooling time fell by approximately 55 percent in the United States from the onset of the pandemic to summer 2021. Globally, UNESCO (2021) estimates that schools were fully closed for an average of 14 weeks in the first pandemic year, rising to 22 weeks when localized closures are included. Denmark therefore belongs to the set of countries with early restrictions but comparatively limited complete closure for the youngest children. Appendix Table A.3 summarizes the policy environment along the margins most relevant for the empirical design: job retention versus active work, public-sector work-from-home mandates versus private-sector discretion, and the age profile of school and childcare closures.

The empirical estimates therefore capture the consequences of entering the pandemic in a more digitally ready occupation within this institutional environment, rather than the effect of remote work in isolation.

2.2 Mental-health care in Denmark

Denmark has a universal, tax-financed health-care system in which all registered residents are covered by public health insurance. General practitioners serve as the first point of contact and act as gatekeepers to most specialized care. Hospitals are predominantly public, while general practitioners and medical specialists typically operate in private practices contracted

with the public insurance system.

Mental-health care is organized as a stepped-care system. For common conditions such as depression and anxiety, patients typically first contact their general practitioner. The general practitioner may assess symptoms, conduct diagnostic or psychometric screening, prescribe medication, or refer the patient onward. Mild cases may be referred to psychotherapy, often provided by private-practice psychologists, while moderate cases may be treated through a combination of psychotherapy and medication. More severe cases may be referred to private-practice psychiatrists or psychiatric hospitals. Psychiatrists primarily treat moderate to severe cases, often through pharmacological treatment, while psychiatric hospitals treat the most severe patients and provide inpatient, outpatient, and emergency psychiatric care (Serena (2025)).

Prescription records capture treated mental-health conditions rather than latent mental health. Changes in prescriptions may reflect underlying morbidity, care-seeking and access, diagnosis, prescribing behavior, or substitution and complementarity between medication and other forms of care. We therefore study prescription outcomes together with broader measures of health-care utilization.

Danish studies are consistent with an acute first-lockdown disruption followed by adaptation, although patterns differ across services. Using hospital and emergency-medical-service records covering 46 percent of the population, Rømer et al. (2021) find lower psychiatric inpatient rates, with the largest decline three weeks into the first lockdown, but no statistically significant overall changes in referrals to mental-health services or suicidal behavior. Grønkjær et al. (2025) compare nationwide mental-health service use from March 2020 to June 2023 with counterfactual trends estimated from the pre-pandemic period. Psychiatric admissions and suicide attempts were below trend, while suicides, patients in private psychiatry or psychology practices, and referrals to psychiatric hospitals were not statistically different from trend. Psychotropic medication use increased. During the first lockdown specifically, psychiatric admissions, private-practice patients, and referrals were below trend. Survey evidence provides complementary context. Using a representative longitudinal survey of Danish adults, Thygesen et al. (2021) document a decline in average mental well-being and an increase in the share with low well-being between autumn 2019 and autumn 2020.

Digital care provided one avenue of adaptation. Denmark entered the pandemic with some existing digital mental-health infrastructure. Internetpsykiatrien, an internet-based cognitive behavioral therapy service for adults with anxiety and depression, had operated in the Region of Southern Denmark before being expanded nationally in 2018 (Mathiasen et al., 2018; Region Syddanmark, 2018). At the beginning of the pandemic, the Danish Organization of General Practitioners enabled video consultations through the *Min Læge* app, and a reimbursement agreement gave GPs a fee for providing video consultations.

During the March 2020 lockdown, video consultations rose abruptly from approximately zero to about 23,500 per week, before declining to roughly 6,000 per week after the lockdown Sandbæk et al. (2021). Register evidence for 2019–2021 confirms that video consultations were concentrated in the early pandemic period and later declined to a small but nonzero share of general-practice contacts; use was higher among younger, more educated, employed, and urban patients (Kirk et al., 2024).

The Danish Health Authority’s subsequent 10-year mental-health plan identified digital services as a priority and noted that COVID-19 highlighted their potential for people with mental disorders (Danish Health Authority, 2022).

This digital margin is relevant for our empirical design. Workers in more digital occupations may have been better positioned to use remote or flexible modes of care. The descriptive utilization patterns below do not suggest a broad post-2019 decline in measured health-care contact, but this does not rule out unmet need or differential barriers to particular services. The utilization outcomes help assess interpretations of the prescription results, but cannot fully separate changes in underlying mental-health need from changes in access or the composition of care.

3 Data and measurement

3.1 Administrative data

Our core data come from population registers maintained by Statistics Denmark (DST), which we link at the individual level using anonymized person identifiers. The *Integrated Database for Labour Market Research* (IDA) provides annual employer–employee matched information on each worker’s primary employment spell, measured in November of each year, including firm identifier, four-digit ISCO occupation code, employment status, and annual wage. Population registers provide demographic characteristics, including date of birth, sex, municipality of residence, and family structure.

3.2 Occupational level of digitalization

We measure occupation-level skill demand using online job postings from Jobindex, Denmark’s largest online job board. The posting data, obtained through the Danish consultancy firm Højbjerg Brauer Schultz (HBS Economics), span 2006–2023 and cover the near universe of Danish online job postings.

We classify each posting’s skill content using the framework developed in Zhu and Hardt (2025), which treats skill extraction as a multi-label classification problem solved with large language models (LLMs). The starting point is a training set of 986 Danish job postings

manually annotated by domain experts across 17 binary skill categories. Using these annotations, Zhu and Hardt develop a prompting strategy in which the LLM reads the full posting text and returns, for each category, a binary prediction together with a supporting text snippet that grounds the classification in the source material. To reduce false positives, the framework uses the model’s own confidence scores, measured by log probabilities: predictions below an optimized confidence threshold are reclassified as negative. A fine-tuned version of the model achieves 93 percent classification accuracy in three-fold cross-validation, compared with 73 percent for traditional keyword-matching methods. The improvement arises because LLMs can interpret context-dependent language, for instance distinguishing postings that require data-analysis skills from postings that merely mention data in passing. We apply the fine-tuned classifier to the full corpus of approximately 4.3 million Jobindex postings using OpenAI’s GPT-4.1 model.

We define a job as demanding a digital skill if the advertisement requests skills in at least one of the following skill categories: networking, data analysis, digital design, generative AI, AI, digital marketing, and programming/software.¹ The main exposure is postings-based occupational digital intensity, Dig_o , defined as the 2019 share of vacancies in occupation o that require at least one of these digital skills. In the empirical specifications, Dig_o is standardized across four-digit occupations. Coefficients therefore measure the effect of a one-standard-deviation increase in the digital intensity of the worker’s 2019 occupation.

Each worker is assigned a baseline four-digit occupation, o_{i0} , from the 2019 employment spell. Exposure is then defined using this baseline occupation. Thus, $Dig_{o_{i0}}$ is fixed at its pre-pandemic value. Workers remain in the sample regardless of subsequent firm, occupation, sector, or employment mobility. This baseline-assignment design avoids redefining exposure using post-treatment choices and gives the main estimates an intention-to-treat interpretation.

3.3 Mental Health

We supplement the administrative registers with individual-level prescription records obtained from the Danish Health Data Authority, which we merge to the population registers using anonymized person identifiers. These records cover prescriptions dispensed at Danish pharmacies and allow us to construct extensive-margin indicators equal to one if an individual fills a prescription in a given drug category during the year. We identify drug categories using ATC codes: anxiolytics (ATC3 = N05B), depression medications (ATC3 = N06A), sleep medications (ATC3 = N05C), gastrointestinal medications (ATC3 = A03B or ATC2 = A03), pain medications (ATC2 = N02), alcohol and substance-abuse treatments (ATC4

¹This approach generally follows the taxonomy of Nania et al. (2019)

= N07BB), and hypertension medications (ATC2 = C02–C09).

As discussed in Section 2.2, our prescription measures capture treated mental-health conditions rather than latent mental health. They identify cases in which symptoms lead to contact with the health-care system and pharmacological treatment. This distinction is important because changes in prescriptions may reflect changes in underlying morbidity, but may also reflect changes in care-seeking, diagnosis, prescribing behavior, or substitution and complementarity between medication and non-pharmacological treatment.

To help interpret prescription effects, we also examine public health-care utilization. These outcomes include GP visits, psychologist and psychiatrist visits, mental-health screening, GP-based talk therapy, and visits to dentists, rheumatologists, and physiotherapists. The Danish registers systematically record health-care services reimbursed through the public insurance system, including contacts with general practitioners, private-practice specialists, hospitals, and prescription drug purchases. However, fully self-paid psychotherapy and some privately insured psychological treatment are not observed in the public registers, because these services do not appear as public reimbursement claims Serena (2025). Together, the prescription and utilization outcomes allow us to distinguish changes in treated mental-health morbidity from broader changes in health-care use and access. Please see the appendix for a more detailed description of these data.

3.4 Work from home

We measure work-from-home potential and use in two complementary ways. The first is employer-side WFH availability, WFH_{ot}^{JP} , defined as the occupation-year share of job postings that advertise work-from-home or remote-work options. The second is realized individual measure of WFH from the Danish Labour Force Survey (AKU), which asks whether respondents worked from home during the last month and distinguishes high-intensity home work (More than half the hours) from lower-intensity arrangements (any). The two measures capture distinct margins: postings reflect the design of newly posted jobs, while AKU captures actual work arrangements among employed workers.

3.5 Sample selection

We focus on the years 2015–2023, inclusive. Our estimation sample consists of workers employed in 2019 in white-collar occupations—managers, professionals, technicians, and clerical workers—who were between ages 29 and 60 in 2019. This restriction keeps the sample away from the main schooling and retirement margins while allowing us to follow workers during and after the COVID-19 shock. These occupations are well represented in online job advertisements and are the relevant set for studying both digital skill demand and

work-from-home arrangements.

3.6 Descriptive statistics

We begin by describing baseline differences across low- and high-digital occupations, then show the full distribution of the continuous digital-readiness measure, and finally plot raw trends for prescription outcomes, health-care utilization, work-from-home measures, and commuting distance. The descriptive splits use the 2019 baseline sample and are intended to summarize selection into more digitally ready occupations before the pandemic. The main estimates continue to use the continuous standardized exposure.

Table 1 reports baseline characteristics by sector and by low versus high occupational digital readiness. By construction, high-digital occupations have substantially higher measured digital readiness. The average digitalization level is 0.436 in the private sector and 0.381 in the public sector, compared with 0.087 and 0.056 in the corresponding low-digital groups. Workers in low- and high-digital occupations are similar in age, but differ in gender composition. High-digital occupations are less female in both sectors, especially in the private sector, where women account for 38.9 percent of workers in high-digital occupations compared with 58.2 percent in low-digital occupations. Wages are also higher in high-digital occupations, especially in the private sector: average hourly wages are 313 DKK in high-digital private-sector occupations compared with 250 DKK in low-digital private-sector occupations. In the public sector, the corresponding difference is smaller, 243 versus 232 DKK. Commuting and work-from-home rows show that high-digital occupations also differed in baseline work organization. Workers in high-digital occupations had longer commuting distances in both sectors, and were more likely to report any WFH in 2019; however, baseline differences in high-intensity WFH—working from home more than half of hours—are small.

Baseline prescription rates are generally somewhat lower in high-digital occupations, particularly in the private sector. For example, depression prescriptions are 4.9 percent among private-sector workers in low-digital occupations and 3.5 percent among those in high-digital occupations. In the public sector, the corresponding difference is smaller, 6.2 versus 5.8 percent. The broad occupational composition also differs across groups. In the private sector, high-digital occupations contain larger shares of managers and professionals and fewer clerical and customer-service workers. In the public sector, high-digital occupations include fewer professionals but more clerical and customer-service workers relative to low-digital occupations. These differences underscore that occupational digital readiness is correlated with baseline worker and job characteristics, which motivates the fixed-effects event-study design used in the main analysis.

Figures A.1 and A.2 plot raw mean prescription rates, measured in percentage, by baseline occupational digital readiness separately for women and men. These figures are descriptive

and should be read as unconditional trends rather than estimates of the effect of occupational digital intensity. They nevertheless provide useful context for the main event-study analysis. For women, low-digital occupations have higher prescription rates than high-digital occupations throughout the period for all four outcomes. This difference is especially visible for depression and sleep prescriptions. Depression prescriptions rise steadily after 2019 in both groups, while the level remains higher among women in low-digital occupations. Sleep prescriptions show a similar post-2019 increase. Anxiety prescriptions decline from 2015 to 2020 and then partially recover, again with persistently higher rates in low-digital occupations. Substance-abuse prescriptions are much less common in levels but increase sharply after 2019 in both groups, with the low-digital group remaining above the high-digital group. The patterns for men are broadly similar. Men in low-digital occupations generally have higher prescription rates than men in high-digital occupations across the full period. Depression prescriptions increase after 2019 for both groups, with the increase somewhat larger among men in low-digital occupations. Anxiety prescriptions fall through 2020 and then partly recover. Sleep prescriptions rise after 2020 in both groups, while the gap between low- and high-digital occupations remains sizable. Substance-abuse prescriptions also rise around and after 2020, though from a low baseline, and remains higher among men in low-digital occupations.

Appendix Figures A.3 and A.4 report raw trends in health-care utilization by baseline occupational digital readiness. GP visits rise around the pandemic onset and remain above 2019 levels through 2023 in both digital-readiness groups, among women and men. GP-based talk therapy, mental-health screening, and psychiatrist visits also show an overall upward pattern over the post-2019 period. Psychologist visits do not share this broad upward pattern, while dental visits fall temporarily in 2020 and subsequently recover. These annual patterns document continued recorded service use, not whether access kept pace with need. Utilization could rise less than need, or access could worsen relatively in one group, even while use increases in both. The raw trends therefore provide context for the prescription estimates but do not rule out an access-related contribution to the relative prescription declines.

The median split in Table 1 is useful for summarizing baseline differences, but it necessarily compresses a more continuous occupational measure. Figure 1 therefore shows the worker-weighted distribution of baseline occupational digital exposure, where exposure is measured at the four-digit occupation level. The figure documents substantial dispersion in pre-pandemic digital readiness. At the upper end of the distribution are occupations in which digital tools and technical skills are central to job tasks, including telecommunications engineering professionals, computer network professionals, architecture, infrastructure, and design work, software and applications development and analysis, and ICT user support tech-

nicians. In these occupations, between 89 and 100 percent of 2019 vacancies required at least one digital skill. At the lower end are occupations with no measured digital requirements in 2019 postings, including public benefits administration, senior officials in legislative bodies, occupational health and hygiene professionals, production management in agriculture and forestry, word-processing clerks, and announcers on radio, television, and other media. Together, the examples at the top and bottom of the distribution show that the measure captures pre-pandemic occupational digital task content rather than simply distinguishing broad sectors, education groups, or white-collar status.

Next we turn to work from home, a potential work-organization change through which occupational digital readiness may have mattered during and after the pandemic. If the digital-readiness measure captures occupations better able to reorganize work remotely, then WFH should increase more strongly in high-digital occupations after 2020. Appendix Figure A.5 plots raw trends for realized and posted work-from-home measures by baseline occupational digital readiness. The first two rows use realized WFH measures from the AKU, while the third row uses the postings-based measure of whether vacancies mention WFH arrangements. The raw trends show a sharp pandemic-era increase in realized WFH, especially in high-digital occupations and especially for working from home more than half of hours. The broader measure of any WFH shows a larger pre-existing gap, with high-digital occupations consistently more likely to involve home work and an additional widening during the pandemic. The postings-based measure rises more gradually but persistently after 2020, particularly in high-digital occupations, suggesting that remote-work arrangements became increasingly embedded in vacancy design after the initial Covid shock.

Appendix Figure A.6 complements the work-from-home evidence by plotting raw trends in commuting distance by baseline occupational digital readiness. This measure captures the distance between workers' residence and workplace, rather than the frequency with which workers physically commute. The figure shows that workers in high-digital occupations have longer commuting distances than workers in low-digital occupations throughout the sample period, for both women and men. Importantly, commuting distance does not decline after the onset of the pandemic. Instead, distances are broadly stable or increasing from 2020 onward, with particularly clear upward movement among men in high-digital occupations. For women, commuting distance also trends upward over the period, and the high-digital group remains substantially above the low-digital group throughout. These raw patterns suggest that the pandemic-era increase in work from home was not accompanied by a shift toward shorter-distance job matches among workers in digitally intensive occupations. Rather, the evidence is consistent with remote-work arrangements reducing the importance of daily physical presence and perhaps expanding the underlying spatial separation between home and workplace.

4 Empirical strategy

Our empirical strategy exploits the pandemic-induced reorganization of work and differences in occupations’ pre-pandemic digital readiness. We use a continuous-exposure difference-in-differences design to compare within-worker changes among workers who entered the pandemic in more versus less digitally intensive occupations. This shares the occupational-exposure logic of Emanuel et al. (2026), who compare outcome changes between workers in remotable and nonremotable occupations rather than according to individuals’ realized remote-work choices.

Let o_{i0} denote worker i ’s four-digit occupation in 2019. Each worker is assigned the pre-pandemic digital intensity of this baseline occupation, $Dig_{o_{i0}}$, standardized across four-digit occupations; the assignment is then held fixed regardless of any subsequent changes in occupation, firm, sector, or employment status. Exposure is therefore predetermined with respect to the pandemic shock and is never updated on the basis of choices made from 2020 onwards. This avoids defining exposure using work arrangements that may themselves respond to changes in mental health.

Under the identifying assumption stated in Section 4.1, the estimates have an intention-to-treat interpretation: they capture the differential consequences of the pandemic and subsequent reorganization of work associated with baseline occupational digital readiness. This reduced-form effect may operate through several channels: the capacity to work from home, commuting requirements, job continuity, task autonomy, household constraints, health-care access, or broader patterns of occupational mobility. It should not be interpreted as the causal effect of an individual’s adoption of remote work.

4.1 Event-study specification

We begin with event-study specifications. These are the primary diagnostic for the design and show the timing of the effects. The event study shows whether outcomes were already trending differentially by baseline occupational digital intensity before COVID, whether there is a break at the pandemic onset, and whether any divergence persists into the post-COVID period.

The event-study specification is

$$Y_{it} = \sum_{\tau \neq 2019} \gamma_{\tau} (Dig_{o_{i0}} \times \mathbb{1}\{t = \tau\}) + \alpha_i + \lambda_t + \varepsilon_{it}. \quad (1)$$

Here, Y_{it} is the outcome for worker i in year t , and the omitted year is 2019. Worker fixed effects absorb time-invariant differences across workers, including permanent differences associated with occupational sorting. Year fixed effects absorb aggregate shocks common to

all workers. Standard errors are clustered by the 2019 four-digit occupation.

The coefficients γ_τ measure year-specific differential changes relative to 2019 associated with a one-standard-deviation increase in baseline occupational digital intensity. The identifying assumption is that, absent the pandemic shock and the associated reorganization of work, workers in more and less digitally intensive 2019 occupations would have followed parallel outcome trends after accounting for worker and year fixed effects. Pre-2020 coefficients that are close to zero and show no systematic trend lend credibility to this assumption. Coefficients for 2020 onwards show whether a differential response emerges at the pandemic onset and whether it persists into the post-COVID period. We discuss how event-study timing informs the mechanism analysis in Section 4.4.

We estimate sex-specific event-study specification in two steps. First, we pool workers across baseline sector to show the overall timing of the relationship between occupational digital intensity and each outcome. Second, we estimate the event studies separately by sex and baseline sector. Gender and sector are substantively important margins: household time demands differed by sex, while WFH was more policy-determined in the public sector and more discretionary in the private sector.

4.2 Difference-in-differences summary

After examining the timing in the event studies, we summarize the effects using a difference-in-differences specification with separate coefficients for the acute COVID and post-COVID periods:

$$Y_{it} = \beta_C (Dig_{o_{i0}} \times Covid_t) + \beta_P (Dig_{o_{i0}} \times PostCovid_t) + \alpha_i + \lambda_t + \varepsilon_{it}, \quad (2)$$

where $Covid_t$ equals one in 2020–2021 and $PostCovid_t$ equals one in 2022–2023, and both equal zero otherwise. The reference period is 2015–2019. Worker and year fixed effects and the clustering of standard errors are the same as in equation (1).

The coefficients β_C and β_P are continuous-exposure difference-in-differences coefficients. They compare within-worker changes in each period relative to 2015–2019 across levels of baseline occupational digital intensity, with magnitudes expressed per one-standard-deviation increase in exposure. Since $Dig_{o_{i0}}$ is fixed within worker, its level is absorbed by the worker fixed effect. Identification relies on the same parallel-trends assumption as the event-study specification. Because exposure is continuous, two distinct assumptions are involved (Callaway et al., 2024). The first is the parallel-trends assumption stated in Section 4.1: absent the pandemic, prescription use in occupations at each level of baseline digital intensity would have evolved in parallel with that in occupations with no measured digital requirements in 2019 postings (a non-trivial set; see Section 3.6). Under this assumption, the differential

post-2019 change at each exposure level relative to zero-exposure occupations is identified. Interpreting the slope coefficients β_C and β_P as the average causal response to a marginal increase in exposure additionally requires a “strong” parallel-trends assumption: the counterfactual evolution of prescription use must not depend on the level of exposure, so that occupations at different baseline levels of digital intensity would have experienced the same average change had they instead shared a common level of exposure. Without it, the linear coefficient combines the causal response to exposure with heterogeneity in effects across exposure levels. Under strong parallel trends, the two-way fixed-effects coefficient is a weighted average of average causal responses across exposure levels, with weights determined by the distribution of baseline exposure in Figure 1. We therefore read β_C and β_P as summaries of the exposure gradient in post-2019 changes rather than as structural dose-response parameters, and we complement them with a coarse binary comparison that does not impose linearity in dose (Section 8.1).

This specification is a parsimonious summary of the event-study profiles. The coefficient β_C summarizes the acute pandemic period, while β_P summarizes the post-COVID period, when formal restrictions had largely ended but work arrangements continued to adjust. The specification is therefore useful for quantifying magnitudes, comparing outcomes, and reporting heterogeneity by sex and baseline sector. We interpret these estimates alongside the event-study evidence on pre-pandemic trends and the timing and persistence of subsequent changes.

4.3 Prescription initiation and cessation

A natural question is whether fewer workers enter treatment, more workers exit treatment, or both. To examine this margin, we decompose annual prescription use into transitions into and out of treatment.

For each medication category m , let X_{it}^m denote an indicator equal to one if individual i fills at least one prescription in category m in year t . We define treatment initiation as

$$Start_{it}^m = 1\{X_{i,t-1}^m = 0, X_{it}^m = 1\},$$

and treatment discontinuation as

$$Stop_{it}^m = 1\{X_{i,t-1}^m = 1, X_{it}^m = 0\}.$$

This exercise has a simple accounting interpretation. Changes in the stock of prescription use can be written as

$$X_{it}^m - X_{i,t-1}^m = Start_{it}^m - Stop_{it}^m.$$

We estimate the same event-study specification as in the baseline analysis, replacing the prescription-use indicator with the corresponding initiation and discontinuation outcomes.

Our preferred specifications use unconditional transition indicators as dependent variables. That is, $Start_{it}^m$ and $Stop_{it}^m$ are defined for all workers, rather than estimating initiation only among prior non-users and discontinuation only among prior users. This preserves the population-level interpretation of the mechanism exercise.

These measures capture annual transitions in recorded prescription use. Initiation may include resumption after a prescription-free year, while cessation denotes a year without a recorded prescription following a year with use; neither measure directly identifies first-ever treatment, treatment duration, or permanent discontinuation.

4.4 Potential mechanisms

The mechanism analysis uses the same baseline-assignment design as the main estimates. We estimate equation (1) for candidate channels and examine their trajectories before and after the pandemic. Departures from pre-pandemic trends provide more informative evidence of pandemic-related adjustment than a smooth continuation of pre-existing differences. Since the event-study coefficients are normalized to 2019, an ongoing differential trend can generate nonzero post-2019 coefficients without a new pandemic response.

Timing is a diagnostic, not a requirement that a mechanism and prescription use follow the same annual pattern. Mechanisms may operate with lags, and temporary changes can have lasting consequences. For example, a temporary reduction in treatment initiation may affect prescription prevalence in subsequent years. We therefore do not require a channel to move contemporaneously with prescriptions or to persist for as long. These comparisons provide suggestive evidence about possible channels, but do not identify their separate causal contributions.

4.4.1 Labor-market dynamics and mobility

We first examine hourly wages, employment in November, days worked, and switching across occupations, sectors, and firms.

Because exposure is assigned using the worker’s 2019 occupation and held fixed thereafter, post-2020 occupation, firm, sector, or employment changes do not mechanically redefine treatment. These mobility responses are part of the intention-to-treat estimand: digital readiness may affect mental health precisely by reducing the need to switch firms, sectors, or occupations, or by allowing workers to remain attached to more adaptable work arrangements.

Conditioning on post-treatment mobility can introduce selection. If workers in more dig-

ital occupations are differentially retained, or if workers in less digital occupations are more likely to move into other jobs, then stayer samples can be informative about whether the main estimates are driven by reallocation. We therefore treat mobility outcomes as both potential channels and diagnostics. The preferred estimates remain the baseline-assignment intention-to-treat effects, while stayer specifications are used as robustness checks in Section 8.1.

