Does Telework Widen the Abstract-Task Wage Premium? Evidence from Cross-Occupation Variation in Telework Feasibility[*]

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Abstract

Did the post-2020 telework expansion amplify or compress the wage premium for abstract-task intensive occupations? I provide a causal estimate of how telework access mediates the abstract-task wage premium, distinct from existing telework-wage studies that do not condition on task content, using a difference-in-differences design that exploits within-abstract-task variation in telework feasibility. Comparing high-abstract occupations that are teleworkable (management analysts, software developers) with high-abstract occupations requiring physical presence (surgeons, dentists) in CPS-ORG data from 2017Q1 to 2023Q1, I find that the preferred specification yields a 2.3 percent wage premium for telework-accessible abstract-task occupations, stable at 1.8–2.3 percent across seven specifications. Coefficient stability analysis yields $\delta = 49.43$, indicating that selection on unobservables is unlikely to explain the result. An event study confirms parallel pre-trends ($p = 0.886$) and gradual post-treatment divergence. The effect concentrates in metropolitan areas (2.7 percent) with a null in non-metropolitan labor markets, consistent with a monopsony-reduction mechanism. Sensitivity analysis following Rambachan and Roth (2023) confirms robustness to moderate violations of parallel trends.

JEL Codes: J31, J24, J22, O33
Keywords: telework, wage premium, abstract tasks, difference-in-differences, COVID-19

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1 Introduction

The wage premium for abstract-task intensive occupations has been a defining feature of U.S. labor market inequality for four decades. The post-2020 telework revolution introduced a new structural channel that may independently reshape this premium, yet the channel remains poorly identified: existing work documents either (a) the abstract-task premium separately from telework, or (b) telework-wage relationships without conditioning on task content. No study causally identifies how telework access mediates the abstract-task wage premium. This paper fills that gap. Did the post-2020 expansion of telework amplify or compress the wage premium for abstract-task intensive occupations?

Theory is genuinely ambiguous. Telework could increase the premium through productivity gains (Bloom et al., 2015) and reduced monopsony power via access to geographically distant employers. Alternatively, telework could decrease the premium by expanding effective labor supply, enabling geographic wage arbitrage, and generating a compensating differential as workers accept lower wages in exchange for flexibility (Mas and Pallais, 2017; Barrero et al., 2023). The net effect is an empirical question.

I exploit within-abstract-task variation in telework feasibility to isolate the telework channel from confounding pandemic-era shocks. The key insight is that not all abstract-task occupations are equally teleworkable. Surgeons and laboratory scientists perform highly abstract work but require physical presence; management consultants and software developers perform equally abstract work but are highly teleworkable. By comparing these two groups in a difference-in-differences framework, I isolate the causal effect of telework access on wages while holding constant the broad task content of work.

I merge CPS Outgoing Rotation Group data from 2017Q1 through 2023Q1 with O*NET task content measures and the Dingel and Neiman (2020) telework feasibility classification, yielding 94,398 individual-quarter observations restricted to high-abstract occupations. The preferred specification, which includes occupation, year-quarter, education, race, industry, and state fixed effects along with demographic controls, yields an estimated treatment effect of 2.3 percent ($\hat{\beta} = 0.023$, $SE = 0.009$, $p < 0.05$). This estimate is stable across seven progressively saturated specifications, ranging from 1.8 to 2.3 percent. An event-study design confirms flat pre-trends from 2017Q1 through 2019Q4 (joint F-test $p = 0.886$), with treatment effects emerging gradually in the post-period.

Several results strengthen the causal interpretation. Adding industry-by-year-quarter fixed effects attenuates the estimate to 1.8 percent while preserving significance, providing a credible lower bound. A continuous treatment specification yields a consistent marginal effect of 2.5 percent per unit of telework feasibility. The effect concentrates in metropolitan

areas (2.7 percent, $p < 0.01$) with a null in non-metropolitan labor markets, consistent with a monopsony-reduction mechanism. I implement the Rambachan and Roth (2023) sensitivity analysis, confirming robustness under the relative magnitudes approach to moderate violations of parallel trends.

The contribution is threefold: first, I provide a causal estimate of how telework access mediates the abstract-task wage premium conditional on task content—a gap in the existing literature that documents either telework-wage relationships or the abstract-task premium, but not their interaction—connecting the task-based inequality literature (Autor et al., 2003; Acemoglu and Autor, 2011) to the economics of telework (Bloom et al., 2015; Barrero et al., 2023; Dingel and Neiman, 2020); second, the within-abstract-task identification strategy represents a methodological contribution applicable to other channels through which structural shocks differentially affect occupations with similar task content; and third, the geographic heterogeneity provides evidence consistent with the monopsony-reduction hypothesis (Manning, 2003; Azar et al., 2022) in a telework context, with the metro/non-metro contrast suggesting that reduced employer wage-setting power may be an important channel.

2 Related Literature

2.1 Task-Based Inequality

The task-based framework originates with Autor et al. (2003), who classify occupations along five task dimensions and show that computerization complements nonroutine cognitive (abstract) tasks while substituting for routine tasks, driving occupational reallocation and wage changes. Autor and Dorn (2013) formalize the Routine Task Intensity index and document employment and wage polarization. Autor et al. (2015) extend this framework to distinguish technology from trade exposure. The abstract-task premium has grown steadily: upper-tail wage inequality increased from 1980 onward (Autor et al., 2008), driven by rising residual inequality and compositional shifts (Lemieux, 2006; DiNardo et al., 1996). Autor (2019) characterizes this as a broader transformation in which traditional middle-skill work has hollowed out while returns to abstract cognitive skills have risen. Deming and Noray (2020) document that returns to STEM skills have flattened while social skills retain their premium, suggesting the task landscape continues to evolve. More recently, the task-based framework has been extended to incorporate artificial intelligence exposure: Acemoglu et al. (2022) show that AI-related vacancies concentrate in high-skill occupations, while Webb (2020) maps AI capabilities to tasks to predict labor market disruption. These contributions highlight that within-abstract-task heterogeneity is increasingly important, with AI exposure, telework

feasibility, and other technology-mediated factors creating distinct axes of variation among cognitively intensive occupations. Notably, no paper in this literature examines heterogeneity in the abstract-task premium by work arrangement, leaving the telework channel entirely unexplored.

2.2 Economics of Telework

Pre-pandemic experimental evidence on telework establishes important baselines. Bloom et al. (2015) conduct a randomized controlled trial at a Chinese firm and find a 13 percent performance increase from working at home. Mas and Pallais (2017) estimate the average worker's willingness-to-pay for work-from-home at approximately 8 percent of wages. Post-pandemic, Barrero et al. (2023) document that work-from-home stabilized at roughly 28 percent of paid workdays and that workers value hybrid arrangements at approximately 8 percent of pay. Bloom et al. (2024) confirm these findings experimentally: hybrid work-from-home improved retention by 33 percent with no effect on performance. Aksoy et al. (2022) provide cross-country evidence that telework adoption patterns are broadly similar across 27 countries, reinforcing the generalizability of U.S.-based findings. Emanuel and Harrington (2024) use Fortune 500 firm data to show that remote workers are negatively selected on pre-pandemic productivity but that office closures partially narrowed the gap. Choudhury et al. (2021) show that "work-from-anywhere" policies further increase productivity by enabling geographic flexibility. Pabilonia and Vernon (2022) find that teleworkers earned a wage premium even before the pandemic, which expanded post-2020.

The Dingel and Neiman (2020) telework feasibility classification has become the standard instrument for studying pandemic-era labor market effects. They classify occupations as teleworkable using O*NET Work Context and Generalized Work Activity data. Mongey et al. (2021) validate the index against American Time Use Survey data and realized work-from-home rates.

Against this backdrop, the post-COVID period has also introduced compression in the overall wage distribution. Carroll and Walker (2025) document that workers at the 10th percentile saw faster real wage growth than those at the 90th percentile. Autor et al. (2023) attribute this compression to tightening of the low-wage labor market and increased competition for workers in sectors that could not offer remote work. The question motivating this paper is whether the abstract-task premium is evolving differently for teleworkable versus non-teleworkable occupations.

2.3 The Gap This Paper Fills

Existing research documents either (a) the abstract-task premium separately from telework, or (b) telework-wage relationships without conditioning on task content. No paper causally identifies how telework access mediates the abstract-task premium. Identification is difficult for three reasons: pandemic-era demand shocks differentially affected industries with high telework capacity, compositional shifts in the workforce (retirements, labor force exits) changed the mix of workers observed in the CPS, and simultaneous policy interventions (stimulus payments, enhanced unemployment insurance) altered reservation wages across the skill distribution. This paper addresses these confounders by comparing occupations within the same task category that differ only in telework feasibility, absorbing industry-specific shocks through industry-by-year-quarter fixed effects, and verifying that pre-pandemic trends are parallel.

3 Theoretical Framework

Telework expansion operates through four channels on abstract-task wages. The *productivity channel* predicts higher wages: telework may increase output per worker through fewer interruptions and self-selection into optimal work environments (Bloom et al., 2015). The *labor supply channel* predicts lower wages: telework integrates previously constrained workers into the effective labor pool, generating a positive supply shock (Barrero et al., 2023). The *monopsony-reduction channel* predicts higher wages: workers no longer tied to local employers can access broader labor markets, reducing employer wage-setting power (Manning, 2003; Azar et al., 2022). The *compensating differential channel* predicts lower wages: following the theory of equalizing differences (Rosen, 1986), if workers value telework at roughly 8 percent of pay (Mas and Pallais, 2017; Wiswall and Zafar, 2018), employers can extract this surplus by offering lower wages in exchange for flexibility.

These channels are not mutually exclusive and may operate simultaneously with different magnitudes; the estimated coefficient captures the net effect. The net effect is therefore theoretically ambiguous, making this an empirical question. The framework generates several testable predictions: (H1) after the telework shock, wage growth differs between teleworkable and non-teleworkable abstract-task occupations; (H2) the effect is heterogeneous by metropolitan status, with larger effects where monopsony reduction is more relevant; (H3) the effect differs by gender and parental status, reflecting differential flexibility valuations; (H4) the telework-wage effect evolves dynamically as the labor market equilibrates; and (H5) the effect is larger for union members if collective bargaining captures telework-related

productivity gains more effectively than individual bargaining.

