NBER WORKING PAPER SERIES

REMOTE WORK AND POLITICAL PREFERENCES:
EVIDENCE FROM U.S. COUNTIES

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Working Paper 32834
http://www.nber.org/papers/w32834

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
August 2024

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NBER Working Paper No. 32834
August 2024
JEL No. D72, H73, H75, J22, J24

ABSTRACT

We examine how trends in remote work vary by U.S. county-level characteristics. Using several data sources with various definitions of remote work, we show that 2016 presidential Democratic vote share is associated with an increase in remote work starting in 2020 and continuing through 2023. An increase in 2016 Democratic vote share of one standard deviation is related to an increase in the likelihood of remote work by 1-2 percentage points and the share of job postings that advertise a remote-work component by about 2 percentage points. These effects are not altered by differences in various COVID policies and outcomes or labor-market indicators. We hypothesize that differences in preferences for workplace flexibility that are correlated with local political tastes opened a gap in remote-work rates once the pandemic-induced "remote-work revolution" substantially increased individuals' opportunities for working from home.

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1. Introduction

Remote work rates in the U.S. are roughly four times higher in 2024 than they were in 2019, a change that was triggered by the COVID-19 pandemic (Barrero, Bloom, and Davis, 2021).[1] What determines who is working from home, either in fully remote or in hybrid work arrangements, as COVID has receded? Recent evidence focuses on both individual- and firm-level factors—such as worker preferences, the feasibility of telework in one's occupation, presence of children, managerial practices, etc. (Alipour, Falck, and Schüller, 2023; Barrero, Bloom, and Davis, 2023; Hansen et al., 2023)—as well as group-level factors such as the cultural characteristics and policies of different countries or regions (Aksoy et al., 2022; Adrjan et al., 2023; Özgüzel, Luca, and Wei, 2023; Zarate et al., 2024).

Geographic differences in political preferences in the U.S. are large and may play a role in shaping remote-work outcomes. COVID quickly became a politicized issue in the U.S., with different messaging on the right and left concerning the severity of the crisis and the proper role of government in addressing it (Allcott et al., 2020; Tyson, 2020). Democratic states initially had higher COVID incidence rates early in the pandemic, but this had flipped by the summer of 2020 as those states generally adopted more stringent measures to curb the spread of the virus (Neelon et al., 2021; Shvetsova et al., 2022) and there was generally more social distancing in Democratic areas (Gollwitzer et al., 2020).[2]

In this paper, we examine the relationship between local political preferences and the persistence of remote work in the wake of the pandemic. Our research question is whether local

[1] https://wfhresearch.com/wp-content/uploads/2024/05/WFHResearch_updates_May2024.pdf.

[2] Individuals living in more Democratic areas of the country were also more likely to be vaccinated, which may have contributed to lower death rates relative to more Republican ones (Wallace, Goldsmith-Pinkham, and Schwartz, 2023; Güss et al., 2023). At the same time, Democrat-voting areas generally imposed longer K-12 school closures than did Republican-voting ones, which may have led to persistent learning shortfalls in the former relative to the latter (Grossmann et al., 2021; Jack and Oster, 2023).

political tastes (proxied by U.S. county Democratic vote share in the 2016 presidential election), are correlated with changes in remote work rates that have persisted beyond the pandemic. We focus on remote work as our outcome of interest given its consequences for workers and their families, firms, and cities. In short, the "remote-work revolution" has the potential to cause lasting changes to the labor market and society. If remote work becomes permanently more common in some areas than others, differences in worker time use and wages, city structure, and firm and worker sorting are likely to follow.[3]

We build on the literature in this area by examining changes in remote work over time by locational characteristics rather than examining cross-sectional differences (as in Zarate et al., 2024). Remote work was increasing (slowly) even prior to the pandemic, and our strategy allows us to examine the possibility of pre-COVID trends that are correlated with our county characteristics of interest. It allows us to include county fixed effects in our models, which eliminates omitted variable bias that is time-invariant across county. Lastly, we can see how county voting preferences affect remote work during the height of the pandemic (2020-2021) and later years (2022-), allowing us to go beyond the results of Adrjan et al. (2023).

We use four different datasets in our analysis, which has the advantage of allowing us to check robustness across different samples and ways of measuring the incidence of remote work. These include the American Community Survey (ACS), American Time Use Survey (ATUS), Current Population Survey (CPS), and job posting data from Hansen et al. (2023). Each of these data sources have unique strengths we highlight below.

[3] For papers on remote work and time use, see Aksoy et al, (2023), Cowan (2024), and Inoue, Ishihata, and Yamaguchi (2024). For remote work and productivity/wages, see Bloom et al. (2015), Choudhury et al. (2024), and Barrero et al. (2022). For remote work and worker sorting, see Bick et al. (2024), Atkin, Schoar, and Shinde (2023) and Hsu and Tambe (2024). For remote work and city structure, see Gupta, Mittal, and Van Nieuwerburgh (2022), Monte, Porcher, and Rossi-Hansberg (2023) and Behrens, Kichko, and Thisse (2024).

We employ event-study models to analyze the trend in remote work from before to after the pandemic by county 2016 presidential voting behavior controlling for a rich set of county and individual controls. Accounting for the "teleworkability" of one's occupation, their college attainment, and the urbanicity of their location (interacted with time), county-level political preferences are correlated with the increase in remote-work rates that begin with the onset of the COVID-19 pandemic. An increase in 2016 Democratic vote share of one standard deviation is related to an increase in the likelihood of remote work by 1-2 percentage points and the share of job postings that advertise a remote-work component by about 2 percentage points in the post-pandemic period. Vote share is also related to the share of total work time individuals spend working from home, though those effects are less precisely estimated and appear to diminish by 2023 (the last year of data available in our individual-level analysis).

We then examine whether these political differences can be explained by differences in COVID policy variables, since areas that had longer lockdowns and school closures, more stringent mask mandates, etc. may have induced more remote work during the height of the pandemic that persisted even after such policies were taken away. Even though some of these variables are correlated with both remote-work changes as well as vote share, their inclusion does not alter the relationship between vote share and remote work. Lastly, we show that adding local economic factors, including the COVID-era unemployment rate as well as a measure of labor-market concentration by county and occupation, also does not explain the effect of vote share on remote work. We conclude that the most likely explanation for our result is underlying differences in preferences for remote work by Democratic vote share that manifested themselves once the pandemic induced a large shift in norms concerning work-from-home.

2. Theoretical considerations

Because of lockdowns to prevent spread of SARS-CoV-2, the cost of experimenting with remote work fell dramatically during the early days of the COVID pandemic, and many workers tried (and firms allowed) work-at-home arrangements during that time (Davis, 2024). Many workers and firms found such arrangements to be beneficial; indeed, many individuals were positively surprised by their productivity at home (Aksoy et al., 2022). Given this, and the potential to save on office space rents, many employers have since planned for modest *increases* in remote work going forward (Barrero, Bloom, and Davis, 2021, 2023; Bick, Blandin, and Mertens, 2023).

Democratic areas of the U.S. generally imposed longer and more stringent lockdown measures and other COVID policies; furthermore, individuals living in these areas were more likely to engage in social distancing and other behavioral changes even conditional on policy stringency.[4] Thus, it is plausible that individuals in jurisdictions with higher Democratic vote shares were more likely to try working remote during the pandemic, which behavior may have persisted beyond the pandemic.

In addition to COVID-related factors, demand for workplace flexibility may vary by political party. Democrats are more likely than Republicans to say that the flexibility to work from home is important in choosing a job, and among those who do work from home, Democrats are more likely to say they would leave the job if they had to return to the office full time (Lee and Bierman, 2023). This may be due in part to average differences between parties in commute times, educational levels, occupational profiles, etc. (Aksoy et al., 2022), which is why we control for these individual factors (and their interactions with time) in our analyses, as described below. Even

[4] Political preferences may be correlated with pandemic behavioral responses due to differences in emphasis on individual freedom relative to other values, (dis)trust of government, and perception of risk/risk preferences (Hsiehchen, Espinoza, and Slovic, 2020). Indeed, individuals who identify as Republican were less likely to alter their behavior to mitigate the risk of spread during the pandemic, including their work arrangements (Allcott et al., 2020; Barbalat and Franck, 2022; Canes-Wrone, Rothwell, and Makridis, 2022; Lamare, Benton, and Tabarani, 2024).

conditional on these factors, Democrats may have higher willingness to pay for workplace flexibility due to different preferences; this could be the case if, for example, if Democratic couples place higher value on equality in work and childcare between partners (Fry et al., 2023).

