Remote Work and the Missing Recovery in Vehicle Sales: The Institutional Fleet Channel*

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Abstract

Remote work idled the vehicle stock organized around office-based work. Using county-level registration data paired with remote job postings and occupation-based shift-share instruments, we show that a ten percentage point rise in the remote work share lowers registrations by 3 to 8 percent. The effect operates entirely through corporate and government fleets, while household purchases show no response. Federal fleet records show that the effects of remote work progate beyond the acquisition channel, with fleets of unchanged size driven less, held longer, and replaced later. In all, remote work accounts for roughly 15 to 30 percent of the persistent decline in national vehicle sales.

Keywords: Remote Work, Durable Goods Consumption, Light Vehicle Sales, Fleet Vehicles, Spatial Spillovers.
JEL classification: D12, J22, R41.

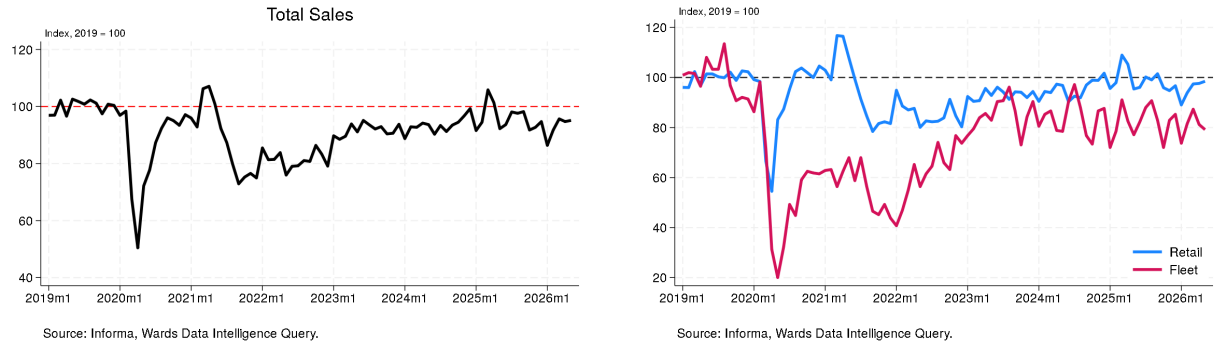
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1 Introduction

Despite a robust macroeconomic recovery from the COVID-19 pandemic, U.S. light vehicle sales have yet to return to pre-pandemic levels as of 2026. As shown in the right panel of Figure 1, light vehicle sales have remained about 8 percent below their pre-pandemic average since 2023, after the likely resolution of supply chain disruptions and despite the earlier support from stimulus payments (Dunbar et al., 2024; Berger et al., 2026). The shortfall is more pronounced for fleet sales, which account for approximately 20 percent of the market and remain about 15 percent below pre-pandemic levels through early 2026 (left panel). Retail vehicle sales are also still modestly subdued, approximately 5 percent below 2019 levels.

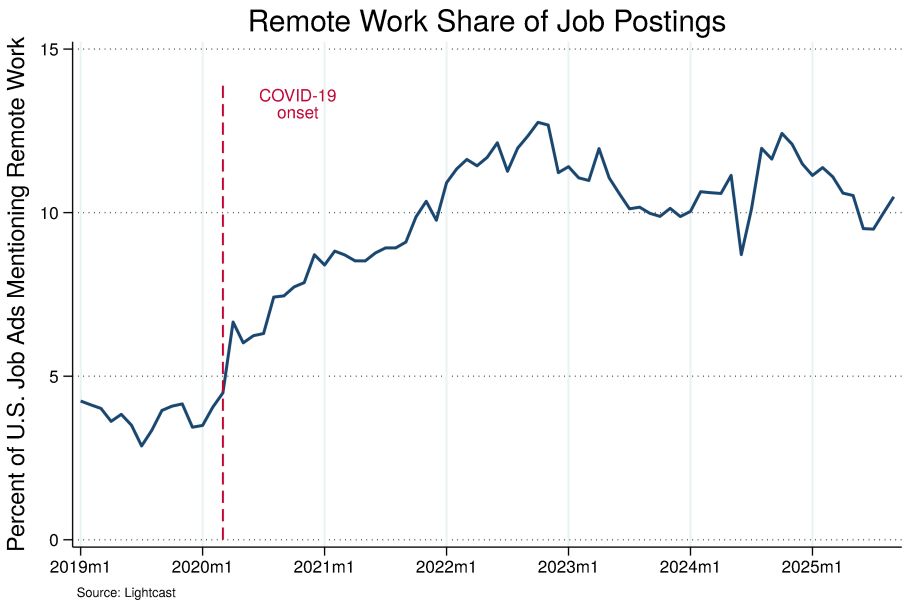
Figure 1: Light Vehicle Sales since 2019



In this paper, we show that part of this missing recovery is structural and we trace it to remote work. Between 2019 and 2021, the share of U.S. workers performing some of their duties from home more than tripled, and roughly a third of paid workdays are still performed from home (Barrero et al., 2023, 2021; Aksoy et al., 2025). Similarly, the share of remote work in job postings, shown in Figure 2 has risen sharply at the onset of the pandemic and stabilized well above its pre-pandemic level.

We test the effect of the shift to remote work on vehicle demand using the universe of new vehicle registrations across U.S. counties from 2019 through 2025, paired with county-level measures of remote work demand from online job postings. The empirical design is a long difference that compares county-level changes in registrations between 2019 and 2024.

Figure 2: Remote Work Share of Job Postings



Note: Monthly share of U.S. online job postings that mention remote work, from January 2019 through late 2025. The vertical line marks the onset of the COVID-19 pandemic. Source: Lightcast, following Hansen et al. (2023).

To address endogeneity, we instrument remote work demand with a Bartik shift-share measure that interacts pre-pandemic county occupational composition with occupation-level remote work ability from Dingel and Neiman (2020) and Montenovo et al. (2022).

We report three main findings. First, the rise in remote work significantly reduced vehicle demand. Counties experiencing a ten percentage point increase in their remote work share see registrations decline by 3 percent in the OLS specification and 6 to 8 percent in the IV specifications. With remote work postings 4.3 percentage points above their pre-pandemic level, these estimates imply that remote work accounts for about 15 to 30 percent of the persistent decline in vehicle sales—roughly 200,000 to 400,000 fewer annual purchases at 2019 sales levels. The IV estimates exceed OLS, and we argue that spatial spillovers contribute to this divergence. Neighboring counties' remote work adoption depresses local registrations with coefficients about twice the own-county effect, so the IV estimates capture general equilibrium adjustments in regional vehicle markets.

Second, the decline operates entirely through institutional fleets. Government fleet

registrations decline by about 20 percent per ten percentage point increase in remote work. Corporate registrations fall by 6-to-10 percent, while household retail purchases show no significant change. Because the raw data bundle rent-a-car fleets with corporate registrations, and rental fleets contracted sharply for reasons tied to travel and supply shocks rather than remote work, we isolate rental throughout and show that the corporate-fleet response survives its exclusion.

Third, remote work has affected not only registrations but also additional margins of adjustment of the vehicle stock. Between 2019 and 2024, federal fleet inventory rose 1 percent while average age increased 9 percent, miles per vehicle fell 12 percent, and disposals dropped 19 percent; acquisitions declined only 2 percent. Cross-sectional analysis using two independent telework measures shows agencies with higher remote work maintain smaller, less-utilized fleets, prolonging vehicle lifespans and reducing disposal rates.