4.4.2 Work from home and commuting distance

We next examine realized WFH from the AKU and postings-based WFH from vacancy text, together with an indicator for whether the workplace is more than 10 km from home, conditional on being employed.

4.4.3 Family outcomes

We also examine family outcomes as a complementary set of mechanisms. WFH may change the organization of household life by reducing commuting time, increasing schedule flexibility, and altering the compatibility of paid work with family responsibilities. These changes could affect marital stability and fertility decisions, and they may also directly influence mental-health needs or the likelihood that distress translates into prescription treatment. We focus on marriage, divorce, and births as margins that capture changes in household formation, relationship stability, and family timing.

5 Results

The organizing question is whether workers who entered the pandemic in more digitally ready occupations experienced different treated mental-health trajectories. We first report the main prescription estimates and their heterogeneity by baseline sector, then examine recorded health-care utilization to help interpret these effects. Within each part, event studies show the timing of the response and provide evidence on pre-pandemic trends, while difference-in-differences estimates summarize magnitudes during 2020–2021 and 2022–2023. Section 6 then examines prescription initiation and cessation, followed by potential channels involving labor-market adjustment, work location, and family outcomes.

5.1 Main prescription outcomes

Figure 2 reports the main event-study estimates from equation (1) for depression, sleep, substance-abuse, and anxiety prescriptions, separately by sex. Throughout this section, the

coefficients should be interpreted as the differential effect associated with a one-standard-deviation increase in the digital intensity of the worker’s 2019 four-digit occupation. The figure is the central diagnostic for the research design. In particular, we use it to assess three features of the results: whether there is little evidence of differential pre-trends before the pandemic, whether there is a clear break at the onset of Covid, and whether any relative effects persist into the post-Covid period. If high- and low-digital occupations were already on differential mental-health trajectories before the pandemic, the post-2020 estimates would be difficult to interpret causally. Instead, the pre-2020 event-study coefficients are generally close to zero and show little evidence of statistically meaningful differential pre-trends for the main prescription outcomes. This is particularly clear for depression, sleep, and substance-abuse prescriptions. Anxiety is noisier and less informative.

After 2020, the event studies show a clear relative divergence by baseline occupational digital intensity. For women, the depression-prescription coefficients move downward after the pandemic onset and become increasingly negative in the post-Covid years. This pattern is important because it shows that the digital-readiness gradient is not confined to the initial lockdown period. The relative decline continues into 2022 and 2023, after formal restrictions had largely ended but remote and hybrid work arrangements were still being renegotiated. Sleep prescriptions display a similar downward profile for women, though the estimates are somewhat less smooth. Substance-abuse prescriptions also decline relatively for women in more digital occupations, especially in the post-Covid period, although the baseline level of this outcome is very low. Anxiety prescriptions do not show the same clean pattern.

The male event-study profiles are weaker. For depression, the post-2020 coefficients are negative but smaller than for women. For sleep and substance-abuse prescriptions, the male estimates are closer to zero. For anxiety, the male profile is modestly negative in some years, but the pattern is not strong enough to support the same interpretation as for women’s depression and sleep outcomes. Overall, Figure 2 shows that the protective digital-readiness gradient in treated mental health is primarily a female result, with more limited and mostly marginal evidence for men.

Table 2 quantifies these patterns using equation (2). As above, each coefficient is the estimated effect of a one-standard-deviation increase in baseline occupational digital intensity. Focusing first on workers in both the public and private sectors, shown in Panel A, women in more digital occupations experience statistically significant relative reductions in depression, sleep, and substance-abuse prescriptions during both the acute Covid period and the post-Covid period. The depression coefficient for women is -0.102 percentage points during 2020–2021 and -0.250 percentage points during 2022–2023, both significant at the 1 percent level. Relative to the pre-2020 mean of 5.880 percent, these estimates correspond to reductions of 1.7 percent during Covid and 4.3 percent after Covid. The post-Covid estimate

is therefore larger both in absolute terms and relative to baseline use.

For men, the pooled depression estimate is smaller and only marginally significant in the post-Covid period. The post-Covid coefficient is -0.093 percentage points, relative to a pre-2020 mean of 3.271 percent, implying a relative effect of about -2.8 percent. This is the clearest evidence of a male reduction in the main prescription table, but it is weaker than the corresponding estimate for women.

Sleep prescriptions show a similar gender pattern. For women, the Covid-period estimate is -0.080 percentage points, significant at the 5 percent level, and the post-Covid estimate is -0.124 percentage points, significant at the 1 percent level. These imply relative effects of -3.0 percent and -4.6 percent. For men, the corresponding estimates are negative but not statistically significant. Substance-abuse prescriptions also decline for women: the pooled estimates are -0.025 percentage points during Covid and -0.050 percentage points post-Covid, both significant at the 1 percent level. Because the pre-2020 mean is only 0.100 percent, these effects are large in proportional terms. They should therefore be interpreted as evidence of a meaningful relative decline on a very low baseline. Anxiety prescriptions estimates are less stable. The female estimates are small and statistically insignificant, while men show small negative estimates, including a marginally significant post-Covid effect of about -2.9 percent relative to the pre-period mean.

Taken together, the main prescription results point to three conclusions. First, there is little evidence that workers in occupations with different levels of digitalization were on different mental-health trajectories before the pandemic, which supports the causal interpretation of the post-2020 estimates. Second, the event studies show a clear break at the onset of Covid in the relative effect of being in a more digital occupation, particularly for women, with significant declines in the use of several mental-health prescriptions. Third, several of these effects persist into the post-Covid period and, for women’s depression prescriptions in particular, become larger after 2021. Overall, the evidence suggests that entering the pandemic in a more digitally intensive occupation reduced prescription-based mental-health treatment, especially for women, and that these effects persisted beyond Covid.

5.2 Sector heterogeneity in prescription outcomes

Figures 3 and 4 split the event studies from equation (1) by baseline sector. This split is substantively important because the mapping from digital readiness to actual work arrangements differed between the public and private sectors. Public-sector workers were more directly affected by formal work-from-home mandates, while private-sector remote work depended more on employer discretion, task feasibility, furlough arrangements and production constraints.

In the public sector, Figure 3 shows relative post-2020 declines in depression and sleep

prescriptions for workers in more digitally intensive 2019 occupations, especially among women. As in Figure 2, the pre-2020 coefficients show little evidence of systematic differential pre-trends. Panel B of Table 2 summarizes these results. For public-sector women, the depression coefficient is -0.125 percentage points during Covid and -0.264 percentage points post-Covid, both statistically significant. These correspond to relative effects of -1.9 percent and -4.1 percent. Sleep prescriptions also decline relatively during Covid, with a coefficient of -0.126 percentage points for women, significant at the 1 percent level, or -4.6 percent relative to the pre-2020 mean; the post-Covid estimate for women is negative but not statistically significant. For public-sector men, the clearest result is for sleep prescriptions: the Covid-period estimate is -0.115 percentage points and the post-Covid estimate is -0.125 percentage points, both statistically significant, implying relative effects of -5.5 percent and -6.0 percent. Public-sector anxiety prescriptions also decline relatively during the Covid period for both women and men, with both estimates statistically significant, though the post-Covid estimates are weaker.

In the private sector, Figure 4 again shows little evidence of systematic pre-trends, followed by relative post-2020 declines for women, especially in substance-abuse prescriptions. Panel C of Table 2 presents the difference-in-differences estimates and shows that the relative reductions in depression and sleep prescriptions for women are smaller during the acute Covid period but become more visible after Covid. Private-sector women have a post-Covid depression coefficient of -0.119 percentage points, significant at the 5 percent level and equal to -2.4 percent relative to the pre-2020 mean, and a post-Covid sleep coefficient of -0.074 percentage points, marginally significant at the 10 percent level and equal to -3.1 percent. The strongest private-sector prescription result is for substance-abuse treatment among women. The Covid-period estimate is -0.031 percentage points and the post-Covid estimate is -0.062 percentage points, both significant at the 1 percent level. Because baseline substance-abuse treatment is rare, the proportional effects are large. For private-sector men, the evidence is weaker and more mixed. Depression and anxiety estimates are generally negative but small and not statistically significant, while substance-abuse prescriptions increase slightly during Covid and are marginally significant in the opposite direction.

Taken together, the sector split shows that the female mental-health gradient is not mechanically generated by one sector alone. More precisely, in the public sector, digital readiness is associated mainly with statistically significant relative declines in women’s depression prescriptions, women’s Covid-period sleep prescriptions, men’s sleep prescriptions, and Covid-period anxiety prescriptions for both sexes. In the private sector, digital readiness is associated mainly with statistically significant relative declines in women’s post-Covid depression and sleep prescriptions and women’s substance-abuse prescriptions in both periods. We next examine whether these sector-specific prescription patterns are accompanied by

differences in recorded health-care utilization.

5.3 Health-care utilization and interpretation of prescription effects

Building on the descriptive evidence in Section 3.6, we examine relative changes in other recorded services alongside the prescription outcomes. The aim is to characterize the composition of care, not to infer access directly from utilization levels. Figure 5 reports event-study estimates from equation (1) for GP utilization, online GP visits, GP-based talk therapy, online talk therapy, mental-health screening, and online mental-health screening, pooling workers across baseline sectors. For most outcomes, the pre-2020 coefficients provide limited evidence of systematic differential trends by baseline occupational digital intensity.

After the pandemic onset, total GP utilization declines relatively for workers in more digitally intensive occupations, particularly for women. These event-study coefficients measure differential changes relative to 2019, net of worker and year fixed effects, rather than absolute changes in utilization. At the same time, the event-study estimates indicate a relative increase in online GP visits after the pandemic began, particularly for women. GP-based talk therapy also increases relatively after 2019, again more clearly for women. The online talk-therapy estimates are noisier and do not provide strong evidence of a statistically significant relative increase. Screening and online screening do not show robust or persistent effects.

The sector-specific estimates in Table 3 show that the talk-therapy increase is concentrated in the private sector. This sector-specific pattern should be interpreted more cautiously, however, because talk therapy among private-sector women displays negative pre-period coefficients relative to the omitted 2019 year. Thus, in the private sector, the post-2020 talk-therapy estimates are best interpreted as evidence that relatively higher use continued after Covid, rather than as a clean Covid-induced break beginning in 2020.

Figure 6 provides additional context. In Panel A, psychologist and psychiatrist visits show relative post-2020 declines, particularly for women. Online psychologist contacts also decline relatively among women, indicating that the increase in online GP contact does not extend uniformly across online services. Panel B of Figure 6 helps distinguish the mental-health-specific utilization response from broader changes in health-care use. Dentist visits increase relatively over time for workers in more digitally intensive baseline occupations, especially among women. Importantly, however, this pattern does not begin sharply in 2020. The pre-2019 coefficients are negative and move steadily toward zero, indicating that high-digital workers were already on an upward relative trend in dental-care use before the pandemic. The post-2020 estimates therefore suggest that Covid did not reverse or disrupt this underlying trend; rather, relative dental-care use continued to rise after 2019. This pattern is informative because it moves in the opposite direction of the prescription and spe-

cialist mental-health results. It suggests that the main findings are not driven by a general post-2020 reduction in all health-care contact among workers in more digitally intensive occupations. Substantively, the continued relative increase in dentist visits may reflect greater flexibility to schedule routine care, higher take-up of preventive care, or broader self-care differences among high-digital workers, particularly women. Physiotherapy shows a short-run relative increase around 2020 followed by relative declines in later years, while rheumatologist visits are close to zero and imprecisely estimated throughout the post-2020 period. Taken together, Panel B supports the interpretation that the prescription results reflect changes in treated mental health and the composition of mental-health-related care, rather than a broad reduction in health-service utilization.

Table 3 quantifies these utilization changes using equation (2). As above, each coefficient is the estimated effect of a one-standard-deviation increase in baseline occupational digital intensity. In the pooled sample shown in Panel A, higher baseline occupational digital intensity is associated with lower relative GP use for both sexes. For women, the Covid-period coefficient is -0.117 visits, significant at the 5 percent level, and the post-Covid coefficient is -0.070 visits, significant at the 10 percent level. For men, the corresponding estimates are -0.062 and -0.060 visits, significant at the 1 and 5 percent levels, respectively. These effects are modest relative to baseline GP use. At the same time, the estimated effect on GP-based talk therapy is positive for women: the coefficient is 0.0051 during Covid, significant at the 1 percent level, and 0.0045 post-Covid, significant at the 5 percent level. Relative to the pre-2020 mean of 0.101, these imply increases of about 5.1 percent and 4.5 percent. Men do not show a comparable pooled increase in talk therapy. GP mental-health screening shows no robust relative effect.

Mental-health specialist use also declines relatively. For women, a one-standard-deviation increase in baseline occupational digital intensity reduces psychiatrist visits in both periods, with the Covid-period and post-Covid estimates both significant at the 5 percent level; the pooled post-Covid effect is -0.0061 visits, or about -10.6 percent relative to the pre-period mean. Psychologist visits also decline for women, with both pooled estimates significant at the 1 percent level and a post-Covid coefficient of -0.0155 visits, or -8.4 percent relative to baseline. For men, psychologist visits decline modestly, with the Covid-period estimate significant at the 5 percent level and the post-Covid estimate marginally significant at the 10 percent level, while the psychiatrist estimates are smaller and not statistically significant. These patterns indicate that the relative decline in prescription use caused by higher baseline occupational digital intensity is accompanied by lower relative use of some specialist services, but not by a uniform decline in all mental-health-related care.

The sector split in Table 3 is also informative. In the public sector, the estimates indicate that a one-standard-deviation increase in baseline occupational digital intensity led to lower

relative GP use during Covid and lower relative specialist use after Covid, with the specialist effects concentrated among women. The Covid-period coefficients for GP visits are negative and statistically significant for both public-sector women and men. On the other hand, the coefficients for public-sector talk therapy are not statistically significant in either period. Public-sector women also show statistically significant post-Covid reductions in psychiatrist and psychologist visits.

The private-sector estimates point to a different utilization pattern. Among workers employed in the private sector in 2019, higher baseline occupational digital intensity is associated with a statistically significant relative increase in GP-based talk therapy. For women, the coefficient is 0.0082 during Covid and 0.0096 post-Covid, both significant at the 1 percent level, corresponding to increases of 9.3 percent and 10.8 percent relative to the pre-2020 mean. Private-sector men also show positive talk-therapy effects in both periods, both significant at the 5 percent level, though the magnitudes are smaller. This increase in talk therapy should not be interpreted as a general increase in health-care utilization. In the same private-sector sample, the estimated effects on total GP visits are negative, especially post-Covid, and psychologist visits decline significantly for women in both periods. The private-sector evidence therefore suggests a change in the composition of care rather than a uniform increase or decrease in utilization: workers in more digital occupations make relatively less use of general GP care and psychologist services, while making relatively more use of GP-based talk therapy.

The utilization evidence weighs against a simple explanation in which the relative prescription declines reflect a broad loss of contact with health-care providers. Although total GP use and some specialist services decline relatively, online GP contact increases among women, and GP-based talk therapy increases particularly in the private sector. The prescription results therefore occur alongside continued—and, for some services, relatively greater—engagement with care. This pattern does not rule out service-specific access barriers, but it does not support a uniform contraction in recorded care.

6 Mechanisms

6.1 Prescription initiation and cessation

We next examine whether the relative decline in prescription use is associated with fewer annual entries into recorded use, more annual exits, or both. This decomposes the prescription outcome rather than identifying an underlying channel. Fewer starts indicate fewer transitions from a prescription-free year into use; higher cessation indicates more transitions out of recorded use in the full sample. In the private sector, where higher baseline digital intensity

is also associated with increased GP-based talk therapy, fewer starts would be consistent with some mental-health needs being addressed through less medication-intensive forms of care, subject to the pre-trend qualification discussed above.

Figure 7 examines whether the relative decline in prescription use is associated with fewer treatment starts among workers in more digitally intensive occupations. For women, depression-prescription starts fall after 2020, with the largest relative reductions appearing in the post-Covid years. Sleep-prescription starts display a similar relative decline, and substance-abuse starts also fall after 2020. Anxiety starts are noisier and do not display the same clear pattern. For men, the estimates for prescription starts are generally flatter and less systematically related to baseline occupational digital readiness.

Figure 8 examines the corresponding event-study estimates for prescription cessation. For women, depression-prescription cessation increases after 2020, and the point estimates are similar in absolute magnitude to the decline in depression starts in some years, particularly around 2021. However, the cessation estimates are less precisely estimated and less persistent, with the relative increase weakening by 2023. For sleep prescriptions, cessation does not show a comparable sustained relative increase, while substance-abuse cessation is noisy because baseline use is rare. Anxiety cessation is also imprecisely estimated.

Taken together, the start-stop decomposition suggests that the relative decline in treated prescription use among women in more digital occupations reflects both margins for depression: fewer annual entries into recorded use and, at least temporarily, more annual exits. For sleep and substance-abuse prescriptions, the evidence points more clearly toward reduced initiation rather than increased cessation.

This distinction is useful in our setting because treatment starts and treatment continuation are likely to capture different margins of health-care interaction. Starting a depression prescription typically requires a new clinical assessment, diagnosis, and treatment decision, whereas continuation of an existing prescription may be less dependent on initiating a new face-to-face contact with the health-care system. In Denmark, prescription renewal can be requested digitally through the Min Læge app and, for ongoing treatment, may involve renewing an existing prescription rather than establishing a new treatment episode. The relative decline in starts is therefore especially informative: it suggests that workers in more digitally intensive occupations experienced fewer annual transitions into pharmacological treatment after the pandemic. The temporary increase in depression cessation points to an additional margin: more annual transitions out of recorded prescription use. Taken together with the utilization evidence, these patterns suggest that the relative prescription declines are unlikely to be driven primarily by a broad reduction in access to care. Instead, the results are more consistent with lower underlying mental-health needs among workers in more digitally intensive occupations, a shift toward less medication-intensive care, or both, reflected

in fewer annual entries into recorded prescription use.

6.2 Labor-market dynamics and mobility

Figures 9–11 report event studies for hourly wages, employment attachment, days worked, and switching across occupations, sectors, and firms. We interpret these outcomes using the framework in Section 4.4.

Panel (a) of Figure 9 shows that, in the pooled sample, hourly wages rise more for workers in more digitally intensive baseline occupations over the sample period. However, the pattern is not a clean pandemic-onset break. The pre-2020 coefficients move steadily toward the omitted 2019 year, and the post-2020 coefficients continue along the same broad upward trajectory. Because hourly wages are defined among those working, these estimates capture a conditional wage margin rather than overall labor-market attachment. This wage panel is best interpreted as evidence of a pre-existing differential wage trajectory by occupational digital intensity, not as evidence of a discrete Covid-timed wage response.

The sector-specific wage panels lead to a similar interpretation. In the public sector, shown in Figure 10, hourly wages follow a relatively smooth pattern around 2020. In the private sector, shown in Figure 11, hourly wages also continue to increase after 2019, also extending the pre-2020 trajectory rather than revealing a sharp break at the onset of Covid. Thus, across sectors, the hourly-wage panels do not provide a stand-alone labor-market mechanism for the prescription results.

Panels (b) and (c) provide related checks on employment attachment and realized labor supply. In the pooled sample, there is limited evidence that employment in November explains the main prescription patterns. The dominant feature of this outcome is the construction of the analysis sample: all workers are selected on employment in 2019, the omitted year. Conditional on that sample restriction, the post-2020 estimates are generally modest, providing limited evidence of a broad employment response.

Turning to days worked, the sector split is more informative than the pooled estimate. In the private sector, shown in Figure 11, the post-2020 coefficients become positive, but this mainly extends the pre-pandemic trajectory: the estimates are already moving steadily upward toward the omitted 2019 year before Covid. The private-sector pattern is therefore better interpreted as the continuation of a pre-existing differential trend, rather than as evidence of a discrete pandemic-induced labor-supply response.

In the public sector, shown in Figure 10, the pattern is different. Days worked displays a clearer shift after 2019, especially for women in more digitally intensive baseline occupations. Unlike the private-sector figure, this movement is less easily explained as a smooth continuation of the pre-period trend. The public-sector days-worked estimates therefore provide the stronger evidence that the pandemic changed realized labor supply along the digital-intensity

margin.

The mobility outcomes, shown in the bottom row of each figure, provide a second diagnostic.² Because treatment is assigned using the worker’s 2019 occupation and then held fixed, post-2020 switching across occupations, sectors, or firms does not mechanically redefine exposure. These outcomes therefore help assess whether the prescription results could be driven by large post-2020 reallocation of initially more digitally exposed workers into different occupations, sectors, or firms.

The mobility panels point to selective, rather than uniform, post-pandemic reallocation. Interpreting the coefficients relative to the omitted 2019 year, the strongest pattern appears among women in the public sector. For this group, the pre-2020 estimates are relatively flat and provide little evidence of differential pre-trends by baseline occupational digital intensity, while the post-2020 coefficients show a clear decline in firm and sector switching among workers in more digitally intensive baseline occupations. Public-sector men do not show a comparable break relative to 2019.

The private-sector mobility results are more difficult to interpret and more heterogeneous. Several of the pre-2020 coefficients differ from zero or trend toward 2019, indicating that workers in more digitally intensive baseline occupations were already on different mobility trajectories before the pandemic. This contrasts with the main prescription results, where the pre-2020 estimates are generally flatter and the post-2020 decline is more clearly timed to the onset of Covid. The private-sector mobility panels therefore provide limited support for mobility as a significant mechanism behind the prescription effects. The decline in one-digit occupational switching among women provides little support for increased reallocation across broad occupational groups. It does not, however, rule out movement between four-digit occupations within those groups. The increase in sector switching among private-sector women, however, suggests a more limited qualification: although these women are not disproportionately changing broad occupations, some may be moving into a different sectoral environment, which could change their realized exposure to work-from-home opportunities or other workplace conditions.

Summing up, the labor-market results do not point to a clear general labor-market mechanism behind the prescription findings. For most groups, wages, employment, days worked, and mobility either follow pre-existing trends or show limited evidence of a distinct pandemic-related response. The main exception is public-sector women, for whom the evidence points to stronger attachment after 2019: days worked increase and firm and sector switching decline, while wages and employment status show little discrete change. Thus, labor-market adjustment may help interpret the public-sector women results, but mainly through stronger

²The switching outcomes are defined only for individuals who are working in both periods used to construct the transition.

attachment to the employer or sector rather than through wages, employment, or occupational reallocation.

6.3 Work from home and commuting distance

The labor-market results provide limited evidence of a general wage or employment channel, although public-sector women show stronger attachment after 2019. We next examine WFH and commuting distance to assess how baseline digital readiness shaped changes in work location. These adjustments may complement changes in labor-market attachment rather than represent mutually exclusive mechanisms.

Figure 12 shows a clear pandemic-timed response in realized WFH. The realized WFH measure, shown in panels (c) and (d), displays a sharp pandemic-timed break. Realized WFH rises sharply in 2020 and 2021 for workers in more digitally intensive baseline occupations and then partially reverses after the acute pandemic years. The magnitudes are larger in the public sector, consistent with the stronger role of formal WFH mandates, but the timing is similar across sectors. Thus, realized WFH maps closely onto the pandemic period: it rises abruptly when the COVID shock reorganized work and then partially recedes.

Appendix Figure A.8 shows that the partial reversal in realized WFH is primarily an intensive-margin adjustment. When WFH is measured as any work from home, rather than working from home more than half of hours, the event-study coefficients remain positive after 2020 and do not display the same sharp post-2021 reversal. The pattern is especially clear for women, including in the private sector, although the estimates are not as precise. This suggests that WFH did not disappear after the acute pandemic period; rather, workers in more digitally intensive occupations became less likely to work from home for a majority of hours while remaining more likely to work from home at least some of the time.

The postings-based WFH measure, shown in panels (a) and (b) captures a different margin. Rather than measuring realized work arrangements among employed workers, it measures whether newly posted jobs mention WFH arrangements. WFH in job postings rises more gradually after 2019 and continues increasing through 2023. This pattern is visible within both sectors, indicating that WFH became increasingly embedded in job design after the pandemic onset. In the sector-specific panels, the public- and private-sector estimates are broadly similar for men. For women, the estimated effects are similar across sectors during the acute pandemic years, but become larger in the public sector in the post-pandemic period.³

³Appendix Figure A.7 reconstructs the postings-based WFH measure separately by sector and four-digit occupation. The results are very similar to the occupation-level postings measure in Figure 12: WFH postings are flat before 2019 and rise sharply after 2020 in more digitally intensive occupations. Allowing postings-based WFH to vary within sector strengthens, rather than weakens, the mechanism evidence, with the public-sector increase becoming somewhat larger while the private-sector increase remains clearly positive.

The commuting-distance panels, shown in panels (e) and (f), provide complementary evidence on the work-location mechanism. The outcome is an indicator for whether a worker’s workplace is more than 10 km from home, conditional on being employed. Before 2019, there is little evidence of a differential pre-trend: the estimates are generally close to zero and do not show systematic divergence between workers in more and less digitally intensive baseline occupations. This changes sharply at the onset of COVID. After 2019, workers in more digitally intensive occupations become more likely to work in jobs located more than 10 km from home, in both sectors.