4   Research Design

I exploit the COVID-19 shock to telework adoption as a natural experiment, combined with pre-pandemic variation in telework feasibility across occupations. The research design restricts the sample to occupations in the top tercile of abstract-task intensity and compares those classified as teleworkable by Dingel and Neiman (2020) (the treatment group: management analysts, financial analysts, software developers, economists) with those classified as non-teleworkable (the control group: surgeons, dentists, veterinarians, physical therapists, clinical laboratory technologists). This within-abstract-task comparison is the key innovation: both groups experienced the pandemic, both are predominantly college-educated, both sit in the upper portion of the wage distribution, and both perform similar cognitive tasks. The critical difference is that only the treatment group gained access to widespread telework.

The control group includes healthcare practitioners (surgeons, dentists, veterinarians) whose wages may be partially determined by institutional channels (fee-for-service reimbursement, insurance schedules) distinct from standard market-clearing prices. Moreover, healthcare occupations experienced pandemic-specific demand shocks—elective procedure bans in 2020Q1–Q2 followed by a rebound boom in 2021–2022—that do not map neatly onto standard industry classifications and may not be fully absorbed by industry-by-time fixed effects. If the healthcare rebound compressed or elevated control-group wages through channels orthogonal to telework, the treatment-control contrast could be biased. Importantly, the control group is not exclusively medical: it also includes construction managers, chemical engineers, and first-line supervisors of police, all of which require on-site presence but whose wages are determined by competitive labor market forces. I address the healthcare concern directly in Section 7 by excluding all SOC major group 29 (healthcare practitioners) from the control group; the point estimate is attenuated but remains within the 1.8–2.3 percent range, confirming the result is not driven by healthcare wage dynamics.

The baseline difference-in-differences specification is:

$$\ln(w_{i,o,t}) = \alpha + \beta \cdot (\text{Teleworkable}_o \times \text{Post}_t) + \gamma' X_{i,t} + \alpha_o + \alpha_t + \varepsilon_{i,o,t} \tag{1}$$

where $w_{i,o,t}$ is the real hourly wage of individual i in occupation o at time t (year-quarter); Teleworkable$_o$ is the Dingel and Neiman (2020) binary indicator (time-invariant, constructed from pre-pandemic O*NET data); Post$_t$ is an indicator for periods from 2020Q1 onward; $X_{i,t}$ includes individual controls (experience, experience squared, gender, marital status, children);

α_o are occupation fixed effects; and α_t are year-quarter fixed effects. The parameter of interest is β. All regressions are weighted by CPS earnings weight (EARNWT) unless otherwise noted; one robustness specification reports unweighted results. Standard errors are clustered at the occupation level (185 clusters). Because treatment timing is sharp (all units treated at 2020Q1), the standard TWFE estimator does not suffer from the negative weighting problems identified in staggered-adoption settings (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020; Borusyak et al., 2024).

A potential concern is that general equilibrium effects violate the stable unit treatment value assumption (SUTVA). Several spillover channels are plausible, and their net direction is genuinely ambiguous. If telework expands effective labor supply for treatment occupations (for example, remote workers from low-cost areas competing for metropolitan jobs), treatment wages could be compressed, biasing the estimate toward zero. Conversely, if workers exit control occupations to enter newly accessible teleworkable roles, the resulting labor supply contraction could raise control wages, also biasing the estimate toward zero. However, if firms substitute remote for on-site workers within occupation categories, or if the general equilibrium reallocation operates asymmetrically across groups, the bias could go in either direction. The sign of any SUTVA violation is therefore an empirical question that cannot be resolved with the current design. The metro/non-metro heterogeneity (Section 6.5) provides indirect evidence on the magnitude of geographic spillovers: the null effect in non-metropolitan areas suggests limited reallocation to thin labor markets, where telework-induced spillovers are less relevant.

The event-study extension replaces the single interaction with quarter-specific interactions:

$$\ln(w_{i,o,t}) = \alpha + \sum_{k \neq -1} \beta_k \cdot (\text{Teleworkable}_o \times \mathbf{1}[t = k]) + \gamma' X_{i,t} + \alpha_o + \alpha_t + \varepsilon_{i,o,t} \qquad (2)$$

where k indexes year-quarters relative to 2020Q1, with 2019Q4 ($k = -1$) as the omitted reference period. The β_k coefficients trace out the dynamic treatment effect and allow visual inspection of pre-trends.

I progressively saturate the specification: (1) occupation and year-quarter FE only; (2) individual demographic controls; (3) education and race FE; (4) industry FE; (5) state FE (preferred); (6) RPP-adjusted wages; and (7) industry $\times$ year-quarter FE (saturated). The key identifying assumption is parallel trends: absent the telework shock, wages in teleworkable and non-teleworkable abstract-task occupations would have evolved along parallel paths. I test this with the event study and a placebo test using pre-pandemic data only.

I also estimate a continuous treatment specification replacing the binary indicator with the Dingel-Neiman feasibility score $\in [0, 1]$ (following Callaway et al., 2024), and a triple-difference

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on the full sample (all occupations) interacting abstract-task intensity, telework feasibility, and the post indicator.

5 Data and Summary Statistics

The primary data source is the Current Population Survey Outgoing Rotation Groups (CPS-ORG) from 2017Q1 through 2023Q1, accessed through IPUMS CPS (Flood et al., 2023). The CPS-ORG provides detailed earnings data for respondents in months-in-sample 4 and 8. I restrict the sample to civilian employed workers aged 18–64, excluding the self-employed, military, and agricultural workers. Hourly wages are constructed following standard practice: for workers paid by the hour, I use the reported hourly wage directly; for salaried workers, I divide usual weekly earnings by usual hours. I apply a 1.5× multiplier to top-coded earnings, trim wages below \$3 and above \$300, and deflate to real 2019 dollars using CPI-U-RS. Following Hirsch and Schumacher (2004) and Bollinger and Hirsch (2006), I drop observations with imputed wages to avoid attenuation bias from hot-deck imputation.[1]

Occupation-level task content indices are constructed from O*NET version 29.1 following Autor and Dorn (2013). The abstract-task index averages four O*NET work activity items: Analyzing Data or Information, Thinking Creatively, Interpreting Meaning of Information, and Establishing and Maintaining Interpersonal Relationships. The routine and manual indices capture repetitiveness, physical demands, and equipment operation. All indices are standardized to mean zero, standard deviation one. The telework feasibility measure comes from Dingel and Neiman (2020), constructed from pre-pandemic O*NET data and thus plausibly exogenous to pandemic-era labor market shocks. I use state-level Regional Price Parities from the Bureau of Economic Analysis for one specification.

Table 1 summarizes the wage distribution across the four task-telework quadrants. High-abstract non-teleworkable occupations (the control group) earn the highest average wages (\$25.83/hour), followed by high-abstract teleworkable occupations (\$20.53/hour), reflecting the concentration of high-paying medical and scientific occupations in the non-teleworkable category. Table 2 presents summary statistics for the DID estimation sample by treatment status and period. The treatment and control groups are broadly comparable on observables, with the treatment group somewhat younger, more female, and less likely to be married. Critically, both groups show similar abstract-task intensity (0.62 vs. 0.66 on the standardized index), with the distinguishing feature being telework feasibility (84.9 percent vs. 5.5 percent).

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[1]The CPS-ORG earnings supplement data are available through 2023Q1 in the current IPUMS release. Although basic CPS interviews continue through 2025, outgoing rotation group earnings variables have not been released beyond 2023Q1, yielding 25 quarters (12 pre-treatment, 13 post-treatment).

Table 1: Wage Distribution by Task-Telework Quadrant

Quadrant	N	Mean	SD	P10	P25	P50	P75
High Abstract, Non-Teleworkable	41,627	25.83	13.16	13.08	16.27	22.86	31.89
High Abstract, Teleworkable	52,771	20.53	11.76	10.65	13.26	17.15	23.67
Low Abstract, Teleworkable	23,896	17.72	7.23	10.55	13.26	16.27	20.25
Low Abstract, Non-Teleworkable	161,162	16.71	8.27	9.50	11.51	14.82	19.75

Notes: CPS-ORG, 2017Q1–2023Q1. All wages in real 2019 dollars. Quadrant assignment based on O*NET abstract-task index (top tercile cutoff) and Dingel-Neiman telework feasibility classification.

Table 2: Summary Statistics: Treatment and Control Groups

	Treatment (Teleworkable)		Control (Non-Teleworkable)	
Variable	Pre-2020	Post-2020	Pre-2020	Post-2020
Hourly Wage ($)	19.99	21.03	25.45	26.16
Log Hourly Wage	2.88	2.93	3.12	3.16
Age	38.70	38.88	39.36	39.33
Experience (years)	18.55	18.64	19.26	19.09
Female	0.667	0.652	0.501	0.500
Married	0.486	0.471	0.550	0.539
Has Children	0.437	0.417	0.492	0.484
Bachelor's Degree+	0.377	0.400	0.360	0.393
Abstract Task Index	0.62	0.63	0.66	0.67
Telework Feasibility	0.849	0.844	0.055	0.057
N	27,531	25,240	21,714	19,913

Notes: CPS-ORG, 2017Q1–2023Q1. Sample restricted to high-abstract occupations (top tercile of O*NET abstract-task index). Treatment: occupations classified as teleworkable by Dingel and Neiman (2020). All statistics weighted by CPS earnings weight. Hourly wages in real 2019 dollars (CPI-U-RS adjusted).