This paper contributes to several literatures. A first strand documents the rise and persistence of remote work (Bloom et al., 2015; Mas and Pallais, 2017; Dingel and Neiman, 2020; Barrero et al., 2021, 2023; Aksoy et al., 2025). A second examines its consequences for real estate and urban form: Ramani and Bloom (2021) document the residential donut, Gupta et al. (2026) estimate large losses in office values, and Delventhal et al. (2023) model the spatial equilibrium response. We provide the first evidence that remote work reshaped durable goods demand. While a large literature studies the determinants of vehicle demand, including gasoline prices, credit conditions, and environmental regulations, almost no work has examined the impact of structural shift to remote work on vehicle markets. Our results show that the relevant margin is not household vehicle ownership per sé but the broader commercial and institutional infrastructure that supports office-based commuting. Government agencies and private firms reduced fleet registrations as the daily concentration of workers in offices diminished. This finding complements Gupta et al. (2026) on commercial real estate: remote work destroyed not only the value of office space but also the vehicle demand that serviced it. Furthermore, the federal fleet data suggested an even larger impact on utilization.

The remainder of the paper proceeds as follows. Section 2 describes the data and empir-

ical strategy, including the construction of the instrument. Section 3 presents the aggregate OLS and IV estimates, their national implications, and the role of spatial spillovers. Section 4.1 identifies the fleet channel, documents the within-metro reallocation of fleets, and traces the year-by-year adjustment. Section 4.3 uses the federal fleet to separate the stock and turnover margins. Section 5 concludes. Robustness checks and additional decompositions appear in the appendix.

2 Data and Empirical Strategy

This section outlines our data and empirical strategy. We construct a county-year panel (2019–2025) combining new vehicle registrations, remote work measures, and county-level control variables including demographic and economic characteristics. We then lay out a long-difference specification with state fixed effects and an occupation-based shift-share instrument that together identify the causal effect of remote work on vehicle demand.

2.1 Data Sources

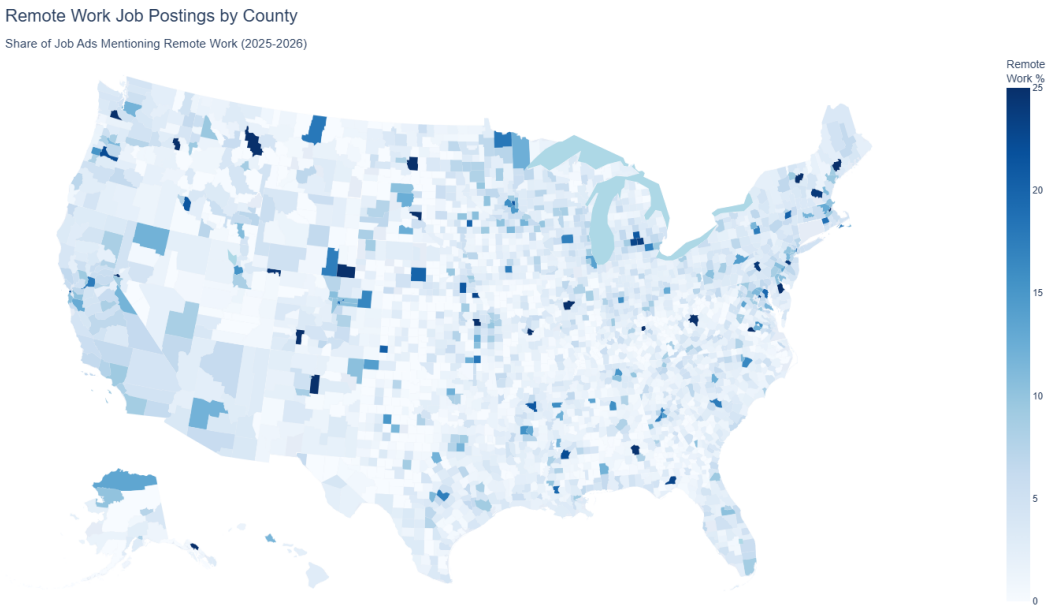
Our analysis combines several administrative and survey-based datasets at the county level for the period 2019–2025, covering all U.S. counties with sufficient data. We describe each data source in turn.

2.1.1 Main Analysis

Vehicle Registrations. Our primary outcome variable captures new vehicle registrations at the county level, obtained from R. L. Polk & Co. This dataset covers the universe of new vehicle registrations in the United States, drawn from state-level data from the Department of Motor Vehicles (DMV). For each registration, we observe the county and month of registration, along with vehicle characteristics such as make, model, and type of registration (e.g., retail, firm, commercial, government). We then aggregate these registrations to county-year totals, our primary unit of analysis. We analyze the period from January 2019 through December 2025, capturing the pre-pandemic baseline, pandemic disruptions, and post-pandemic recovery.

Remote Work. Our remote work measure is also defined at the county-year level, matching the structure of the vehicle registration data. We draw on Hansen et al. (2023), who identify whether online vacancy postings collected by Lightcast allow a new hire to work remotely.[1] This measure captures both employer demand for remote work and the occupational composition of the local labor market that makes remote work feasible. Importantly, Barrero et al. (2023) show that job posting shares track closely household survey measures of actual remote work, and the cross-sectional variation across counties aligns with the occupation-level remote ability indexes developed by Dingel and Neiman (2020) and Montenovo et al. (2022). Figure 3 illustrates the substantial cross-county variation in remote work postings, which our design exploits. Remote work opportunities are heavily concentrated in metropolitan counties with knowledge-intensive occupational structures, while remaining sparse across much of the rural areas.

Figure 3: Remote Work Job Postings by County



Note: County-level share of job postings that mention remote work. Darker shading denotes a higher remote work share. Source: Lightcast, following Hansen et al. (2023).

[1]The measure is available at `wfhmap.com`.

2.1.2 Instrumental Variable Strategy

Occupational Composition and Remote Ability. For the instrumental variable, we obtain county-level occupational composition from the American Community Survey (ACS) 2014–2018 five-year estimates via the U.S. Census Bureau's Public Use Microdata Sample (PUMS). We deliberately use these pre-period data to avoid endogeneity concerns. The PUMS version provides individual-level records with six-digit Standard Occupational Classification (SOC) codes and Public Use Microdata Area (PUMA) geography codes. We allocate workers to counties using a tract-based crosswalk that maps 2010 PUMAs to counties with 2010 population-weighted allocation factors.[2] This procedure yields employment counts by detailed SOC occupation for each county, covering 524 distinct occupations. For occupation-level remote work ability, we use the classifications constructed by Montenovo et al. (2022) and Dingel and Neiman (2020), who analyze Department of Labor's O*NET task requirements to determine which occupations can be performed remotely.[3] We match these scores to PUMS ACS occupations using a cascading merge at the six-digit or five-digit level, yielding an overall match rate of 82.3 percent.

2.1.3 Control Variables

Population. We obtain county-level annual population estimates from the U.S. Census Bureau's Population Estimates Program.[4] These estimates provide July 1 population counts for all counties, based on births, deaths, and net migration. Because population growth is a key determinant of vehicle demand, we control for changes in county population while weighting all regressions by 2010 population.

Income. We obtain county-level personal income from the Bureau of Economic Analysis (BEA) Regional Economic Accounts. The BEA produces annual estimates of total personal

[2]The crosswalk is constructed from the 2010 Census Tract-to-PUMA relationship file combined with 2010 ACS tract-level population data. For each PUMA, we compute the 2010 population share residing in each county and use these shares as allocation weights.