This post-COVID increase is consistent with the WFH mechanism. The estimates should not be interpreted as showing that workers necessarily spent more time commuting, or even that they commuted more frequently. Rather, they suggest that remote work weakened the link between residential location and workplace location: when workers do not need to travel to the workplace every day, it becomes less costly to live farther from work, to remain in a job after moving farther away, or to accept a job located farther from home. The indicator cannot distinguish among these margins, but the timing of the increase is informative. The absence of a clear pre-trend, followed by a sharp break at COVID, mirrors the timing of the realized WFH response and aligns with the timing of the prescription effects. This supports the interpretation that the main effects are linked to pandemic-induced changes in work location, commuting constraints, and schedule flexibility.

Appendix Tables A.1 and A.2 report the corresponding difference-in-differences estimates for work from home and use them to scale the reduced-form prescription estimates. The results confirm that baseline occupational digital readiness strongly predicts the reorganization of work after 2020. Using actual WFH, a one-standard-deviation increase in baseline occupational digital readiness raises WFH during the Covid period by 10.2 percentage points for public-sector women and 8.6 percentage points for public-sector men. The corresponding private-sector estimates are smaller but still large and highly significant, at 5.5 percentage points for women and 6.0 percentage points for men. In the post-Covid period, the actual WFH estimates are smaller and less precise, likely reflecting the survey-based nature of the data. The public-sector estimates are about 2.6–2.8 percentage points for both women and men and are not statistically significant, while the private-sector estimate remains positive and marginally significant for women but is close to 1 percentage point and statistically insignificant for men.

The postings-based WFH measure shows a more persistent adjustment. During Covid, digital readiness increased WFH mentions in vacancies by around 2 percentage points in the public sector and by about 1 percentage point in the private sector, regardless of sex. After Covid, the postings-based estimates rise further and are highly significant in both sectors: roughly 5 percentage points in the public sector for both women and men, and between

2.9 and 3.7 percentage points in the private sector for women and men, respectively. The same appendix tables also report a simple scaling exercise that divides the reduced-form prescription estimates by the corresponding WFH estimates. This exercise is useful for assessing magnitudes, but it should not be interpreted as an instrumental-variables estimate of the causal effect of WFH. A causal interpretation would require digital readiness to affect prescriptions only through WFH, excluding the other channels considered here.

The WFH estimates line up with the prescription results in timing, but not in a simple dose-response pattern across groups. The clearest alignment is in the public sector during Covid, where baseline digital readiness generated the largest increase in actual WFH and where prescription use also declined relatively across several outcomes for both women and men. However, the cross-group magnitudes are not proportional. Private-sector workers also experienced sizable increases in actual WFH during Covid, but the corresponding prescription effects are weaker, more outcome-specific, and in some cases positive for men. Similarly, in the post-Covid period, the actual WFH estimates are smaller and less precise, while some prescription effects persist, especially for public-sector women’s depression prescriptions and public-sector men’s sleep prescriptions. The postings-based measure shows a more persistent digital-readiness gradient in WFH opportunities after Covid, but this persistence does not map mechanically into uniform prescription declines across all groups.

6.4 Family outcomes

The WFH and commuting-distance evidence points to a reorganization of work location after COVID: workers in more digitally intensive occupations became more likely to work from home, and distance to the workplace became less binding. We next ask whether these work-location changes are reflected in broader family outcomes. These outcomes are not alternative labor-market mechanisms, but potential downstream margins through which changes in commuting, flexibility, and household time allocation may have affected treated mental health.

Figure 13 reports event-study estimates for marriage, divorce, and births. Red lines report estimates for women and blue lines report estimates for men. The marriage panels provide the least evidence of a COVID-timed response. In the public sector, the estimates remain close to zero before and after 2019 for both women and men. In the private sector, marriage rises after 2019, especially for women, but the increase appears to continue the pre-period trend rather than break discretely at COVID. Marriage therefore does not appear to be the family margin most closely linked to the decline in mental-health prescriptions.

The divorce panels show clearer post-COVID movement, especially for women. For private-sector women, divorce falls sharply after 2019 among workers in more digitally intensive baseline occupations, with little evidence of a differential pre-trend. For public-sector

women, divorce also declines after 2019, although the early 2015–2016 estimates make the pre-period less clean. From the years immediately preceding COVID, however, the estimates move toward zero and then turn negative after 2019. The responses for men are smaller in both sectors. These patterns point to increased marital stability among women in more digitally intensive occupations, which aligns with the decline in mental-health prescriptions after COVID.

Births also change after COVID, but the timing requires some care. Births observed in 2020 partly reflect pre-pandemic conceptions, so births in 2021 and later are more informative about fertility decisions made after the onset of COVID. In the public sector, births rise after 2019 for women in more digitally intensive baseline occupations and continue increasing in the years most relevant for post-COVID fertility decisions. The pre-period does not show a comparable upward trend. This pattern is consistent with the WFH mechanism: public-sector workers experienced especially large increases in realized WFH, and public-sector women show some of the strongest and most persistent prescription declines. Remote work may have relaxed commuting and time constraints in ways that made childbirth more compatible with continued employment.

The private-sector birth pattern is different. For private-sector women, the estimates rise around 2020–2021, but once the lag between conception and birth is considered, the post-COVID pattern is weaker than in the public sector and does not show the same sustained increase. Relative to the pre-pandemic trajectory, it may instead reflect a weaker or declining fertility response. This contrast suggests that the birth response is not a generic consequence of digital readiness, but varies by sector in a way that broadly mirrors the stronger public-sector WFH and prescription patterns.

The birth response may also matter directly for measured prescription use. Pregnancy, childbirth, and breastfeeding can change treatment and prescribing decisions, so an increase in births among public-sector women could mechanically contribute to lower observed prescriptions even if underlying mental-health needs do not fall one-for-one. This channel should therefore be interpreted as part of the broader work-family adjustment rather than as direct evidence that fertility itself improves mental health. It also cannot explain the prescription effects for men, which reinforces the interpretation that births are one component of a wider set of mechanisms linked to WFH, commuting, and flexibility.

The family-outcome evidence therefore complements the WFH and commuting results. Marriage shows little discrete response around COVID. Divorce and births, in contrast, move for women after 2019 in directions consistent with greater marital stability and altered family timing. These patterns are consistent with changes in work location and household time allocation, but do not establish whether family changes mediate the prescription effects or occur alongside them.

7 Heterogeneity by baseline characteristics

Tables 4 and 5 report heterogeneity by baseline subgroup. These estimates should be interpreted as descriptive subgroup evidence rather than as formal tests of differences across groups, since precision varies substantially across cells. Overall, the results are most consistent with a flexibility-based interpretation: the negative estimates are most systematic among workers for whom remote or flexible work was likely more valuable, including professionals, workers with longer commutes, workers outside large cities, and, to some extent, married workers.

The professional subgroup shows one of the clearest patterns. In both sectors, estimates for professionals are often negative and statistically significant, especially among women and especially for depression, anxiety, sleep, and substance-abuse prescriptions. This is consistent with the idea that baseline digital readiness mattered because it enabled changes in work organization after Covid, such as remote work, greater schedule flexibility, or more autonomy over work routines.

The commuting and location results point in a similar direction. Workers with baseline commutes above 10 kilometers show relatively consistent negative estimates, particularly among women. Workers outside Copenhagen, Frederiksberg, and Aarhus also tend to show negative estimates, especially after Covid. These patterns suggest that digital readiness may have been especially valuable where it reduced commuting burdens or relaxed location constraints.

The household-related results are more mixed. Married workers show a relatively stable negative sign pattern, especially among women. By contrast, the estimates for workers with children under age 19 are less systematic and often imprecise. The estimates for workers with young children are somewhat more negative in some cells, but not consistently enough to support a narrow childcare mechanism. Taken together, the household results are more consistent with a broader flexibility mechanism than with an effect operating specifically through children.

The estimates for workers living alone differ by sex and sector. Several coefficients are negative, but among private-sector men living alone, higher baseline occupational digital intensity is associated with a statistically significant relative increase in depression prescriptions during 2020–2021 and a marginally significant relative increase in substance-abuse prescriptions during 2022–2023. These results leave room for adverse effects among some workers living alone, although they do not identify isolation as the mechanism. Living alone is also distinct from being unmarried, since unmarried workers may cohabit or live with others.

One exception to the broadly negative pattern is public-sector managers. Among women managers in the public sector, digital readiness is associated with a positive and statistically

significant anxiety estimate during Covid. Male public-sector managers show a different pattern, with negative estimates for anxiety during Covid and for depression and anxiety after Covid. This suggests that managerial responsibilities may have interacted differently with digital work by gender, potentially reflecting greater coordination burdens or blurred work boundaries for women managers during the pandemic.

The heterogeneity results reinforce the interpretation that digital readiness mattered through changes in work organization. The most consistent negative estimates appear in groups for whom remote or flexible work was likely more feasible or valuable. The evidence does not support a simple childcare story, while married workers show a relatively consistent negative pattern. Qualifications to the broadly negative prescription effects include relative increases in some outcomes among private-sector men living alone and in anxiety prescriptions among public-sector women managers during Covid.

8 Robustness and additional outcomes

8.1 Pre-trend, functional-form, and stayer diagnostics

Appendix Figure A.9 reports event-study specifications that allow for one-digit occupation trends. This exercise addresses the concern that the baseline estimates might partly reflect broad differences across occupational groups rather than the post-2020 effect of baseline occupational digital readiness. The resulting estimates demonstrate similar patterns to the main event studies: depression, sleep, and substance-use prescriptions continue to decline relatively after 2020 among workers in more digitally intensive occupations, especially women, while the anxiety results remain less stable. The fact that these declines remain after allowing broad occupation groups to follow their own time trends strengthens the interpretation that the results reflect a COVID-timed break associated with digital readiness, rather than broad differences across major occupational groups.

A further check on functional form uses the median split of Table 1. Because baseline exposure is fixed within worker, the change over time in the gap between the high- and low-exposure groups in Appendix Figures A.1 and A.2 corresponds, up to panel imbalance, to a binary-exposure difference-in-differences with worker and year fixed effects, and it does not rely on linearity in dose. For women, the high-low gap in depression prescriptions widens after 2019, with a smaller widening for sleep and substance-abuse prescriptions and no widening for anxiety; for men, the depression gap also widens. The pattern in the coarse binary comparison is therefore consistent with the sign and the outcome ranking of the continuous estimates, although the raw figures do not deliver confidence intervals for the change in the gap. Implementing the dose-response estimators proposed by Callaway et al.

(2024), which would replace the linear slope with nonparametric estimates of the response at each exposure level, is left for future work.

Appendix Figures A.10 and A.11 report stayer diagnostics. The baseline estimates follow workers regardless of subsequent employment changes. This is the preferred intention-to-treat design because mobility may itself be a response to the pandemic and to baseline occupational digital readiness. Conditioning on post-2020 mobility can therefore change the estimand and introduce selection. Nevertheless, stayer specifications are useful diagnostics for assessing whether the main findings are mechanically driven by post-COVID reallocation.

The occupation-stayer specification restricts the sample to workers who remain in their baseline occupation, addressing the concern that the results are driven by workers leaving less digitally adaptable occupations. The sector-stayer specification restricts the sample to workers who remain in their baseline public or private sector, addressing movement across institutional regimes. Across these diagnostics, the qualitative pattern is broadly similar to the baseline estimates, although precision declines in the restricted samples. The persistence of the main pattern among stayers suggests that the results are not explained by large post-2020 reallocation across occupations or sectors, while the preferred interpretation remains the baseline intention-to-treat design.

8.2 Other stress-linked medications and health-service diagnostics

Appendix Figure A.12 reports additional medication and health-service outcomes. The first row examines prescription-based outcomes that are not psychiatric medications but are plausibly linked to stress-related morbidity: pain medication, gastrointestinal medication, and hypertension medication. These outcomes provide a useful boundary condition for the main results. Although some estimates move after 2020, the profiles also show pre-existing differential trends and do not display the same clean COVID-timed break as the primary mental-health prescription outcomes. The evidence therefore does not suggest a broad pandemic-timed decline across all stress-linked somatic medication categories. Instead, the main effects appear more specific: they are concentrated in core treated mental-health outcomes, sleep medication, and substance-use treatment, rather than reflecting a generalized decline in medication use among workers in more digitally intensive baseline occupations.

9 Conclusion

This paper shows that pre-pandemic occupational digital readiness shaped treated mental-health trajectories among Danish white-collar workers during and after COVID-19. Workers in more digitally intensive occupations experienced relative declines in prescription use, with

the clearest and most persistent effects for women’s depression and sleep prescriptions. In 2022–2023, a one-standard-deviation increase in baseline digital intensity reduced women’s antidepressant use by 0.250 percentage points, or 4.3 percent of the pre-pandemic mean. Effects for men were smaller and less systematic. Event studies show little evidence of differential pre-trends for the main prescription outcomes, followed by divergence after 2019.

Workers in more digitally intensive baseline occupations experienced a sharp relative increase in realized work from home in 2020–2021 and a continuing rise in advertised remote-work opportunities through 2023. Their increased likelihood of holding jobs more than 10 km from home is consistent with less binding location constraints. These work-location changes provide stronger supporting evidence than labor-market differences that largely predate the pandemic.

The prescription declines include fewer treatment starts among women, while the utilization evidence makes a generalized loss of health-care contact an unlikely primary explanation. The data do not fully distinguish lower underlying need from a shift toward less medication-intensive care. Pre-trends in private-sector talk therapy and some family outcomes limit a pandemic-onset interpretation of those margins. The design identifies the reduced-form effect of baseline digital readiness on treatment, not the separate effect of working from home on latent mental health. The continuous-exposure coefficients also summarize a gradient across occupations rather than a dose-response function; interpreting them as marginal effects of exposure requires the stronger parallel-trends assumption discussed in Section 4.2.

The international evidence also cautions against a uniform interpretation. Emanuel et al. (2026) find increased isolation, mental distress, and prescription use in the United States in an analysis that excludes 2020–2021, making our post-COVID estimates the closer comparison. Cowan and Spearing (2026) find that adverse relative changes in loneliness and mental-health symptoms in the United Kingdom disappear by 2023 despite persistently higher remote work. Differences in occupational coverage, outcome measures, adjustment horizons, and institutions suggest possible explanations for the contrasting findings; the cross-country comparison does not identify which are responsible.

These findings also bear on return-to-office requirements. Returning workers to the office need not return them to their pre-pandemic circumstances: households may have reorganized residential locations and daily routines around greater flexibility. Our estimates do not identify the effects of return-to-office requirements, but they motivate evaluating worker health and potential adjustment costs alongside productivity and workplace interaction.

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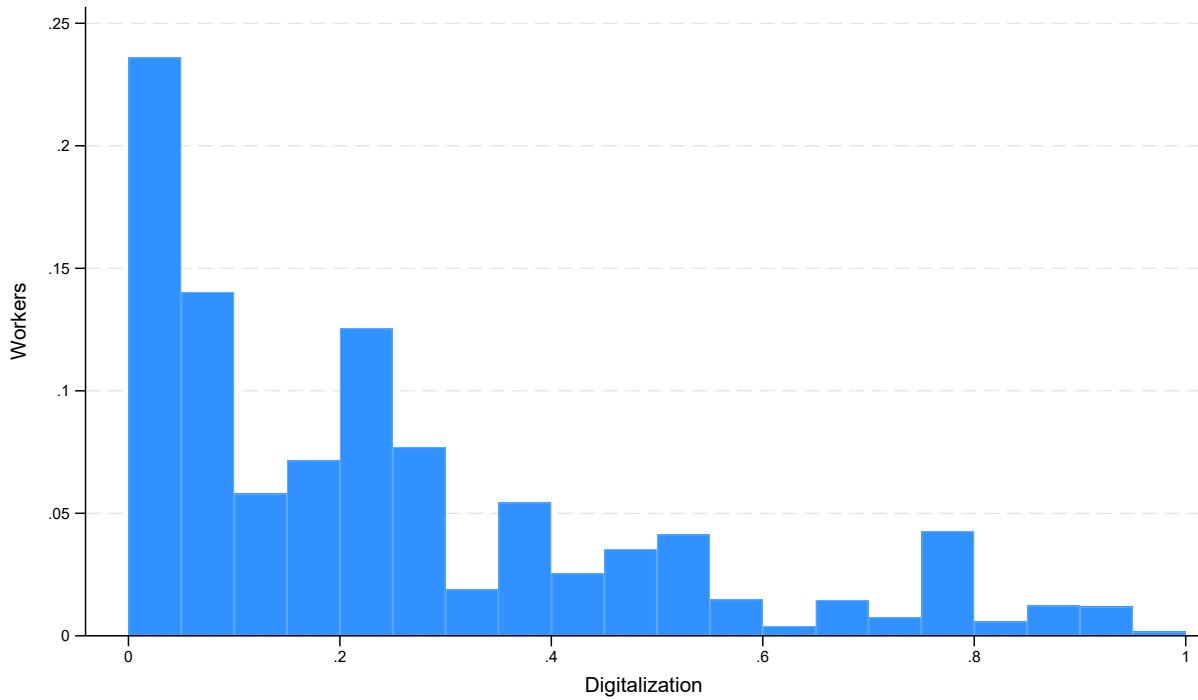
Tables and Figures

Table 1: 2019 worker characteristics by occupational digital readiness

	Private sector		Public sector	
	Low	High	Low	High
Age	46.43	46.40	46.64	47.01
Female (%)	58.2	38.9	77.3	63.8
Married/partnered (%)	53.1	55.6	55.5	54.3
Has children 18 or younger (%)	17.5	18.8	21.0	17.1
Wage, DKK	250.75	313.45	232.47	243.45
Digital intensity (fraction)	0.087	0.436	0.056	0.381
Depression prescription (fraction)	0.049	0.035	0.062	0.058
Anxiety prescription (fraction)	0.008	0.006	0.010	0.011
Sleep prescription (fraction)	0.021	0.017	0.026	0.024
Substance-abuse prescription (fraction)	0.002	0.001	0.002	0.002
Managers (fraction)	0.072	0.144	0.026	0.050
Professionals (fraction)	0.330	0.449	0.853	0.451
Technician/associate professionals (fraction)	0.313	0.254	0.107	0.167
Clerical support and customer service (fraction)	0.285	0.153	0.014	0.332
Commuting distance, km	24.36	28.25	18.24	21.12
WFH, job posting (fraction)	0.029	0.047	0.023	0.030
WFH, actual more than half of hours (fraction)	0.080	0.088	0.075	0.084
WFH, actual any (fraction)	0.363	0.512	0.367	0.472
Workers	218,635	442,053	330,578	116,926
Four-digit occupations	129	112	115	105

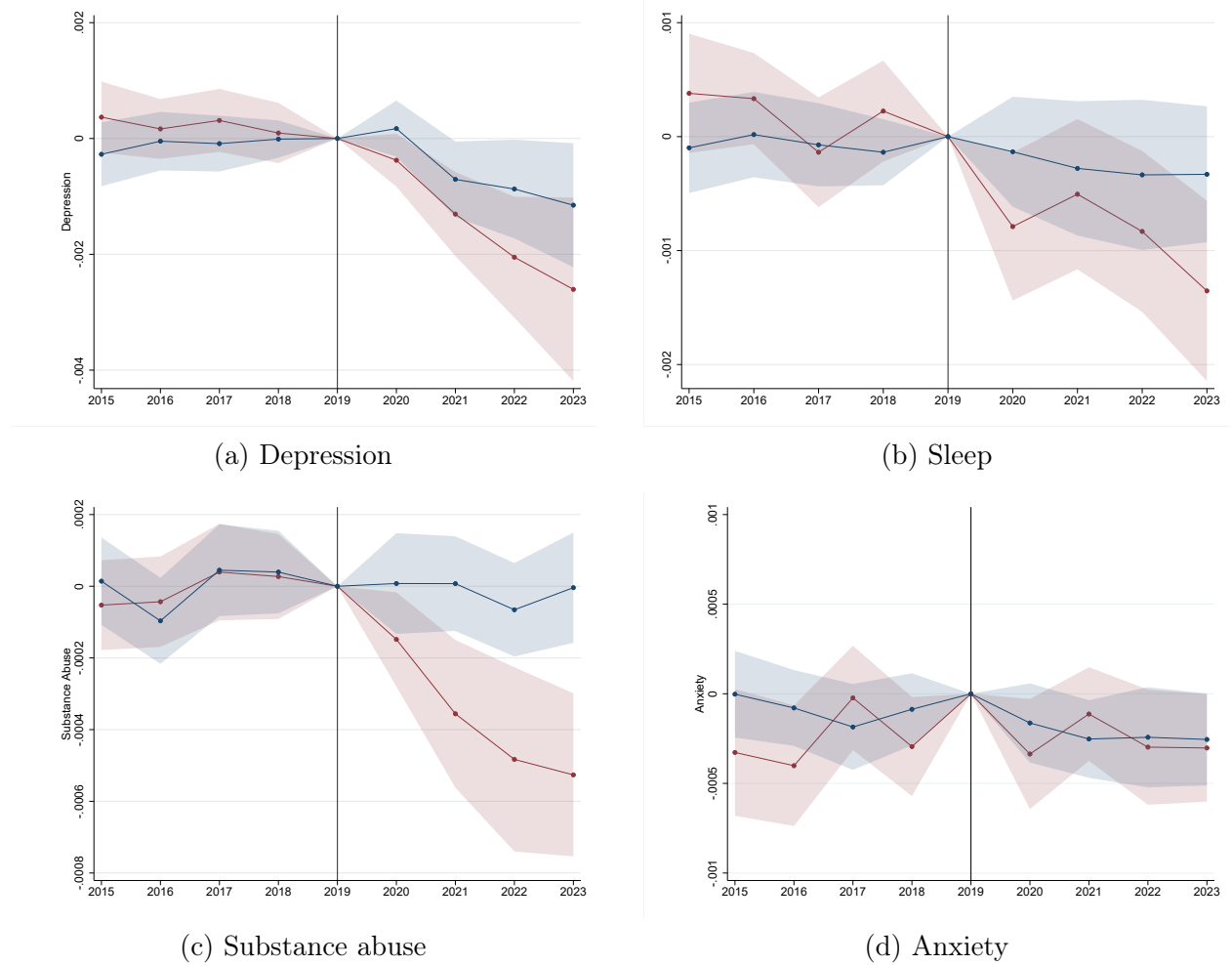
Notes: Sample consists of workers employed in 2019 in one-digit DISCO groups 1–4 and aged 29–60 at baseline. High and low digital readiness are defined relative to the median of postings-based 2019 occupational digital intensity in the worker’s four-digit baseline occupation. The split is descriptive only; the main estimates use standardized continuous occupational digital intensity. Depression, anxiety, sleep, and substance-abuse prescription rows report fractions, not percentages. Digitalization level is the average postings-based digital intensity of the worker’s four-digit baseline occupation. One-digit occupation rows report the fraction of workers in each broad occupation group. Commuting distance is measured in kilometers. Work-from-home rows report fractions.

Figure 1: Worker-Weighted Distribution of Occupational Digital Exposure



Notes: The figure shows the distribution of baseline occupational digital exposure across workers. We define a vacancy as requiring a digital skill if it requests skills in at least one of the following categories: networking, data analysis, digital design, generative AI, artificial intelligence, digital marketing, or programming/software. This classification broadly follows the taxonomy of Nania et al. (2019). The main exposure is postings-based occupational digital intensity, Dig_o , defined as the 2019 share of vacancies in occupation o that require at least one of these digital skills.