The goal of our paper is to examine how local political affiliation (via Democratic vote share in the 2016 presidential election) correlates with the evolution of remote-work rates in the U.S. conditional on individual demographic and socioeconomic factors that are likely to affect work-from-home opportunities and demand.[5] We then examine how other county-level characteristics affect this relationship. Principal among these is COVID policy measures and outcomes (for the reasons we describe above). We also examine whether labor-market conditions mediate the relationship between local politics and remote-work rates. We describe the variables used to proxy for these characteristics in the next section.

3. Data

We utilize three U.S. repeated cross-sectional individual survey datasets: the American Community Survey (ACS); the American Time Use Survey (ATUS); and the Current Population Survey (CPS). In each case, we use the datasets that are harmonized over time by the Integrated Public Use Microdata Series (IPUMS).[6] In all datasets, we restrict attention to 25-59 year-olds

[5] The decision to work from home depends on arrangements that can be worked out with one's employer as well as how often one's colleagues (wish to) work remotely. For example, if a worker's colleagues regularly work from home, the benefits of going into the office herself falls. It then follows that multiple equilibria are possible across co-worker teams, firms, and regions (Monte, Porcher, and Rossi-Hansberg, 2023). The idea that remote work behavior is likely determined in part by remote work decisions made by firms, co-workers, friends and neighbors, etc. imply that the political preferences in the area a person lives may affect their outcomes above and beyond their personal preferences.

[6] **ACS**: Steven Ruggles, Sarah Flood, Matthew Sobek, Daniel Backman, Annie Chen, Grace Cooper, Stephanie Richards, Renae Rodgers, and Megan Schouweiler. IPUMS USA: Version 15.0 [dataset]. Minneapolis, MN: IPUMS, 2024. https://doi.org/10.18128/D010.V15.0.

ATUS: Sarah M. Flood, Liana C. Sayer, Daniel Backman, and Annie Chen. American Time Use Survey Data Extract Builder: Version 3.2 [dataset]. College Park, MD: University of Maryland and Minneapolis, MN: IPUMS, 2023. https://doi.org/10.18128/D060.V3.2.

CPS: Sarah Flood, Miriam King, Renae Rodgers, Steven Ruggles, J. Robert Warren, Daniel Backman, Annie Chen, Grace Cooper, Stephanie Richards, Megan Schouweiler, and Michael Westberry. IPUMS CPS: Version 11.0 [dataset]. Minneapolis, MN: IPUMS, 2023. https://doi.org/10.18128/D030.V11.0.

(i.e., those who are in their prime working years). We also restrict the analysis to individuals with non-missing county identifiers; in each of these datasets, to protect the identities of respondents, county identifiers are obscured when there is an insufficient number of observations in that county-time period. County identifiers are available for 40-60% of all respondents (depending on the dataset) and naturally skew toward larger (less rural) counties.

We add to these individual-level datasets information on job postings by county from Hansen et al. (2023). As we describe below, each dataset has valuable characteristics for our study.

American Community Survey

We employ annual data from the ACS from 2015-2023 (the last year the data is publicly available). The ACS is the largest of our datasets, with over one million observations per year. Our measure of remote work comes from a question regarding individuals' transportation to work that is asked across all years in our sample period—in particular, one of the possible answers is "worked at home." The percentage of individuals providing this answer varies from 3.4% in 2015 to 14.4% in 2021. It is unclear how individuals with hybrid work arrangements answer this question, but it stands to reason that those indicating "worked at home" skew toward those who work remotely full-time.

American Time Use Survey

We also employ ATUS data from 2015-2023 (the last year the data is publicly available). The ATUS is a 24-hour time diary in which the respondent reports the activities they were doing between 4:00 am of the first day and 4:00 am of the following day. Respondents are randomly sampled from individuals who completed the Current Population Survey (CPS) and take the ATUS between two and five months after their final CPS interview. We focus on the share of overall work time spent at home (conditional on working a positive amount)—remote-work minutes divided by the total minutes an individual spent working in a given day (remote work share). This measure has the advantage of capturing both the extensive and intensive margins of remote work.[7] The tradeoff, relative to the ACS, is a much small sample size of roughly 10,000 total.

Current Population Survey

We employ basic monthly data from the Current Population Survey from October 2022 to November 2023 due to the availability of two questions related to remote work in those months of the CPS. The first question asks whether the individual "teleworked or worked at home for pay prior to when the COVID-19 pandemic started (February 2020)." The second question asks whether the individual "teleworked or worked at home for pay" in the last week of the survey reference month. The first question is available only from October 2022 to November 2023, while the second question is available from October 2022 to March 2024. We measure a *change* toward remote work by defining a binary variable that is equal to one if an individual indicates that she was not working at home in February 2020 but is working at home in her survey reference month. About 11% of individuals in our sample experienced this transition.

Data on Job Postings

Finally, we use data obtained from Hansen et al. (2023) on the share of online job vacancy postings that explicitly allow remote-work arrangements (hybrid or fully remote). The job posting data comes from Lightcast, which collects data from more than 50,000 sources. Hansen et al. (2023) then develop a machine learning method to detect postings in which remote work is advertised. We use the 3-month moving average of the share of vacancies with remote-work possibilities by county and month (as provided by the authors). This data begins in January 2019 and runs through September 2024.

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[7] In results available upon request, we do not find that Democratic vote share has a significant post-COVID effect on total work time (the denominator of remote-work share).

2016 Presidential Election Results

County-level data on the 2016 presidential election comes from the MIT Election Data and Science Lab.[8] We use the Democratic (Clinton) vote share in each county as our measure of the county's political preferences (similar to Zarate et al., 2024).

COVID Pandemic Policy and Outcome Indices

We use several COVID-related policy and outcome variables by county (or, when not available by county, state) in our analysis. First, we use the average proportion of public K-12 schools that were closed in each county during the 2020-2021 period. This measure is derived from the school closures database in Parolin and Lee (2021). The authors consider a school to be closed if there is at least a 50 percent year-over-year decrease in in-person visits compared to the same month in 2019 (the pre-pandemic baseline), as determined using aggregated and anonymized mobile phone traffic data from *SafeGraph*.

We also use a measure of COVID overall policy stringency from the widely cited Oxford COVID-19 Government Response Tracker (OxCGRT) (Hale et al., 2021). Because these variables are only available at the state (rather than county) level, we match them to individuals' states rather than counties of residence. We employ the authors' Stringency Index, which is also used in Adrjan et al. (2023). This is a composite measure of school closures, workplace closures, canceled public events, gathering restrictions, public transportation restrictions, stay-at-home orders, restrictions on internal and international movement and public information campaigns. This index takes a normalized value between 0 and 100 for every day starting January 1st, 2020 running through December 31, 2022. We take the average value of the index over 2020 and 2021 as our measure

[8] MIT Election Data and Science Lab, 2018, "County Presidential Election Returns 2000-2020", https://doi.org/10.7910/DVN/VOQCHQ, Harvard Dataverse, V12, UNF:6:KNR0/XNVzJC+RnAqIx5Z1Q== [fileUNF]

of state-level COVID policy stringency. Though closures and other restrictions were not unheard of in 2022, they had become very rare. For example, schools were closed less than 1% of the time; business closures and stay-at-home orders essentially did not occur in 2022. Thus, we believe that 2022 represents the first year that can be considered "post-COVID restrictions."