[3]Section 2.2.2 provides more details on these measures while discussing our IV specifications.

[4]We use the Vintage 2019 estimates (2010 census base) for 2018–2019 and the Vintage 2024 estimates (2020 census base) for 2020–2024, both from the Census Bureau's Population Estimates Program county-level datasets.

income and per capita personal income for all U.S. counties, compiled from a combination of administrative records including tax returns, unemployment insurance records, and Social Security data. Personal income includes wages and salaries, proprietors' income, dividends, interest, rents, and transfer payments. We use these data to construct changes in county income over our sample period, a key control for local economic conditions that may affect both vehicle demand and the presence of remote work arrangements.

Establishment, Employment, and Payroll Data. We obtain county-level measures of business activity from the Bureau of Labor Statistics' Quarterly Census of Employment and Wages (QCEW). The QCEW provides near-universal coverage of U.S. employment, drawing from state unemployment insurance tax records that cover approximately 95 percent of civilian jobs. We measure county-level business activity using the number of establishments, total employment, and average weekly payroll for each county and year.

Metropolitan Geography. For the within-metropolitan analysis we assign counties to Core-Based Statistical Areas using the Census Bureau's 2023 delineation crosswalk. The crosswalk defines metropolitan groupings only. All registration and remote-work variables are the county-level series described above.

2.1.4 Looking into the Mechanisms

Federal Fleet and Telework. We use the General Services Administration (GSA)'s Federal Fleet Report, the annual census of federal motor vehicles, to investigate dynamics in the federal fleet.[5] From this source, we observe year-end inventory, average vehicle age, miles driven per vehicle, and acquisition and disposal counts by vehicle segment for each federal agency. We match fleet data to agency telework using two sources: the Office of Personnel Management's Federal Employee Viewpoint Survey (FEVS), which reports respondent-weighted telework share (24 agencies, FY2021–2023), and the OPM's annual Status of Telework, which details agencies' telework shares in the Federal Government

[5]Fiscal year 2019 through 2024 editions are available at https://www.gsa.gov/policy-regulations/policy/motor-vehicle-management-policy/ffr-open-data-set-library.

Reports to Congress (37 agencies, FY2019–2024).[6]

Automotive Fleet Top 300 Fleet Survey. We use the Automotive Fleet Top 300 Commercial Fleets survey to measure firm-level commercial vehicle fleets. The survey is conducted annually and identifies the largest commercial fleets in the United States, with eligibility generally requiring a fleet of at least 1,000 on-road vehicles; public-sector fleets are excluded. The published data report fleet size and composition by vehicle type. We use the listings for 2019–2024 to construct a panel of vehicle holdings for large commercial fleets.

Our main analysis sample includes 3,014 U.S. counties with complete data for both vehicle registrations and remote work measures. This final sample covers approximately 99 percent of the U.S. population.

2.2 Empirical Strategy

We now turn to our empirical strategy for estimating the causal effect of remote work on vehicle demand. We first present the main OLS framework and then discuss our instrumental variable strategy, which addresses concerns with endogeneity and reverse causality.

2.2.1 OLS Framework

Our goal is to understand the response of vehicle demand to remote work. Several features of the pandemic period complicate the identification of this relationship. Stimulus payments, semiconductor shortages, and other supply chain disruptions, all affected vehicle demand during 2020–2022, and these shocks may have interacted with the shift to remote work. Moreover, vehicle registrations remain below pre-pandemic levels as of early 2026, suggesting possible ongoing adjustment dynamics.

To isolate the longer-term relationship between remote work and vehicle demand while filtering out these transitory factors, we employ a long-difference specification comparing

[6]Data availability constraints limit FEVS to 2021–2023. The OPM's annual Status of Telework measure is a self-reported administrative count, with some cross-agency variation in the definition of telework.

2019 to post-pandemic outcomes. Our baseline specification uses 2024 as the post-pandemic benchmark, identifying a five-year comparison window. We assess robustness using alternative post-pandemic periods, specifically 2023–2024, 2024–2025, or 2023–2025, to ensure our results are not driven by idiosyncratic shocks such as lingering semiconductor supply effects or tariff announcements that may have differentially affected specific years.

The dependent variable is constructed as the difference between the log of annual registrations in the post-pandemic period and the log of annual registrations in 2019. The baseline five-year horizon between 2019 and 2024 likely provides sufficient time for both remote work arrangements and vehicle purchasing patterns to stabilize while limiting the influence of transitory shocks. Our specification includes additional controls for changes in population and other county-level factors that may influence vehicle registrations and correlate with remote work adoption, such as changes in income and demographic composition.

We estimate the following long-difference specification:

$$\Delta \ln \text{Regs}_c = \beta_0 + \beta_1 \cdot \Delta\text{RemoteWork}_c + \beta_2 \cdot \Delta X_c + \gamma_s + \varepsilon_c \tag{1}$$

where c indexes counties, s indexes states, the dependent variable is the log change in vehicle registrations from 2019 to 2024, $\Delta\text{RemoteWork}_c$ is the change in remote work share in percentage points over the same period, ΔX_c captures changes in county-level characteristics that may affect vehicle demand, γ_s are state fixed effects, and ε_c is the error term.

State fixed effects absorb all state-level variation, including regional differences in pandemic severity and policy responses, state-level economic conditions and labor market trends, geographic variation in industrial composition, and state-specific vehicle market characteristics such as taxation and registration policies. Furthermore, the long-difference design also differences out all time-invariant county characteristics, such as geography, baseline infrastructure, and persistent cultural factors. With these controls, β_1 identifies the within-state relationship between remote work adoption and vehicle demand. We find that 88 percent of the variation in remote work adoption is within-state (83 percent when weighted by 2010 population), indicating that within-state comparisons capture the

relevant policy and economic margin.

2.2.2 Instrumental Variable Strategy

While our long-difference specification with state fixed effects and controls for local conditions addresses many potential confounders, concerns about endogeneity remain. Remote work demand likely reflects local economic conditions, industrial composition, and labor market characteristics that may independently affect vehicle demand. For example, rapidly growing counties may adopt more remote work and purchase more vehicles simultaneously, generating a spurious positive correlation. Conversely, reverse causality may arise if vehicle market conditions influence firms' remote work policies—for instance, if higher commuting costs or rising vehicle prices push employers to offer remote arrangements. Additionally, measurement error in our job-posting-based remote work measure could attenuate OLS estimates.

To address these concerns, we employ an instrumental variable strategy that isolates variation in remote work demand driven by the pre-pandemic county occupation composition. Specifically, we construct a Bartik shift-share instrument that predicts county-level remote work demand using pre-pandemic occupational composition and the remote ability of each occupation:

$$Z_c = \sum_o s_{oc}^{\text{pre}} \times A_o \tag{2}$$

where s_{oc}^{pre} is the share of county c's pre-pandemic employment in occupation o (from 2014–2018 PUMS ACS), and A_o is the measure of remote work ability of occupation o.

We use two complementary measures of remote work ability, A_o, from Dingel and Neiman (2020) and Montenovo et al. (2022), both constructed from the ONET database of occupational task requirements.[7] Dingel and Neiman (2020) classify occupations as not teleworkable when ONET work-context or work-activity requirements involve inherently physical tasks—operating equipment, handling materials, working outdoors, or providing in-person services—concluding that 37 percent of U.S. jobs could be done from home in early 2020. Montenovo et al. (2022) classify occupations as remoteable when O*NET

[7] Langemeier and Tito (2022) analyzes the features of the two remote work ability measures in detail.

respondents rate email, memos, or telephone as important or very important to the job. These measures capture different dimensions of teleworkability—physical task requirements versus communication-tool reliance—and are positively but imperfectly correlated, which is why we report both throughout.