Figure 2: Event-study estimates of mental-health prescription dynamics by baseline occupational digital intensity



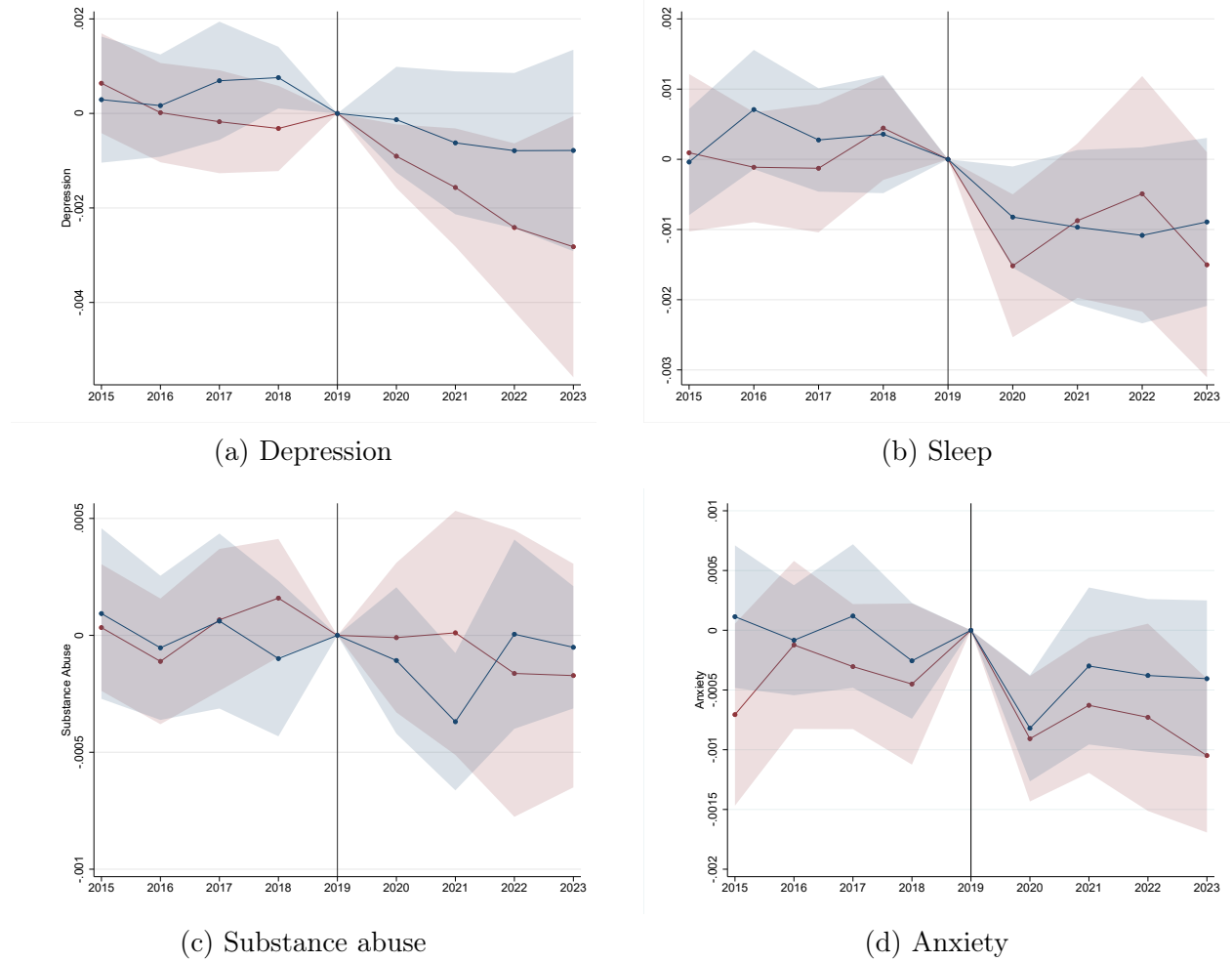
Notes: The figure reports event-study coefficients from equation (1). The treatment is standardized occupational digital intensity in the worker's 2019 four-digit occupation. Estimates are normalized to zero in 2019 and are estimated separately by sex, with worker and year fixed effects. Outcomes are indicators equal to one if the worker fills at least one prescription in the indicated medication category during the year. Coefficients are reported in probability units. Shaded regions or bars denote 95 percent confidence intervals. Standard errors are clustered at the 2019 four-digit occupation level.

Table 2: Difference-in-differences estimates for mental-health prescriptions

	Depression		Sleep		Substance Abuse		Anxiety	
	Women	Men	Women	Men	Women	Men	Women	Men
PANEL A: BOTH SECTORS								
COVID effect, 2020–2021 (pp)	−0.102*** (0.0323)	−0.0182 (0.0278)	−0.0801** (0.0309)	−0.0146 (0.0300)	−0.0247*** (0.00914)	0.000587 (0.00572)	−0.00259 (0.0126)	−0.0137* (0.00754)
Post-COVID effect, 2022–2023 (pp)	−0.250*** (0.0696)	−0.0930* (0.0498)	−0.124*** (0.0352)	−0.0275 (0.0344)	−0.0500*** (0.0122)	−0.00369 (0.00530)	−0.0100 (0.0125)	−0.0178* (0.00914)
Observations	5,444,739	4,114,755	5,444,739	4,114,755	5,444,739	4,114,755	5,329,503	4,028,081
Pre-2020 mean (%)	5.880	3.271	2.694	1.725	0.0998	0.189	1.140	0.614
Relative COVID effect (fraction of mean)	−0.0173	−0.00556	−0.0297	−0.00847	−0.247	0.00312	−0.00227	−0.0222
Relative Post-Covid Effect (fraction of mean)	−0.0426	−0.0284	−0.0461	−0.0159	−0.501	−0.0196	−0.00881	−0.0290
PANEL B: PUBLIC SECTOR								
COVID effect, 2020–2021 (pp)	−0.125** (0.0581)	−0.0753 (0.0775)	−0.126*** (0.0478)	−0.115*** (0.0418)	−0.00310 (0.0263)	−0.0234* (0.0127)	−0.0464** (0.0229)	−0.0539** (0.0235)
Post-COVID effect, 2022–2023 (pp)	−0.264** (0.120)	−0.117 (0.102)	−0.105 (0.0826)	−0.125** (0.0566)	−0.0199 (0.0330)	−0.00201 (0.0155)	−0.0580* (0.0317)	−0.0368 (0.0282)
Observations	2,800,264	987,325	2,800,264	987,325	2,800,264	987,325	2,800,264	987,325
Pre-2020 mean (%)	6.502	4.397	2.773	2.078	0.107	0.237	1.217	0.830
Relative COVID effect (fraction of mean)	−0.0193	−0.0171	−0.0456	−0.0554	−0.0289	−0.0986	−0.0381	−0.0650
Relative Post-Covid Effect (fraction of mean)	−0.0405	−0.0265	−0.0380	−0.0599	−0.185	−0.00845	−0.0477	−0.0443
PANEL C: PRIVATE SECTOR								
COVID effect, 2020–2021 (pp)	−0.0418 (0.0301)	0.0299 (0.0214)	−0.0217 (0.0313)	0.0366 (0.0275)	−0.0309*** (0.00767)	0.0111* (0.00563)	0.0235* (0.0130)	−0.00580 (0.00841)
Post-COVID effect, 2022–2023 (pp)	−0.119** (0.0519)	−0.0149 (0.0337)	−0.0739* (0.0389)	0.0364 (0.0261)	−0.0616*** (0.0125)	0.00105 (0.00590)	0.0146 (0.0143)	−0.0135 (0.00997)
Observations	2,529,239	3,040,756	2,529,239	3,040,756	2,529,239	3,040,756	2,529,239	3,040,756
Pre-2020 mean (%)	5.073	2.874	2.362	1.533	0.0832	0.163	1.053	0.543
Relative Covid Effect (fraction of mean)	−0.00824	0.0104	−0.00920	0.0238	−0.371	0.0678	0.0224	−0.0107
Relative Post-Covid Effect (fraction of mean)	−0.0235	−0.00517	−0.0313	0.0237	−0.740	0.00645	0.0138	−0.0249

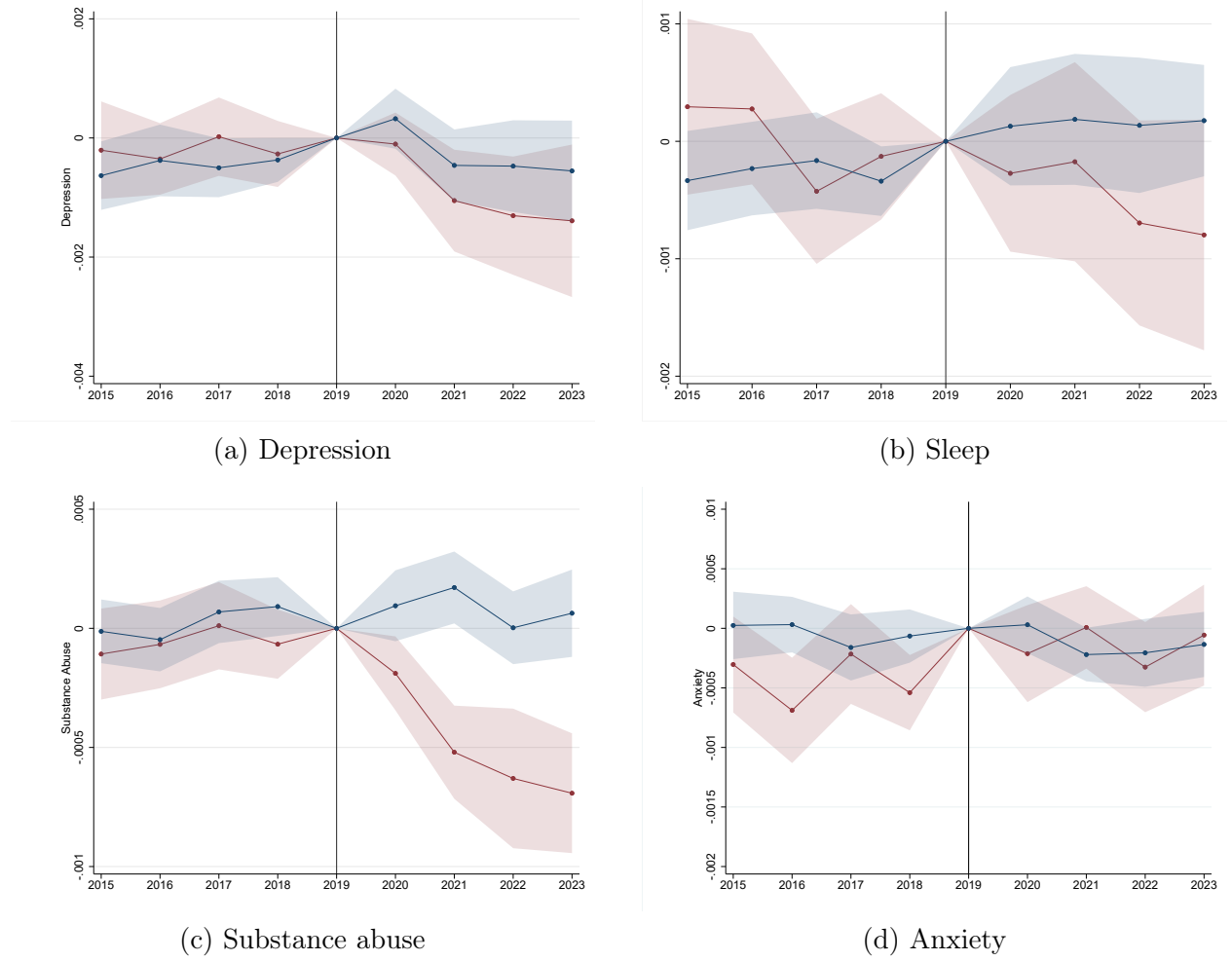
Notes: Standard errors clustered at the 2019 four-digit occupation level are in parentheses. The treatment is standardized baseline occupational digital intensity interacted with indicators for the Covid period, 2020–2021, and the post-Covid period, 2022–2023, estimated separately by sex and baseline sector. Panel A pools sectors; Panels B and C restrict to workers in the baseline public and private sectors, respectively. Prescription outcomes are annual indicators for filling at least one prescription in the indicated category. Coefficients and their standard errors are reported in percentage points (pp); pre-2020 means are reported as percentages. Relative effects are ratios of the corresponding coefficient to the pre-2020 mean. “Pre-2020 Mean” is the pre-Covid outcome mean for the estimation sample. Relative effects divide the corresponding coefficient by this mean. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure 3: Public sector: event-study estimates of treated mental-health by baseline occupational digital intensity



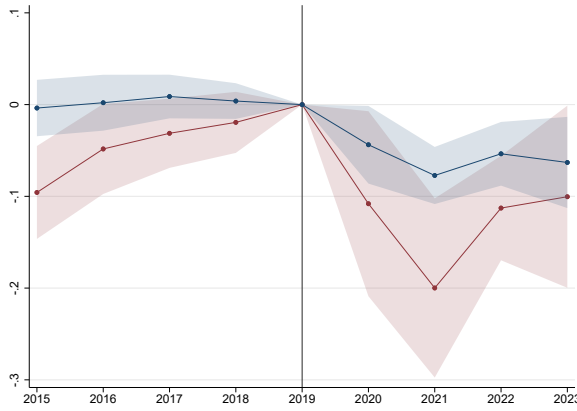
Notes: The figure reports event-study coefficients from equation (1), estimated separately by sex within the indicated baseline sector. The treatment is standardized occupational digital intensity in the worker's 2019 four-digit occupation. Outcomes are indicators equal to one if the worker fills at least one prescription in the indicated medication category during the year. Coefficients are reported in probability units. Estimates are normalized to zero in 2019. Standard errors are clustered at the 2019 four-digit occupation level.

Figure 4: Private sector: event-study estimates of treated mental-health by baseline occupational digital intensity

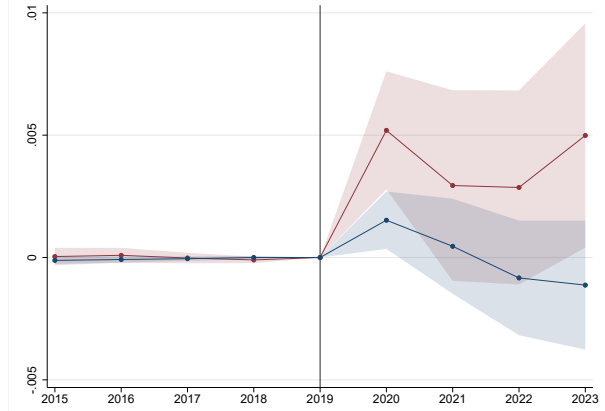


Notes: The figure reports event-study coefficients from equation (1), estimated separately by sex within the indicated baseline sector. The treatment is standardized occupational digital intensity in the worker's 2019 four-digit occupation. Outcomes are indicators equal to one if the worker fills at least one prescription in the indicated medication category during the year. Coefficients are reported in probability units. Estimates are normalized to zero in 2019. Standard errors are clustered at the 2019 four-digit occupation level.

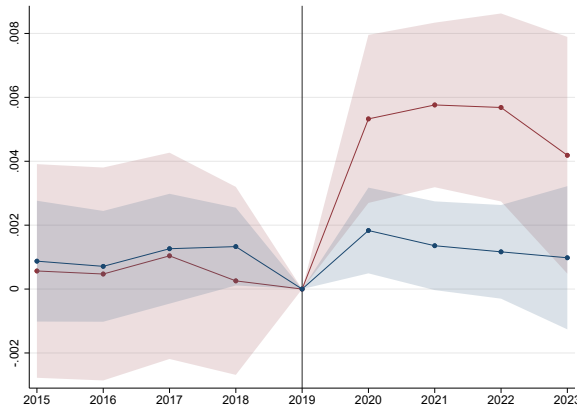
Figure 5: Event-study estimates of GP utilization by baseline occupational digital intensity



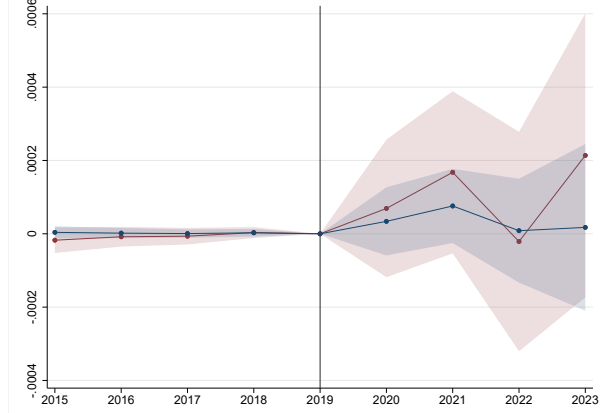
(a) All GP visits



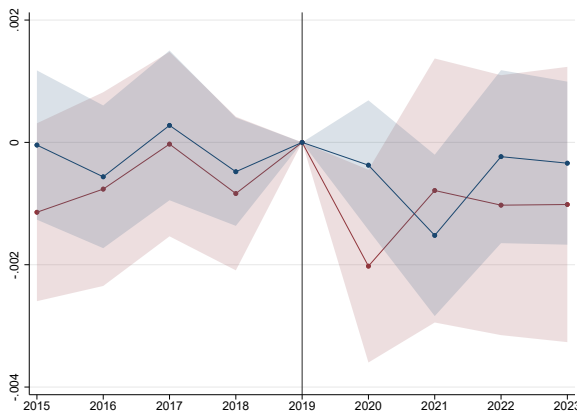
(b) Online GP visits



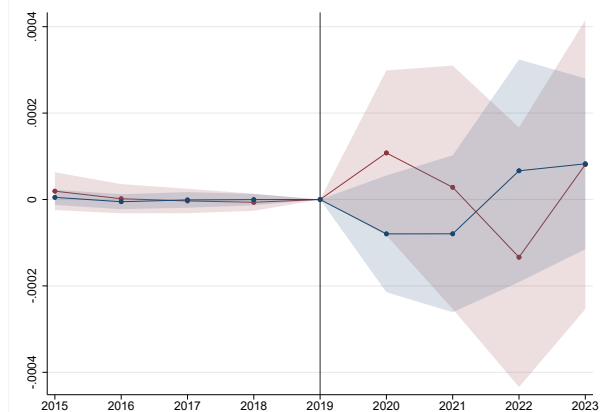
(c) Talk therapy



(d) Online talk therapy



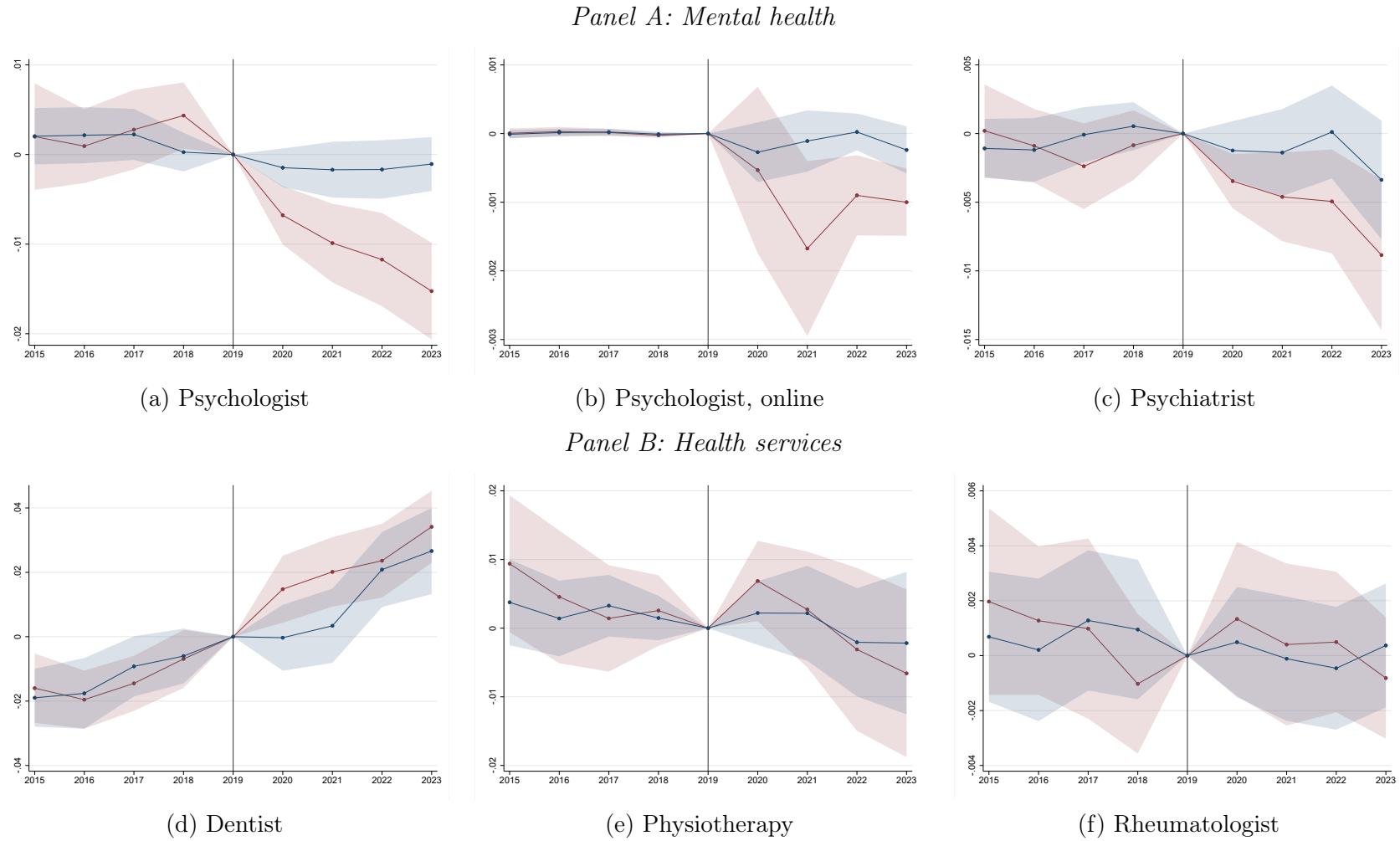
(e) Screening



(f) Online screening

Notes: The figure reports event-study coefficients from equation (1). The treatment is standardized occupational digital intensity in the worker's 2019 four-digit occupation. Estimates are normalized to zero in 2019 and are estimated separately by sex, with worker and year fixed effects. Outcomes are annual counts of the indicated health-care contacts or services. Coefficients are expressed in contacts or services per worker per year. Talk therapy and screening refer to GP-based services. Shaded regions or bars denote 95 percent confidence intervals. Standard errors are clustered at the 2019 four-digit occupation level.

Figure 6: Mental health and health-service outcomes by baseline occupational digital intensity



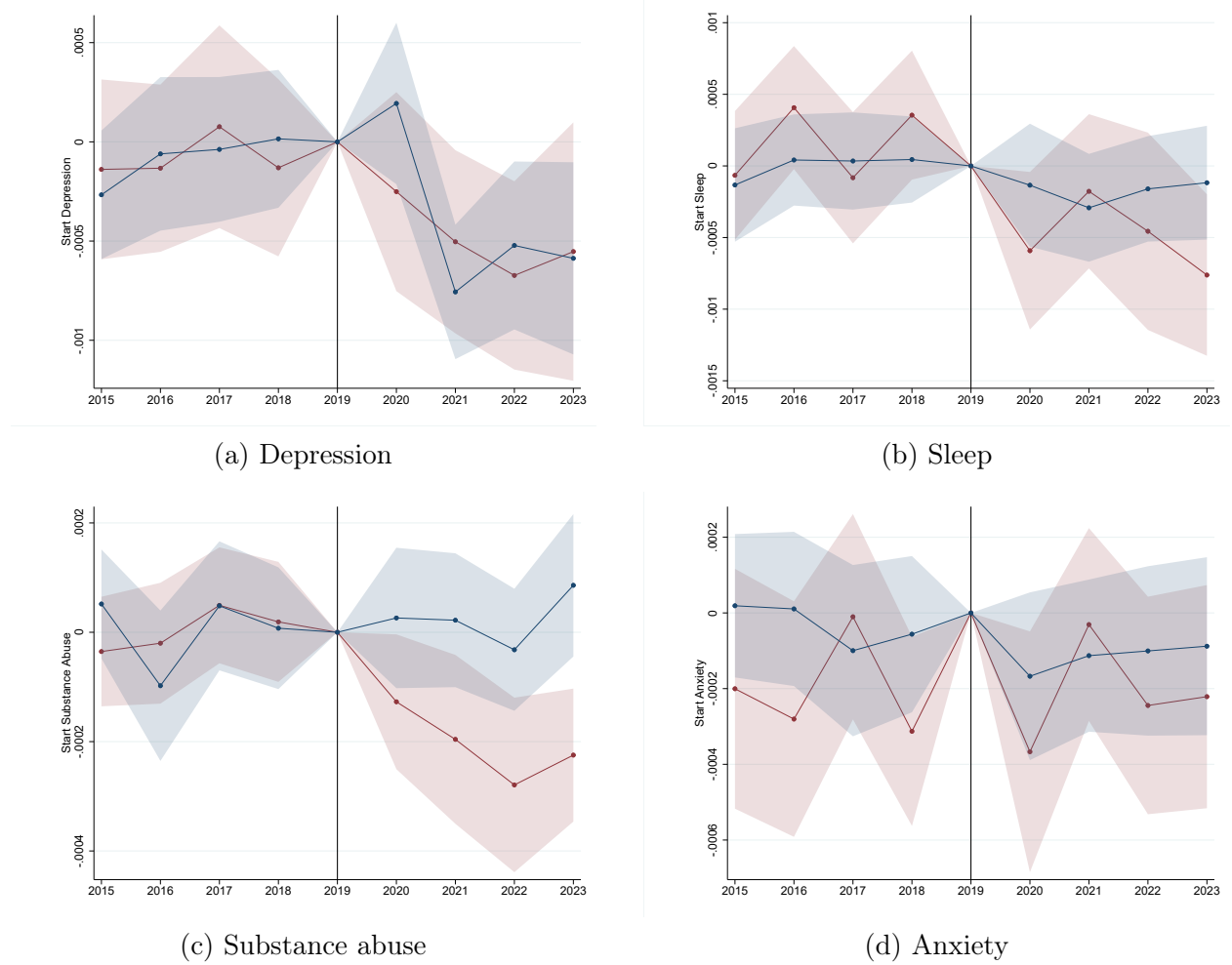
Notes: The figure reports event-study coefficients from equation (1). The treatment is standardized occupational digital intensity in the worker's 2019 four-digit occupation. Estimates are normalized to zero in 2019 and are estimated separately by sex, with worker and year fixed effects. Outcomes are annual counts of the indicated health-care contacts or services. Coefficients are expressed in contacts or services per worker per year. Talk therapy and screening refer to GP-based services. Shaded regions or bars denote 95 percent confidence intervals. Standard errors are clustered at the 2019 four-digit occupation level.