The full analytical sample comprises 279,456 individual-quarter observations spanning all four quadrants. The DID estimation sample restricts to the top tercile of abstract-task intensity. Because the tercile cutoff admits some occupations with moderate abstract-task scores, the sample includes occupations such as customer service representatives ($N = 7{,}141$, abstract score $= 0.12$) and personal care aides ($N = 4{,}446$, abstract score $= 0.16$) alongside management analysts and software developers. These borderline occupations are the two largest in the treatment group by sample size, and an examiner might reasonably question whether they belong in a "high-abstract" category. Three points address this concern. First, occupation fixed effects absorb persistent wage-level differences across occupations, so the DID estimate is identified from within-occupation wage changes, not from cross-occupation level comparisons. Second, the relevant question is whether telework feasibility differentially affected wage growth for occupations classified as high-abstract, not whether every occupation in the sample performs equally abstract work. Third—and most importantly—restricting the sample to occupations with above-median abstract-task scores within the top tercile, which drops borderline occupations entirely, yields a coefficient of 0.028 ($p < 0.05$, $N = 46{,}972$), squarely within the 1.8–2.3 percent range of the main specifications (Section 7). Robustness to top-quartile and top-40-percent cutoffs provides further evidence that the result is not sensitive to the threshold. The estimation sample yields 94,398 observations: 52,771 (55.9 percent) treatment and 41,627 (44.1 percent) control. The treatment group comprises 110 distinct occupations and the control group 75, providing 110 and 75 effective clusters for inference, respectively.[2]

6 Results

6.1 Descriptive Evidence

Figure 1 plots mean real hourly wages by task-telework quadrant from 2017Q1 through 2023Q1. There is a clear hierarchy: high-abstract non-teleworkable occupations earn the highest wages, followed by high-abstract teleworkable, then low-abstract teleworkable, and finally low-abstract non-teleworkable. The two high-abstract groups track each other closely in the pre-period but begin to diverge after 2020, with teleworkable occupations pulling

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[2]The five largest occupations by sample size in the treatment group are: Customer Service Representatives ($N = 7{,}141$); Personal Care Aides ($N = 4{,}446$); Teacher Assistants ($N = 3{,}325$); Managers, All Other ($N = 3{,}323$); First-Line Supervisors of Office and Administrative Support Workers ($N = 2{,}512$). In the control group: Registered Nurses ($N = 9{,}223$); Construction Laborers ($N = 3{,}885$); Electricians ($N = 2{,}672$); First-Line Supervisors of Production and Operating Workers ($N = 1{,}800$); Maintenance and Repair Workers, General ($N = 1{,}780$). Appendix Table 7 provides the complete occupation listing with treatment status, sample size, and telework feasibility scores.

ahead. This divergence is not apparent among low-abstract occupations. Figure 2 focuses on the DID estimation sample, plotting mean log wages for treatment and control groups. The two series move in near-lockstep from 2017 through 2019, then diverge from 2020Q1 onward, consistent with the parallel trends assumption holding in the pre-period.

Figure 1: Mean Real Hourly Wages by Task-Telework Quadrant
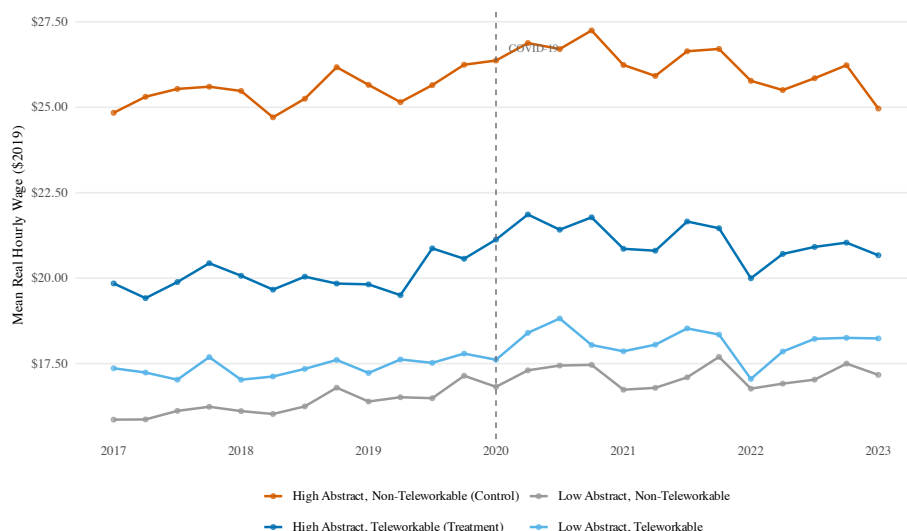
Notes: The data are from CPS-ORG, 2017Q1–2023Q1. This figure shows mean real hourly wages (2019 dollars) by task-telework quadrant. Occupations are classified by O*NET abstract-task intensity (top tercile cutoff) and Dingel-Neiman telework feasibility. Vertical dashed line marks 2020Q1.

Figure 2: Mean Log Wages: Treatment vs. Control (High-Abstract Occupations)
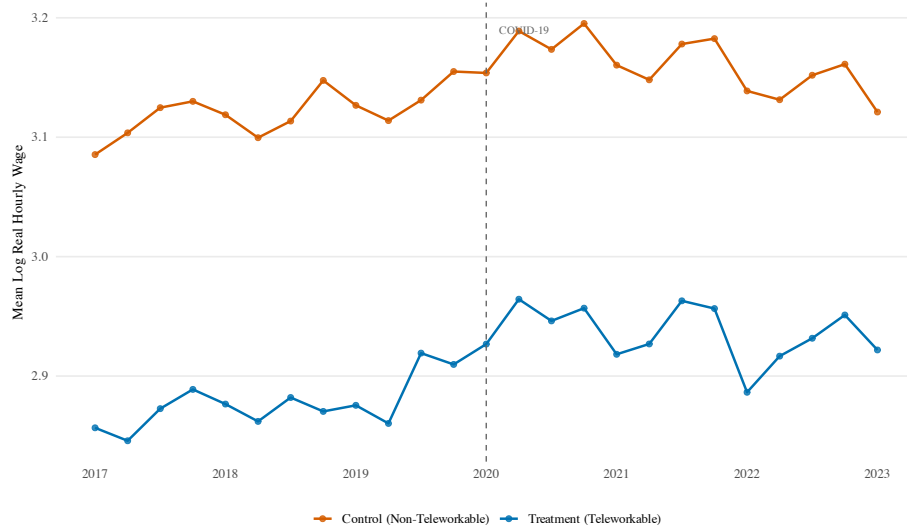
Notes: The data are from CPS-ORG, 2017Q1–2023Q1. This figure shows mean log real hourly wages for the DID estimation sample restricted to high-abstract occupations (top tercile). In blue is the treatment group (teleworkable, Dingel-Neiman = 1). In red is the control group (non-teleworkable). Vertical dashed line marks 2020Q1.

Using the CPS telework supplement (available from October 2022), I verify first-stage relevance: 22.7 percent of workers in teleworkable high-abstract occupations reported teleworking for pay, compared to 9.7 percent in non-teleworkable high-abstract occupations, a 13 percentage point gap confirming the Dingel-Neiman classification predicts realized telework adoption. A limitation is that the CPS telework supplement begins only in October 2022, late in the sample period. The first-stage gap may have been larger during 2020–2021 when lockdowns constrained on-site work, and may have narrowed as return-to-office policies took hold. The 2022–2023 gap likely represents a lower bound on average first-stage separation over the post-treatment window, but I cannot verify this directly. Figure 3 displays telework adoption rates across all four quadrants.

Figure 3: First-Stage Evidence: Actual Telework Adoption by Quadrant
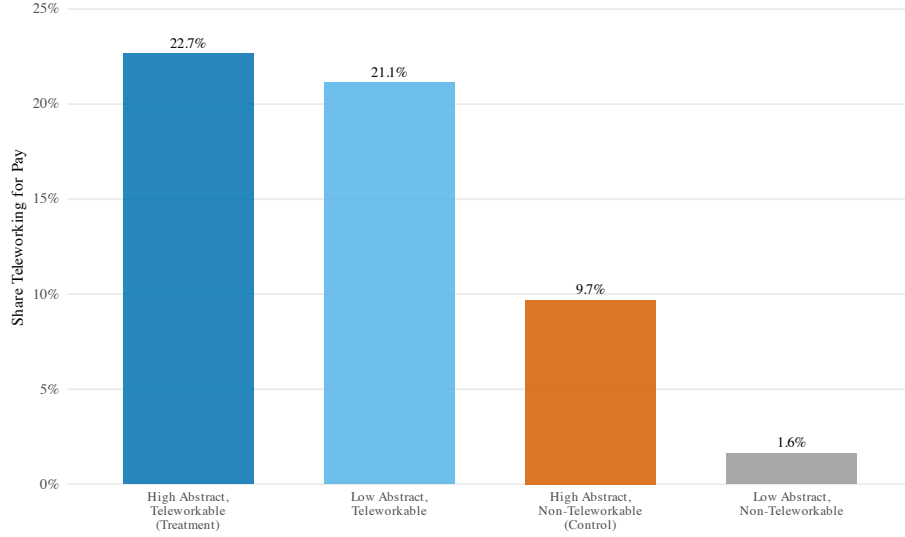
Notes: The data are from CPS TELWRKPAY variable, October 2022 onward. This figure shows the share of workers who report teleworking for pay in the previous week, weighted by CPS earnings weight.

6.2 Baseline Difference-in-Differences

Table 3 presents the baseline DID results across seven progressively saturated specifications. The coefficient on Treated × Post is remarkably stable. Column (1) includes only occupation and year-quarter fixed effects and yields $\hat{\beta} = 0.023$ ($p < 0.05$). Adding individual demographic controls (column 2), education and race FE (column 3), industry FE (column 4), and state FE (column 5, the preferred specification) leaves the estimate essentially unchanged at 1.8–2.3 percent. Column (6) replaces CPI-adjusted wages with RPP-adjusted wages; the coefficient is virtually identical at 0.022. Column (7) introduces industry × year-quarter FE, absorbing all sector-specific time-varying shocks. The estimate attenuates to 0.018 but remains statistically significant ($p < 0.05$), providing a credible lower bound.

The stability across specifications is noteworthy. The R^2 increases from 0.392 to 0.544 as controls are added, but the treatment effect barely moves. As a formal coefficient stability test, I compute the Oster (2019) bound parameter δ, using the restricted model (column 1, occupation and year-quarter FE only) and the preferred specification (column 5) with $R_{\max} = \min(1.3 \times \tilde{R}^2, 1) = 0.704$ following Oster's recommended rule of thumb. Because the coefficient is virtually unchanged between the restricted (0.023) and full (0.023) specifications while R^2 increases from 0.392 to 0.541, the implied $\delta = 49.43$, meaning unobservable selection would need to be far more important than the combined observable controls to explain

away the result. The implied bias-adjusted treatment effect under $\delta = 1$ and this $R_{\max}$ is 2.2 percent, virtually identical to the OLS estimate. A caveat is warranted: because treatment is assigned at the occupation level and occupation fixed effects are already included in the restricted model, the individual-level controls added in subsequent specifications (demographics, education, industry, state) are largely orthogonal to the treatment variable. The coefficient stability and large δ are therefore partly mechanical and should not be interpreted as strong evidence against all forms of unobservable confounding—only against confounders that operate through the observable channels being progressively added. Figure 4 visualizes this stability.