The instrument's relevance requires that counties concentrated in teleworkable occupations adopted remote work more extensively after 2020, which our strong first-stage results confirm. The exclusion restriction requires that pre-pandemic occupational composition affects vehicle registrations only through remote work adoption, conditional on state fixed effects and other controls. This assumption would be violated if counties with a larger share of high-teleworkable occupations experienced differential income or taste shocks that directly affected vehicle demand. State fixed effects absorb many such threats by restricting identification to within-state comparisons.

We estimate all specifications using ordinary least squares (and two-stage least squares for the IV) with 2010 population weights. Standard errors are clustered at the state level (Moulton, 1990).

3 Results

This section presents the baseline results. We describe the merged county panel, report the main OLS and IV estimates, quantify the aggregate implications, and document the spatial spillovers that help account for the gap between IV and OLS estimates.

Before turning to the regression results, Table 1 summarizes what the merged county panel shows, reporting population-weighted summary statistics for our long-difference sample of 3,014 counties. Between 2019 and 2024, the average county saw the share of job postings that allow remote work rise by 4.3 percentage points, from a 2019 baseline of 2.9 percent to 7.2 percent. Crucially for identification, this increase varied widely across counties: the cross-county standard deviation of 3.9 percentage points is nearly as large as the mean, and adoption ranges from essentially no change to increases exceeding 30 percentage points. It is this dispersion in remote work adoption, rather than the common national rise, that our within-state design exploits. Over the same period, vehicle registrations fell

by about 6 percent on average.

The remaining rows characterize the sample. The average county registered roughly 5,600 new vehicles in 2019, but this figure is highly right-skewed—the standard deviation exceeds the mean and the largest counties register more than 50,000—underscoring why we weight by population throughout. The two instruments, the employment-weighted remote-communication and teleworkability shares, average 0.29 and 0.20 with modest cross-county dispersion, consistent with their role as pre-determined, occupation-based predictors of subsequent remote work adoption.

Table 1: Summary Statistics: Long-Difference Variables

Variable	Mean	Std. Dev.	Min	Max	N
Δ Remote Postings	4.3	3.9	−78.9	34.1	3,014
Δ Log Registrations	−0.06	0.13	−0.90	1.54	3,014
Avg Reg., 2019	5,588	9,712	0.83	51,723	3,014
Avg Remote Postings, 2019	2.87	2.07	0	82.49	3,014
Rem. Comm. Share	0.29	0.03	0.16	0.40	3,014
Teleworkability Share	0.20	0.02	0.07	0.33	3,014

Note: Statistics are population-weighted using 2010 county population. Δ Remote Postings is expressed in percentage points and Avg Remote Postings is in percent. Rem. Comm. Share and Teleworkability Share are employment-weighted remote ability measures constructed from the 2018 O*NET survey based on the definition of Montenovo et al. (2022) and Dingel and Neiman (2020), respectively. The sample includes all U.S. counties with non-missing data for both periods.

Table 2 reports OLS and IV estimates of the effect of remote work on vehicle demand. In the OLS specification with only state fixed effects (column 1), a one percentage point increase in remote work share is associated with a 0.3 percent decline in vehicle registrations.

Figure 4 provides a visual representation of the OLS relationship. The binned scatter plot groups counties into 100 population-weighted percentiles based on their change in remote work share, with each point representing approximately one percent of the U.S. population. The downward-sloping pattern is clear and consistent across the entire distribution. The linear fit tracks the data closely, indicating that the relationship is not driven

Table 2: Main Results: OLS and IV Estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS		First Stage		Reduced Form		2SLS	
	Δ ln Regs		Δ Remote Post.		Δ ln Regs		Δ ln Regs	
Δ Remote Post.	-0.311***	-0.312***					-0.779***	-0.591***
	(0.101)	(0.104)					(0.186)	(0.155)
Δ Population		0.484***	-0.125***	-0.117***	0.557***	0.539***	0.459***	0.469***
		(0.112)	(0.039)	(0.037)	(0.122)	(0.122)	(0.111)	(0.109)
Δ Income		0.466***	0.030	0.015	0.472***	0.474***	0.495***	0.484***
		(0.105)	(0.044)	(0.039)	(0.105)	(0.105)	(0.100)	(0.100)
Rem. Comm. IV			0.482***		-0.375***			
			(0.037)		(0.095)			
Teleworkable IV				0.617***		-0.365***		
				(0.037)		(0.100)		
First-stage F			168	278				
R^2	0.256	0.354	0.291	0.359	0.354	0.353	0.118	0.132
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
2010 Pop. W.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	3,014	3,014	3,014	3,014	3,014	3,014	3,014	3,014

Note: Dependent variable is the log change in vehicle registrations between 2024 and 2019, except in Columns 5-6 where it is the change in the share of remote job postings. Columns 1-2 report OLS estimates. Columns 3-4 present the reduced-form regression of log registrations directly on measures of remote work ability, the Bartik instruments, bypassing any measure of realized remote work. Columns 5-6 report the first-stage regression of actual remote work change on the two Bartik instruments. Columns 7-8 report two-stage least squares, instrumented with the remote-communication (Column 7) and teleworkability (Column 8) measures, respectively; within the reduced-form (Columns 3-4) and first-stage (Columns 5-6) pairs the instrument in use is the one whose coefficient row is populated. The Bartik instruments are the pre-pandemic county employment share in each occupation (from ACS 2014-2018 PUMS) weighted by occupation-level measures of remote ability (Rem. Comm. IV based on Montenovo et al. (2022) and Teleworkable IV based on Dingel and Neiman (2020)), using the characteristics from the 2018 O*NET survey. All columns also include the change in heating and cooling degree days to control for differences in weather patterns. Without state fixed effects (not shown), the OLS coefficient is -0.381^{***} (0.118). All specifications use 2010 population weights. Standard errors clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

by outliers or nonlinearities.[8]

Column 2 adds controls for population and income growth, key determinants of vehicle sales that correlate with remote work adoption. Both variables enter positively as expected, yet the remote work coefficient remains essentially unchanged. This stability suggests our

[8] Winsorizing the change in remote posting share at the 1st and 99th percentiles, which implies excluding 60 counties, leaves the estimates essentially unchanged: the OLS coefficient is −0.38 with and without controls, and the two-stage least squares estimates are −0.80 (Rem. Comm.) and −0.60 (Teleworkable), compared with −0.78 and −0.59 in Table 2.