Table 3: Difference-in-differences estimates for mental-health care and utilization diagnostics

	Psychiatrist		Psychologist		GP Visit		Screening		Talk Therapy	
	Women	Men	Women	Men	Women	Men	Women	Men	Women	Men
PANEL A: BOTH SECTORS										
Covid: Treat×1(2020 or 2021)	−0.00326** (0.00151)	−0.000978 (0.00123)	−0.0103*** (0.00176)	−0.00286** (0.00117)	−0.117** (0.0473)	−0.0624*** (0.0174)	−0.000895 (0.000994)	−0.000778 (0.000677)	0.00508*** (0.00117)	0.000773 (0.000586)
Post-Covid: Treat×1(2022 or 2023)	−0.00609** (0.00245)	−0.00127 (0.00174)	−0.0155*** (0.00255)	−0.00265* (0.00146)	−0.0702* (0.0423)	−0.0604** (0.0259)	−0.000506 (0.00116)	−0.000123 (0.000814)	0.00448** (0.00193)	0.000251 (0.00111)
Observations	5,329,503	4,028,081	5,329,503	4,028,081	5,329,503	4,028,081	5,329,503	4,028,081	5,329,503	4,028,081
Pre-2020 mean (annual count)	0.0573	0.0393	0.184	0.0661	9.970	4.836	0.0519	0.0288	0.101	0.0419
Relative Covid Effect (fraction)	−0.0568	−0.0249	−0.0559	−0.0433	−0.0117	−0.0129	−0.0172	−0.0270	0.0505	0.0184
Relative Post-Covid Effect (fraction)	−0.106	−0.0324	−0.0838	−0.0401	−0.00704	−0.0125	−0.00974	−0.00427	0.0445	0.00599
PANEL B: PUBLIC SECTOR										
Covid: Treat×1(2020 or 2021)	−0.00558 (0.00400)	0.000773 (0.00267)	−0.00355 (0.00351)	−0.00202 (0.00438)	−0.194** (0.0771)	−0.0907** (0.0385)	−0.00260 (0.00206)	−0.00142 (0.00166)	0.00000 (0.00313)	0.000855 (0.00179)
Post-Covid: Treat×1(2022 or 2023)	−0.00958** (0.00403)	−0.000899 (0.00371)	−0.0123*** (0.00403)	−0.000665 (0.00460)	−0.0890 (0.0814)	−0.0751 (0.0675)	−0.00207 (0.00207)	−0.00206 (0.00161)	−0.000923 (0.00308)	−0.00210 (0.00212)
Observations	2,800,264	987,325	2,800,264	987,325	2,800,264	987,325	2,800,264	987,325	2,800,264	987,325
Pre-2020 mean (annual count)	0.0658	0.0620	0.220	0.101	10.09	4.952	0.0559	0.0361	0.111	0.0541
Relative Covid Effect (fraction)	−0.0848	0.0125	−0.0162	−0.0200	−0.0192	−0.0183	−0.0465	−0.0394	0.000184	0.0158
Relative Post-Covid Effect (fraction)	−0.146	−0.0145	−0.0561	−0.00656	−0.00882	−0.0152	−0.0370	−0.0570	−0.00832	−0.0389
PANEL C: PRIVATE SECTOR										
Covid: Treat×1(2020 or 2021)	−0.000319 (0.00165)	0.000486 (0.00129)	−0.00642*** (0.00242)	−0.00161* (0.000890)	−0.0490 (0.0573)	−0.0335*** (0.0122)	0.000832 (0.000703)	0.000000 (0.000446)	0.00823*** (0.000957)	0.00125** (0.000522)
Post-Covid: Treat×1(2022 or 2023)	0.000435 (0.00209)	0.000685 (0.00129)	−0.00852*** (0.00246)	−0.000963 (0.00113)	−0.0849*** (0.0323)	−0.0608*** (0.0200)	0.00118 (0.000871)	0.00116** (0.000536)	0.00958*** (0.00129)	0.00215** (0.000845)
Observations	2,529,239	3,040,756	2,529,239	3,040,756	2,529,239	3,040,756	2,529,239	3,040,756	2,529,239	3,040,756
Pre-2020 mean (annual count)	0.0479	0.0319	0.145	0.0545	9.836	4.798	0.0475	0.0265	0.0890	0.0380
Relative Covid Effect (fraction)	−0.00667	0.0152	−0.0442	−0.0295	−0.00499	−0.00698	0.0175	0.000129	0.0925	0.0329
Relative Post-Covid Effect (fraction)	0.00908	0.0215	−0.0587	−0.0177	−0.00863	−0.0127	0.0249	0.0439	0.108	0.0566

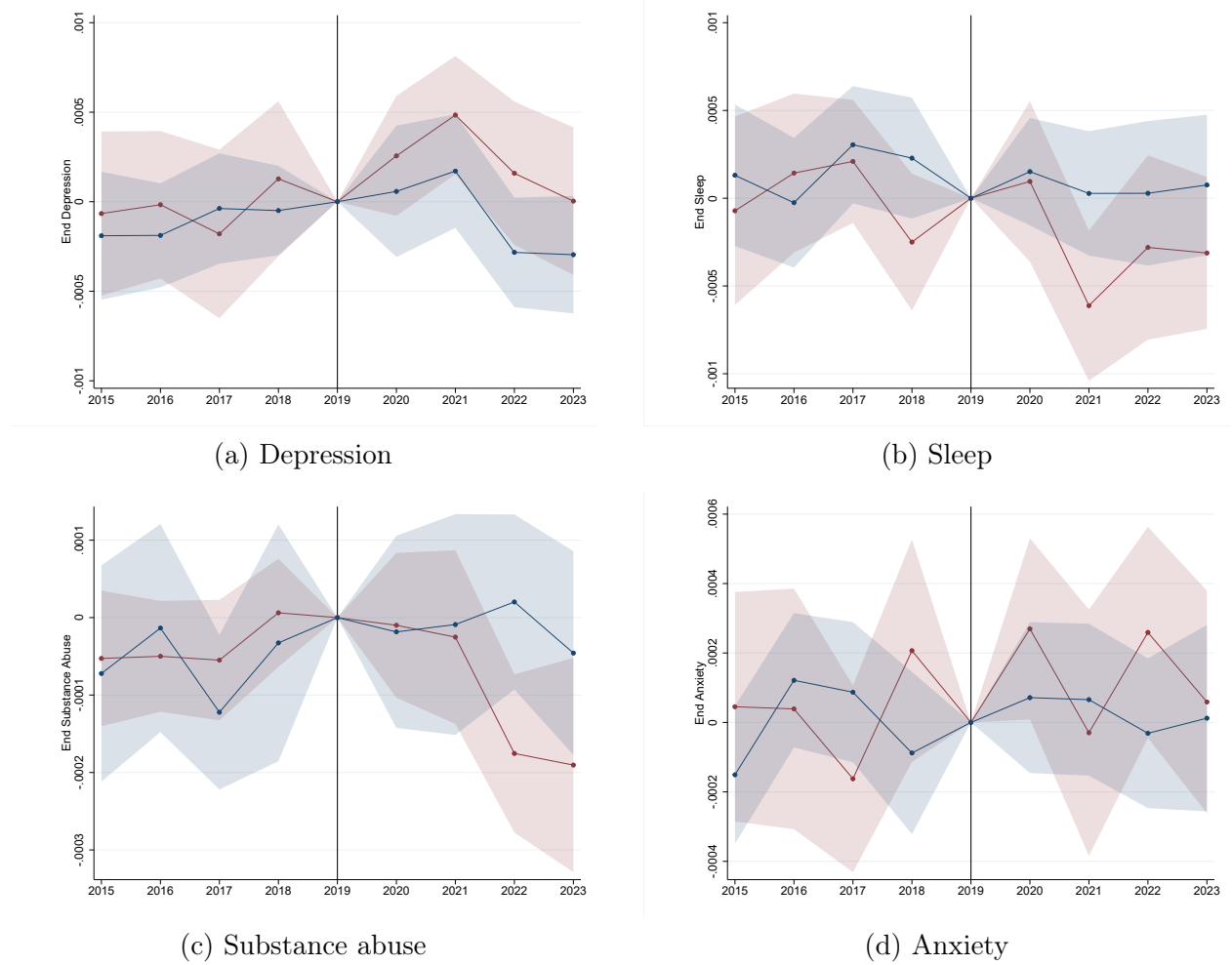
Notes: Standard errors clustered at the 2019 four-digit occupation level in parentheses. The treatment is standardized baseline occupational digital intensity interaction with the indicated period and estimated separately by sex and baseline sector. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure 7: Event-study estimates of mental-health prescription starts by baseline occupational digital intensity



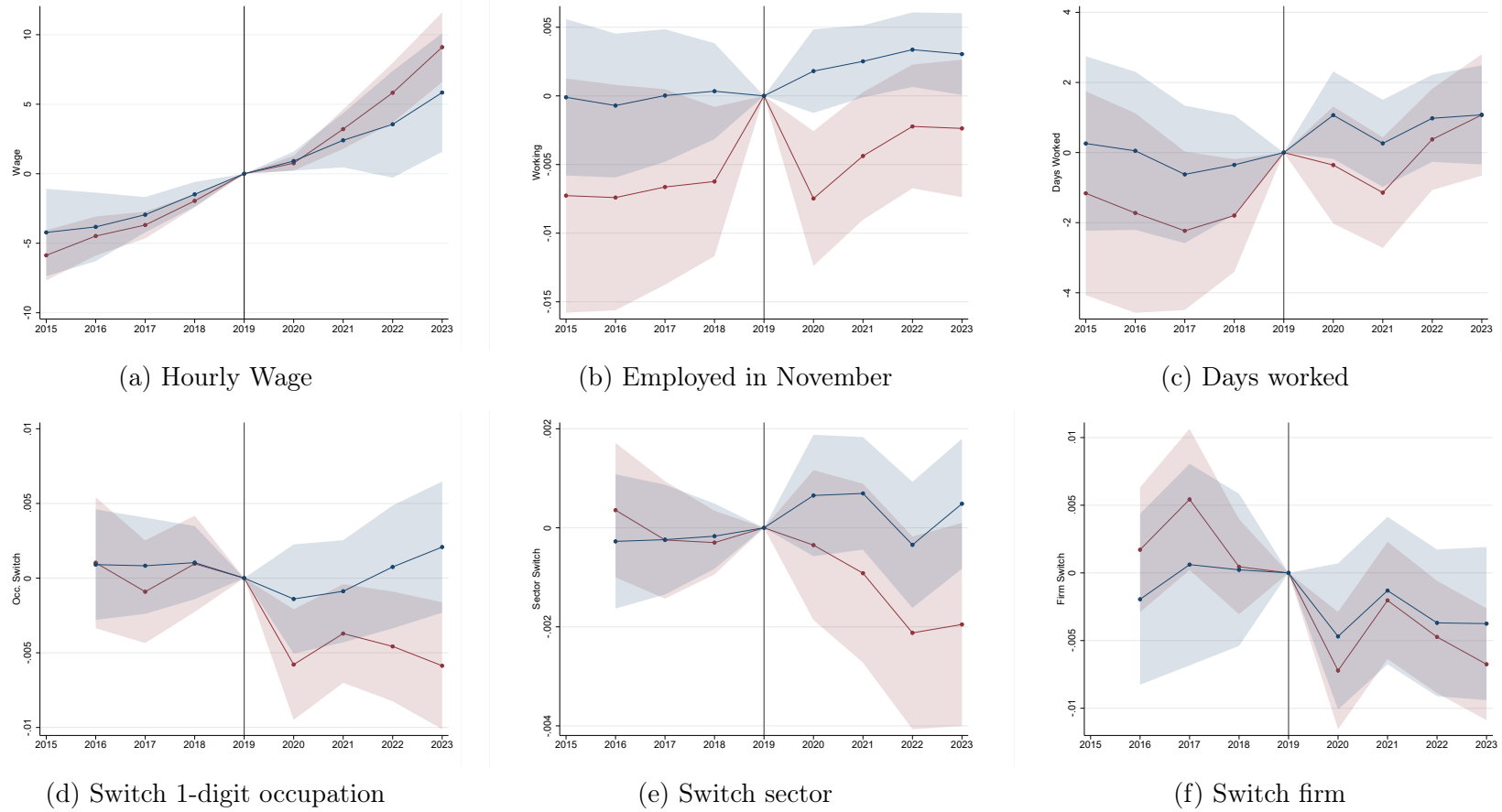
Notes: The figure reports event-study coefficients from equation (1). The treatment is standardized occupational digital intensity in the worker's 2019 four-digit occupation. Estimates are normalized to zero in 2019 and are estimated separately by sex, with worker and year fixed effects. For each medication category, the outcome equals one if the worker fills at least one prescription in the current year but none in the previous year. This transition indicator is defined for the full estimation sample, not only for previous non-users, and does not necessarily identify first-ever treatment. Shaded regions or bars denote 95 percent confidence intervals. Standard errors are clustered at the 2019 four-digit occupation level.

Figure 8: Event-study estimates of mental-health prescription cessation by baseline occupational digital intensity



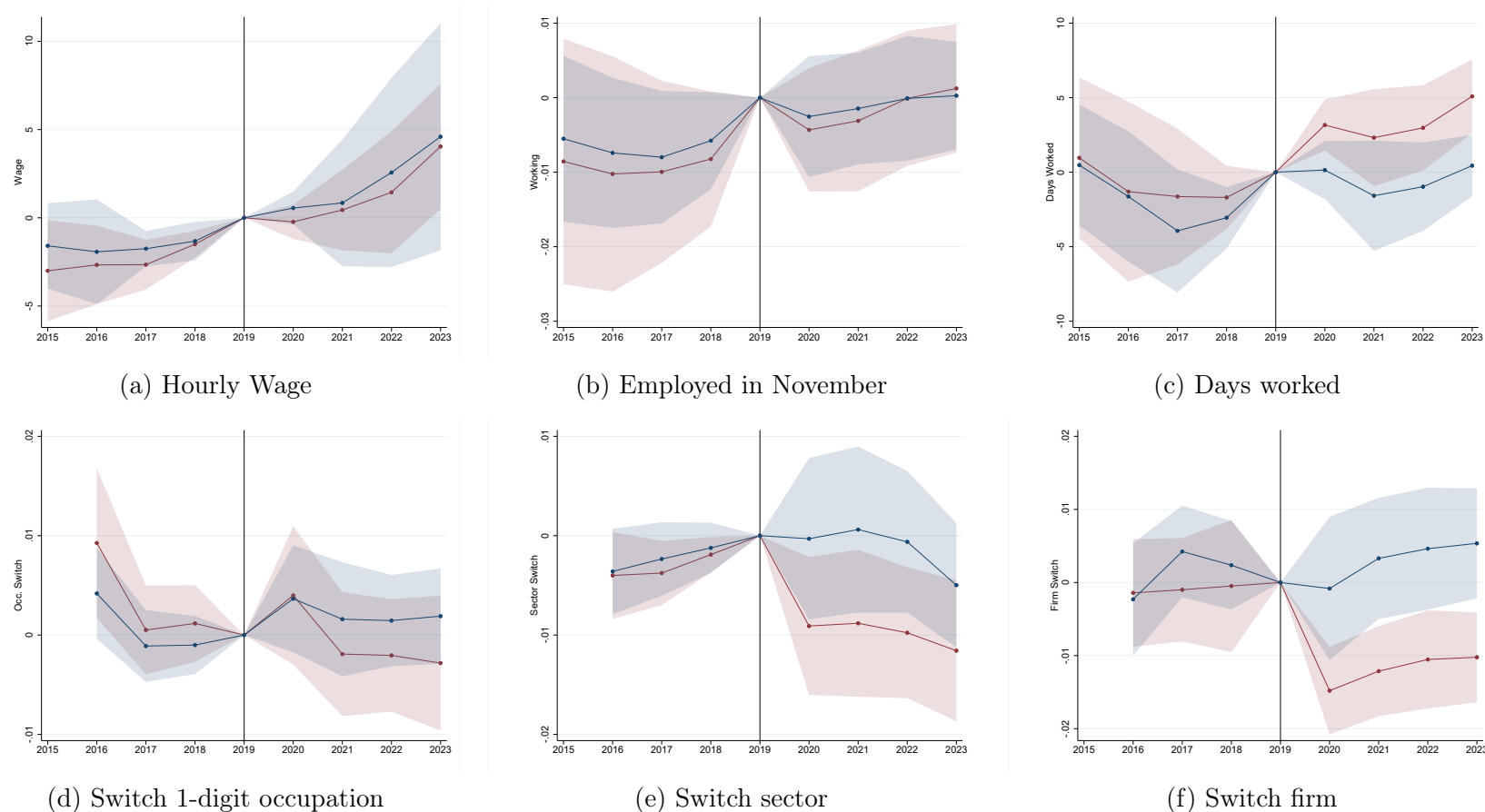
Notes: The figure reports event-study coefficients from equation (1). The treatment is standardized occupational digital intensity in the worker's 2019 four-digit occupation. Estimates are normalized to zero in 2019 and are estimated separately by sex, with worker and year fixed effects. For each medication category, the outcome equals one if the worker fills no prescription in the current year after filling at least one in the previous year. This transition indicator is defined for the full estimation sample, not only for previous users, and does not necessarily indicate permanent treatment cessation. Shaded regions or bars denote 95 percent confidence intervals. Standard errors are clustered at the 2019 four-digit occupation level.

Figure 9: Event-study estimates of labor-market dynamics by baseline occupational digital intensity



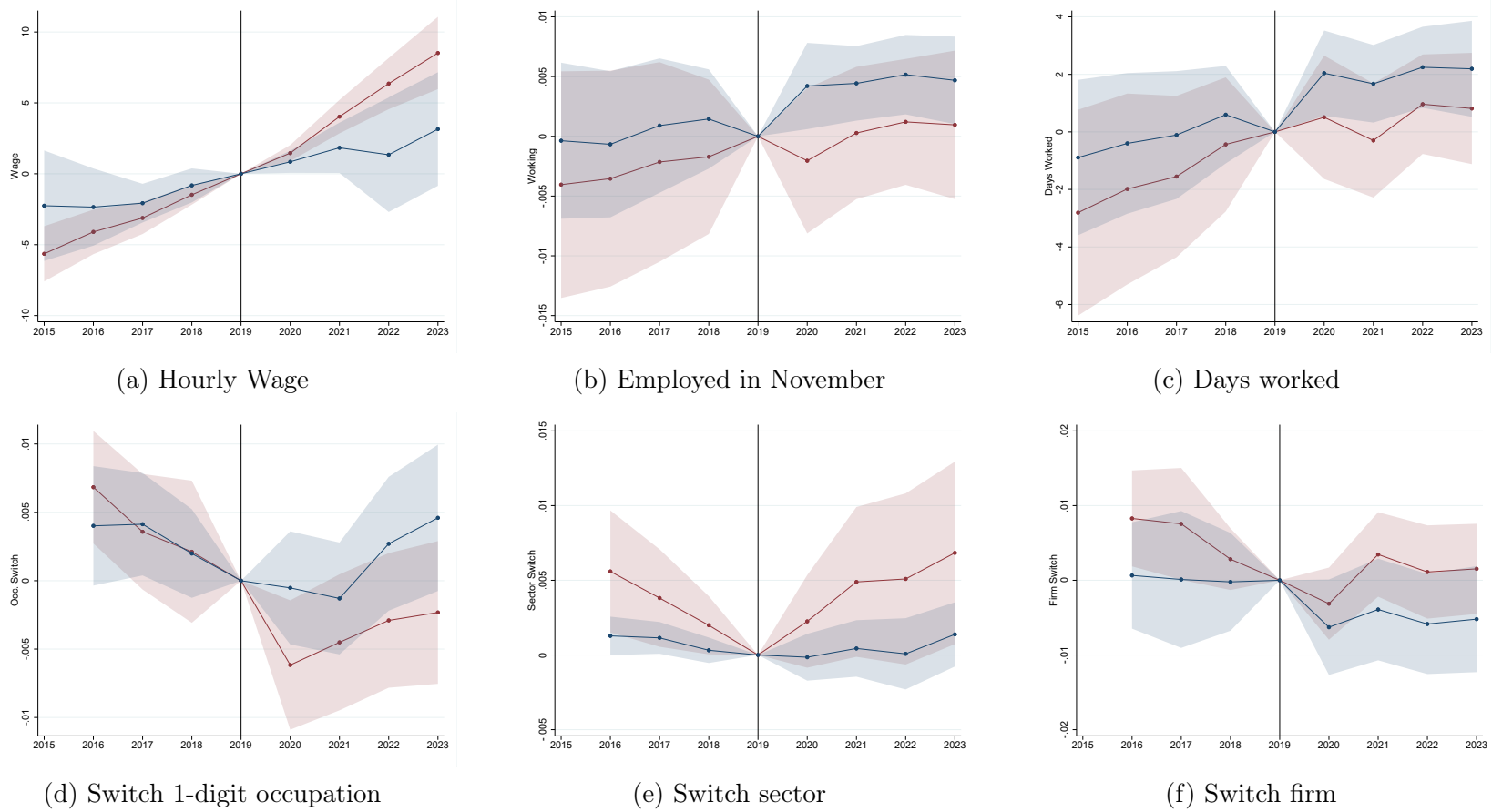
Notes: The figure reports event-study coefficients from equation (1). The treatment is standardized occupational digital intensity in the worker's 2019 four-digit occupation. Estimates are normalized to zero in 2019 and are estimated separately by sex, with worker and year fixed effects. In panel (a), hourly-wage coefficients are expressed in DKK per hour. In panel (c), coefficients are expressed in days worked per year. Employment in November and the occupation-, sector-, and firm-switching outcomes are binary indicators; their coefficients are reported in probability units, so 0.01 corresponds to 1 percentage point. Hourly wages are measured among workers who are working. Switching outcomes are defined only for individuals employed in both periods used to construct the transition. Shaded regions or bars denote 95 percent confidence intervals. Standard errors are clustered at the 2019 four-digit occupation level.

Figure 10: Event-study estimates of labor-market dynamics by baseline occupational digital intensity, public sector



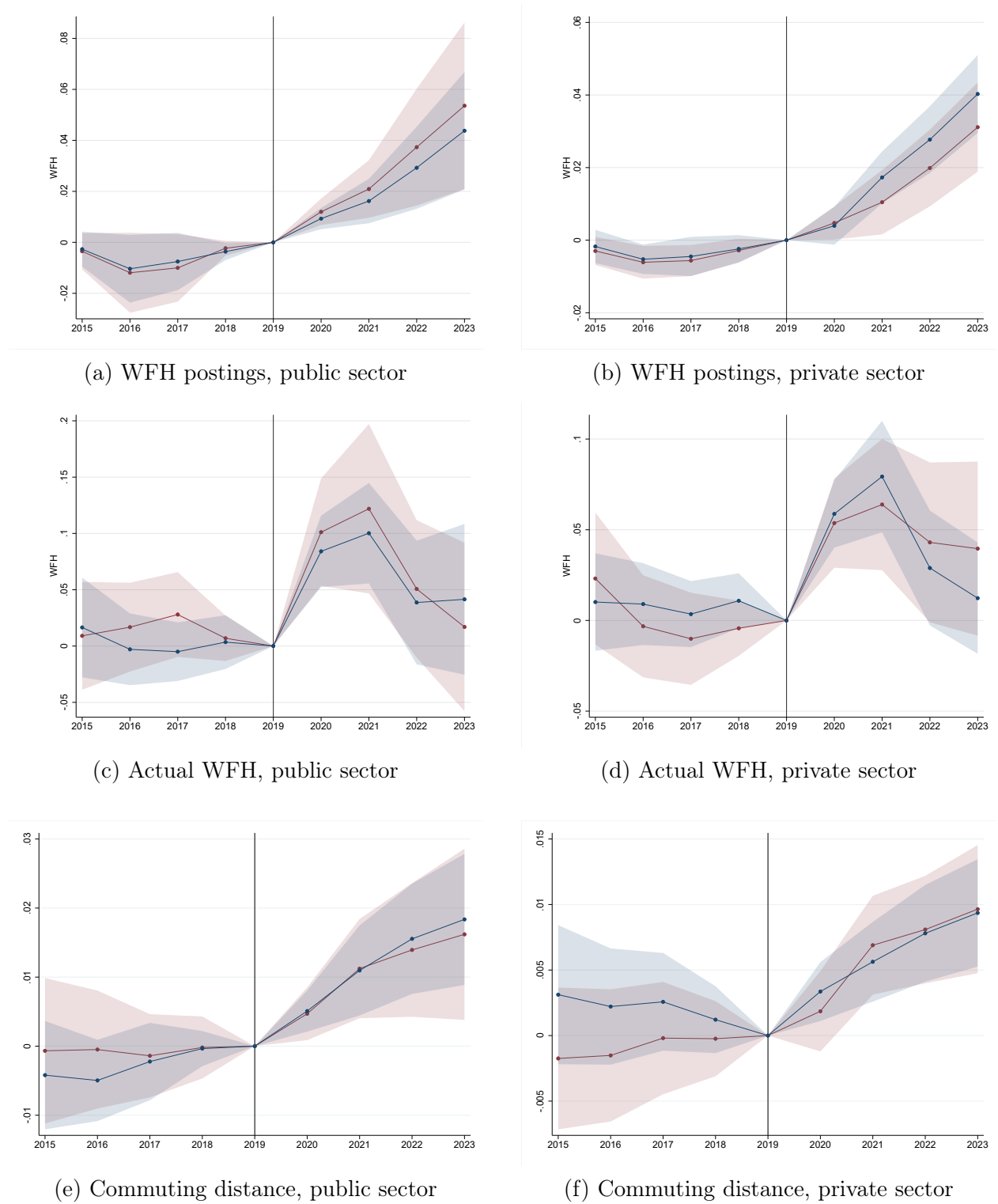
Notes: The figure reports event-study coefficients from equation (1). The treatment is standardized occupational digital intensity in the worker's 2019 four-digit occupation. Estimates are normalized to zero in 2019 and are estimated separately by sex, with worker and year fixed effects. In panel (a), hourly-wage coefficients are expressed in DKK per hour. In panel (c), coefficients are expressed in days worked per year. Employment in November and the occupation-, sector-, and firm-switching outcomes are binary indicators; their coefficients are reported in probability units, so 0.01 corresponds to 1 percentage point. Hourly wages are measured among workers who are working. Switching outcomes are defined only for individuals employed in both periods used to construct the transition. Shaded regions or bars denote 95 percent confidence intervals. Standard errors are clustered at the 2019 four-digit occupation level.