Additionally, we control for the cumulative number of confirmed COVID-19 cases and deaths per capita in each county by the end of 2021 based on the data from the COVID-19 data repository maintained by the Center for Systems Science and Engineering at Johns Hopkins University (Dong et al., 2020). Lastly, we use a measure of exposure to others during the pandemic developed by Couture et al. (2022) using PlaceIQ smartphone movement data.[9] This index measures how many encounters the average smartphone user had with other smartphone users in a particular county on a particular day from January 20, 2020 through the end of August 2021. We utilize this index aggregated over 2020 as our measure of county-level inter-person contact during the height of the pandemic.

Labor Market Concentration

To account for the possibility that remote work arrangements, which workers typically value but (some) firms view as less productive, were more likely to grow in counties with tighter labor markets, we use a measure of labor-market concentration that is specific to both occupations and counties from Choi and Marinescu (2024). Using data on job postings from Lightcast (formerly Burning Glass Technologies), the authors construct a Herfindahl-Hirschman Index (HHI) for each six-digit Standard Occupational Classification (SOC) occupation by county and

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[9] This data has been made publicly available by the authors at: https://github.com/COVIDExposureIndices/COVIDExposureIndices.

quarter.[10] We use a crosswalk between SOC codes and Census occupation codes to merge this HHI measure (averaged over the year 2019) with our data.[11]

Occupational Telework Potential

A key control variable in our analysis is how feasible telework is in one's occupation. We employ the measure from Dingel and Neiman's (2020) paper that uses pre-COVID job characteristics to determine whether jobs can be performed from home; aggregating to higher occupation levels, a "teleworkability" index score between 0 and 1 can be assigned to each occupation. This index has been used extensively in the literature and cross-validated by employee self-reports on remote work feasibility in their job (Alipour, Falck, and Schüller, 2023). To assign the Dingel and Neiman (2020) index to each occupation in our datasets, we first use the Standard Occupational Classification (SOC) crosswalk provided by the authors.[12] We then use a crosswalk from SOC codes to Census occupation codes (2018 Census Occupation Code Lists).[13] In our analysis below, we generally restrict attention to those individuals who report an occupation (even if they are not currently working). This means we lose roughly 10-20% of the total sample (depending on the dataset) because the individual either does not report an occupation or because they have a military occupation, which we cannot match to Dingel and Neiman's (2020) index.

4. Empirical Strategy

Using our four data sources, we examine how trends in remote work from before to after the COVID pandemic vary by county characteristics including 2016 presidential election Democratic vote share.

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[10] These data are publicly available at: https://data.mendeley.com/datasets/45r6ps4r68/1.
[11] The authors construct both a lower-bound HHI and an upper-bound HHI based on how job postings with missing employer names are treated; we use the former for our analysis.
[12] https://github.com/jdingel/DingelNeiman-workathome/blob/master/onet_to_BLS_crosswalk/output/onet_teleworkable_blscodes.csv
[13] https://www.census.gov/topics/employment/industry-occupation/guidance/code-lists.html

4.1 Event-study specification

For our analysis using ACS and ATUS, we employ the following event-study specification:

$$y_{ict} = \sum_{t \neq 2019} (county\_char_c \cdot D_t)\beta_t + \gamma_c + \delta_t + X_{ict}\lambda + \epsilon_{ict}\,. \tag{1}$$

In this equation, y_{ict} is the remote-work outcome of individual i living in county c observed in year t; $county\_char_c$ is a vector of county voting, policy, and economic characteristics; D_t is an indicator that takes a value of "1" for each year t in our sample frame (2015-2023, with 2019 as the excluded base year); γ_c is a county fixed effect; δ_t is a year fixed effect; X_{ict} is a vector of individual and county-level controls; and ϵ_{ict} is the error term. The coefficients of interest are the β_t's, which indicate how outcomes evolve depending on the county characteristics of interest.

The main variable of interest in $county\_char_c$ is the county's percentage of votes in the 2016 presidential election for the Democratic candidate (Clinton), but we also examine effects of the COVID policy and behavior variables as well as economic circumstances described in the last section. Throughout our analysis, we standardize these variables by subtracting their mean and dividing by the standard deviation (so that their units are standard deviations).

Because our treatment variables of interest are continuous, with no "never treated" states, interpreting the β_t's as causal under treatment effect heterogeneity requires more than the typical "parallel trends" assumption underlying a two-way fixed (TWFE) estimator of a binary treatment (Callaway, Goodman-Bacon, and Sant'Anna, 2024). This is an issue that has generally gone unrecognized in the TWFE literature until recently. Callaway, Goodman-Bacon, and Sant'Anna (2024) introduce a "strong" parallel trends assumption, which states that the treatment effect of a certain "dose" of $county\_char_c$ for those units that experience that dose is the average treatment effect of that dose among all units (and this is true for all potential doses of the treatment). This implies no selection into different doses of the treatment based on treatment effects.[14]

For our analysis using CPS, we observe whether a respondent reports working remotely in their interview month (from October 2022-November 2023) as well as in February 2020. As our outcome of interest, we focus on a binary outcome that is equal to "1" if the individual was *not* working remotely in February 2020 but *is* working remotely in their interview month (and "0" otherwise).[15] Thus, the outcome is whether an individual became a remote worker sometime after the onset of the COVID pandemic. We analyze this outcome as a function of county-level COVID policy stringency and voting preferences as well as county- and individual-level controls:

$$y_{icm} = \sum_{q(m)} \left(county\_char_c \cdot D_{q(m)}\right)\beta_{q(m)} + \delta_{q(m)} + X_{icm}\lambda + \epsilon_{icm}\,. \tag{2}$$

In this case, y_{icm} is the remote-work outcome of individual i living in county c observed in month-year m (e.g., July 2023); $D_{q(m)}$ is an indicator that takes a value of "1" for each of five quarters[16] covering the months that the combination of these questions was asked in the CPS; $\delta_{q(m)}$ is a quarter-year fixed effect; other variables are the same as in Equation (1). It is important to note that there is no county fixed effect in Equation (2) as it is collinear with COVID-era county characteristics (which do not vary with time).[17]

In all of our analyses, we cluster standard errors at the county level and include individual demographics (age and its square, dummies for sex, race, Hispanic ethnicity, marital status, and

[14] If the strong parallel trends assumption holds, the TWFE β_t's represent a weighted average of causal responses at different doses (change in a unit's potential outcome with a marginal increase in the dose) with non-negative weights that sum to one.
[15] Less than 2% of respondents indicate that they were working from home in February 2020 but are not working from home in their interview month.
[16] These quarters are: October-December 2022; January-March 2023; April-June 2023; July-September 2023; October-November 2023.
[17] In Equation (1), omitting the year 2019 interactions allows us to include county fixed effects in the model. There is no natural equivalent "base period" in Equation (2).

location urbanicity[18]) and labor-market characteristics (educational attainment; occupation and industry at the Census 4-digit level). We also interact urbanicity, college completion, and whether an occupation is considered fully "teleworkable" (according to Dingel and Neimann, 2020) with year. This is because the relationship between these variables and remote work also likely changes after COVID. Because some counties have a higher percentage of individuals living in urban areas (where remote work is more common), with college degrees, or working in occupations and industries that lend themselves to remote work, it is also important to control for the interactions between these variables with year dummies. This is especially true if our county characteristics of interest (e.g., voting behavior) are correlated with these variables, since leaving them out could cause omitted variable bias in our estimates of the effects of those characteristics on remote work. Table 1 includes mean values of individual characteristics by time period (pre- vs. post-COVID) and dataset; it also includes means and standard deviations of county- and state-level characteristics used in the analysis.[19]

In our first specification, we only include Democratic vote share in $county\_char_c$. In the second specification, we add COVID related policy variables—county-level school closure length and the state-level COVID stringency index. We do this not only to examine the effect of these variables themselves but to see to what extent they affect the relationship between vote share and remote work. In our third specification, we add state-by-time fixed effects; this controls for all state-level factors that may have affected the evolution of remote work in a given location. In other

[18] Categories for this IPUMS-created measure include 1) Not in metropolitan area, 2) In central/principal city of metropolitan area, 3) Not in central/principal city of metropolitan area, 4) In metropolitan area but central/principal city status indeterminable, and 5) Metropolitan area status indeterminable.