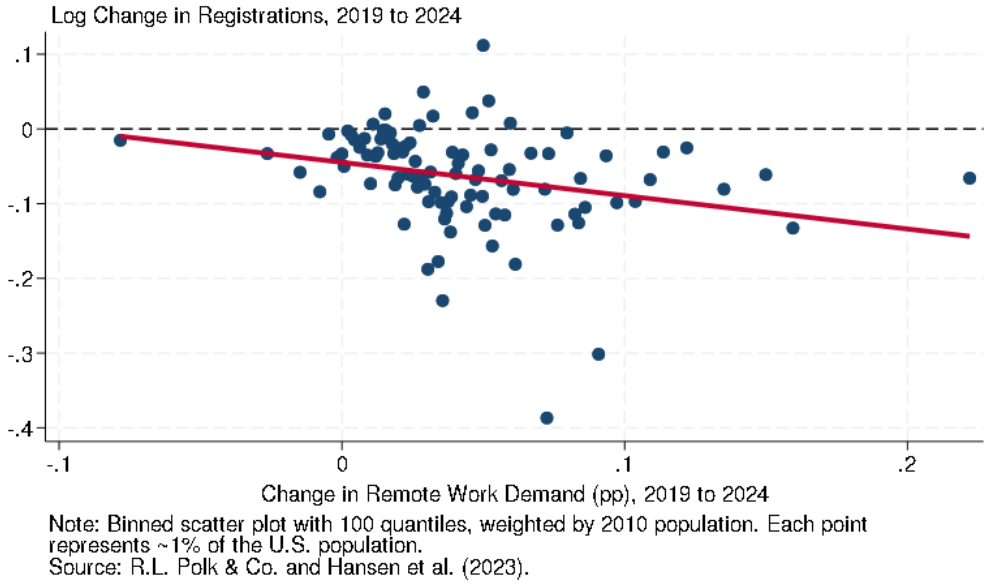


Figure 4: Remote Work and Vehicle Registrations: Binned Scatter Plot

Note: Each point represents a population-weighted percentile (1 percent of U.S. population) of counties grouped by change in remote work share from 2019 to 2024. The vertical axis shows the log change in vehicle registrations over the same period; the horizontal axis shows the change in remote work share (percentage points). The line shows the linear fit corresponding to the bivariate regression, with no other controls or state fixed effects.

estimate captures more than compositional shifts in county characteristics, though we address further endogeneity concerns through instrumental variables.

Our instrumental variable strategy exploits counties' pre-pandemic occupational composition to isolate exogenous variation in remote work adoption. Columns 3-4 report the first stage: counties with occupational structures concentrated in teleworkable jobs adopted remote work more extensively after 2020, with strong first-stage F-statistics confirming instrument relevance.[9]

Columns 5-6 present the reduced form, which relates pre-pandemic occupational structure directly to vehicle registration changes. The negative coefficients establish that counties with high telework potential experienced relative declines in registrations, consistent

[9]Figure H.1 visualizes both first stages: binning counties by their pre-pandemic occupational teleworkability (left panel) or remote-communication share (right panel) reveals a tight, positive, near-linear relationship with the subsequent 2019–2024 change in remote work.

with the mechanism and supporting the identifying assumption.

The two-stage least squares estimates in columns 7-8 combine these relationships to identify the causal effect of remote work. Both instruments produce negative coefficients that are larger than the OLS estimates, with differences significant at the 10 percent level. The consistency across two distinct measures of teleworkability strengthens confidence in the causal relationship. The larger IV magnitude point to attenuation bias in the OLS specification, likely operating through two channels: simultaneity, if economic growth drives both remote work adoption and vehicle registrations, and measurement error in our posting-based measure. Additionally, the IV estimates may capture spatial spillovers if remote work adoption in neighboring counties affects local vehicle demand, a mechanism we document in subsection 3.1.

Before interpreting magnitudes, we first validate a key identifying assumption underlying our long-difference design. If counties that later adopted more remote work were already on different vehicle demand trajectories before the pandemic, our estimates would be biased. To test for parallel pre-trends, we leverage vehicle registration data from 2018. We regress the change in log registrations from 2018 to 2019 on the future change in remote work from 2019 to 2024. If future remote work adoption were correlated with pre-existing trends, counties that would eventually adopt more remote work should already show differential vehicle demand growth in 2018–2019, before remote work became widespread. We find no such correlation: The coefficient on future remote work is 0.026 and statistically insignificant.[10]

Turning to economic magnitudes and taking the more conservative IV estimate from column 8, a one standard deviation increase in the remote work demand, roughly consistent with the average increase in remote postings between 2019 and 2024, reduces vehicle registrations by 17 percent of a standard deviation, holding income and population constant.

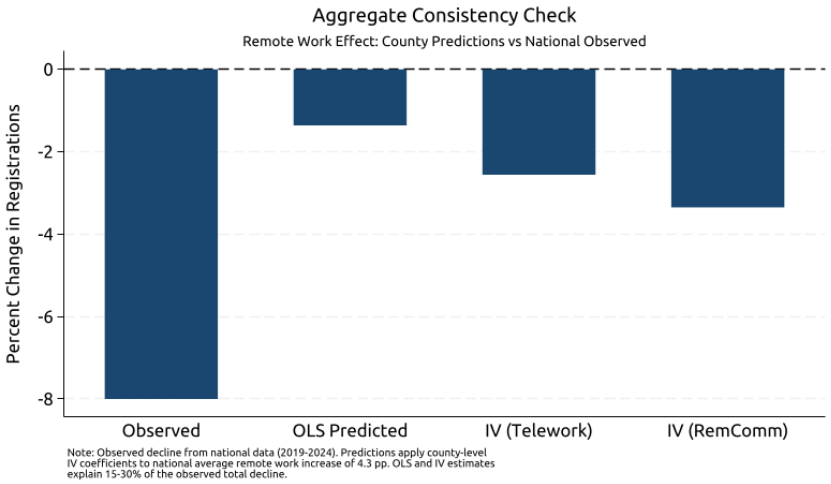
To place these county-level estimates in broader context, we can extrapolate their aggregate implications for the U.S. market. Applying the county-level IV coefficient to the national 4.3 percentage point increase in remote work provides a rough partial-equilibrium

[10]The result is virtually unchanged if we instead measure the future change in remote work from 2019 to the 2023–2025 average, where the coefficient is 0.021.

benchmark of a 2.5 percent decline in registrations, around 30 percent of the overall decline in sales from 2019 through 2024—or roughly 400,000 fewer vehicles at 2019 sales levels. The OLS coefficient implies a more modest effect, accounting for about 15 percent of the overall decline, or 200,000 units. This back-of-the-envelope calculation suggests that remote work adoption has been a quantitatively important factor in the post-pandemic slowdown in vehicle sales. However, this calculation should be interpreted with caution, as it applies a cross-sectional estimate to an aggregate outcome and abstracts from general equilibrium responses, compositional shifts across states, and concurrent macro-level forces such as interest rates, vehicle prices, and fleet aging that may have also contributed to the sales shortfall.

Figure 5 summarizes this extrapolation against the observed national decline. Applying the county-level coefficients to the aggregate increase in remote work reproduces between roughly one-sixth (OLS) and one-third (the Teleworkable IV) of the observed drop in registrations; the Rem. Comm. IV delivers an even larger share, at about two-fifths. The exercise confirms that remote work is a first-order contributor, even as the majority of the decline in sales has been shaped by other forces.

Figure 5: Aggregate Consistency: County Predictions vs. National Decline



Note: The observed bar is the national decline in registrations from 2019 to 2024. The predicted bars apply the county-level OLS and IV coefficients to the national 4.3 percentage point increase in the remote work share. Source: authors' calculations.

Finally, the aggregate estimates are robust along several dimensions, which we document in Appendix A. Substituting a survey-based work-from-home measure from the American Community Survey for the posting-based measure leaves the coefficients essentially unchanged. Controlling for a shift-share measure of county exposure to the 2021–2022 semiconductor shortage does not move the estimates, and extending the long difference to 2025, well after supply had normalized, leaves them if anything somewhat larger, the opposite of what a transitory supply story would predict. The estimates are also insensitive to conditioning on pre-pandemic income levels, and exposure-robust standard errors following Borusyak et al. (2022) if anything sharpen inference.