Figure 11: Event-study estimates of labor-market dynamics by baseline occupational digital intensity, private sector



Notes: The figure reports event-study coefficients from equation (1). The treatment is standardized occupational digital intensity in the worker's 2019 four-digit occupation. Estimates are normalized to zero in 2019 and are estimated separately by sex, with worker and year fixed effects. In panel (a), hourly-wage coefficients are expressed in DKK per hour. In panel (c), coefficients are expressed in days worked per year. Employment in November and the occupation-, sector-, and firm-switching outcomes are binary indicators; their coefficients are reported in probability units, so 0.01 corresponds to 1 percentage point. Hourly wages are measured among workers who are working. Switching outcomes are defined only for individuals employed in both periods used to construct the transition. Shaded regions or bars denote 95 percent confidence intervals. Standard errors are clustered at the 2019 four-digit occupation level.

Figure 12: Event-study estimates of WFH and commuting distance by baseline occupational digital intensity



Notes: Panels (a) and (b) use the occupation-year share of job postings advertising WFH. Panels (c) and (d) use an indicator for working from home for more than half of working hours during the preceding month, measured in the AKU. Panels (e) and (f) use an indicator for a home-work distance greater than 10 km, conditional on employment; they do not report effects on distance measured in kilometres or on commuting frequency. All coefficients are reported in probability or share units, not percentage points; 0.01 corresponds to 1 percentage point.

Table 4: Covid and post-Covid heterogeneity estimates by baseline subgroup, public sector

Sub-group	Depression		Anxiety		Sleep		Substance abuse	
	Wom.	Men	Wom.	Men	Wom.	Men	Wom.	Men
PANEL A: COVID PERIOD, 2020–2021								
Main result	−0.125** (0.0581)	−0.0753 (0.0775)	−0.0464** (0.0229)	−0.0539** (0.0235)	−0.126*** (0.0478)	−0.115*** (0.0418)	−0.00310 (0.0263)	−0.0234* (0.0127)
Not living in big city	−0.170*** (0.052)	−0.019 (0.082)	−0.105*** (0.033)	−0.010 (0.031)	−0.133** (0.056)	−0.062 (0.040)	0.019 (0.026)	−0.027* (0.016)
Commute >10 km	−0.171*** (0.069)	−0.069 (0.076)	−0.147*** (0.032)	−0.041 (0.028)	−0.158** (0.066)	−0.121** (0.059)	0.016 (0.024)	−0.042** (0.018)
Firm would be furloughable	0.296 (0.464)	−0.160 (0.293)	0.496* (0.294)	−1.101* (0.631)	1.036* (0.606)	0.368 (0.446)	−0.009 (0.128)	−0.164 (0.100)
Had children	−0.063 (0.524)	−0.072 (0.075)	0.143 (0.345)	−0.070* (0.036)	−0.059 (0.686)	−0.043 (0.061)	−0.064* (0.037)	−0.021 (0.021)
Had young children	−0.133* (0.073)	0.033 (0.102)	−0.066 (0.044)	−0.014 (0.038)	−0.096** (0.043)	−0.046 (0.069)	−0.011 (0.019)	−0.037 (0.026)
Living alone	−0.223* (0.115)	0.194 (0.131)	−0.113 (0.092)	−0.039 (0.054)	−0.210 (0.179)	−0.091 (0.098)	0.030 (0.052)	−0.071 (0.049)
Married	−0.145** (0.062)	−0.148** (0.058)	−0.098*** (0.031)	−0.090*** (0.027)	−0.085* (0.048)	−0.156*** (0.056)	−0.006 (0.024)	−0.012 (0.022)
Manager	0.046 (0.243)	−0.028 (0.258)	0.393*** (0.126)	−0.281* (0.143)	0.007 (0.306)	0.489** (0.192)	0.070 (0.078)	0.117 (0.094)
Professionals	−0.112*** (0.039)	−0.070 (0.065)	−0.095*** (0.028)	−0.060** (0.024)	−0.129*** (0.043)	−0.124*** (0.045)	−0.012 (0.018)	−0.027* (0.014)
Technicians	−0.053 (0.188)	0.134 (0.144)	0.001 (0.090)	−0.008 (0.048)	0.012 (0.165)	0.068 (0.075)	0.063 (0.086)	−0.027 (0.030)
Clerical	−0.208 (0.163)	−0.227 (0.461)	0.126 (0.167)	−0.289 (0.328)	−0.168 (0.268)	0.386 (0.392)	−0.020 (0.032)	0.082 (0.235)
PANEL B: POST-COVID PERIOD, 2022–2023								
Main result	−0.264** (0.120)	−0.117 (0.102)	−0.0580* (0.0317)	−0.0368 (0.0282)	−0.105 (0.0826)	−0.125** (0.0566)	−0.0199 (0.0330)	−0.00201 (0.0155)
Not living in big city	−0.340** (0.133)	−0.146 (0.117)	−0.123*** (0.039)	−0.003 (0.035)	−0.126 (0.095)	−0.118* (0.063)	0.004 (0.032)	−0.004 (0.020)
Commute >10 km	−0.305** (0.141)	−0.124 (0.104)	−0.143*** (0.050)	−0.031 (0.039)	−0.139 (0.121)	−0.124 (0.077)	0.002 (0.035)	−0.010 (0.023)
Firm would be furloughable	0.920* (0.534)	−2.595** (1.065)	0.833** (0.383)	−1.334 (0.800)	1.935*** (0.719)	−1.158 (0.735)	0.136 (0.132)	−0.181 (0.114)
Had children	0.526 (0.914)	−0.093 (0.093)	−0.508 (0.364)	−0.059 (0.039)	−0.379 (0.835)	−0.025 (0.075)	−0.015 (0.015)	−0.022 (0.023)
Had young children	−0.216 (0.155)	−0.047 (0.115)	−0.041 (0.047)	−0.006 (0.037)	−0.045 (0.108)	−0.081 (0.105)	−0.025 (0.029)	−0.038 (0.031)
Living alone	−0.361 (0.227)	0.234 (0.169)	−0.220** (0.106)	0.012 (0.072)	−0.111 (0.194)	−0.185* (0.109)	0.007 (0.060)	0.033 (0.055)
Married	−0.228* (0.124)	−0.147* (0.088)	−0.106** (0.041)	−0.050 (0.034)	−0.095 (0.074)	−0.135* (0.081)	−0.016 (0.032)	−0.019 (0.020)
Manager	−0.434 (0.572)	−0.822*** (0.150)	0.265 (0.160)	−0.451** (0.185)	0.172 (0.372)	0.423* (0.235)	−0.106 (0.101)	0.107 (0.123)
Professionals	−0.257** (0.105)	−0.139 (0.094)	−0.083** (0.035)	−0.032 (0.031)	−0.104 (0.087)	−0.139** (0.065)	−0.031 (0.022)	−0.004 (0.015)
Technicians	−0.236 (0.228)	0.376*** (0.120)	−0.148 (0.093)	−0.037 (0.055)	0.049 (0.173)	0.092 (0.104)	0.001 (0.061)	−0.005 (0.030)
Clerical	−0.669* (0.364)	−1.036** (0.436)	0.019 (0.126)	−0.152 (0.297)	0.078 (0.361)	0.441 (0.489)	−0.216*** (0.055)	−0.062 (0.284)

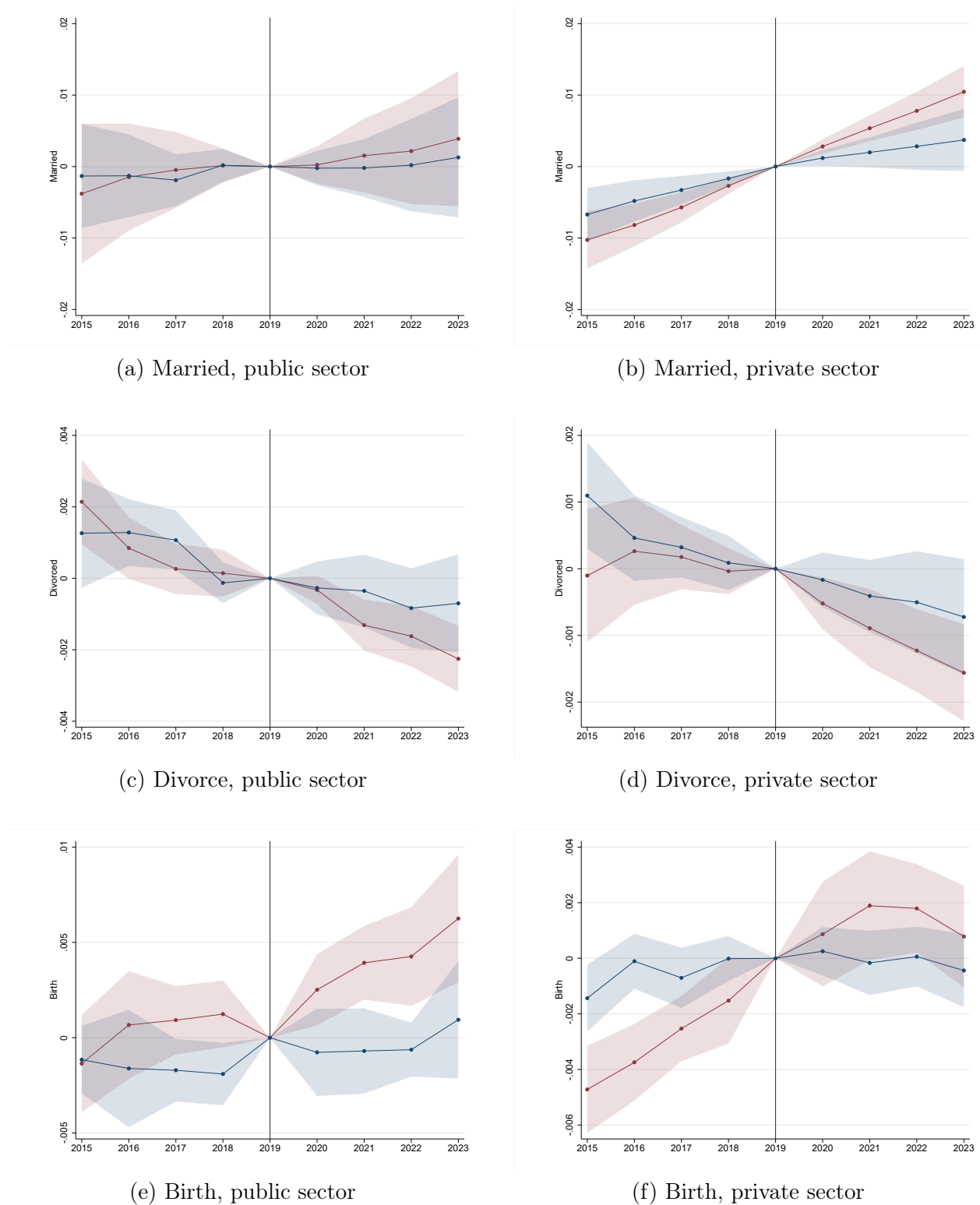
Notes: The table reports Covid-period and post-Covid estimates from the public sector specification. Each coefficient is the estimated effect of a one-standard-deviation increase in baseline occupational digital intensity for the indicated subgroup, sex, and outcome. Coefficients and standard errors are reported in percentage points. Subgroups are defined at baseline in 2019; *Commute >10 km* is based on one-way baseline commuting distance and *Had young children* is based on children under age 6. Standard errors, clustered at the 2019 four-digit occupation level, are reported in parentheses. The shaded rows report the corresponding main estimates from the sector-specific DID table. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 5: Covid and post-Covid heterogeneity estimates by baseline subgroup, private sector

Sub-group	Depression		Anxiety		Sleep		Substance abuse	
	Wom.	Men	Wom.	Men	Wom.	Men	Wom.	Men
PANEL A: COVID PERIOD, 2020–2021								
Main result	−0.0418 (0.0301)	0.0299 (0.0214)	0.0235* (0.0130)	−0.00580 (0.00841)	−0.0217 (0.0313)	0.0366 (0.0275)	−0.0309*** (0.00767)	0.0111* (0.00563)
Not living in big city	−0.076 (0.046)	−0.019 (0.032)	−0.017 (0.021)	−0.008 (0.012)	−0.039 (0.038)	0.015 (0.029)	−0.029*** (0.011)	0.019** (0.008)
Commute >10 km	−0.070 (0.045)	0.000 (0.037)	−0.022 (0.024)	−0.011 (0.012)	−0.113*** (0.043)	0.012 (0.031)	−0.030*** (0.011)	0.022*** (0.008)
Firm would be fur-loughable	0.017 (0.055)	−0.035 (0.041)	−0.026 (0.030)	−0.007 (0.023)	0.033 (0.054)	0.029 (0.046)	−0.027** (0.012)	0.010 (0.014)
Had children	0.288 (0.639)	−0.031 (0.028)	−0.077 (0.298)	0.002 (0.013)	1.617*** (0.520)	−0.031 (0.028)	−0.124 (0.211)	0.029*** (0.008)
Had young children	−0.093* (0.056)	−0.041 (0.034)	0.011 (0.037)	−0.024 (0.019)	0.033 (0.043)	−0.013 (0.023)	−0.003 (0.011)	0.010 (0.009)
Living alone	−0.101 (0.083)	0.114** (0.052)	−0.051 (0.045)	0.012 (0.032)	−0.097 (0.095)	0.083 (0.057)	−0.047** (0.023)	0.027 (0.019)
Married	−0.088** (0.034)	−0.048 (0.031)	−0.036 (0.024)	−0.003 (0.015)	−0.064 (0.044)	−0.001 (0.030)	−0.029*** (0.010)	0.016** (0.008)
Manager	−0.148 (0.099)	−0.023 (0.115)	0.051 (0.118)	−0.027 (0.026)	0.027 (0.169)	−0.042 (0.068)	−0.013 (0.049)	−0.032 (0.028)
Professionals	−0.074* (0.040)	−0.030 (0.029)	−0.014 (0.020)	0.000 (0.016)	−0.074* (0.043)	−0.007 (0.032)	−0.049*** (0.009)	0.019** (0.010)
Technicians	0.085 (0.073)	0.025 (0.080)	0.009 (0.047)	−0.008 (0.022)	0.076 (0.069)	0.078 (0.049)	0.005 (0.024)	0.027* (0.014)
Clerical	−0.215* (0.105)	−0.173** (0.083)	−0.073** (0.029)	−0.077** (0.038)	0.090 (0.109)	−0.187** (0.086)	0.001 (0.026)	−0.026 (0.029)
PANEL B: POST-COVID PERIOD, 2022–2023								
Main result	−0.119** (0.0519)	−0.0149 (0.0337)	0.0146 (0.0143)	−0.0135 (0.00997)	−0.0739* (0.0389)	0.0364 (0.0261)	−0.0616*** (0.0125)	0.00105 (0.00590)
Not living in big city	−0.164** (0.070)	−0.063 (0.045)	−0.046** (0.021)	−0.021* (0.012)	−0.102** (0.050)	0.005 (0.028)	−0.066*** (0.017)	0.010 (0.009)
Commute >10 km	−0.154* (0.078)	−0.052 (0.052)	−0.040* (0.024)	−0.010 (0.014)	−0.152*** (0.053)	0.014 (0.028)	−0.059*** (0.018)	0.017 (0.010)
Firm would be fur-loughable	−0.149 (0.091)	−0.072 (0.076)	−0.005 (0.037)	−0.014 (0.024)	−0.023 (0.063)	0.047 (0.039)	−0.044** (0.019)	0.002 (0.014)
Had children	−0.018 (0.872)	−0.090* (0.048)	−0.036 (0.447)	−0.008 (0.016)	0.702 (0.436)	0.006 (0.028)	−0.324 (0.402)	0.007 (0.010)
Had young children	−0.176* (0.100)	−0.109** (0.051)	0.037 (0.030)	−0.025 (0.023)	−0.006 (0.056)	0.024 (0.027)	−0.037** (0.016)	−0.011 (0.011)
Living alone	−0.167 (0.112)	0.021 (0.073)	−0.061 (0.058)	−0.014 (0.038)	−0.174* (0.099)	0.067 (0.062)	−0.082*** (0.029)	0.033* (0.017)
Married	−0.188*** (0.057)	−0.075 (0.047)	−0.028 (0.030)	−0.004 (0.018)	−0.118** (0.058)	0.021 (0.030)	−0.068*** (0.016)	0.006 (0.009)
Manager	−0.072 (0.163)	−0.067 (0.125)	0.036 (0.121)	−0.052* (0.028)	0.078 (0.189)	−0.031 (0.090)	0.035 (0.069)	−0.081*** (0.028)
Professionals	−0.170*** (0.065)	−0.084* (0.046)	−0.025 (0.022)	−0.013 (0.017)	−0.146*** (0.054)	−0.035 (0.034)	−0.087*** (0.017)	0.017 (0.011)
Technicians	0.106 (0.178)	0.079 (0.086)	0.006 (0.032)	−0.004 (0.020)	0.011 (0.095)	0.066 (0.049)	0.002 (0.033)	0.022 (0.014)
Clerical	−0.134 (0.198)	−0.339* (0.175)	−0.109** (0.041)	−0.077 (0.063)	0.034 (0.158)	0.116 (0.102)	−0.029 (0.036)	−0.039 (0.029)

Notes: The table reports Covid-period and post-Covid estimates from the private sector specification. Each coefficient is the estimated effect of a one-standard-deviation increase in baseline occupational digital intensity for the indicated subgroup, sex, and outcome. Coefficients and standard errors are reported in percentage points. Subgroups are defined at baseline in 2019; *Commute >10 km* is based on one-way baseline commuting distance and *Had young children* is based on children under age 6. Standard errors, clustered at the 2019 four-digit occupation level, are reported in parentheses. The shaded rows report the corresponding main estimates from the sector-specific DID table. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

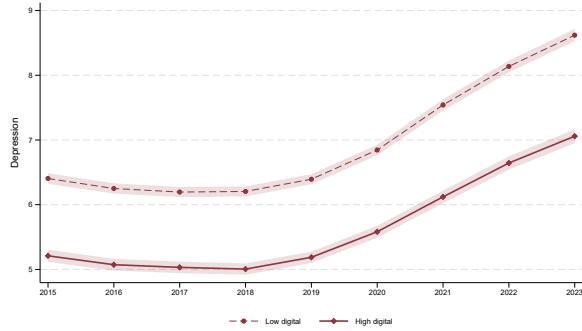
Figure 13: Event-study estimates of broader life outcomes by baseline occupational digital intensity



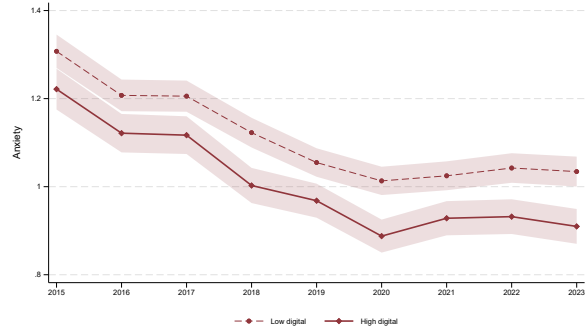
Notes: The figure reports event-study coefficients from equation (1). The treatment is standardized occupational digital intensity in the worker's 2019 four-digit occupation. Estimates are normalized to zero in 2019 and are estimated separately by sex, with worker and year fixed effects. Outcomes are binary indicators for the indicated family outcomes. Coefficients are reported in probability units. Shaded regions or bars denote 95 percent confidence intervals. Standard errors are clustered at the 2019 four-digit occupation level.

Appendix

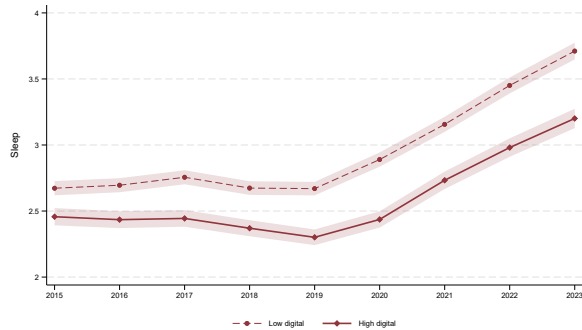
Figure A.1: Descriptive prescription trends by baseline occupational digital readiness: women



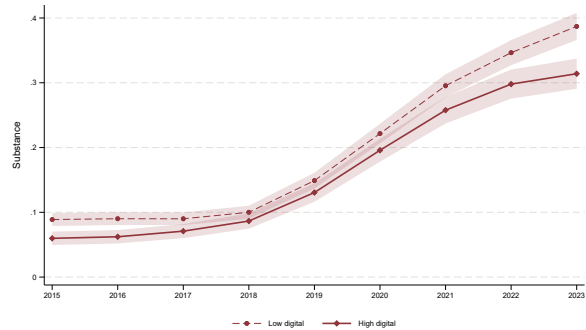
(a) Depression



(b) Anxiety



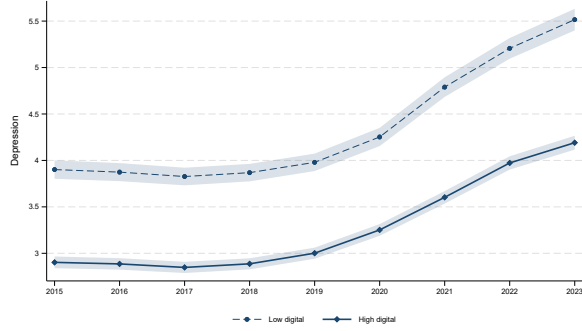
(c) Sleep



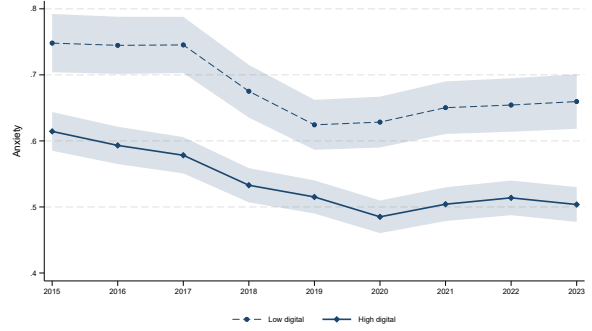
(d) Substance abuse

Notes: Figures show raw mean prescription rates for women by low and high baseline occupational digital readiness. Outcomes are shown in percent. The sample covers 2015–2023 and consists of workers employed in 2019 in white-collar occupations—managers, professionals, technicians, and clerical workers—who were ages 29–60 in 2019. Low and high digital readiness are defined using a median split of 2019 postings-based occupational digital intensity in the worker’s four-digit baseline occupation. The figures are descriptive and do not condition on worker fixed effects or other controls.