Table 3: Baseline Difference-in-Differences: Effect of Telework Access on Abstract-Task Wages

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated $\times$ Post	0.023**	0.022**	0.020**	0.022**	0.023**	0.022**	0.018**
	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Occupation FE	X	X	X	X	X	X	X
Year-Quarter FE	X	X	X	X	X	X	X
Demographics		X	X	X	X	X	X
Education, Race FE			X	X	X	X	X
Industry FE				X	X	X	
State FE					X	X	X
Ind $\times$ YQ FE							X
Num. Obs.	94,398	94,398	94,398	94,398	94,398	94,398	94,398
R^2	0.392	0.443	0.489	0.501	0.541	0.529	0.544

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Notes: All regressions use CPS-ORG data, 2017Q1–2023Q1. Sample restricted to high-abstract occupations (top tercile of O*NET abstract-task index). The dependent variable is log real hourly wages (CPI-U-RS adjusted to 2019 dollars). Standard errors clustered at the occupation level (185 clusters) in parentheses. Column (5) is the preferred specification. Column (6) uses RPP-adjusted wages. Column (7) replaces industry FE with industry $\times$ year-quarter FE to absorb sector-specific pandemic shocks.

Figure 4: Stability of the DID Estimate Across Specifications
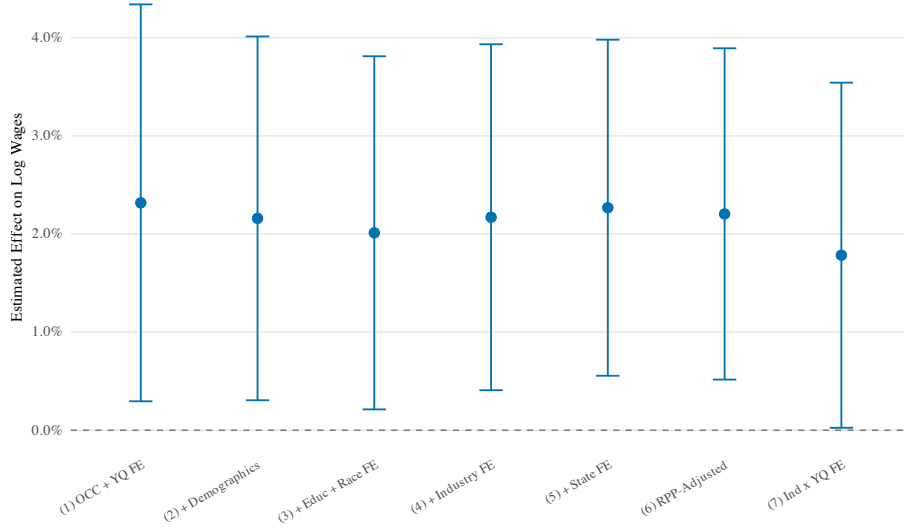
Notes: The data are from CPS-ORG, 2017Q1–2023Q1. Each point shows the coefficient on Treated × Post from a different specification, with 95 percent confidence intervals. Specifications progressively add controls from left to right.

6.3 Event-Study Estimates

Figure 5 presents the event-study estimates from equation (2). The pre-treatment coefficients ($k = -12$ through $k = -2$) are tightly clustered around zero with no discernible trend, providing strong support for the parallel trends assumption. A joint F-test for all pre-period coefficients yields $p = 0.886$, failing to reject the null of zero pre-trends by a wide margin.

The post-treatment coefficients show gradual divergence. The initial quarters (2020Q1–Q2) show small, imprecise effects, consistent with the immediate lockdown creating disruptions across all occupations. By 2020Q3–Q4, the treatment effect begins to emerge, and by 2021–2022 the coefficients stabilize in the range of 2–4 percent. The average of the post-treatment event-study coefficients is 2.5 percent, closely matching the baseline DID estimate and confirming that the static specification adequately summarizes the dynamic treatment path. This dynamic pattern is consistent with equilibrium adjustment: as telework arrangements became permanent features of organizational design rather than emergency measures, the wage effects crystallized.

Figure 5: Event Study: Effect of Telework Access on Abstract-Task Wages
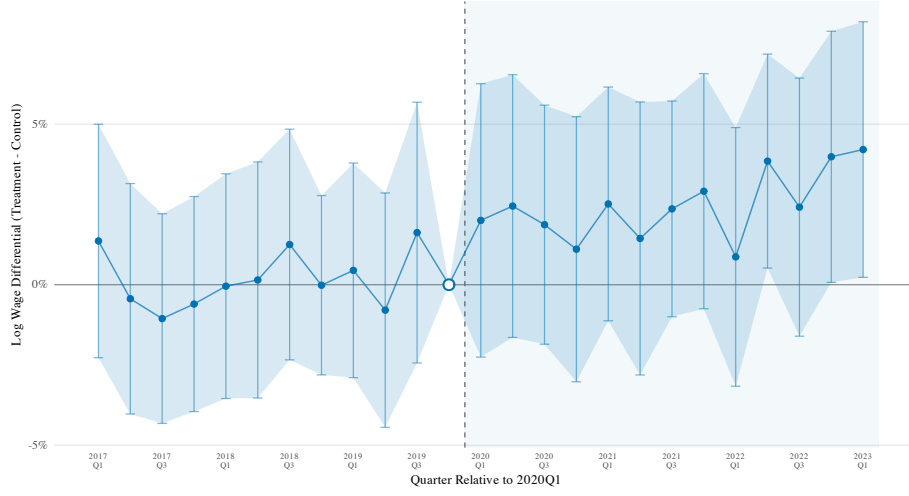
Notes: The data are from CPS-ORG, 2017Q1–2023Q1. This figure shows coefficients from equation (2) with 95 percent confidence intervals. Reference period: 2019Q4. Blue shading indicates post-treatment. All specifications include occupation, year-quarter, education, race, industry, and state FE. Standard errors clustered at the occupation level.

Figure 6 overlays the baseline event study with the saturated specification (adding industry × year-quarter FE). The pre-trends remain flat in both. The post-treatment coefficients are attenuated in the saturated specification, consistent with some of the effect operating through industry-level channels, but the broad pattern of gradual divergence is preserved. The joint F-test for pre-trends in the saturated specification yields $p = 0.703$.

Figure 6: Event Study: Baseline vs. Saturated Specification
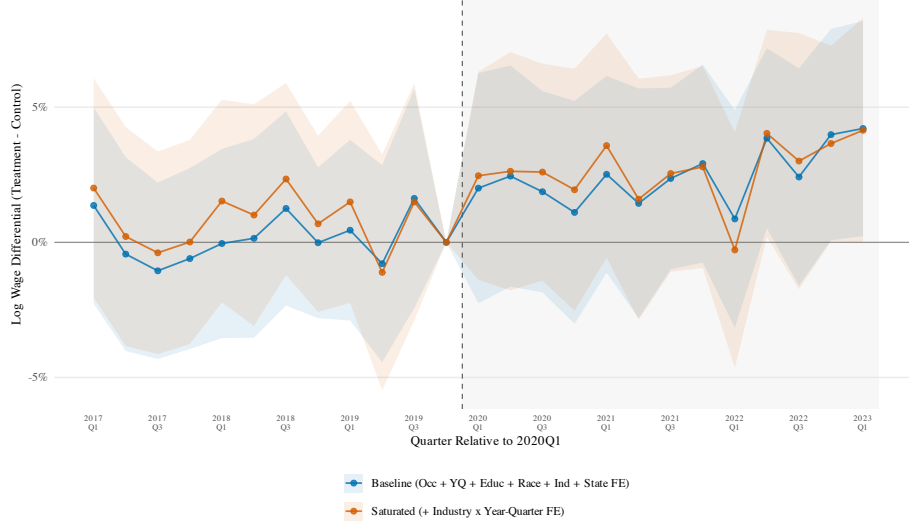
Notes: The data are from CPS-ORG, 2017Q1–2023Q1. Both specifications include occupation, year-quarter, education, race, and state FE. In blue is the baseline specification. In orange is the saturated specification, which additionally includes industry × year-quarter FE. Standard errors clustered at the occupation level.

6.4 Continuous Treatment and Triple Difference

Table 4 reports results from the continuous treatment specification and the triple-difference. The continuous treatment DID within the high-abstract sample yields $\hat{\beta} = 0.025$ ($p < 0.05$): a one-unit increase in telework feasibility (from fully non-teleworkable to fully teleworkable) leads to a 2.5 percent wage premium post-pandemic. This provides a dose-response interpretation consistent with the binary specification. The triple-difference, estimated on the full sample (all occupations), yields 0.020 on the Abstract × Teleworkable × Post interaction, confirming the telework premium is specific to abstract-task intensive occupations.

To assess whether the dose-response relationship is approximately linear, I split the Dingel-Neiman feasibility score into terciles within the high-abstract sample and interact each tercile indicator with the post-treatment dummy. Relative to the lowest-feasibility tercile, the middle tercile shows a coefficient of 0.025 (SE = 0.008) and the highest tercile shows 0.022 (SE = 0.010). Both are positive and statistically significant, confirming a dose-response relationship, though the coefficients are similar in magnitude. This pattern is consistent with a threshold effect: once telework feasibility crosses a moderate level, additional feasibility does not proportionally increase the wage premium, suggesting the mechanism operates through access to telework rather than intensity of telework.

Figure 7 presents the continuous treatment event study, showing the same pattern of flat pre-trends and gradual post-treatment divergence.

Table 4: Continuous Treatment and Triple-Difference Estimates

	(1) Continuous DID (High-abstract sample)	(2) Triple-Difference (Full sample)	(3) Binned Feasibility (High-abstract sample)
TW Feasibility × Post	0.025**		
	(0.010)		
Abstract × TW Feas. × Post		0.020**	
		(0.009)	
Mid Tercile × Post			0.025***
			(0.008)
High Tercile × Post			0.022**
			(0.010)
Num. Obs.	94,398	279,456	94,398
R^2			0.541
FE		Occ, YQ, Educ, Race, Ind, St	

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Notes: All regressions use CPS-ORG data, 2017Q1–2023Q1. Standard errors clustered at the occupation level (185 clusters) in parentheses. TW Feasibility is the continuous Dingel-Neiman score $\in [0, 1]$. Column (3) splits the feasibility score into terciles within the high-abstract sample; the omitted category is the lowest-feasibility tercile. All specifications include individual demographic controls.