3.1 The Role of Spatial Spillovers

We now turn to spatial spillovers, which provide an additional explanation for the IV-OLS divergence. Remote work adoption in neighboring counties may affect local vehicle demand, causing the IV estimates to capture both direct and spillover effects through spatially correlated changes.

Table 3 presents results from specifications designed to directly test for spatial spillovers and assess their contribution to our baseline IV estimates. We define neighboring counties as those sharing a border according to the 2010 county adjacency files and construct measures of average remote work demand in these adjacent counties. Column 1 provides the most direct evidence, including both own-county and neighbor-county changes in remote work postings as separate regressors. The results reveal a striking pattern: while own-county remote work adoption reduces vehicle registrations, neighboring counties' remote work adoption exhibits an even larger negative effect. The neighbor effect is more than twice the magnitude of the own-county effect, though estimated with less precision given the smaller variation in spatially averaged measures.

Columns 2 and 3 confirm this finding using reduced-form specifications that separately include measures of remote work ability for own county and neighboring counties based on their respective occupational composition. Both instruments reveal the same pattern: the neighbor coefficient substantially exceeds the own-county coefficient, with neighboring

Table 3: Remote Work and Vehicle Demand: Quantifying Spillover Effects

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	Red. Form	Red. Form	OLS	2SLS	
Δ Remote Post.	-0.251**					
	(0.102)					
Δ Adj Remote Post.	-0.552*					
	(0.289)					
Rem. Comm. IV		-0.230**				
		(0.109)				
Adj. Rem. Comm. IV		-0.645**				
		(0.252)				
Teleworkable IV			-0.210			
			(0.131)			
Adj. Teleworkable IV			-0.516*			
			(0.257)			
Diff. Remote Post.				-0.177*	-0.002	-0.002
				(0.097)	(0.534)	(0.534)
Δ Population	0.500***	0.563***	0.558***	0.487***	0.417***	0.452***
	(0.118)	(0.121)	(0.124)	(0.113)	(0.106)	(0.104)
Δ Income	0.475***	0.500***	0.486***	0.454***	0.491***	0.472***
	(0.104)	(0.100)	(0.101)	(0.106)	(0.104)	(0.101)
First-stage F					15	15
R^2	0.358	0.364	0.359	0.349	0.047	0.112
State FE	Yes	Yes	Yes	Yes	Yes	Yes
2010 Pop. W.	Yes	Yes	Yes	Yes	Yes	Yes
N	3,014	3,014	3,014	3,014	3,014	3,014

Note: Dependent variable is the log change in vehicle registrations between 2024 and 2019. Column 1 reports OLS estimates including own-county and adjacent-county changes in remote postings as separate regressors. Columns 2-3 present reduced-form regressions of log registrations on the own-county and adjacent-county Bartik instruments (Rem. Comm. IV based on Montenovo et al. (2022) and Teleworkable IV based on Dingel and Neiman (2020)), constructed from pre-pandemic county employment shares by occupation (ACS 2014-2018 PUMS) weighted by occupation-level remote ability from the 2018 O*NET survey. Column 4 reports OLS estimates using the differential remote work measure, defined as the own-county change in the share of remote postings minus the average change in adjacent counties. Columns 5-6 report two-stage least squares estimates of the differential specification, instrumenting the differential remote work measure with the corresponding differential in the Bartik instruments. Adjacent counties are defined by the 2010 county adjacency files. All specifications use 2010 population weights. Standard errors clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

exposure roughly twice as large.

To isolate variation in remote work adoption that is orthogonal to neighbors' pat-

terns, we construct a differential measure defined as the difference between own-county and neighboring-county changes in remote work demand. This measure captures relative remote work adoption, that is, instances where a county experiences an increase in remote work that differs from the trend in surrounding counties. Column 4 shows that this differential measure exhibits a negative coefficient of -0.18, suggesting that counties adopting remote work faster than their neighbors experience relative declines in vehicle registrations. This coefficient is smaller in magnitude than both the baseline OLS and IV estimates, as expected when the specification isolates relative adoption by netting out the spatially correlated variation that drives spillovers. Columns 5 and 6 implement IV strategies using this differential specification, instrumenting the relative remote work measure with the analogous differential in pre-pandemic occupational teleworkability between own and neighboring counties. In a specification designed to minimize spillover effects, the IV estimates are insignificant.

These findings carry important implications for interpreting our baseline IV estimates. First, they confirm that spatial spillovers operate in the same direction as direct effects with both own and neighboring remote work adoption reducing local vehicle demand. This rules out scenarios where spillovers might offset or mask direct effects and instead suggests that the mechanisms through which remote work reduces vehicle demand may extend across county boundaries. Second, the substantial magnitude of spillover effects suggests that our baseline IV estimates may partly capture general equilibrium effects encompassing broad spatial adjustments in regional vehicle markets and mobility patterns. Third, the presence of spillovers has implications for policy and welfare analysis. The impact of remote work on vehicle demand extends beyond individual counties, creating externalities that could affect neighboring jurisdictions—whether through reduced congestion benefits, changes in retail and service location patterns, or integration of vehicle markets.

4 Mechanisms

4.1 Heterogeneity by Registration Type

To understand the mechanisms through which remote work affects vehicle demand, we distinguish between two fundamentally different types of vehicle registrations. Fleet registrations comprise vehicles owned by organizations—federal, state, and local governments; private firms; commercial operators; rental companies; and vehicle manufacturers themselves. Retail registrations, by contrast, represent vehicles purchased or leased by individual households.[11] This decomposition is crucial because it separates organizational asset decisions from household ownership choices, allowing us to identify whether remote work operates through business fleet optimization or changes in consumer vehicle demand.

Table 4 presents this decomposition. Remote work adoption has large, negative effects on fleet registrations (columns 1-3). The OLS estimate indicates that a one percentage point increase in remote work share reduces fleet registrations by 1.3 percent, more than four times the magnitude of the overall effect reported; the instrumental variable estimates are even larger in magnitude. These estimates suggest that the causal effect of remote work on fleet vehicle demand is substantial: a one percentage point increase in remote work may lead firms and organizations to reduce their vehicle fleets by up to 4 percent. In contrast, remote work has no detectable effect on retail vehicle registrations. The OLS estimate is economically small and statistically indistinguishable from zero. The IV estimates switch sign to become slightly positive, remaining near zero and statistically insignificant.

Two observations validate these results.[12] First, weighting our estimated coefficients by the fleet and retail registration shares closely reproduces the aggregate effect in Table 2.[13] Second, geographic concentration of fleet registrations does not drive these results. Fleet registrations are only modestly more concentrated than retail, with the top

[11] Fleet and retail registrations account for roughly 25 and 75 percent of 2019 total registrations, respectively.

[12] Appendix B reports the yearly results. The fact that the extimate of the fleet effects remains fairly consistent overtime with the exception of 2020 supports our identification strategy.

[13] The share-weighted sum approximates but does not exactly equal the aggregate effect. Because fleet registrations decline more sharply than their share of total registrations, the weighted sum modestly overstates the aggregate coefficient by roughly one-sixth.