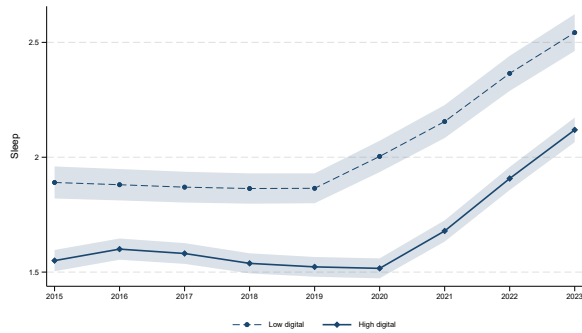
Figure A.2: Descriptive prescription trends by baseline occupational digital readiness: men



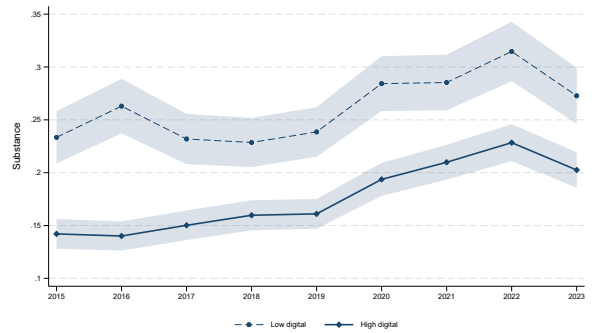
(a) Depression



(b) Anxiety



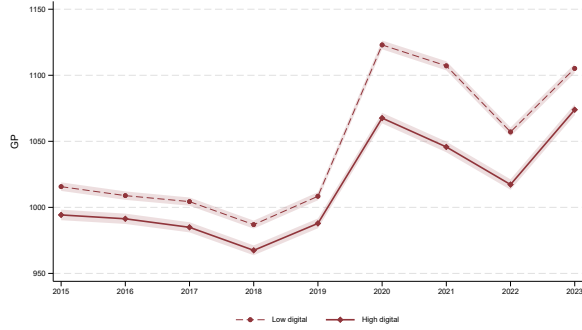
(c) Sleep



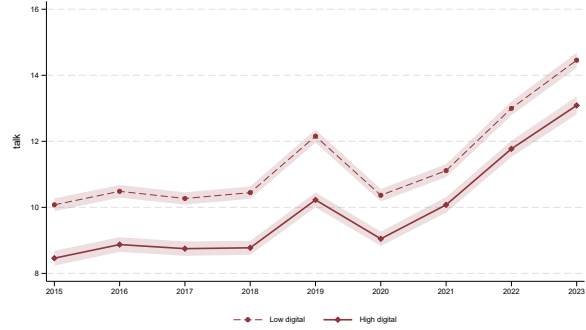
(d) Substance abuse

Notes: Figures show raw mean prescription rates for men by low and high baseline occupational digital readiness. Outcomes are shown in percent. The sample covers 2015–2023 and consists of workers employed in 2019 in white-collar occupations—managers, professionals, technicians, and clerical workers—who were ages 29–60 in 2019. Low and high digital readiness are defined using a median split of 2019 postings-based occupational digital intensity in the worker’s four-digit baseline occupation. The figures are descriptive and do not condition on worker fixed effects or other controls.

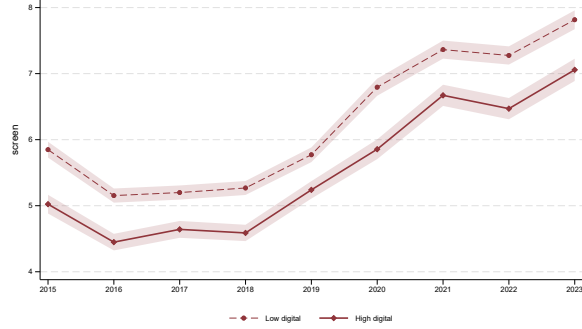
Figure A.3: Descriptive utilization trends by baseline occupational digital readiness: women



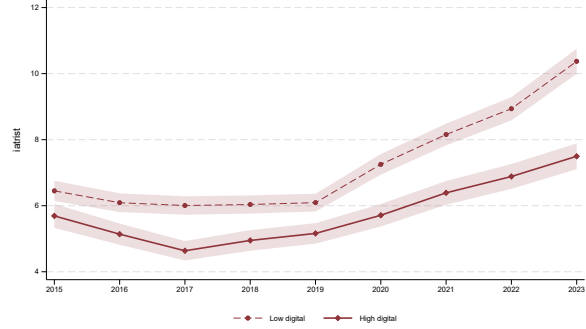
(a) GP visits



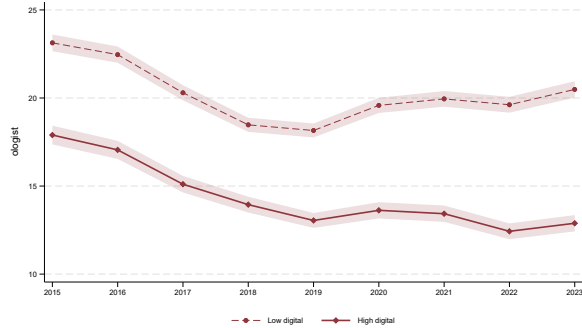
(b) Talk Therapy



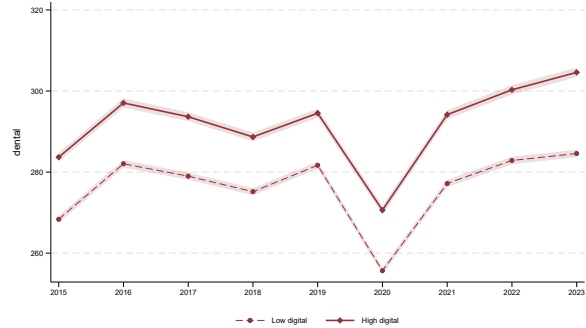
(c) Mental Health Screening



(d) Psychiatrist



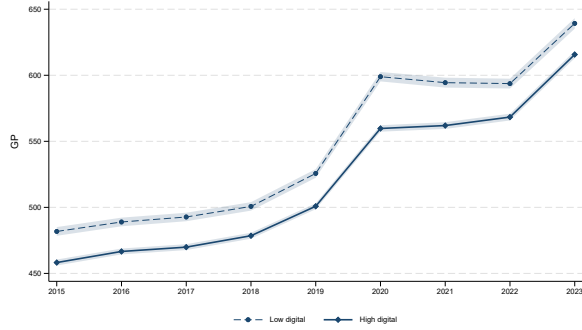
(e) Psychologist



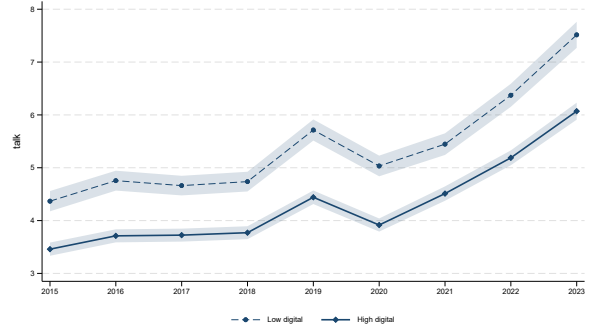
(f) Dental

Notes: Figures show raw mean utilization for women by low and high baseline occupational digital readiness. The sample covers 2015–2023 and consists of workers employed in 2019 in white-collar occupations—managers, professionals, technicians, and clerical workers—who were ages 29–60 in 2019. Low and high digital readiness are defined using a median split of 2019 postings-based occupational digital intensity in the worker’s four-digit baseline occupation. The figures are descriptive and do not condition on worker fixed effects or other controls.

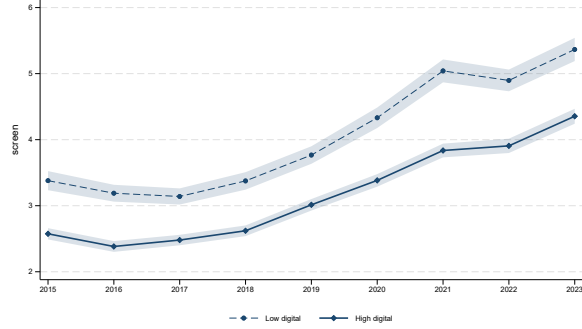
Figure A.4: Descriptive utilization trends by baseline occupational digital readiness: men



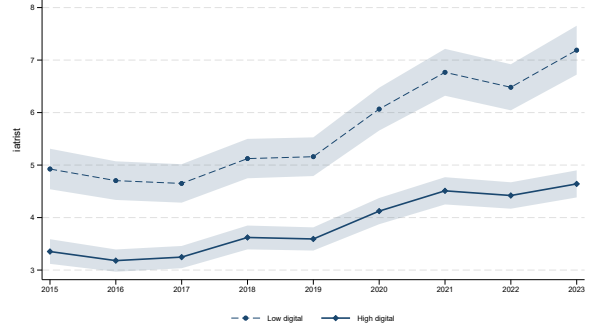
(a) GP visits



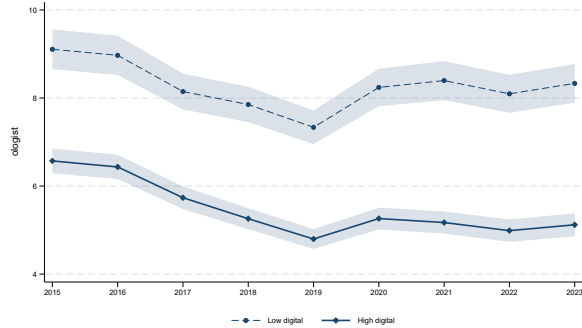
(b) Talk Therapy



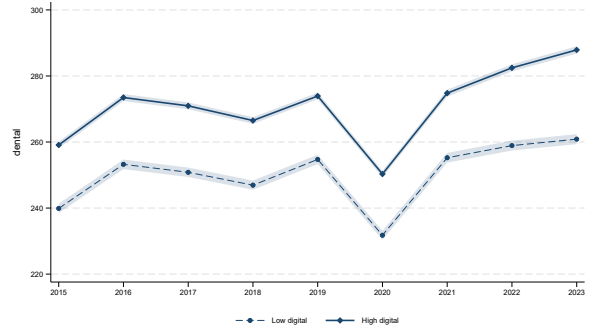
(c) Mental Health Screening



(d) Psychiatrist



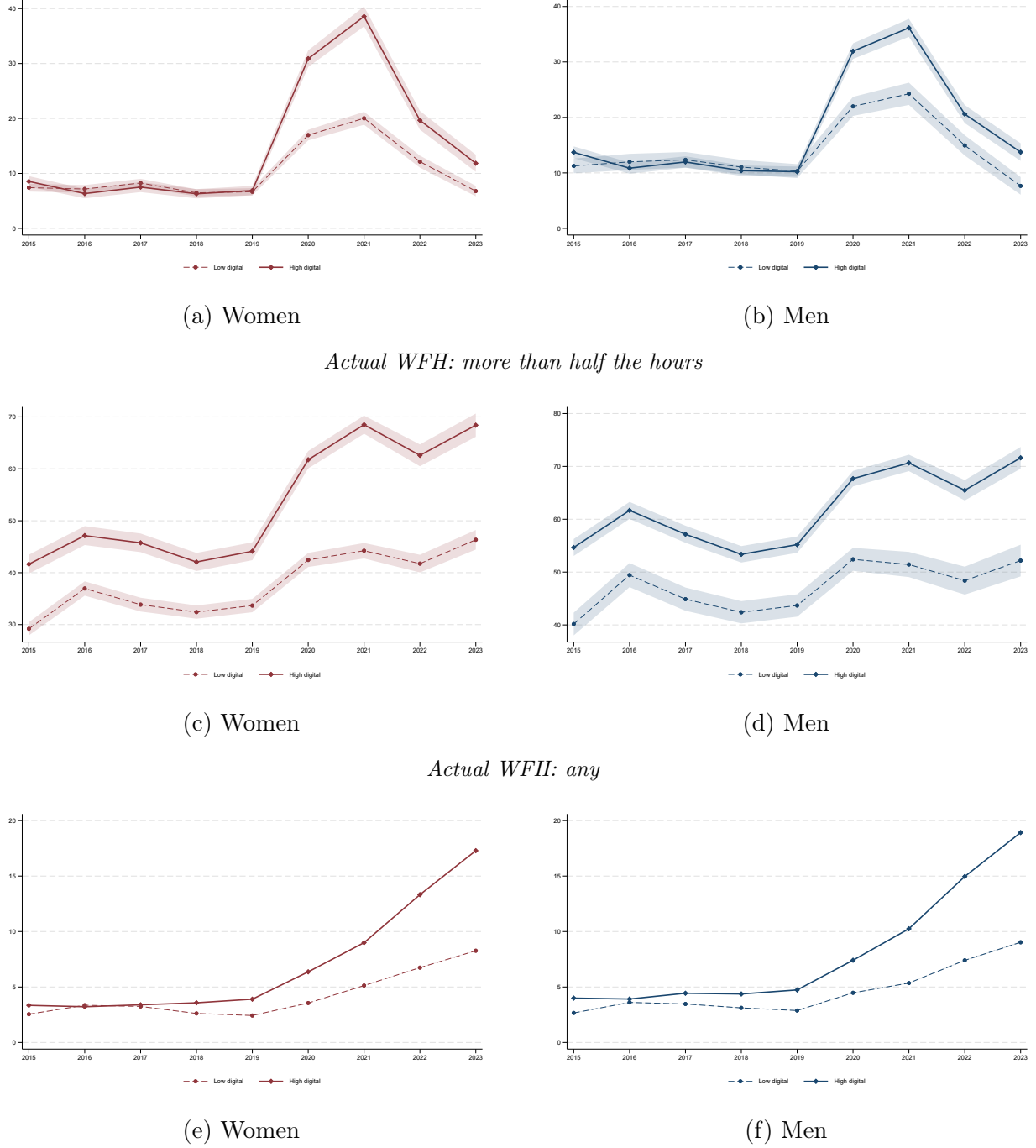
(e) Psychologist



(f) Dental

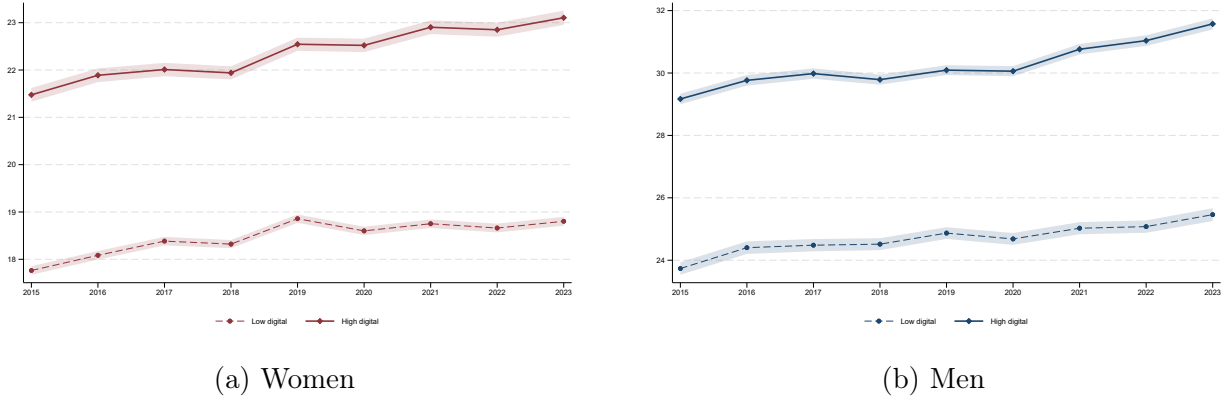
Notes: Figures show raw mean utilization for men by low and high baseline occupational digital readiness. The sample covers 2015–2023 and consists of workers employed in 2019 in white-collar occupations—managers, professionals, technicians, and clerical workers—who were ages 29–60 in 2019. Low and high digital readiness are defined using a median split of 2019 postings-based occupational digital intensity in the worker’s four-digit baseline occupation. The figures are descriptive and do not condition on worker fixed effects or other controls.

Figure A.5: Descriptive work-from-home trends by baseline occupational digital readiness



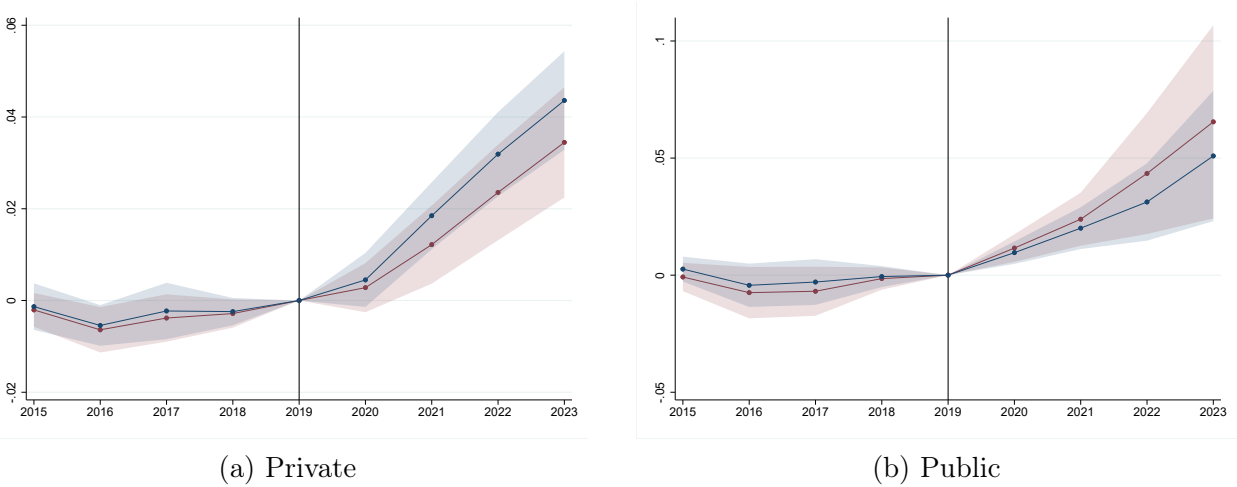
Notes: Figures show raw mean work-from-home rates by low and high baseline occupational digital readiness. Women are shown in the first column and men in the second column. Outcomes are shown in percentage points. The sample covers 2015–2023 and consists of workers employed in 2019 in white-collar occupations—managers, professionals, technicians, and clerical workers—who were ages 29–60 in 2019. Low and high digital readiness are defined using a median split of 2019 postings-based occupational digital intensity in the worker’s four-digit baseline occupation. The figures are descriptive and do not condition on worker fixed effects or other controls.

Figure A.6: Descriptive commuting trends by baseline occupational digital readiness



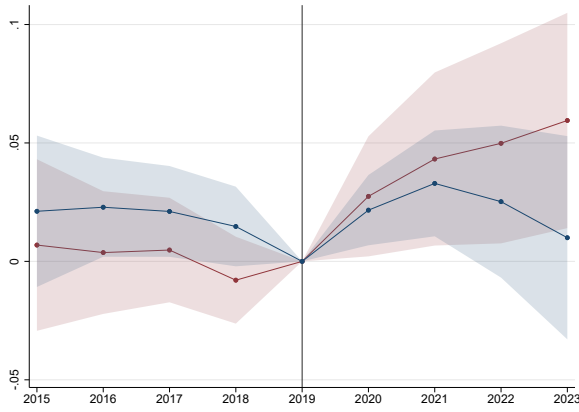
Notes: Figures show raw commuting distance by low and high baseline occupational digital readiness. Women are shown in the left panel and men in the right panel. The sample covers 2015–2023 and consists of workers employed in 2019 in white-collar occupations—managers, professionals, technicians, and clerical workers—who were ages 29–60 in 2019. Low and high digital readiness are defined using a median split of 2019 postings-based occupational digital intensity in the worker’s four-digit baseline occupation. The figures are descriptive and do not condition on worker fixed effects or other controls.

Figure A.7: Event-study estimates of sector specific posting based WFH by baseline occupational digital intensity

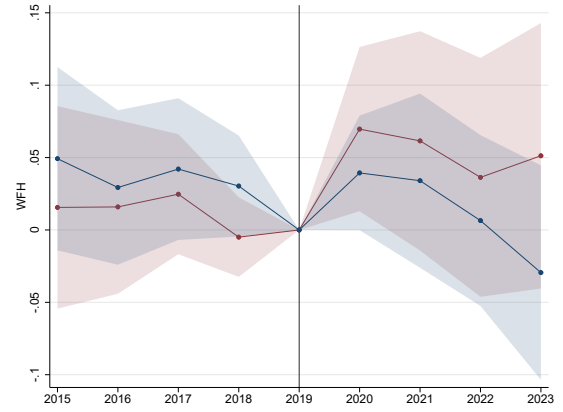


Notes: The figure reports event-study coefficients from equation (1) with the indicated labor-market or WFH measure as the outcome. The treatment is standardized occupational digital intensity in the worker’s 2019 four-digit occupation. Estimates are normalized to zero in 2019 and estimated separately by sex. Standard errors are clustered at the 2019 four-digit occupation level.

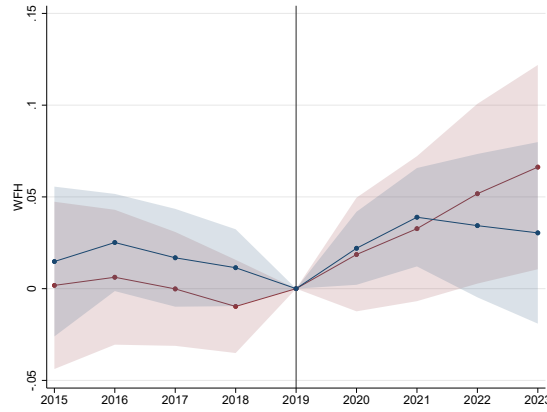
Figure A.8: Event-study estimates of any WFH by baseline occupational digital intensity



(a) Actual WFH, any level



(b) Actual WFH, public sector



(c) Actual WFH, private sector

Notes: The figure reports event-study coefficients from equation (1) with the indicated labor-market or WFH measure as the outcome. The treatment is standardized occupational digital intensity in the worker's 2019 four-digit occupation. Estimates are normalized to zero in 2019 and estimated separately by sex. Standard errors are clustered at the 2019 four-digit occupation level.