Figure 7: Continuous Treatment Event Study
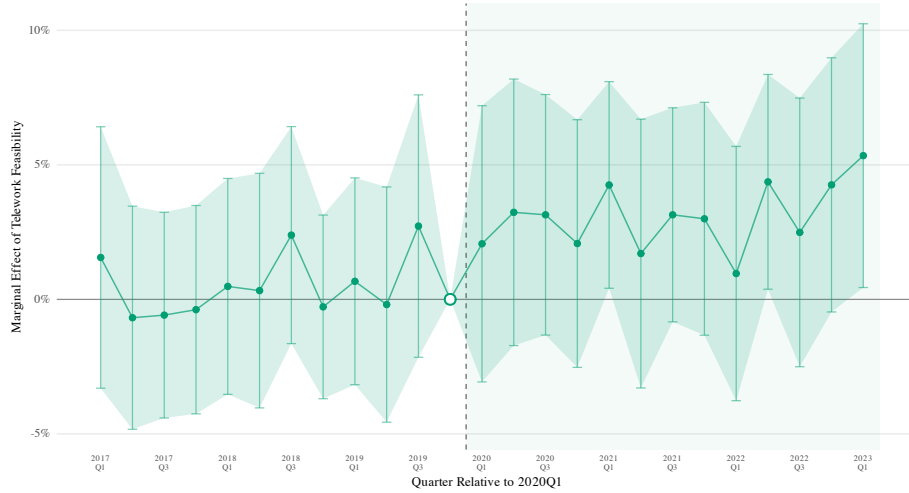
Notes: The data are from CPS-ORG, 2017Q1–2023Q1. This figure shows the marginal effect of a one-unit change in telework feasibility at each quarter. Reference period: 2019Q4. Green shading indicates post-treatment.

6.5   Heterogeneity

Table 5 and Figure 8 present split-sample estimates across four dimensions. The most striking result is geographic: in metropolitan areas, the treatment effect is 2.7 percent, statistically significant ($p < 0.01$); in non-metropolitan areas, the point estimate is indistinguishable from zero ($p = 0.99$). This is consistent with the monopsony-reduction mechanism (H2): telework expands effective labor markets most where employer concentration constrains wages. Figure 12 in the Appendix presents separate event studies for metropolitan and non-metropolitan subsamples, confirming that the divergence concentrates in urban labor markets while non-metropolitan areas show flat treatment-control dynamics throughout. The metro subsample shows flat pre-trends followed by clear post-2020 divergence, while the non-metro subsample shows no discernible pattern in either period, with wide confidence intervals reflecting the smaller sample.

The effect is somewhat larger for women (2.4 percent, $p < 0.05$) than for men (2.0 percent, $p = 0.17$), consistent with women placing higher value on telework flexibility (Mas and Pallais, 2017; Wiswall and Zafar, 2018). Workers with a bachelor's degree or higher show a coefficient of 2.8 percent compared to 2.1 percent for those without, suggesting the premium operates across the education distribution within high-abstract occupations. Union members exhibit the largest effect at 4.2 percent ($p < 0.01$, $N = 11,754$), compared to 1.6 percent for non-union workers ($p < 0.10$, $N = 81,104$), potentially reflecting union bargaining

incorporating telework-related productivity gains into wage contracts more effectively than individual bargaining. The union subsample is substantially smaller, limiting statistical power for the equality test. Formal Wald tests of coefficient equality show that none of the four subgroup differences reach conventional significance levels: metro/non-metro ($z = 1.50$, $p = 0.133$), gender ($z = -0.25$, $p = 0.806$), education ($z = 0.43$, $p = 0.668$), and union ($z = 1.59$, $p = 0.113$). The metro and union splits approach marginal significance, consistent with economically meaningful heterogeneity that the split samples lack power to detect. Splitting by age reveals similar effects across career stages: the treatment effect is 1.8 percent for workers under 40 ($p < 0.10$) and 2.3 percent for workers 40 and older ($p < 0.05$), with no significant difference ($z = -0.32$, $p = 0.750$). The absence of age heterogeneity suggests the premium is broadly based across career stages rather than concentrated among workers with particular demographic profiles.

Because the Dingel-Neiman classification captures telework feasibility rather than actual adoption, the baseline estimate is an intent-to-treat (ITT) effect. I emphasize that the ITT is the policy-relevant parameter: it captures the wage consequences of being in an occupation where telework became feasible, regardless of individual adoption. A Wald rescaling to a LATE is not informative here: the first-stage gap is modest (13.0 percentage points), 9.7 percent of control occupations also report teleworking (substantial non-compliance), and the thin first stage produces a mechanically inflated ratio.

Table 5: Heterogeneity in the Telework-Wage Effect

	Gender		Geography		Education		Union Status		Age	
	Male	Female	Metro	Non-Metro	BA+	No BA	Union	Non-Union	<40	≥40
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treated × Post	0.020	0.024**	0.027***	0.000	0.028**	0.021*	0.042***	0.016*	0.018*	0.023**
	(0.014)	(0.011)	(0.010)	(0.015)	(0.012)	(0.011)	(0.014)	(0.009)	(0.009)	(0.011)
Num. Obs.	37,806	56,589	75,487	17,878	36,105	58,287	11,754	81,104	48,985	45,413
R^2	0.501	0.576	0.543	0.564	0.462	0.503	0.574	0.530	0.557	0.518

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$

Notes: All regressions use CPS-ORG data, 2017Q1–2023Q1. Each column reports the coefficient on Treated × Post from a separate regression on the indicated subsample. All specifications include occupation, year-quarter, education (where applicable), race, industry, and state fixed effects plus individual demographic controls. Standard errors clustered at the occupation level in parentheses.

Figure 8: Heterogeneity in the Telework-Wage Effect
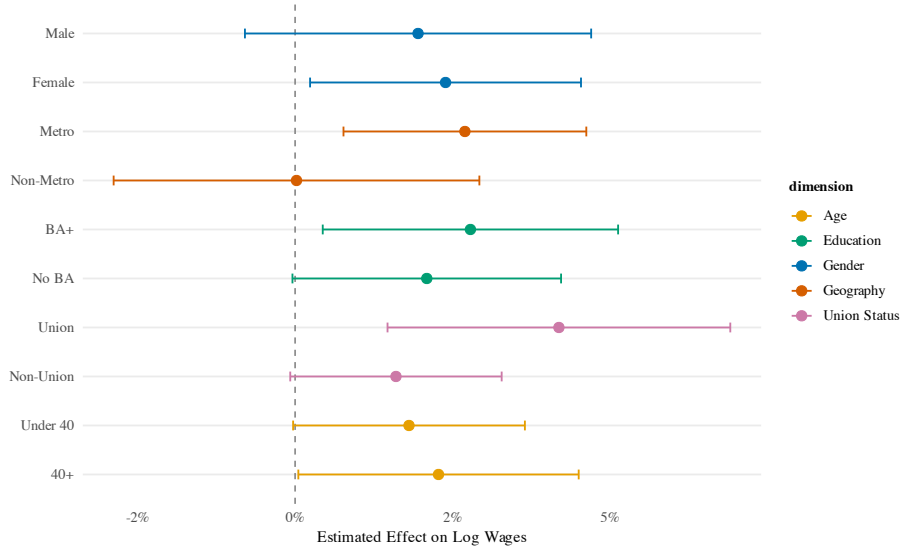
Notes: The data are from CPS-ORG, 2017Q1–2023Q1. Each point is from a separate regression on the indicated subgroup, with 95 percent confidence intervals. All specifications include occupation, year-quarter, education, race, industry, and state FE.

7 Robustness and Sensitivity

Table 6 and Figure 9 summarize results from the robustness specifications. A placebo test applying the DID to pre-pandemic data (2017–2019) with a fake treatment at 2018Q1 yields 0.004 ($p = 0.62$), confirming the treatment effect is not an artifact of pre-existing differential trends. Dropping the 2020Q2–Q4 quarters (the acute lockdown period) yields 0.024 ($p < 0.01$), suggesting the result is not driven by compositional disruptions during the pandemic peak. Using the top quartile (rather than top tercile) of abstract-task intensity yields 0.018 ($p < 0.10$), while the top 40 percent yields 0.025 ($p < 0.01$). Double-clustering standard errors by occupation and state yields virtually identical significance. Unweighted regression yields 0.016 ($p < 0.05$).

A concern with the control group is that healthcare practitioners (SOC major group 29), including surgeons, dentists, and veterinarians, may have wages determined by institutional or regulatory factors rather than competitive labor market forces. Healthcare occupations also experienced distinctive pandemic demand shocks: elective procedure bans suppressed revenue and potentially wages in early 2020, followed by a strong rebound in 2021–2022 driven by pent-up demand. These shocks do not map cleanly onto industry-by-time fixed effects because healthcare wage determination reflects fee schedules, insurance reimbursement rates, and

facility-specific revenue dynamics that vary within industry codes. If the healthcare rebound artificially depressed control-group wage growth relative to counterfactual, the treatment effect would be overstated. Excluding all SOC major group 29 occupations from the control group yields a coefficient of 0.019 ($p < 0.10$, $N = 82,373$). The attenuation from 0.023 to 0.019 likely reflects reduced statistical power rather than a substantive change in the estimate: excluding SOC major group 29 removes approximately 12,000 observations from the control group, reducing the effective cluster count. The point estimate remains within the 1.8–2.3 percent range of the main specifications, confirming that the result is not driven by the healthcare wage structure.

Restricting the sample to prime-age workers (25–54) addresses concerns about differential retirement or labor force entry patterns during the pandemic. The preferred specification on this subsample yields 0.018 ($p < 0.05$, $N = 66,521$), within the range of the main estimates.

Perhaps the most important robustness check concerns the tercile cutoff itself. The two largest treatment occupations by sample size—customer service representatives and personal care aides—have abstract-task scores of 0.12 and 0.16, respectively, well below the treatment-group mean. If the result were driven by these borderline occupations (whose telework feasibility may proxy for something other than abstract-task content), the estimate would weaken substantially when they are dropped. Restricting the sample to occupations above the median abstract-task score within the top tercile eliminates all such borderline cases and yields 0.028 ($p < 0.05$, $N = 46,972$). This estimate is squarely within the 1.8–2.3 percent range of the main specifications and, if anything, slightly larger in magnitude, confirming that borderline occupations attenuate rather than inflate the baseline estimate.