Table 4: Fleet vs. Retail Registrations

	(1)	(2)	(3)	(4)	(5)	(6)
	Fleet			Retail		
	OLS	IV		OLS	IV	
		Rem. Comm.	Telew.		Rem. Comm.	Telew.
Δ Remote	-1.322***	-3.732***	-2.852***	-0.055	0.012	0.011
	(0.341)	(0.449)	(0.507)	(0.064)	(0.189)	(0.153)
Δ Population	0.406	0.278	0.325	0.537***	0.541***	0.541***
	(0.256)	(0.250)	(0.239)	(0.071)	(0.069)	(0.068)
Δ Income	0.654**	0.805***	0.750**	0.455***	0.451***	0.451***
	(0.296)	(0.298)	(0.284)	(0.068)	(0.064)	(0.066)
First-stage F		163.3	271.7		163.4	271.7
R^2	0.117	-0.036	0.016	0.625	0.299	0.299
N	3,025	3,025	3,025	3,028	3,028	3,028

Note: The dependent variable in all columns is the log change in registrations (2019 to 2024 average). Each column reports a separate regression of registrations on the change in remote work share, where registrations are restricted to fleet transactions (all non-retail types: government, firm, commercial, rental, and dealer/manufacturer) in columns 1-3 and retail transactions (individual purchases and leases) in columns 4-6. Columns 1 and 4 report OLS estimates; the remaining columns report IV estimates, instrumented within each pair with the remote-communication measure (Rem. Comm., Montenovo et al., 2022) in the first column and teleworkability (Telew., Dingel and Neiman, 2020) in the second. First-stage F is the Kleibergen-Paap Wald rk statistic. The sample here is slightly larger than the 3,014 counties in Table 2 because this decomposition conditions on a lighter control set (income and weather) rather than the full establishment, employment, and pay controls that define the headline estimation sample; the fleet and retail columns differ from one another by a few counties because not every registration type is observed in every county. All specifications include state fixed effects, population weights, controls for income and heating- and cooling-degree days, and state-clustered standard errors. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

ten counties holding about 15 percent of fleet registrations versus 10 percent of retail, and the fleet estimates are unchanged when the most fleet-intensive counties are excluded. Third, we corroborate the fleet channel using an industry-based approach rather than our occupation-based remote work measure. Appendix D presents a shift-share design exploiting county-level variation in industry teleworkability, with counties whose 2019 employment was concentrated in teleworkable industries seeing systematically larger fleet declines.

The absence of effects on retail registrations indicates that households have not substantially altered their vehicle ownership decisions in response to an increase in remote work

demand, at least in the 5-year window in our analysis. This finding may initially appear puzzling, given that remote work reduces commuting frequency and might be expected to reduce workers' need to own a vehicle. However, several factors may account for the muted household response. First, households adjust vehicle ownership slowly due to high transaction costs and strong habits. Second, vehicles provide value beyond commuting as most household travel is unrelated to work.[14] Third, remote work may have enabled households to relocate further from employment centers or to less dense, more automobile-dependent areas where vehicle ownership remains essential even with reduced commuting frequency.[15] Finally, uncertainty about the permanence of remote work arrangements may have led households to maintain their existing vehicle stock as an option value.

Having established that the aggregate effect operates entirely through fleet registrations, we can further examine which organizational fleets are adjusting. Fleet registrations can be decomposed into four types: government (federal, state, and local), private firms, commercial operators (vehicles used in delivery of goods and services), and rental companies. We exclude rental companies from this analysis because they faced a distinct pandemic-related shock, a collapse in travel demand, likely unrelated to remote work.[16]

Table 5 highlights that the fleet response varies by organizational type.[17] Government vehicle fleets show the strongest adjustments. The estimates indicate that federal, state, and local agencies reduced their vehicle fleets substantially in response to remote work adoption: government registrations declined by 2 percent per percentage point of higher remote work demand.

Private firm vehicle registrations also show significant declines in response to an increase

[14]The 2022 NextGen National Household Travel Survey (NHTS) Passenger Origin-Destination (OD) data indicate that approximately 28 percent of passenger trips are work-related. The NextGen NHTS classifies passenger trips as work or non-work based on the observed origin and destination of trips and the identification of workers' home and workplace locations.

[15]For example, Ramani and Bloom (2021) suggest that remote work made suburban and exurban locations more attractive.

[16]Appendix C presents results for rental registrations separately and for the combined firm-plus-rental category, with somewhat larger effects.

[17]Appendix Table F.2 present the full specification with both IV strategies and the full set of controls. The two IV strategies deliver pretty similar results, thus we have decided to highlight only the results based on the Dingel and Neiman (2020) classification.

Table 5: Remote Demand and Fleet Registrations: A Deeper Dive

	(1)	(2)	(3)	(4)	(5)	(6)
	Government		Firm		Commercial	
	OLS	IV (Telew.)	OLS	IV (Telew.)	OLS	IV (Telew.)
Δ Remote	-1.871***	-2.133***	-0.579***	-1.048**	0.075	-0.113
	(0.592)	(0.789)	(0.168)	(0.413)	(0.283)	(0.537)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
First-stage F		182.6		213.5		210.7
R^2	0.142	0.060	0.426	0.105	0.116	0.018
N	2,540	2,540	3,005	3,005	2,837	2,837

Note: The dependent variable in all columns is the log change in registrations (2019 to 2024 average). Each column reports a separate regression of registrations on the change in remote work share, where registrations in each column are restricted to the indicated transaction type. The firm category here comprises firm lease and firm purchase registrations and excludes rental (rent-a-car) registrations, which the raw data group together with firm transactions; because rental fleets are large and adjust for reasons plausibly distinct from commuting, we isolate them and analyze them separately in Appendix C. Columns 1, 3, and 5 report OLS estimates; the even columns report IV estimates using the teleworkability Bartik instrument (Telew., Dingel and Neiman, 2020); the alternative remote-communication instrument (Montenovo et al., 2022) delivers similar results, reported in Appendix Table F.2. First-stage F is the Kleibergen-Paap Wald rk statistic. Sample sizes vary because some registration types are not present in all counties. All specifications include state fixed effects, population weights, and state-clustered standard errors. The "Controls" row indicates that each specification additionally conditions on the change in the number of establishments, employment, average weekly pay, income, population, and heating- and cooling-degree days; the full set of control coefficients is reported in Appendix Table F.2. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

to remote work demand, with effects between 0.6 and 1 percent.

Commercial vehicle registrations, instead, show no systematic response to remote work adoption. This null result aligns with the operational role these vehicles play: they serve functions related to transporting goods, making deliveries, and providing services to customers. Unlike employee vehicles or commuter fleets, commercial vehicle demand depends on the scale of business operations and customer service needs, which may have been largely unaffected by whether back-office employees work remotely.

In these regressions, we include a richer set of firm-specific controls beyond the population and income variables used in our baseline specifications. Specifically, we control for changes in the number of establishments, total employment, and average weekly payroll at the county level. These variables capture local business activity and organizational scale,

which may independently affect fleet vehicle demand and could potentially correlate with remote work adoption patterns. The inclusion of these controls ensures that our estimated remote work effects reflect changes in vehicle intensity conditional on overall business expansion or contraction, rather than simply picking up broader trends in local economic activity.

To keep the table readable we collapse these controls into a single "Controls" indicator and report their full coefficients in Appendix Table F.2, where they display sensible patterns. Establishment growth positively predicts government and firm registrations but not commercial vehicles, employment growth enters positively for government fleets, payroll growth strongly predicts firm and commercial registrations, and income and population growth predict firm registrations, consistent with corporate fleets scaling with local economic activity. Crucially, the remote work coefficients remain negative and significant for both government and firm fleets even after controlling for these firm-specific factors, indicating that fleet reductions operate independently of general business conditions.