[19] In the ACS, the percentage of individuals classified as white decreases markedly from before to after 2020, while the percentage of those whose race is classified as "other" (that is, not white, black, or Asian) rises by a similar magnitude. This is likely principally due to the changes in questionnaire and coding practices adopted by the Census in 2020: https://www.census.gov/newsroom/blogs/random-samplings/2021/08/improvements-to-2020-census-race-hispanic-origin-question-designs.html.

words, in this specification, we only utilize differences in counties within the same state for identification of the county characteristics of interest. In our fourth and last specification, we exclude state-by-time fixed effects but include several additional county-level characteristics (interacted with year): COVID cases and deaths per capita, the COVID-era exposure index, the average county unemployment rate over the 2020-2021 period, and the labor-market concentration index. Some of these variables are potentially endogenous to vote share and/or COVID policies, but we include them in this last specification to again examine how they alter the estimated relationship between vote share and our remote-work outcomes.

5. Results

5.1 Analysis of potential composition effects

Before we examine our main regression results following Equations (1) and (2), we assess the possibility that changes in the composition of workers across counties could affect our results. A potential problem with interpreting the effects of county Democratic vote share on remote work as causal is if there is selective migration across counties. If, for example, individuals who were inclined to work remotely were more likely to relocate to counties with stronger Democratic political preferences since 2020 even for reasons other than politics (such as differences in the occupational or industrial make-up of local economies), our estimates of the causal effects described above would be contaminated. To examine the possibility that the characteristics of individuals living in counties with a larger Democratic vote share have changed since 2020, we use ACS data and as dependent variables an individual's occupational teleworkability index score, whether she is a college graduate, and whether she is currently employed. On the right-hand side of these regressions, we include 2016 county Democratic vote share (interacted with year) as well as county and year fixed effects and individual-level controls. If vote share became more correlated with these characteristics starting in 2020, it would suggest that composition changes across county might drive our remote-work results.

Table 2 contains the results using the ACS. There is little evidence of trends by 2016 Democratic vote share in teleworkability index, college attainment, or employment prior to COVID. Relative to 2019 (the base year), there is also no evidence of a differential increase (by state Democratic vote share) in individuals' occupational teleworkability index score or likelihood of being a college graduate. There appears to be a reduction in the likelihood of employment associated with vote share following the onset of the pandemic that largely dissipates by 2023. The largest effect is seen in 2021, with a 0.8 percentage point reduction in employment for an increase in one standard deviation in Democratic vote share. This is small relative to the pre-COVID mean of 0.77, but we nevertheless include county-level COVID-era (2020-2021) unemployment rates (interacted by year) as a control in one of our regression models to account for the possibility that relative changes in remote work in more Democratic counties are correlated with employment outcomes.

We conclude from our analysis of the association between individual characteristics and Democratic vote share that changes in the composition of workers across counties are unlikely to drive our results on remote work below.

5.2 Analysis using ACS data

We now turn to estimating Equation (1) using our largest individual-level dataset (ACS). Our measure of remote work in ACS is whether individuals indicated that their transportation to/from work was "work from home." The results are contained in Table 3. In the first column, only 2016 Democratic vote share (interacted with year) is included as a county-level characteristic. In the second column, we add both the county school closure index and the state COVID policy stringency index. In column 3, we additionally add state-by-year fixed effects; in column 4, these are removed but several additional county-level characteristics (with year interactions) are added: COVID cases and deaths per capita, the COVID-era mobility index (indicating the degree of adherence to social distancing policies and/or personal behavior changes during the pandemic), the COVID-era unemployment rate, and the pre-COVID labor-market concentration index by occupation (HHI).

Estimates of the effects of Democratic vote share by year are quite consistent across columns. First, there is some indication of a slightly higher remote-work rate in more Democratic counties in pre-COVID years; however, these effects are very small and, if anything, get even smaller up through 2019 (the base year), suggesting that there is no pre-COVID trend in remote work by vote share. Second, starting in 2020, there is a significant association between vote share and remote work of roughly 2 percentage points in 2020 and 2021 and 1 percentage point in 2022 and 2023 (for an increase in the Democratic vote share of one standard deviation). Though these effects appear to diminish somewhat from 2021 to 2023, the (smaller) effects in the latter year are still statistically significant at the 1% level.

The fact that inclusion of additional county- and state-level controls does not alter the relationship between Democratic vote share and remote work is noteworthy. This is not because these characteristics are uncorrelated with vote share; in a regression of vote share on these characteristics, the r-squared is 0.66, indicating that they explain roughly two-thirds of the variation in vote share. It is also not because these characteristics have no impact on remote work. In Figure 1, point estimates for both vote share and a set of other county characteristics are plotted for the years we analyze (other characteristics are not included in the figure as their association with remote work is generally insignificant). There are positive effects of both vote share (as already discussed) and school closure intensity on remote work following the pandemic. In the latter case, it is plausible that when more schools were closed for a longer period, individuals living in those counties were more likely to begin working remotely during that time (generally 2020 and 2021) and then continue working remotely even after schools re-opened.[20] This result is consistent with findings from international and cross-sectional state-level variation in COVID response policies that remote-work jobs are now more common in regions that imposed stricter lockdowns during the pandemic (Adrjan et al., 2023; Zarate et al., 2024).

On the other hand, greater inter-person contact during the pandemic (as proxied by the COVID exposure index) is negatively associated with remote work starting in 2020, though this effect fades and becomes statistically insignificant in later years. This is intuitive, as counties in which individuals curbed their in-person interactions with others less were also places in which individuals were less likely to take up or stick with remote work after lockdowns were lifted. Lastly, the labor-market concentration index is strongly negatively associated with remote work in the post-pandemic period, especially starting in 2021. This suggests that when employers had more monopsony power over an occupation in a local labor market, they suppressed remote-work opportunities (consistent with the idea that at least some employers find remote work to be less productive or desirable). However, more evidence is needed to understand this association.

When we focus only on those workers in our sample in fully teleworkable occupations (according to Dingel and Neiman, 2020), unsurprisingly, all effects shown in Figure 1 get somewhat larger (as demonstrated in Figure 2). This is consistent with the idea that the feasibility of remote work in one's occupation largely determines her ability to take advantage of shifts in opportunities to work from home more generally.

[20] Of course, we would need panel data to more carefully examine this possibility.

Returning to the results of Table 3, the fact that the association between 2016 Democratic vote share and remote-work rates in the post-COVID period does not appear to be due to differences in COVID policy/outcome differences or local labor market conditions leads us to believe that preferences for remote work that vary by political affiliation play an important role in explaining our results. As discussed in Section 2, there are differences by political affiliation in the value workers place on being able to work remotely part or all of the time. The great experiment with remote work caused by the COVID pandemic appears to have opened a gap in remote-work rates across geographic areas with different average political differences where none existed before because remote-work opportunities were so much rarer prior to 2020, the initial proliferation of remote work as a way to curb the spread of COVID became politicized, or both.

5.3 Analysis using ATUS data

In Table 4, we use ATUS data to analyze the exact same regression results as in Table 3 but with a change in the measure of remote work: in this case, it is the share of an individual's total work time that is from home rather than a workplace. Note that this means our ATUS sample is composed only of individuals who report working a positive amount on the day of their time diary.[21]

The results in Table 4 suggest, as did the ACS results in Table 3, that 1) there is little evidence of pre-trends in the effects of county Democratic vote share (though in two specifications there are large and significant effects in 2018, and 2) the inclusion of additional state- and county-level controls (moving from column 1 to column 4) does little to change the post-COVID relationship between vote share and remote work. A few differences between the ACS and ATUS analysis stand out. First, the sample size using ATUS is dramatically smaller than with ACS. This

[21] In results available upon request, we do not find that Democratic vote share has a significant post-COVID effect on total work time (the denominator of remote-work share).

may be responsible for the fact that individual coefficients are less precisely estimated with ATUS, and some post-COVID coefficients are not statistically significant at conventional levels. Second, the effect of Democratic vote share in 2023 appears to fall close to zero, indicating that vote-share effects may be dissipating over time. This is a similar pattern to what we see in the ACS, though in that case 2023 vote-share effects are still sizeable and statistically significant. In our analysis of job postings described in Section 5.5, we will see that according to that measure of remote-work activity, the effect of Democratic vote share since COVID has not dropped off over time.