Table 6: Robustness Checks

	Preferred (1)	Placebo (2)	No 2020 (3)	Top 25% (4)	Top 40% (5)	Dbl. Clust. (6)	Unwgt. (7)	Ind×YQ (8)	No HC (9)	Prime (10)	High Abs. (11)
Treated × Post	0.023**		0.024***	0.018*	0.025***	0.023***	0.016**	0.018**	0.019*	0.018**	0.028**
	(0.009)		(0.009)	(0.010)	(0.008)	(0.008)	(0.008)	(0.009)	(0.010)	(0.009)	(0.013)
Placebo × Post		0.004									
		(0.008)									
Num. Obs.	94,398	49,245	85,269	70,136	112,561	94,398	94,398	94,398	82,373	66,521	46,972
R^2	0.541	0.554	0.542	0.517	0.530	0.541	0.545	0.544	0.484	0.518	0.533

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$

Notes: All regressions use CPS-ORG data. Standard errors clustered at the occupation level in parentheses. Column (1) reproduces the preferred specification from Table 3. Column (2) uses pre-pandemic data only (2017–2019) with a placebo treatment at 2018Q1. Column (3) excludes 2020Q2–Q4. Columns (4)–(5) use alternative abstract-task thresholds. Column (6) double-clusters by occupation and state. Column (7) is unweighted. Column (8) includes industry × year-quarter FE. Column (9) excludes healthcare practitioners (SOC major group 29) from the control group. Column (10) restricts to prime-age workers (25–54). Column (11) restricts to occupations above the median abstract-task score within the top tercile.

Figure 9: Robustness of the Baseline DID Estimate
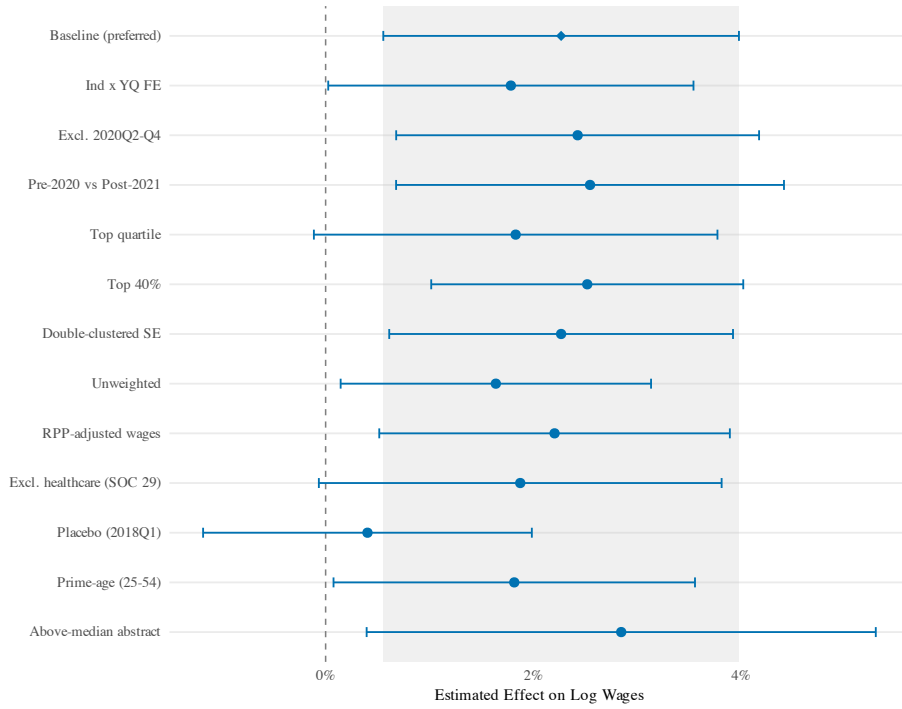
Notes: The data are from CPS-ORG, 2017Q1–2023Q1. Each point shows the coefficient on Treated × Post from a different specification, with 95 percent confidence intervals. Shaded band shows the 95 percent CI of the preferred specification. The placebo test uses pre-pandemic data with a fake treatment at 2018Q1.

Figure 10 plots kernel densities of log wages for treatment and control groups before and after the pandemic. The post-period density for the treatment group shifts rightward relative to the pre-period, while the control group shows a smaller shift, consistent with the DID estimate capturing a genuine shift in the conditional wage distribution rather than an artifact of outliers or distributional shape changes.

Figure 10: Log Wage Distribution: Treatment vs. Control, Pre vs. Post
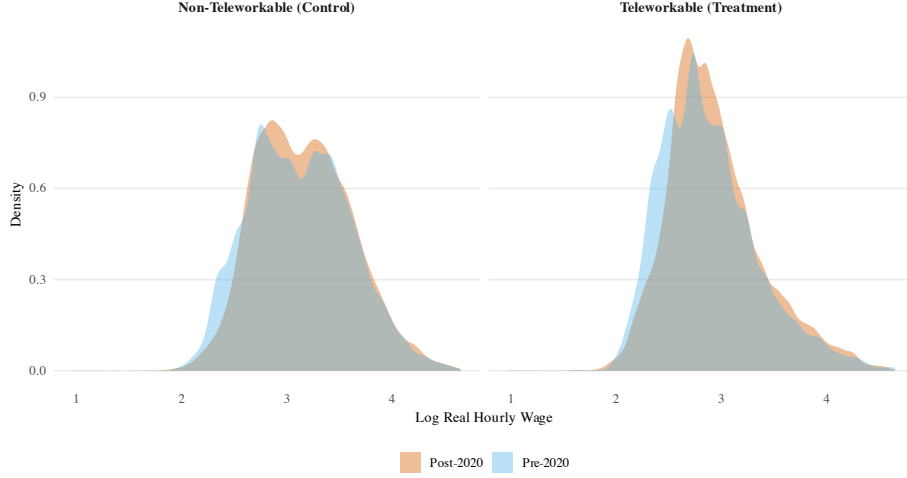
Notes: The data are from CPS-ORG, 2017Q1–2023Q1. This figure shows kernel density estimates, weighted by CPS earnings weight. On the left is the teleworkable (treatment) group. On the right is the non-teleworkable (control) group.

I implement the Rambachan and Roth (2023) sensitivity analysis (Honest DID) to assess robustness to violations of the parallel trends assumption. The relative magnitudes restriction assumes the maximum post-treatment violation is at most $\bar{M}$ times the maximum pre-treatment violation. At $\bar{M} = 0$ (standard parallel trends), the 95 percent CI for the first post-treatment period is $[-0.021, 0.061]$, and the average post-treatment effect interval is $[-0.004, 0.053]$. Under the relative magnitudes approach, the results remain informative for moderate values of $\bar{M}$, excluding zero up to approximately $\bar{M} = 1.5$—that is, the conclusion survives even if post-treatment trend violations are 50 percent larger than the maximum observed pre-treatment deviation.

The smoothness restriction (Δ^{SD}), which bounds the second derivative of the violation path, yields a CI of $[-0.027, 0.034]$ at $M = 0$ (linear extrapolation of pre-trends), which includes zero. This divergence between the two approaches warrants careful discussion. The Δ^{SD} approach extrapolates a linear trend from the pre-period forward; when pre-treatment coefficients are very close to zero (as they are here—the joint F-test yields $p = 0.886$), even tiny deviations from exact linearity generate a non-trivial extrapolated violation that quickly widens confidence intervals for moderate-sized treatment effects. Rambachan and Roth (2023) note that the two restrictions answer different questions: Δ^{RM} asks whether post-treatment violations of the kind observed pre-treatment could explain the result, while Δ^{SD} asks whether a smooth extrapolation of pre-trends could do so. For this application, the relative magnitudes approach may be more informative because it directly benchmarks

against observed pre-period behavior. However, the fact that the effect does not survive the smoothness restriction is a genuine limitation. The result should be interpreted as robust to moderate violations of parallel trends under the relative magnitudes framework, but fragile under the smoothness framework—a distinction that readers should weigh based on which assumption they find more plausible for this setting (Figure 11).

Figure 11: Honest DID Sensitivity Analysis (Rambachan and Roth, 2023)

(a) Relative Magnitudes

(b) Smoothness Restriction
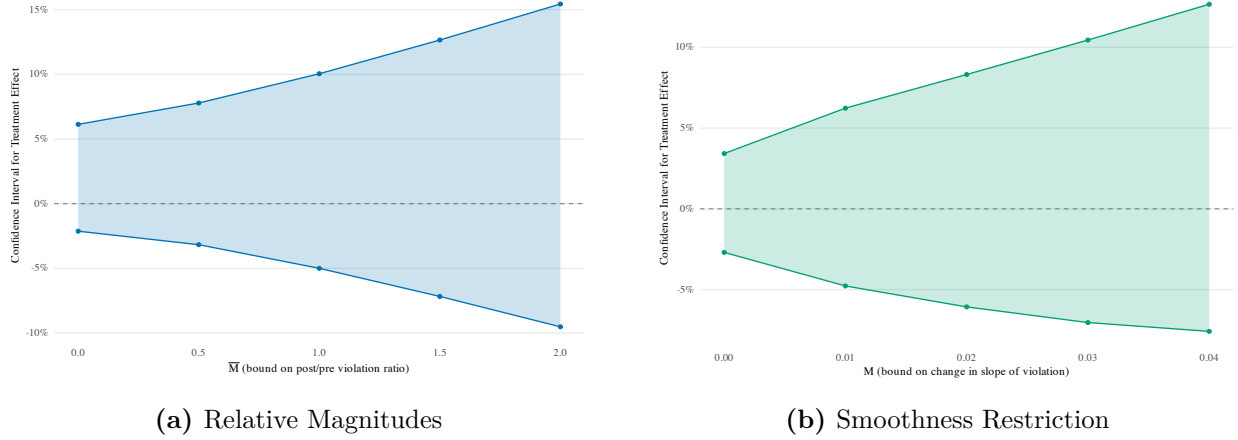
Notes: The data are from CPS-ORG, 2017Q1–2023Q1. On the left, robust CIs under $\Delta^{RM}(\bar{M})$, bounding the max post-treatment violation relative to max pre-treatment violation. On the right, robust CIs under $\Delta^{SD}(M)$, bounding the change in slope of the violation path. Both panels show 95 percent CIs for the first post-treatment period.