Table A.1: WFH-scaled difference-in-differences estimates for mental-health prescriptions: public sector

	Depression		Sleep		Substance Abuse		Anxiety	
	Women	Men	Women	Men	Women	Men	Women	Men
PANEL A: AKU WFH DENOMINATOR								
<i>Covid estimates, 2020–2021</i>								
Mental-health estimate	−0.126** (0.058)	−0.076 (0.078)	−0.127*** (0.048)	−0.115*** (0.042)	−0.0031 (0.026)	−0.023* (0.013)	−0.046** (0.023)	−0.054** (0.023)
WFH estimate	10.220*** (2.502)	8.556*** (1.606)	10.220*** (2.502)	8.556*** (1.606)	10.220*** (2.502)	8.556*** (1.606)	10.220*** (2.502)	8.556*** (1.606)
Scaled estimate	−0.012* (0.0064)	−0.0088 (0.0092)	−0.012** (0.0056)	−0.013** (0.0055)	−0.0003 (0.0026)	−0.0027* (0.0016)	−0.0045* (0.0025)	−0.0063** (0.0030)
<i>Post-Covid estimates, 2022–2023</i>								
Mental-health estimate	−0.264** (0.120)	−0.117 (0.102)	−0.106 (0.083)	−0.125** (0.057)	−0.020 (0.033)	−0.0020 (0.016)	−0.058* (0.032)	−0.037 (0.028)
WFH estimate	2.806 (2.380)	2.620 (2.654)	2.806 (2.380)	2.620 (2.654)	2.806 (2.380)	2.620 (2.654)	2.806 (2.380)	2.620 (2.654)
Scaled estimate	−0.094 (0.091)	−0.045 (0.060)	−0.038 (0.043)	−0.048 (0.053)	−0.0071 (0.013)	−0.0008 (0.0060)	−0.021 (0.021)	−0.014 (0.018)
PANEL B: JOB-POSTING WFH DENOMINATOR								
<i>Covid estimates, 2020–2021</i>								
Mental-health estimate	−0.126** (0.058)	−0.076 (0.078)	−0.127*** (0.048)	−0.115*** (0.042)	−0.0031 (0.026)	−0.023* (0.013)	−0.046** (0.023)	−0.054** (0.023)
WFH estimate	2.157*** (0.541)	1.728*** (0.420)	2.157*** (0.541)	1.728*** (0.420)	2.157*** (0.541)	1.728*** (0.420)	2.157*** (0.541)	1.728*** (0.420)
Scaled estimate	−0.058* (0.031)	−0.044 (0.046)	−0.059** (0.027)	−0.067** (0.029)	−0.0014 (0.012)	−0.014* (0.0081)	−0.021* (0.012)	−0.031** (0.016)
<i>Post-Covid estimates, 2022–2023</i>								
Mental-health estimate	−0.264** (0.120)	−0.117 (0.102)	−0.106 (0.083)	−0.125** (0.057)	−0.020 (0.033)	−0.0020 (0.016)	−0.058* (0.032)	−0.037 (0.028)
WFH estimate	5.055*** (1.508)	4.098*** (1.057)	5.055*** (1.508)	4.098*** (1.057)	5.055*** (1.508)	4.098*** (1.057)	5.055*** (1.508)	4.098*** (1.057)
Scaled estimate	−0.052* (0.028)	−0.029 (0.026)	−0.021 (0.018)	−0.030* (0.016)	−0.0039 (0.0067)	−0.0005 (0.0038)	−0.011 (0.0071)	−0.0090 (0.0073)

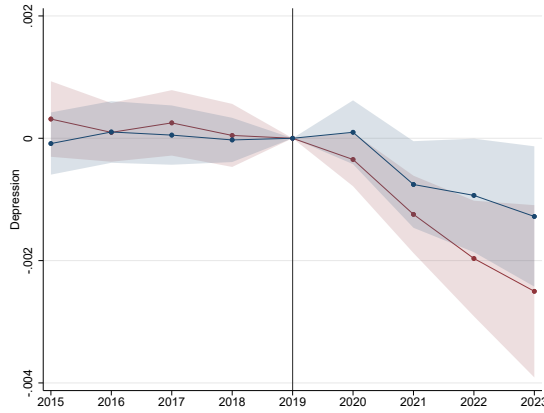
Notes: Standard errors are in parentheses. Mental-health estimates are the reduced-form DID estimates reported in the main mental-health prescription table 2. WFH estimates are DID estimates from equation (2), using either realized WFH from the AKU or the job-posting WFH measure as the dependent variable. Scaled estimates divide the mental-health estimate by the corresponding WFH estimate for the same sex, outcome, period, sector, and WFH denominator. Standard errors for scaled estimates are computed using a first-order delta-method approximation based on the underlying standard errors of the mental-health and WFH estimates. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.2: WFH-scaled difference-in-differences estimates for mental-health prescriptions: private sector

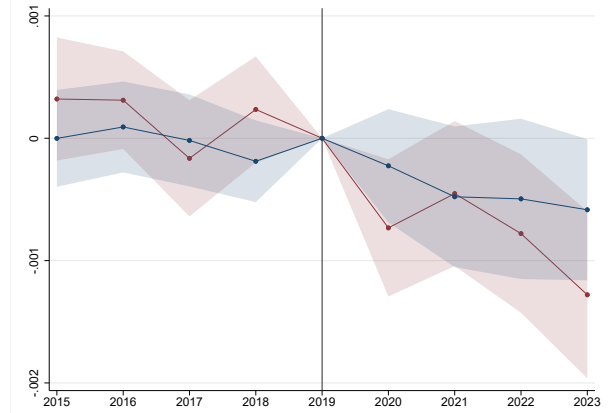
	Depression		Sleep		Substance Abuse		Anxiety	
	Women	Men	Women	Men	Women	Men	Women	Men
PANEL A: AKU WFH DENOMINATOR								
<i>Covid estimates, 2020–2021</i>								
Mental-health estimate	−0.042 (0.030)	0.030 (0.021)	−0.022 (0.031)	0.037 (0.028)	−0.031*** (0.0077)	0.011** (0.0056)	0.024* (0.013)	−0.0058 (0.0084)
WFH estimate	5.490*** (1.293)	5.980*** (0.978)	5.490*** (1.293)	5.980*** (0.978)	5.490*** (1.293)	5.980*** (0.978)	5.490*** (1.293)	5.980*** (0.978)
Scaled estimate	−0.0076 (0.0058)	0.0050 (0.0037)	−0.0040 (0.0058)	0.0061 (0.0047)	−0.0056*** (0.0019)	0.0019* (0.0010)	0.0043* (0.0026)	−0.0010 (0.0014)
<i>Post-Covid estimates, 2022–2023</i>								
Mental-health estimate	−0.120** (0.052)	−0.015 (0.034)	−0.074* (0.039)	0.036 (0.026)	−0.062*** (0.012)	0.0011 (0.0059)	0.015 (0.014)	−0.014 (0.0100)
WFH estimate	3.487* (1.970)	0.994 (1.204)	3.487* (1.970)	0.994 (1.204)	3.487* (1.970)	0.994 (1.204)	3.487* (1.970)	0.994 (1.204)
Scaled estimate	−0.034 (0.024)	−0.015 (0.039)	−0.021 (0.016)	0.037 (0.052)	−0.018* (0.011)	0.0011 (0.0061)	0.0042 (0.0047)	−0.014 (0.019)
PANEL B: JOB-POSTING WFH DENOMINATOR								
<i>Covid estimates, 2020–2021</i>								
Mental-health estimate	−0.042 (0.030)	0.030 (0.021)	−0.022 (0.031)	0.037 (0.028)	−0.031*** (0.0077)	0.011** (0.0056)	0.024* (0.013)	−0.0058 (0.0084)
WFH estimate	1.085*** (0.301)	1.318*** (0.247)	1.085*** (0.301)	1.318*** (0.247)	1.085*** (0.301)	1.318*** (0.247)	1.085*** (0.301)	1.318*** (0.247)
Scaled estimate	−0.039 (0.030)	0.023 (0.017)	−0.020 (0.029)	0.028 (0.022)	−0.029*** (0.011)	0.0084* (0.0046)	0.022 (0.013)	−0.0044 (0.0064)
<i>Post-Covid estimates, 2022–2023</i>								
Mental-health estimate	−0.120** (0.052)	−0.015 (0.034)	−0.074* (0.039)	0.036 (0.026)	−0.062*** (0.012)	0.0011 (0.0059)	0.015 (0.014)	−0.014 (0.0100)
WFH estimate	2.868*** (0.564)	3.650*** (0.454)	2.868*** (0.564)	3.650*** (0.454)	2.868*** (0.564)	3.650*** (0.454)	2.868*** (0.564)	3.650*** (0.454)
Scaled estimate	−0.042** (0.020)	−0.0041 (0.0093)	−0.026* (0.015)	0.010 (0.0073)	−0.022*** (0.0061)	0.0003 (0.0016)	0.0051 (0.0051)	−0.0037 (0.0028)

Notes: *Notes:* Standard errors are in parentheses. Mental-health estimates are the reduced-form DID estimates reported in the main mental-health prescription table 2. WFH estimates are DID estimates from equation (2), using either realized WFH from the AKU or the job-posting WFH measure as the dependent variable. Scaled estimates divide the mental-health estimate by the corresponding WFH estimate for the same sex, outcome, period, sector, and WFH denominator. Standard errors for scaled estimates are computed using a first-order delta-method approximation based on the underlying standard errors of the mental-health and WFH estimates. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

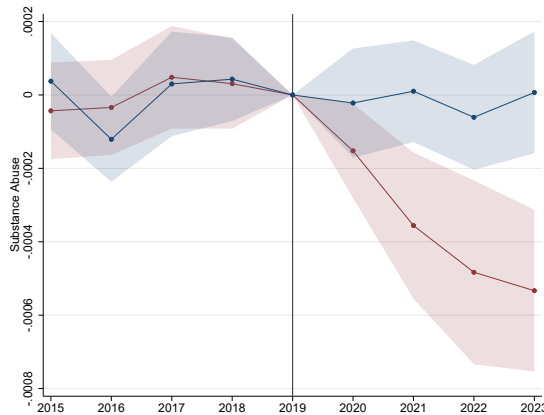
Figure A.9: Mental-health event studies controlling for one-digit occupation trends



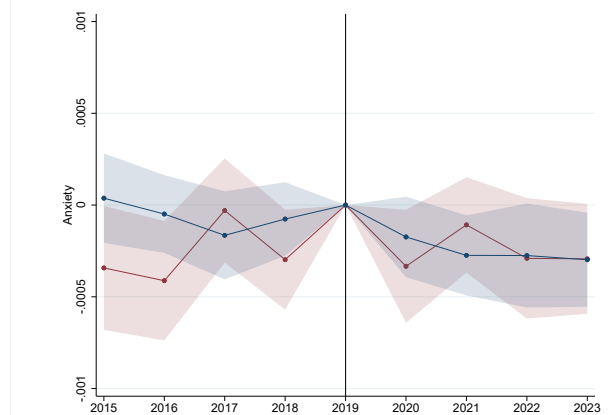
(a) Depression



(b) Sleep



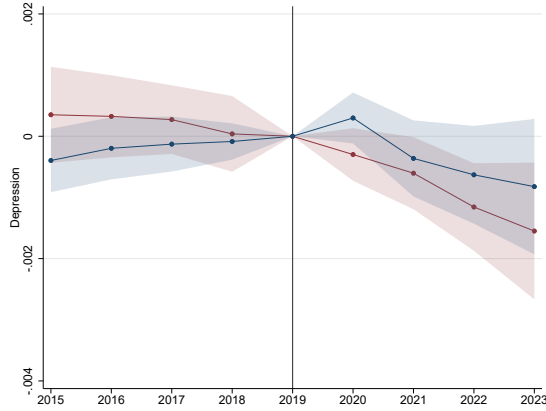
(c) Substance abuse



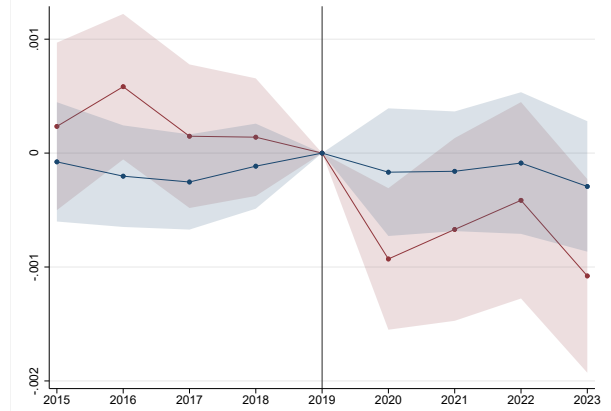
(d) Anxiety

Notes: The figure reports event-study coefficients from equation (1). The treatment is standardized occupational digital intensity in the worker's 2019 four-digit occupation. Estimates are normalized to zero in 2019 and are estimated separately by sex, with worker and year fixed effects. Outcomes are indicators equal to one if the worker fills at least one prescription in the indicated medication category during the year. Coefficients are reported in probability units. Shaded regions or bars denote 95 percent confidence intervals. Standard errors are clustered at the 2019 four-digit occupation level.

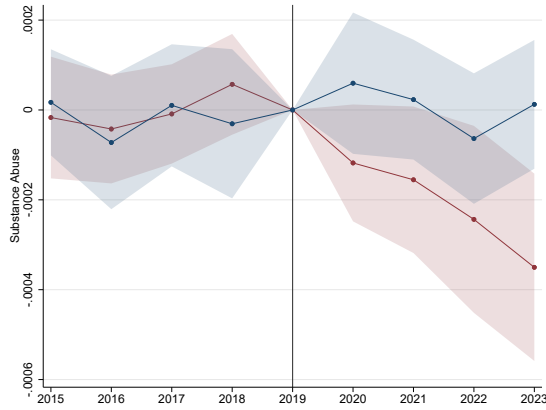
Figure A.10: Mental-health event studies for occupation stayers



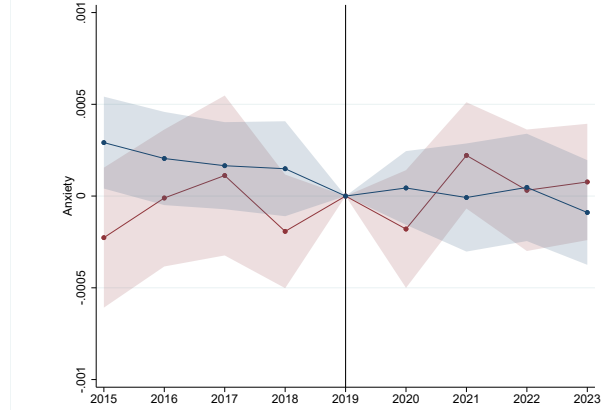
(a) Depression



(b) Sleep



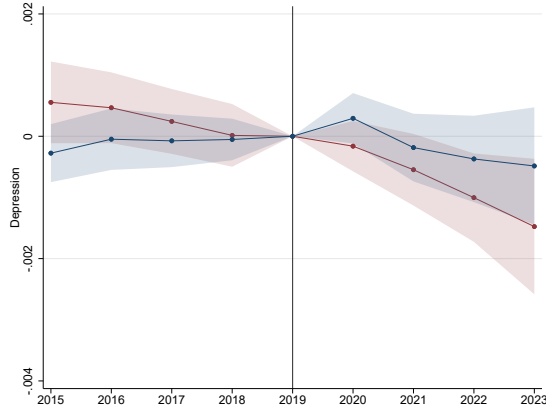
(c) Substance abuse



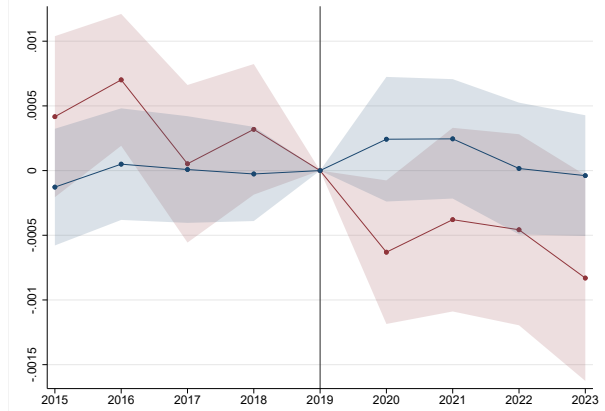
(d) Anxiety

Notes: The figure reports event-study coefficients from equation (1). The treatment is standardized occupational digital intensity in the worker's 2019 four-digit occupation. Estimates are normalized to zero in 2019 and are estimated separately by sex, with worker and year fixed effects. Outcomes are indicators equal to one if the worker fills at least one prescription in the indicated medication category during the year. Coefficients are reported in probability units. Shaded regions or bars denote 95 percent confidence intervals. Standard errors are clustered at the 2019 four-digit occupation level.

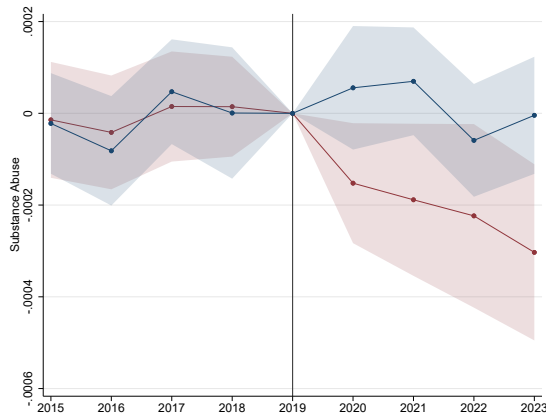
Figure A.11: Mental-health event studies for sector stayers



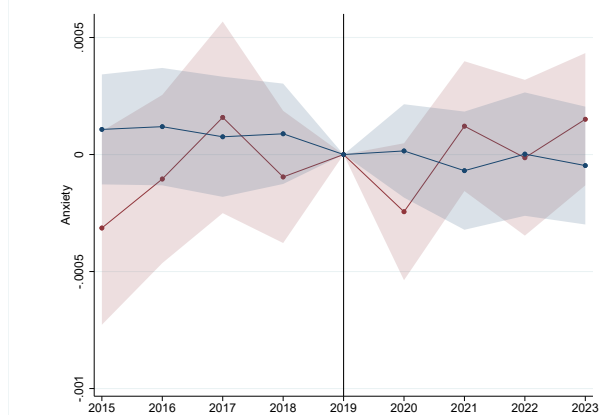
(a) Depression



(b) Sleep



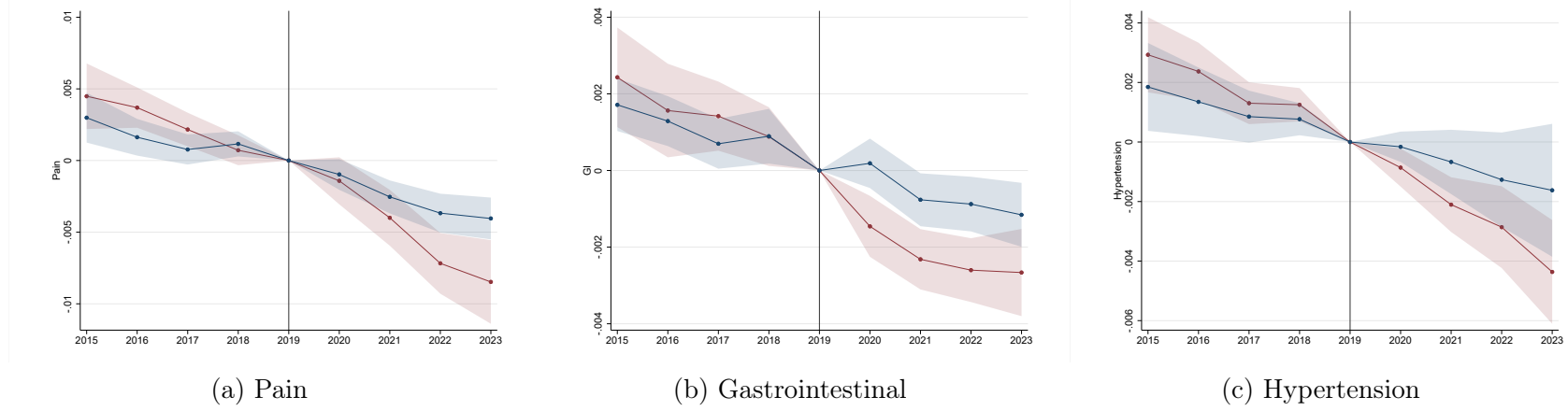
(c) Substance abuse



(d) Anxiety

Notes: The figure reports event-study coefficients from equation (1). The treatment is standardized occupational digital intensity in the worker's 2019 four-digit occupation. Estimates are normalized to zero in 2019 and are estimated separately by sex, with worker and year fixed effects. Outcomes are indicators equal to one if the worker fills at least one prescription in the indicated medication category during the year. Coefficients are reported in probability units. Shaded regions or bars denote 95 percent confidence intervals. Standard errors are clustered at the 2019 four-digit occupation level.

Figure A.12: Event-study estimates for other medication outcomes by baseline occupational digital intensity



Notes: The figure displays event-study coefficients from estimating equation (1). The coefficients come from a single regression interacting year indicators with pre-pandemic occupational digital intensity, measured in 2019, and sex, along with individual fixed effects. Occupational digital intensity is the standardized share of job postings within a 4-digit occupation that require digital skills. Estimates are normalized to zero in 2019, the omitted reference year, and therefore represent differential changes in the outcome associated with a one-standard-deviation increase in 2019 occupational digital intensity. Bars indicate 95% confidence intervals. Standard errors are clustered at the 4-digit occupation of employment in 2019.

Table A.3: COVID-19 Policy Environment: Denmark, the United States, and Empirical Relevance

Policy dimension	Denmark	United States	Relevance for empirical interpretation
<i>Panel A. Employment protection and income stabilization</i>			
Pre-pandemic labor-market model	Flexicurity system: relatively flexible hiring and dismissal rules combined with generous income insurance and active labor-market policy.	More decentralized labor-market institutions, weaker income insurance, and no comparable national short-time work infrastructure prior to the pandemic.	Baseline employment protection differs from pandemic-era job retention. In Denmark, the key pandemic margin was not ordinary dismissal protection but temporary preservation of worker–firm matches.
Job-retention instrument	Wage-compensation scheme for private firms facing large layoffs. Eligible firms could retain affected employees on payroll while receiving wage subsidies.	Paycheck Protection Program (PPP): forgivable loans to firms, mainly small businesses, conditional in part on payroll retention.	Both policies supported employment relationships, but the Danish scheme was more directly tied to furlough and worker–firm match preservation, while PPP operated through firm liquidity and payroll incentives.
Furlough versus active work	Furloughed workers under the Danish wage-compensation scheme were generally preserved in employment but did not work during the compensation period.	PPP support was not equivalent to a worker-level furlough state; retained workers could continue working, be temporarily inactive, or face reduced hours depending on firm decisions.	Mechanism analyses should distinguish remote-work continuity from furlough-based job retention. Employment preservation need not imply continued work from home.
Household income support	Wage compensation was supplemented by support for self-employed workers, sick-leave reimbursement, and compensation for fixed costs and lost revenue.	CARES Act support included Economic Impact Payments, expanded unemployment-insurance eligibility, and the Federal Pandemic Unemployment Compensation (FPUC) supplement.	Income shocks were dampened in both settings, but through different channels: Denmark relied more on job-retention and welfare-state instruments; the U.S. relied more on direct transfers and unemployment insurance.
<i>Panel B. Social distancing, workplace restrictions, and work from home</i>			
Policy centralization	Early national restrictions were introduced in March 2020, including closures of schools, childcare, universities, and many public-facing activities.	Restrictions were primarily state and local. There was no single federal lockdown, and timing and stringency varied substantially across jurisdictions.	Cross-country comparisons should not treat the U.S. as having a single policy regime. Denmark provides a more nationally coordinated institutional environment.
Public-sector work from home	Non-critical public-sector employees were sent home to work during the first lockdown.	Public-sector WFH policies varied across federal, state, and local employers.	Baseline sector changes the mapping from occupational digital readiness to realized remote work. Public-sector mandates compressed part of the adoption margin in Denmark.
Private-sector work from home	Private employers were strongly encouraged to send employees home to work where feasible, but retained greater discretion over implementation outside directly closed activities.	Private-sector WFH adoption depended on state/local restrictions, employer decisions, and occupational feasibility.	In the private sector, occupational digital readiness likely maps more directly into realized WFH than in the public sector, where formal mandates were stronger.

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Table A.3: COVID-19 Policy Environment: Denmark, the United States, and Empirical Relevance (*continued*)

Policy dimension	Denmark	United States	Relevance for empirical interpretation
Contact-intensive activity	Many public-facing and close-contact activities were closed or restricted during the first lockdown and subsequent waves.	Closures of public-facing businesses varied across states and over time.	Sectoral exposure to restrictions may affect earnings, days worked, and employment independently of remote-work feasibility.
<i>Panel C. Schools, childcare, and household time constraints</i>			
Initial school and childcare closures	Schools and daycare institutions closed during the first lockdown in March 2020.	Most schools shifted rapidly toward remote instruction in spring 2020, but policies were decentralized across states, districts, and schools.	Closures generated a household production shock that is not captured by workplace restrictions alone.
Reopening of young children	Reopening was phased by age and grade. Early childhood education and care, special education, and younger pupils were prioritized from mid-April 2020.	There was no federal age-sequenced reopening framework. Reopening was highly heterogeneous and often more prolonged, especially in large districts.	The Danish setting provides substantial but relatively shorter complete disruption for young children than many comparison settings. This matters for parental labor supply and sex-specific estimates.
Older pupils and later restrictions	Older pupils remained in remote instruction longer than younger pupils, and later restrictions were more targeted and episodic.	Many U.S. students experienced remote or hybrid schooling into the 2020–2021 academic year.	Child age is a relevant source of heterogeneity: young-child childcare constraints were acute but shorter in Denmark; older-child schooling disruptions were more prolonged.
International comparison	Denmark combined early restrictions with relatively rapid reopening for young children and extensive income stabilization.	The U.S. combined large fiscal transfers with more decentralized public-health and schooling policies.	Denmark is not best characterized as a minimal-restriction case. Its distinctiveness lies in the combination of early restrictions, high administrative capacity, job-retention policy, and comparatively short complete closures for young children.

Notes: WFH = work from home. PPP = Paycheck Protection Program. FPUC = Federal Pandemic Unemployment Compensation. ECEC = early childhood education and care. The table summarizes institutional margins relevant to the empirical design rather than providing a complete chronology of pandemic policy. “Furlough” refers to workers preserved in employment but not actively working under the Danish wage-compensation scheme.

Sources: Cross-national containment and closure policies are summarized from the Oxford COVID-19 Government Response Tracker (Hale et al., 2021). Danish flexicurity background is from Kreiner and Svarer (2022). Danish wage-compensation details and evidence on employment retention are from Borgensgaard (2022). Danish public/private WFH rules and first-lockdown workplace measures are from Søsted and Videbæk Munkholm (2020). U.S. PPP evidence is from Autor et al. (2022); U.S. unemployment-insurance replacement rates are from Ganong et al. (2020). Danish school and childcare closures and reopening are from OECD (2020) and Lundtofte (2021). U.S. school-mode evidence is from National Center for Education Statistics (2022). Global school-closure comparisons are from UNESCO (2021). The motivation for emphasizing young children in school-closure interpretation is consistent with Fuchs-Schündeln et al. (2022).

Health-care utilization is constructed from the Danish health-service register. The construction code reads annual SSSY files for 2010–2023 and uses the specialty and service-code fields to classify visits. General-practitioner visits are identified by specialty prefix 80, psychiatrist visits by prefix 24, psychologist visits by prefix 63, physiotherapy by prefix 62, dentist visits by prefix 50, and rheumatology by prefix 06. GP-based screening is identified using GP service code 2149, and GP-based talk therapy using service codes 6101 and 6201. The code also identifies online GP contacts, GP email and telephone contacts, online GP screening and talk therapy, and video/email/telephone contacts with psychiatrists or psychologists. These utilization variables are collapsed to person-year counts and merged to the main worker panel, with missing utilization records set to zero.

The utilization outcomes are used to discipline interpretation of the prescription outcomes. Lower prescription use among workers in more digitally ready occupations could reflect improved mental health, reduced access to care, or substitution toward other forms of treatment. The GP, psychologist, psychiatrist, online-care, screening, and talk-therapy variables therefore help separate a broad access story from a change in the composition of treatment.