5.4 Analysis using CPS data

The last of our individual datasets is the CPS, which contains yet another measure of remote-work behavior that we analyze as an outcome in our regressions. This variable is equal to "1" if the individual was *not* working remotely in February 2020 but *is* working remotely in their interview month (and "0" otherwise). Results corresponding to estimation of regression equation (2) are contained in Table 5. In this case, we cannot observe pre-COVID interactions between Democratic vote share and time period because our dependent variable is not observed prior to COVID. We can see, however, how the relationship between vote share and switching to remote work evolves from late 2022 to late 2023.

The results suggest a strong positive effect of vote share on switching to remote work that peaks in early 2023 (at about 3 percentage points for an increase in vote share of one standard deviation) and is consistent across specification. Consistent with our ATUS results, the effect of vote share diminishes by late 2023, especially once additional controls are added in columns 2-4.

Overall, the results using ACS, ATUS, and CPS tell a consistent story: counties with a higher pre-COVID Democratic vote share experienced a larger increase in remote-work rates during the pandemic, and that effect has persisted to a lesser extent as the pandemic receded.

5.5 Analysis using job posting data

In Table 6, we analyze a different kind of data: the 3-month moving average of the share of online job vacancies with remote-work advertising by county and month (from Hansen et al., 2023). Because this data was collected by the authors only starting in 2019, we cannot analyze the pre-treatment period as we do with our ACS and ATUS analyses. However, with data through 2024, we can see how the effect of Democratic vote share is changing long after the onset of the pandemic.

In these regressions, we estimate equation (1) with the share of job postings (at the county-month level) that advertise remote-work possibilities. We also exclude individual-level covariates from the right-hand side of equation (1).

The results in Table 6 indicate that an increase of one standard deviation in Democratic vote share is associated with a roughly 1.5-2 percentage point increase in the share of job postings with explicit remote-work opportunities starting in 2021. These are large effects, with the pre-COVID (2019) mean being about 3% of all job postings and the post-COVID mean about 7% of all postings. Once again, estimates are affected very little by the inclusion of additional controls in columns 2-4.[22] Lastly, the effects are persistent through 2024, suggesting that by this measure of remote work opportunities, the association with a higher Democratic vote share in one's county of residence has remained strong.

5.6 Analysis at the state level

[22] One important note is the large reduction in sample size when moving from columns 1-3 to column 4 in Table 6. This is due to the fact that this analysis is not restricted to larger urban counties as it is with our individual-level data (due to county identifiers being obscured for those in smaller counties). This means that we can use all counties in the regressions corresponding with columns 1-3; however, when we add the COVID mobility index to the regression (column 4), it reduces to the sample to larger counties again because that variable is only available for those counties.

In our county-level analysis, we are unable to use roughly half of the observations in our individual datasets because county identifiers are not included for those in counties with insufficient observations (typically smaller, more rural counties). This leads us to try our analysis at the state level as an additional robustness check, which has the advantage of letting us include all individuals in the data (since state of residence is given for all individuals in all three data sources). The disadvantage is that using state-level Democratic vote share (and other characteristics) does not make use of within-state variation in these variables; furthermore, because they are averaged over all areas within a state, there is a greater degree of measurement error between our proxies and what an individual actually experiences in her local area.

The state-level results utilize the state COVID policy stringency index (as in the county-level regressions) and state-level equivalents of 2016 presidential Democratic vote share, the COVID-era unemployment rate, COVID cases and deaths through 2021, and the 2020 device exposure (mobility) index. In addition, we make use of two additional policy indices provided by OxCGRT: the economic support index (cash payments for job loss/inability to work and debt/contract relief) and the mandatory vaccination index (for certain classes of workers, such as medical personnel). We do not control for the labor-market concentration index as it is a local measure of occupation-specific employer concentration.

The results of this exercise are contained in Tables 7 (ACS), 8 (ATUS), and 9 (CPS). Moving from left to right across columns, we add additional state-level controls (as we did with county controls in our county-level regressions). The ACS results are very similar across columns, and the estimated effects sizes mirror those from our county-level analysis in Table 3 (though the state-level results are in some cases slightly smaller in magnitude). State-level ATUS results in Table 8 are somewhat smaller in magnitude but more consistently individually significant, likely owing to the more than doubling of the sample size. Including additional state-level covariates also tends to render the coefficients on Democratic vote share in the post-COVID period larger; in the most saturated specification (column 4), even the 2023 interaction with vote share is significant at the 5% level.

Turning to the CPS results in Table 9, we again see results that are highly consistent with their county-level counterparts (in Table 5), though they are somewhat smaller in magnitude, especially in earlier quarters. However, in the state-level results, there is more consistent evidence that the effect of vote share on remote-work transitions persist through the end of the sample window (the last quarter of 2023).

6. Conclusion

We investigate how pre-COVID county Democratic vote share (in the 2016 presidential election) affects the growth of remote work during and after the COVID-19 pandemic. Across several different data sources and measures of remote work, we find that a larger Democratic vote share is associated with a relative increase in remote work rates in a county starting in 2020 and persisting for several years after. A natural question is whether differences in COVID-19 policies and/or local economic circumstances, which are correlated with political preferences, drive this relationship. Though we cannot fully rule out the possibility that additional factors that are correlated with vote share explain our findings, differences in occupational teleworkability, college attainment, COVID-era policies, and local economic conditions (such as the concentration of local labor markets) do not account for the association between vote share and remote work we observe. This is true even though many of these variables themselves are strongly predictive of remote-work rates in wake of the pandemic.

We hypothesize that differences in the valuation of remote work that are correlated with political preferences caused individuals in more Democratic counties to begin working remotely in greater numbers during the early phase of the pandemic—when new opportunities to work from home arose—and that this behavior carried over into later years. Local political preferences have the potential to change the equilibrium associated with remote work in a given area, since each worker and firm's decisions about remote work arrangements depend in part on group-level demand and supply for it.

As political polarization has risen in the U.S. (Tyler and Iyengar, 2024), several issues surrounding individuals' work lives have become more culturally sensitive (see, for example, Kempf and Cassidy, 2025). While polarization surrounding the COVID-19 pandemic may have been the impetus for polarized beliefs regarding the value of remote work and remote-work policies, remote work itself has now become a politically charged subject.[23] Because we do not observe political preferences or values at the individual level nor do we have panel data on the same individuals before and after the onset of COVID-19, our ability to explore the exact mechanisms underlying our estimated effects is limited. We believe this is an important direction for future research.