8   Conclusion

Teleworkable abstract-task occupations experienced a 2.3 percent relative wage premium following the pandemic-induced telework expansion, translating to roughly \$0.41–\$0.52 per hour, or \$855–\$1,090 per year for a worker earning the sample mean of approximately \$23 per hour. This estimate is stable across seven progressively saturated specifications (1.8–2.3 percent), supported by flat pre-trends, a null placebo test, and consistent continuous-treatment and triple-difference results. The positive sign indicates that channels increasing wages (productivity gains and monopsony reduction) dominate channels decreasing wages (supply expansion and compensating differentials).

Several limitations warrant discussion. The sample window ends at 2023Q1, and longer-run effects may differ as return-to-office policies and competitive dynamics evolve. The Dingel-Neiman classification captures feasibility, not actual adoption; the treatment is intent-to-treat and may understate the effect of actual telework. SUTVA violations are possible through

general equilibrium effects, and as discussed in Section 4, the direction of any resulting bias is ambiguous. The Honest DID analysis reveals that under the smoothness restriction the CI includes zero, though the more economically motivated relative magnitudes approach confirms robustness for plausible violation magnitudes. This underscores that moderate-sized effects in DID designs require careful attention to parallel trends credibility and to the choice of sensitivity framework.

The heterogeneity results help distinguish between competing mechanisms. The metro/non-metro split strongly supports the monopsony-reduction channel: telework reduces effective labor market concentration in dense urban areas where commuting constraints previously limited workers' outside options. The union premium (4.2 versus 1.6 percent) suggests institutions matter for translating telework-related productivity gains into wages.

These findings interact in an informative way with the concurrent literature on post-pandemic wage compression. Autor et al. (2023) document that the post-2020 labor market compressed the overall wage distribution, with workers at the 10th percentile experiencing faster real wage growth than those at the 90th percentile—a reversal of four decades of widening inequality. Carroll and Walker (2025) confirm this compression pattern using CPS data over a similar window. My results are not contradictory but complementary: the post-COVID labor market simultaneously compressed wages *across* the skill distribution (through tightening of the low-wage labor market and enhanced outside options for non-teleworkable service workers) while creating new inequalities *within* skill groups at the top (through telework-mediated monopsony reduction for abstract-task workers). The net effect on upper-tail inequality depends on which force dominates. The 1.8–2.3 percent within-abstract-task premium I estimate is modest relative to the 7–8 percent compression at the 10th-vs-90th percentile documented by Autor et al., suggesting that the compression force dominated overall but that telework access opened a new margin of within-group inequality that existing frameworks—which treat all abstract-task workers symmetrically—do not capture.

These findings have direct implications for return-to-office (RTO) policies. If telework access raises wages through reduced monopsony power, as the metropolitan concentration of effects suggests, mandating physical presence could restore employer wage-setting power and compress abstract-task wages. Recent high-profile mandates, including Amazon's five-day return-to-office policy announced in September 2024, underscore the relevance of these findings: if the 1.8–2.3 percent premium reflects reduced monopsony power, full RTO mandates may partially reverse this wage gain. Firms designing hybrid work policies should recognize that the 1.8–2.3 percent premium represents the market's partial pricing of remote work flexibility, a benchmark for calibrating the productivity-flexibility tradeoff. The null effect in non-metropolitan areas suggests the telework revolution has not yet equalized labor market

opportunities across space, at least within the abstract-task segment.

Future work should extend the analysis as longer CPS-ORG data become available, enabling assessment of whether the premium persists or dissipates as remote work norms solidify, and investigate firm-level mechanisms using matched employer-employee data to disentangle the productivity, monopsony-reduction, and compensating differential channels.

Data Availability Statement

The Current Population Survey Outgoing Rotation Group (CPS-ORG) microdata are available through IPUMS CPS (https://cps.ipums.org). O*NET task content data are available from the National Center for O*NET Development (https://www.onetcenter.org). The Dingel-Neiman telework feasibility classification is available at https://github.com/jdingel/DingelNeiman-workathome. Bureau of Economic Analysis Regional Price Parities are available at https://www.bea.gov. Replication code is available from the author upon request.

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A Additional Tables

Table 7: Occupations in the Estimation Sample

Occupation	SOC	N	Group	Abstract	TW Score
Customer Service Representatives	43-4051	7,141	Treatment	0.12	0.61
Personal Care Aides	39-9021	4,446	Treatment	0.16	0.75
Teacher Assistants	25-9041	3,325	Treatment	0.85	0.75
Managers, All Other	11-9199	3,323	Treatment	1.05	0.86
First-Line Supervisors of Office and Adm	43-1011	2,512	Treatment	0.49	1.00
Accountants and Auditors	13-2011	1,827	Treatment	0.81	1.00
Other Teachers and Instructors	25-3000	1,708	Treatment	0.83	1.00
Preschool and Kindergarten Teachers	25-2010	1,670	Treatment	0.72	0.87
Elementary and Middle School Teachers	25-2020	1,437	Treatment	0.72	0.87
Other Office/Admin Support Workers	43-9199	1,410	Treatment	0.86	1.00
Designers	27-1020	1,260	Treatment	0.23	0.82
Human Resources Workers	13-1070	1,048	Treatment	0.22	0.87
Financial Managers	11-3031	1,035	Treatment	0.99	1.00
First-Line Supervisors of Non-Retail Sal	41-1012	959	Treatment	0.26	1.00
Residential Advisors	39-9030	857	Treatment	0.16	0.75
Medical and Health Services Managers	11-9111	828	Treatment	1.38	1.00
General and Operations Managers	11-1021	774	Treatment	1.02	1.00
Postsecondary Teachers	25-1000	652	Treatment	1.42	0.96
Marketing and Sales Managers	11-2020	616	Treatment	0.93	1.00
Tax Preparers/Financial Specialists	13-2070	615	Treatment	0.60	0.75
Business Operations Specialists, All Oth	13-1199	584	Treatment	0.61	0.85
Property, Real Estate, and Community Ass	11-9141	566	Treatment	0.24	1.00
Supervisors, Transportation Workers	53-1000	562	Treatment	0.16	0.79
Interviewers, Except Eligibility and Loa	43-4111	550	Treatment	0.80	1.00
Claims Adjusters and Examiners	13-1030	541	Treatment	0.22	0.87
Management Analysts	13-1111	539	Treatment	1.92	1.00
Civil Engineers	17-2051	501	Treatment	0.73	1.00

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Table 7 continued

Occupation	SOC	N	Group	Abstract	TW Score
Social and Community Service Managers	11-9151	497	Treatment	0.63	1.00
Sales Representatives, Services, All Oth	41-3099	475	Treatment	0.32	0.50
Transportation, Storage, and Distributio	11-3071	473	Treatment	0.85	0.71
Purchasing Agents	13-1023	407	Treatment	0.96	1.00
Market Research Analysts and Marketing S	13-1161	319	Treatment	1.88	1.00
Librarians	25-4021	314	Treatment	0.39	0.80
Secondary School Teachers	25-2030	313	Treatment	0.72	0.87
Personal Financial Advisors	13-2052	304	Treatment	1.57	1.00
Computer and Information Systems Manager	11-3021	301	Treatment	0.80	1.00
Order Clerks	43-4151	293	Treatment	0.17	1.00
Special Education Teachers	25-2050	252	Treatment	0.72	0.87
Other Healthcare Practitioners	29-9000	244	Treatment	0.64	0.67
Other Personal Care Workers	39-9099	240	Treatment	0.16	0.75
Administrative Services Managers	11-3011	237	Treatment	0.79	0.82
Training and Development Specialists	13-1151	234	Treatment	1.20	1.00
Lawyers	23-1011	234	Treatment	1.33	1.00
Purchasing Managers	11-3061	231	Treatment	0.99	1.00
Electrical/Electronics Engineers	17-2070	219	Treatment	1.08	0.78
Logisticians	13-1081	216	Treatment	1.03	1.00
Broadcast/Sound Engineers	27-4010	215	Treatment	0.48	0.83
Speech-Language Pathologists	29-1127	202	Treatment	1.46	1.00
Physical Scientists, All Other	19-2099	200	Treatment	1.38	1.00
Human Resources Managers	11-3121	193	Treatment	1.52	1.00
Other Media and Comm. Workers	27-3090	190	Treatment	0.51	0.56
Probation Officers and Correctional Trea	21-1092	183	Treatment	0.29	1.00
Cost Estimators	13-1051	165	Treatment	1.00	1.00
Financial Analysts	13-2051	163	Treatment	0.60	0.75
Biological Scientists	19-1020	163	Treatment	1.31	0.55
Private Detectives and Investigators	33-9021	161	Treatment	0.60	1.00
Chief Executives	11-1011	156	Treatment	1.96	1.00
Editors	27-3041	152	Treatment	0.59	1.00
Misc. Mathematical Scientists	15-2090	149	Treatment	1.23	1.00
Operations Research Analysts	15-2031	148	Treatment	1.39	1.00

Table 7 continued

Occupation	SOC	N	Group	Abstract	TW Score
Photographers	27-4021	147	Treatment	0.45	1.00
Telemarketers	41-9041	141	Treatment	0.54	1.00
Securities, Commodities, and Financial S	41-3031	137	Treatment	0.68	1.00
News Analysts and Reporters	27-3020	129	Treatment	0.51	0.56
Artists and Related Workers	27-1010	126	Treatment	0.23	0.82
Architects	17-1010	125	Treatment	0.96	0.85
Environmental Scientists	19-2040	120	Treatment	1.17	0.78
Public Relations Specialists	27-3031	118	Treatment	1.41	1.00
Insurance Underwriters	13-2053	116	Treatment	0.73	1.00
Human Resources Assistants, Except Payro	43-4161	114	Treatment	0.31	1.00
Aerospace Engineers	17-2011	103	Treatment	1.37	1.00
Medical Scientists	19-1040	102	Treatment	1.31	0.55
Chemists and Materials Scientists	19-2030	99	Treatment	1.17	0.78
Surveyors and Cartographers	17-1020	97	Treatment	0.96	0.85
Compensation, Benefits, and Job Analysis	13-1141	94	Treatment	0.52	1.00
Misc. Social Scientists	19-3090	93	Treatment	1.57	0.85
Librarians	25-4010	87	Treatment	0.39	0.80
Misc. Media/Comm. Equipment Workers	27-4030	86	Treatment	0.48	0.83
Other Religious Workers	21-2099	78	Treatment	0.40	1.00
Psychologists	19-3030	69	Treatment	1.57	0.85
Financial Specialists, All Other	13-2099	68	Treatment	1.09	1.00
Directors, Religious Activities and Educ	21-2021	65	Treatment	0.76	1.00
Appraisers and Assessors of Real Estate	13-2021	63	Treatment	0.60	0.75
Conservation Scientists/Foresters	19-1030	63	Treatment	1.31	0.55
Cargo and Freight Agents	43-5011	63	Treatment	0.23	1.00
Technical Writers	27-3042	59	Treatment	0.75	1.00
Statistical Assistants	43-9111	59	Treatment	0.86	1.00
Announcers	27-3010	55	Treatment	0.51	0.56
Credit Analysts	13-2041	54	Treatment	0.56	1.00
Training and Development Managers	11-3131	53	Treatment	1.55	1.00
Architectural and Engineering Managers	11-9041	46	Treatment	1.01	0.56
Public Relations and Fundraising Manager	11-2031	44	Treatment	0.93	1.00