[23] https://www.bloomberg.com/news/articles/2024-03-02/remote-work-jobs-how-the-office-debate-became-a-culture-war-flashpoint

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Figure 1:

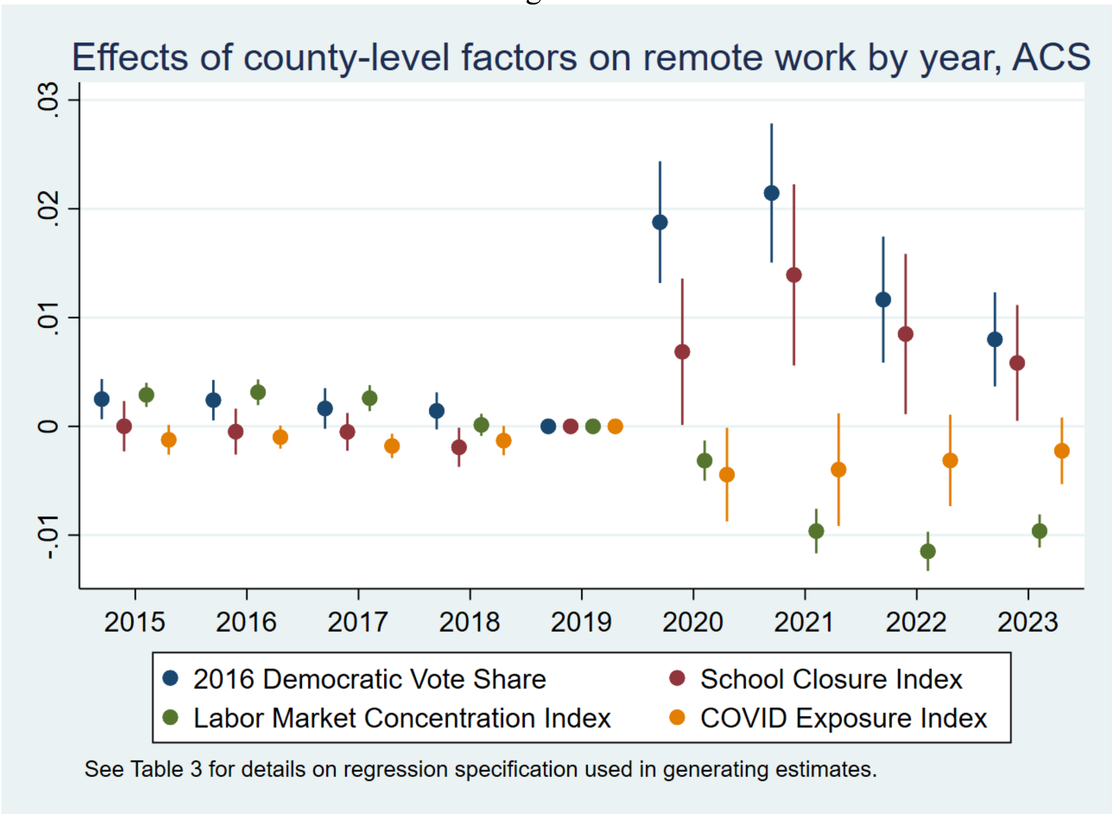

Figure 2:

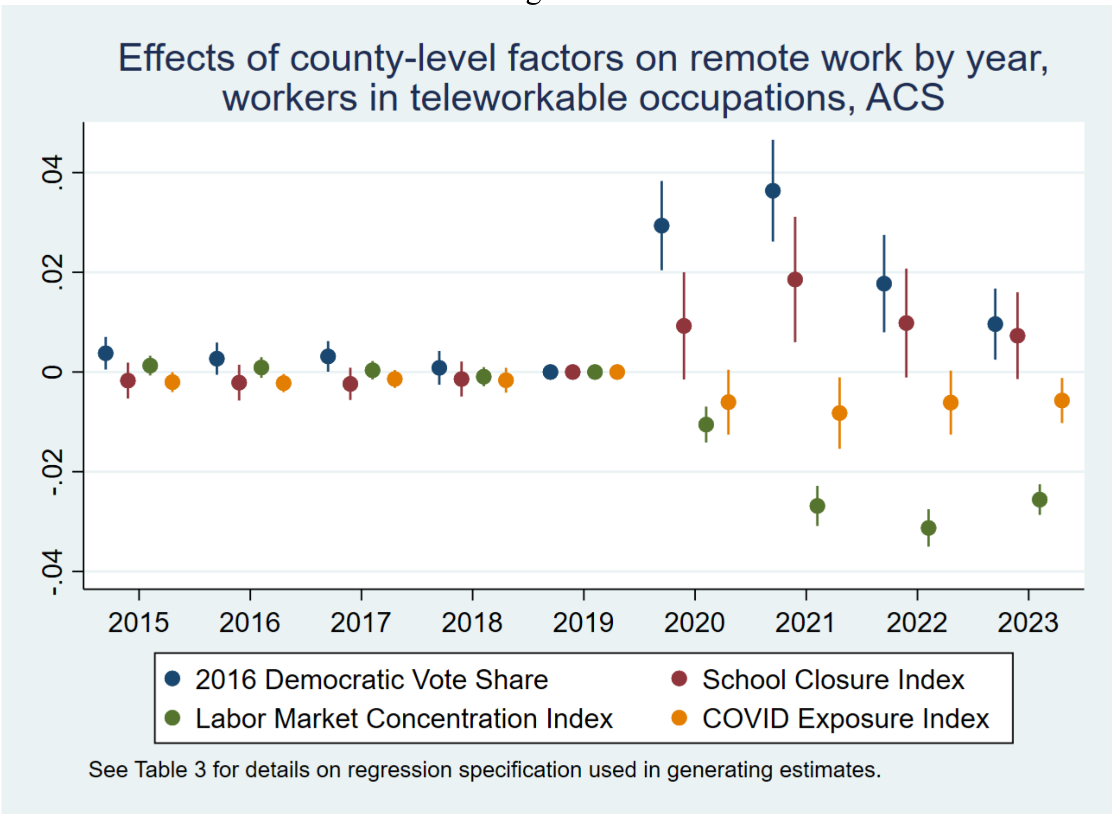

Table 1:
Summary statistics by time period and dataset

	ACS		ATUS		CPS	
	Pre-COVID	Post-COVID	Pre-COVID	Post-COVID	Pre-COVID	Post-COVID
Individual characteristics	mean	mean	mean	mean	mean	mean
Age	41.51	41.37	41.70	41.56	N/A	41.58
Fully teleworkable occupation	0.37	0.39	0.41	0.44	N/A	0.39
Employed	0.78	0.79	0.80	0.83	N/A	0.80
Working from home (indicates "work from home" as response to transportation to work)	0.04	0.14	N/A	N/A	N/A	N/A
Daily share of work time from home	N/A	N/A	0.17	0.34	N/A	N/A
Transitioned to remote work after COVID	N/A	N/A	N/A	N/A	N/A	0.11
High school graduate	0.20	0.18	0.22	0.21	N/A	0.25
Some college	0.29	0.27	0.23	0.19	N/A	0.23
College graduate	0.37	0.41	0.44	0.50	N/A	0.45
White	0.67	0.54	0.76	0.75	N/A	0.72
Black	0.14	0.14	0.14	0.13	N/A	0.14
Asian	0.09	0.09	0.07	0.10	N/A	0.11
Other race	0.10	0.23	0.03	0.02	N/A	0.03
Hispanic ethnicity	0.22	0.23	0.23	0.24	N/A	0.24
Married	0.52	0.52	0.57	0.56	N/A	0.54
Female	0.51	0.50	0.51	0.50	N/A	0.50
In metropolitan area	0.99	0.99	0.99	0.99	N/A	0.99
County/state characteristics	Mean	SD	Mean	SD	Mean	SD
2016 presidential Democratic vote share	0.58	0.16	0.59	0.17	0.59	0.16
COVID stringency index (2020-2021)	46.04	3.97	45.90	3.88	46.91	4.02
School closure percentage (2020-2021)	0.38	0.13	0.40	0.13	0.40	0.13
Unemployment rate (2020-2021)	7.18	2.05	7.31	2.11	7.43	2.11
COVID deaths per 1,000 residents (2020-2021)	2.22	0.92	2.35	0.93	2.37	0.93
COVID cases per 1,000 residents (2020-2021)	159.15	34.25	162.74	37.64	164.16	38.62
Mobility index (2020)	118.01	52.45	115.24	56.76	116.31	57.10
HHI by county and occupation (2019)	1,193.11	1,846.53	1,260.24	1,948.86	1,164.81	1,781.80

All values are weighted by survey sample weights.

Table 2:

The effect of 2016 presidential democratic vote share on teleworkability, college graduation, and employment, by year, ACS

	Occupational teleworkability	College graduate	Employed
Pre-COVID			
2016 Democratic vote share x (year==2015)	0.000	0.000	-0.001
	(0.001)	(0.001)	(0.001)
2016 Democratic vote share x (year==2016)	-0.000	-0.000	-0.001
	(0.001)	(0.001)	(0.001)
2016 Democratic vote share x (year==2017)	-0.001	-0.001	-0.002**
	(0.001)	(0.001)	(0.001)
2016 Democratic vote share x (year==2018)	-0.000	0.001	-0.001
	(0.001)	(0.001)	(0.001)
Post-COVID			
2016 Democratic vote share x (year==2020)	0.001	0.001	-0.006***
	(0.001)	(0.001)	(0.002)
2016 Democratic vote share x (year==2021)	-0.001	-0.001	-0.008***
	(0.001)	(0.001)	(0.002)
2016 Democratic vote share x (year==2022)	0.001	-0.000	-0.003***
	(0.001)	(0.001)	(0.001)
2016 Democratic vote share x (year==2023)	-0.002*	0.001	-0.002*
	(0.001)	(0.001)	(0.001)
Pre-COVID mean of dependent variable	0.41	0.34	0.77
Observations	6,774,125	7,660,818	7,660,818

Notes: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable in column 1 is the Dingel and Neimann teleworkability index score of each individual's 4-digit Census occupation, which takes a value between 0 and 1; in column 2, it is equal to "1" if the individual is a college graduate (and "0" otherwise); in column 3, it is equal to "1" if the individual is currently employed (and "0" otherwise). 2016 county-level Democratic vote share has been normalized to units of standard deviations. All regressions are weighted by ACS sample weights. All regressions include county and year fixed effects, age and its square, and dummies for sex, race, Hispanic ethnicity, marital status, urbanicity, educational attainment (except column 2), occupation (except columns 1 and 3), and industry (except column 3). Standard errors are clustered at the county level.

Table 3:

The effect of 2016 county-level presidential democratic vote share on remote work, by year, ACS

	I	II	III	IV
Pre-COVID				
2016 Democratic vote share x (year==2015)	0.002***	0.002**	0.001	0.003***
	(0.001)	(0.001)	(0.001)	(0.001)
2016 Democratic vote share x (year==2016)	0.002**	0.002**	0.002	0.002**
	(0.001)	(0.001)	(0.001)	(0.001)
2016 Democratic vote share x (year==2017)	0.001**	0.002**	0.001	0.002*
	(0.001)	(0.001)	(0.001)	(0.001)
2016 Democratic vote share x (year==2018)	0.001*	0.002**	0.002*	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Post-COVID				
2016 Democratic vote share x (year==2020)	0.020***	0.018***	0.017***	0.019***
	(0.003)	(0.003)	(0.004)	(0.003)
2016 Democratic vote share x (year==2021)	0.025***	0.019***	0.018***	0.021***
	(0.004)	(0.004)	(0.005)	(0.003)
2016 Democratic vote share x (year==2022)	0.014***	0.011***	0.011***	0.012***
	(0.003)	(0.003)	(0.004)	(0.003)
2016 Democratic vote share x (year==2023)	0.009***	0.007***	0.008***	0.008***
	(0.002)	(0.003)	(0.003)	(0.002)
Observations	6,769,281	6,769,281	6,769,281	6,610,622
Included controls (interacted by year):				
Occupational teleworkability, college graduation, and urbanicity	Yes	Yes	Yes	Yes
County average school closure and State COVID policy stringency indices	No	Yes	Yes	Yes
State by year fixed effects	No	No	Yes	No
Labor-market HHI, unemployment rate, COVID cases and deaths, mobility index	No	No	No	Yes

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The dependent variable is equal to "1" if the individual indicates that they "worked at home" in response to a question about their transportation to work (and "0" otherwise). The pre-COVID mean of the dependent variable is 0.04 and the post-COVID mean is 0.13. All county-level characteristics have been normalized so their units are standard deviations. All regressions are weighted by ACS sample weights. All regressions include county and year fixed effects, age and its square, and dummies for sex, race, Hispanic ethnicity, marital status, urbanicity, educational attainment, occupation, and industry. Standard errors are clustered at the county level.

Table 4:
The effect of 2016 county-level presidential democratic vote share on share of daily work time spent at home, by year, ATUS

	I	II	III	IV
Pre-COVID				
2020 Democratic vote share x (year==2015)	0.013	-0.003	0.011	-0.028
	(0.019)	(0.030)	(0.042)	(0.029)
2020 Democratic vote share x (year==2016)	0.004	-0.011	-0.010	-0.026
	(0.018)	(0.032)	(0.040)	(0.034)
2020 Democratic vote share x (year==2017)	0.023	-0.013	0.015	-0.033
	(0.017)	(0.029)	(0.039)	(0.026)
2020 Democratic vote share x (year==2018)	0.046**	0.035	0.074**	0.018
	(0.018)	(0.026)	(0.035)	(0.025)
Post-COVID				
2020 Democratic vote share x (year==2020)	0.067***	0.086**	0.081	0.064*
	(0.022)	(0.038)	(0.052)	(0.036)
2020 Democratic vote share x (year==2021)	0.065***	0.058	0.035	0.044
	(0.022)	(0.040)	(0.046)	(0.039)
2020 Democratic vote share x (year==2022)	0.064***	0.074**	0.102**	0.054*
	(0.019)	(0.035)	(0.044)	(0.032)
2020 Democratic vote share x (year==2023)	0.012	0.013	0.014	-0.006
	(0.021)	(0.033)	(0.044)	(0.031)
Observations	9,924	9,924	9,917	9,685
Included controls (interacted by year):				
Occupational teleworkability, college graduation, and urbanicity	Yes	Yes	Yes	Yes
County average school closure and State COVID policy stringency indices	No	Yes	Yes	Yes
State by year fixed effects	No	No	Yes	No
Labor-market HHI, unemployment rate, COVID cases and deaths, mobility index	No	No	No	Yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is equal to number of minutes the individual reported working for pay *from home* in their interview day divided by the total number of minutes worked in the same day. The pre-COVID mean of the dependent variable is 0.16 and the post-COVID mean is 0.32. All county-level characteristics have been normalized so their units are standard deviations. All regressions are weighted by ATUS sample weights. All regressions include county and year fixed effects, age and its square, and dummies for sex, race, Hispanic ethnicity, marital status, urbanicity, educational attainment, occupation, and industry. Standard errors are clustered at the county level.

Table 5:

The effect of 2016 county-level presidential democratic vote share on remote work transition, by quarter, CPS

	I	II	III	IV
2020 Democratic vote share x (quarter==4 & year==2022)	0.022***	0.023***	0.019***	0.023***
	(0.005)	(0.007)	(0.007)	(0.006)
2020 Democratic vote share x (quarter==1 & year==2023)	0.025***	0.031***	0.028***	0.029***
	(0.004)	(0.006)	(0.006)	(0.005)
2020 Democratic vote share x (quarter==2 & year==2023)	0.024***	0.026***	0.019***	0.026***
	(0.003)	(0.005)	(0.006)	(0.005)
2020 Democratic vote share x (quarter==3 & year==2023)	0.015***	0.016***	0.013**	0.016***
	(0.004)	(0.006)	(0.005)	(0.006)
2020 Democratic vote share x (quarter==4 & year==2023)	0.021***	0.010	0.001	0.009
	(0.004)	(0.007)	(0.008)	(0.006)
Observations	207,237	207,237	207,237	200,625
Included controls (interacted by quarter-year):				
Occupational teleworkability, college graduation, and urbanicity	Yes	Yes	Yes	Yes
County average school closure and State COVID policy stringency indices	No	Yes	Yes	Yes
State by quarter-year fixed effects	No	No	Yes	No
Labor-market HHI, unemployment rate, COVID cases and deaths, mobility index	No	No	No	Yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is equal to "1" if the individual indicates that they have shifted from not working at home in February 2020 to working at home at the month of interview (and "0" otherwise). The mean of the dependent variable is 0.11. All county-level characteristics have been normalized so their units are standard deviations. All regressions are weighted by CPS final basic weights. All regressions include quarter-year fixed effects, age and its square, and dummies for sex, race, Hispanic ethnicity, marital status, urbanicity, educational attainment, occupation, and industry. Standard errors are clustered at the county level. The last quarter of 2023 only includes October and November due to the unavailability of the work at home question, in reference to February 2020, beginning December 2023.