Table 7 continued

Occupation	SOC	N	Group	Abstract	TW Score
Recreational Therapists	29-1125	40	Treatment	0.57	0.75
Environmental Engineers	17-2081	39	Treatment	0.96	1.00
Computer Hardware Engineers	17-2061	36	Treatment	1.04	1.00
Agricultural and Food Scientists	19-1010	34	Treatment	1.31	0.55
Advertising and Promotions Managers	11-2011	25	Treatment	0.33	1.00
Judicial Law Clerks	23-1012	22	Treatment	0.60	1.00
Buyers and Purchasing Agents	13-1021	21	Treatment	0.38	1.00
Petroleum Engineers	17-2171	21	Treatment	1.36	1.00
Biomedical Engineers	17-2031	19	Treatment	1.17	1.00
Atmospheric and Space Scientists	19-2021	18	Treatment	1.32	1.00
Financial Examiners	13-2061	15	Treatment	0.52	1.00
Economists	19-3011	15	Treatment	1.34	1.00
Natural Sciences Managers	11-9121	14	Treatment	0.88	1.00
Compensation and Benefits Managers	11-3111	13	Treatment	0.97	1.00
Sales Engineers	41-9031	13	Treatment	0.62	1.00
Marine Engineers and Naval Architects	17-2121	12	Treatment	0.59	0.52
Astronomers and Physicists	19-2010	7	Treatment	1.17	0.78
Actuaries	15-2011	5	Treatment	1.03	1.00
Registered Nurses	29-1141	9,223	Control	1.26	0.00
Construction Laborers	47-2061	3,885	Control	0.29	0.00
Electricians	47-2111	2,672	Control	0.76	0.00
First-Line Supervisors of Production and	51-1011	1,800	Control	0.16	0.00
Maintenance and Repair Workers, General	49-9071	1,780	Control	0.16	0.00
Medical Assistants	31-9092	1,759	Control	0.38	0.00
Social Workers	21-1020	1,587	Control	0.84	0.48
Police and Sheriff's Patrol Officers	33-3051	1,494	Control	0.61	0.00
Counselors	21-1010	1,435	Control	0.84	0.48
Tellers	43-3071	993	Control	0.18	0.00
Chefs and Head Cooks	35-1011	893	Control	0.86	0.00
Correctional Officers and Jailers	33-3010	825	Control	0.35	0.03
Construction Managers	11-9021	669	Control	0.61	0.00
Social and Human Service Assistants	21-1093	627	Control	0.12	0.00
Education Administrators	11-9030	591	Control	0.55	0.28
Physical Therapists	29-1123	542	Control	0.78	0.00

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Table 7 continued

Occupation	SOC	N	Group	Abstract	TW Score
Aircraft Mechanics and Service Tech-nicia	49-3011	530	Control	0.31	0.00
First-Line Supervisors of Mechanics, Ins	49-1011	484	Control	0.18	0.00
First-Line Supervisors of Housekeep-ing a	37-1011	477	Control	0.35	0.00
Hairdressers, Hairstylists, and Cos-metol	39-5012	472	Control	0.25	0.00
Engineers, All Other	17-2199	437	Control	0.78	0.47
Phlebotomists	31-9097	427	Control	0.19	0.00
Water and Wastewater Treatment Plant and	51-8031	424	Control	0.47	0.00
Compliance Officers	13-1041	370	Control	0.57	0.47
Highway Maintenance Workers	47-4051	358	Control	0.15	0.00
Loan Interviewers and Clerks	43-4131	340	Control	0.54	0.00
Respiratory Therapists	29-1126	334	Control	0.15	0.00
Physical Therapist Assistants	31-2020	328	Control	0.57	0.00
Occupational Therapists	29-1122	322	Control	0.83	0.00
Misc. Physicians	29-1129	298	Control	1.39	0.25
Reservation and Transportation Ticket Ag	43-4181	290	Control	0.18	0.00
Nurse Practitioners	29-1171	277	Control	1.26	0.00
Mechanical Engineers	17-2141	258	Control	0.86	0.27
First-Line Supervisors of Landscap-ing, L	37-1012	241	Control	0.14	0.00
Industrial Production Managers	11-3051	224	Control	0.57	0.00
Dietitians and Nutritionists	29-1031	211	Control	0.87	0.00
Physicians and Surgeons	29-1060	211	Control	0.76	0.00
Detectives and Criminal Investiga-tors	33-3021	211	Control	0.84	0.21
Eligibility Interviewers, Government Pro	43-4061	206	Control	0.14	0.00
Construction and Building Inspec-tors	47-4011	196	Control	0.37	0.00
First-Line Supervisors of Police and Det	33-1012	195	Control	0.83	0.00
Physician Assistants	29-1071	194	Control	0.86	0.00
Meeting, Convention, and Event Planners	13-1121	191	Control	0.84	0.00

Table 7 continued

Occupation	SOC	N	Group	Abstract	TW Score
Farmers, Ranchers, and Other Agricultura	11-9013	185	Control	0.53	0.00
Opticians, Dispensing	29-2081	170	Control	0.94	0.00
Industrial Engineers	17-2110	161	Control	0.94	0.34
Lodging Managers	11-9081	156	Control	0.49	0.00
Advertising Sales Agents	41-3011	148	Control	0.55	0.00
Tax Preparers	13-2082	141	Control	0.32	0.00
First-Line Supvrs, Protective Svc	33-1099	130	Control	0.62	0.00
Producers and Directors	27-2012	125	Control	0.37	0.35
Tax Examiners and Collectors, and Revenu	13-2081	123	Control	0.24	0.00
Occupational Therapy Assistants	31-2010	114	Control	0.57	0.00
First-Line Supervisors of Correctional O	33-1011	105	Control	0.41	0.00
First-Line Supervisors of Fire Fighting	33-1021	93	Control	0.61	0.00
Biological Technicians	19-4021	68	Control	0.27	0.00
Fundraisers	13-1131	60	Control	1.58	0.00
Fire Inspectors and Prevention	33-2020	60	Control	0.21	0.00
Animal Trainers	39-2011	54	Control	0.13	0.00
Nurse Anesthetists	29-1151	51	Control	1.19	0.00
Radiation Therapists	29-1124	50	Control	0.45	0.00
Urban and Regional Planners	19-3051	37	Control	1.68	0.00
Veterinarians	29-1131	37	Control	0.42	0.00
Chemical Engineers	17-2041	33	Control	1.19	0.00
Optometrists	29-1041	31	Control	1.32	0.00
Parking Enforcement Workers	33-3041	31	Control	0.16	0.00
Gaming Managers	11-9071	28	Control	0.27	0.00
Morticians, Undertakers, and Funeral Dir	39-4031	28	Control	0.38	0.00
Audiologists	29-1181	26	Control	1.25	0.00
Materials Engineers	17-2131	25	Control	1.12	0.00
Health Diagnosing and Treating Practitio	29-1199	21	Control	0.95	0.14
Emergency Management Directors	11-9161	17	Control	1.04	0.00
Dentists	29-1020	15	Control	0.76	0.00
Chiropractors	29-1011	12	Control	0.49	0.00
Electronic Equipment Installers and Repa	49-2096	11	Control	0.31	0.00

Notes: Occupations sorted by sample size within each group. Abstract is the standardized O*NET abstract-task index. TW Score is the Dingel-Neiman telework feasibility score. Entries labeled "SOC XX-XXXX" represent aggregated occupation categories in the IPUMS CPS coding that lack a direct match to detailed O*NET occupation titles.

B   Additional Figures

Figure 12: Event Study by Metropolitan Status
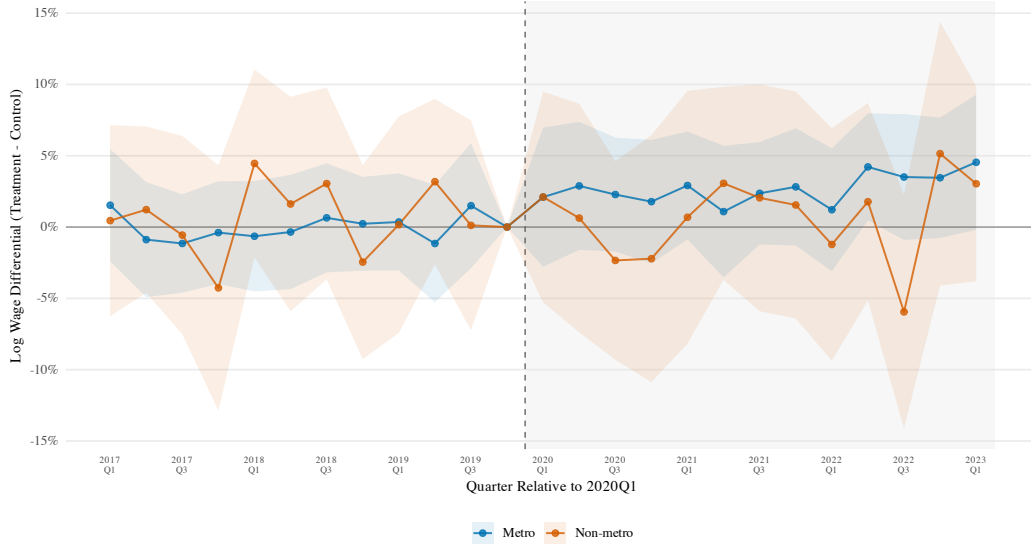
Notes: The data are from CPS-ORG, 2017Q1–2023Q1. This figure shows separate event-study estimates for metropolitan (blue) and non-metropolitan (orange) subsamples. Both specifications include occupation, year-quarter, education, race, industry, and state fixed effects. Standard errors clustered at the occupation level. Reference period: 2019Q4.