Table 6:

The effect of 2016 county-level presidential democratic vote share on remote work, by year, job postings

	I	II	III	IV
Post-COVID				
2016 Democratic vote share x (year==2020)	0.873***	0.705***	0.610***	0.746***
	(0.046)	(0.093)	(0.084)	(0.092)
2016 Democratic vote share x (year==2021)	1.673***	1.617***	1.404***	1.796***
	(0.108)	(0.172)	(0.168)	(0.178)
2016 Democratic vote share x (year==2022)	2.206***	2.004***	1.755***	2.125***
	(0.158)	(0.256)	(0.234)	(0.273)
2016 Democratic vote share x (year==2023)	2.133***	2.069***	1.791***	2.257***
	(0.175)	(0.234)	(0.211)	(0.252)
2016 Democratic vote share x (year==2024)	1.935***	1.853***	1.647***	2.024***
	(0.154)	(0.205)	(0.193)	(0.215)
Observations	18,444	18,246	18,240	12,054
Included controls (interacted by year):				
County average school closure and State COVID policy stringency indices	No	Yes	Yes	Yes
State by year fixed effects	No	No	Yes	No
Unemployment rate, COVID cases and deaths, mobility index	No	No	No	Yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the 3-month moving average percentage of online job postings that explicitly allow remote-work arrangements originating from a county in a given month. The pre-COVID mean of the dependent variable is 3% and the post-COVID mean is 7%. All county-level characteristics have been normalized so their units are standard deviations. All regressions are weighted by 2020 county populations. All regressions include county and year fixed effects. Standard errors are clustered at the county level.

Table 7:

The effect of 2016 state-level presidential democratic vote share on remote work, by year, ACS

	I	II	III	IV
Pre-COVID				
2016 Democratic vote share x (year==2015)	0.002***	0.002**	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
2016 Democratic vote share x (year==2016)	0.001***	0.001**	0.001	0.000
	(0.000)	(0.001)	(0.001)	(0.001)
2016 Democratic vote share x (year==2017)	0.001**	0.001	0.001	-0.000
	(0.000)	(0.001)	(0.001)	(0.001)
2016 Democratic vote share x (year==2018)	0.001***	0.001	0.000	0.000
	(0.000)	(0.001)	(0.001)	(0.001)
Post-COVID				
2016 Democratic vote share x (year==2020)	0.011***	0.011***	0.009*	0.009***
	(0.002)	(0.003)	(0.005)	(0.003)
2016 Democratic vote share x (year==2021)	0.014***	0.016***	0.015**	0.016***
	(0.003)	(0.004)	(0.006)	(0.005)
2016 Democratic vote share x (year==2022)	0.007***	0.010***	0.012***	0.012***
	(0.003)	(0.002)	(0.004)	(0.003)
2016 Democratic vote share x (year==2023)	0.002	0.006**	0.008**	0.008***
	(0.002)	(0.002)	(0.003)	(0.003)
Observations	10,906,557	10,906,557	10,906,557	10,906,557
Included controls (interacted by year):				
Occupational teleworkability, college graduation, and urbanicity	Yes	Yes	Yes	Yes
State COVID policy stringency index	No	Yes	Yes	Yes
State mandatory vaccination and economic support indices	No	No	Yes	Yes
State-level COVID unemployment rate, cases and deaths, mobility index	No	No	No	Yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is equal to "1" if the individual indicates that they "worked at home" in response to a question about their transportation to work (and "0" otherwise). The pre-COVID mean of the dependent variable is 0.04 and the post-COVID mean is 0.13. All state-level characteristics have been normalized so their units are standard deviations. All regressions are weighted by ACS sample weights. All regressions include state and year fixed effects, age and its square, and dummies for sex, race, Hispanic ethnicity, marital status, urbanicity, educational attainment, occupation, and industry. Standard errors are clustered at the state level.

Table 8:

The effect of 2016 state-level presidential democratic vote share on remote work, by year, ATUS

	I	II	III	IV
Pre-COVID				
2016 Democratic vote share x (year==2015)	0.009	0.025**	0.021	0.026
	(0.011)	(0.012)	(0.014)	(0.017)
2016 Democratic vote share x (year==2016)	-0.006	0.009	-0.002	0.003
	(0.010)	(0.012)	(0.015)	(0.019)
2016 Democratic vote share x (year==2017)	0.000	0.013	0.029	0.035*
	(0.012)	(0.014)	(0.018)	(0.019)
2016 Democratic vote share x (year==2018)	0.011	0.010	0.001	-0.007
	(0.009)	(0.014)	(0.014)	(0.016)
Post-COVID				
2016 Democratic vote share x (year==2020)	0.018	0.028*	0.027	0.036
	(0.011)	(0.016)	(0.021)	(0.023)
2016 Democratic vote share x (year==2021)	0.029**	0.042***	0.037**	0.047***
	(0.011)	(0.012)	(0.016)	(0.017)
2016 Democratic vote share x (year==2022)	0.018	0.036**	0.036**	0.039*
	(0.014)	(0.015)	(0.017)	(0.021)
2016 Democratic vote share x (year==2023)	0.007	0.019	0.031*	0.041**
	(0.014)	(0.013)	(0.016)	(0.019)
Observations	22,623	22,623	22,623	22,623
Included controls (interacted by year):				
Occupational teleworkability, college graduation, and urbanicity	Yes	Yes	Yes	Yes
State COVID policy stringency index	No	Yes	Yes	Yes
State mandatory vaccination and economic support indices	No	No	Yes	Yes
State-level COVID unemployment rate, cases and deaths, mobility index	No	No	No	Yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is equal to number of minutes the individual reported working for pay *from home* in their interview day divided by the total number of minutes worked in the same day. The pre-COVID mean of the dependent variable is 0.16 and the post-COVID mean is 0.32. All state-level characteristics have been normalized so their units are standard deviations. All regressions are weighted by ATUS sample weights. All regressions include state and year fixed effects, age and its square, and dummies for sex, race, Hispanic ethnicity, marital status, urbanicity, educational attainment, occupation, and industry. Standard errors are clustered at the state level.

Table 9:

The effect of 2016 state-level presidential democratic vote share on remote work transition, by quarter, CPS

	I	II	III	IV
2016 Democratic vote share x (quarter==4 & year==2022)	0.014***	0.012**	0.015**	0.011**
	(0.004)	(0.005)	(0.007)	(0.004)
2016 Democratic vote share x (quarter==1 & year==2023)	0.017***	0.015***	0.019**	0.015**
	(0.005)	(0.006)	(0.008)	(0.006)
2016 Democratic vote share x (quarter==2 & year==2023)	0.015***	0.015***	0.019***	0.016***
	(0.004)	(0.005)	(0.007)	(0.005)
2016 Democratic vote share x (quarter==3 & year==2023)	0.013***	0.015***	0.014*	0.009
	(0.004)	(0.004)	(0.008)	(0.005)
2016 Democratic vote share x (quarter==4 & year==2023)	0.014***	0.014***	0.015*	0.010*
	(0.004)	(0.005)	(0.007)	(0.005)
Observations	494,941	494,941	494,941	494,941
Included controls (interacted by quarter-year):				
Occupational teleworkability, college graduation, and urbanicity	Yes	Yes	Yes	Yes
State COVID policy stringency index	No	Yes	Yes	Yes
State mandatory vaccination and economic support indices	No	No	Yes	Yes
State-level COVID unemployment rate, cases and deaths, mobility index	No	No	No	Yes

Notes: *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is equal to "1" if the individual indicates that they have shifted from not working at home in February 2020 to working at home at the month of interview (and "0" otherwise). The mean of the dependent variable is 0.11. All state-level characteristics have been normalized so their units are standard deviations. All regressions are weighted by CPS final basic weights. All regressions include quarter-year fixed effects, age and its square, and dummies for sex, race, Hispanic ethnicity, marital status, urbanicity, educational attainment, occupation, and industry. Standard errors are clustered at the state level. The last quarter of 2023 only includes October and November due to the unavailability of the work at home question, in reference to February 2020, beginning December 2023.