4.2 Government Fleets: Smaller Stock or Slower Turnover?

The preceding sections established three points. Registrations fell in high-remote-work counties in the post-pandemic period; the decline is concentrated in fleet rather than retail transactions, with government fleets carrying the largest coefficient. What the registration data cannot tell us is whether organizations are operating smaller fleets or simply replacing vehicles less frequently.

This distinction follows directly from the evolution of the vehicle stock. As Greenspan and Cohen (1999) emphasize, the stock evolves according to

$$V_t = V_{t-1} + \text{Registrations}_t - \text{Scrap}_t, \tag{3}$$

where V_t is the number of vehicles in operation at time t, Registrations_t represents new vehicles entering the stock and are a proxy for vehicle sales, and Scrap_t is the number of vehicles leaving the stock because they are scrapped or otherwise retired. Thus, registrations increase the vehicle stock, while scrappage reduces it. The number of vehicles scrapped depends in part on the characteristics and use of the existing stock: Greenspan and Cohen

(1999) emphasize the roles of vehicle age and utilization, with higher utilization generating greater physical wear and tear and therefore contributing to earlier retirement.[18] Remote work can affect both sides of this process. Greater remote work may reduce the demand for vehicles but also reduce the utilization of vehicles that remain in an organization's fleet. Lower utilization reduces physical wear and tear, potentially extending vehicle lifespans and reducing the rate at which existing vehicles are scrapped and replaced. Organizations may therefore respond to greater remote work by keeping vehicles longer, reducing the rate at which existing vehicles are replaced even without an immediate reduction in the size of the fleet. A decline in registrations could consequently reflect either a reduction in fleet size (lower V_t), delayed replacement (lower Scrap_t and longer vehicle holding periods), or both. Both channels represent reductions in vehicle demand and are consistent with the estimates reported so far, but they may differ in their persistence and in their implications for the level and composition of the vehicle stock.

Separating these channels requires data on vehicle stocks, age, and utilization, which the Polk registration data does not contain. Government fleets provide an exception. They are the segment with the largest estimated response and the only fleet for which inventory, vehicle age, and mileage are published. We therefore use the federal fleet as a case where the underlying channel can be observed directly.

The Federal Fleet Report includes data on all three margins for government fleets. Table 6 reports the 2019 to 2024 change for the Total Civilian Agencies.[19] Federal acquisitions fell by 2 percent over this period, consistent with our county-level registration findings. More revealing, however, are the changes in vehicle age and utilization. Average age rose by nearly 9 percent, miles per vehicle fell by 12 percent, and disposals dropped by almost 19 percent. These patterns point to a significant decline in scrappage: federal agencies kept their vehicles longer and drove them less. By contrast, the inventory stock increased slightly, rising by just under 1 percent.

These data illustrate the mechanism in equation (3). The aggregate decline in gov-

[18] Greenspan and Cohen (1999) note that repair costs and incentives to replace older vehicles can also affect scrappage decisions.

[19] Appendix E presents more details on individual segments.

Table 6: Federal Fleet: Stock, Age, and Utilization, 2019–2024

Quantity	2019	2024	% change
Inventory (stock), Total Civ. Agencies	245,950	248,155	+0.9
Average age (years), passenger vehicles	6.3	6.8	+8.9
Miles per vehicle, passenger vehicles	8,910	7,824	−12.2
Disposals, Total Civ. Agencies	29,927	24,337	−18.7
Acquisitions, Total Civ. Agencies	29,006	28,422	−2.0

Note: All rows cover the Total Civilian Agencies aggregate of the GSA Federal Fleet Report for fiscal years 2019 and 2024, which excludes the Department of Defense and other non-civilian agencies. Inventory is the year-end stock from Table 2-1, average age from Table 2-9, miles per vehicle from Table 4-2, and acquisitions and disposals from Tables 5-4 and 5-5; age and miles refer to the passenger subtotal. The acquisition and disposal counts are single-year flows and are not expected to reconcile arithmetically with the five-year change in inventory, because the report records agency-owned acquisitions and disposals while the inventory also reflects GSA-leased and commercially leased vehicles that enter and leave without an agency purchase or disposal, as well as reclassifications across years. Cycle lengthening predicts a roughly flat stock, rising average age, falling miles per vehicle, and falling disposals, which is the pattern observed here. These are descriptive time series rather than estimated coefficients; they corroborate the county instrumental-variable estimates rather than replacing them. Segment-level detail is reported in Appendix Table E.1.

ernment acquisitions seems to have operated through the scrappage margin: reduced fleet utilization extended vehicle lifespans and lowered the replacement rate, even as the stock itself increased modestly.

The evidence so far documents only what happened to federal fleets between 2019 and 2024 but does not establish a quantitative relationship between those changes and remote work. To address this, we combine the fleet data with two agency-level measures of telework intensity.

The first is the respondent-weighted share of employees reporting regular telework in the Federal Employee Viewpoint Survey (FEVS). We match 24 civilian agencies across fiscal years 2021 to 2023, capturing the period when remote work arrangements became established post-pandemic. The second is the administrative telework share from OPM's Status of Telework Reports to Congress, which we match to 37 agencies over fiscal years 2019 to 2024. This measure provides a finer agency roster and widens the panel to include both the pre-pandemic baseline and the pandemic surge in telework. The two sources define telework differently: one as a survey response on self-reported behavior and the

other as a formal administrative count.

Both deliver the same reduced-form fact, reported in Table 7. Agencies with higher telework shares maintain significantly smaller fleets and drive them less, with consistently negative estimates across both measures despite their different definitions. The estimates rely on cross-sectional variation across agencies rather than within-agency changes over time, as the limited number of observations precludes a panel design. They nonetheless provide direct corroboration of the county-level results, linking telework to fleet outcomes at the organizational level.

Table 7: Remote Work and Government Fleets

	FEVS	OPM
	(1)	(2)
Log inventory	-6.767^{***}	-5.102^{***}
	(0.555)	(0.372)
Log miles per vehicle	-0.842^{***}	-0.496^{***}
	(0.136)	(0.154)
Years	2021–23	FY2019–24
Agencies	24	37
N	72	209

Note: Each cell is a separate regression of the log of a federal agency's total fleet outcome on that agency's telework share, pooling agency-years with year fixed effects, agency-size weights, and heteroskedasticity-robust standard errors in parentheses. Column 1 uses the OPM Federal Employee Viewpoint Survey respondent-weighted share reporting any regular telework, over the 24 civilian agencies matchable for fiscal years 2021 to 2023, weighted by respondent counts. Column 2 uses the administrative telework share from OPM's Status of Telework in the Federal Government Reports to Congress, weighted by agency employment. The OPM panel is unbalanced: 37 civilian agencies over fiscal years 2019 to 2024 yield 213 agency-years rather than the 222 a balanced roster would give, because five agencies are absent from the OPM roster in at least one year, and four further rows drop for missing sedan inventory on the fleet side, leaving $N = 209$. Outcomes are year-end inventory (Federal Fleet Report Table 2-1) and miles per vehicle (Table 4-2). Because agency telework varies mostly across agencies rather than within, the coefficients are identified by cross-agency variation, and we read them as direct-survey and administrative corroboration on small, potentially confounded samples rather than as identified causal evidence. Segment-level estimates are reported in Appendix Table E.2. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

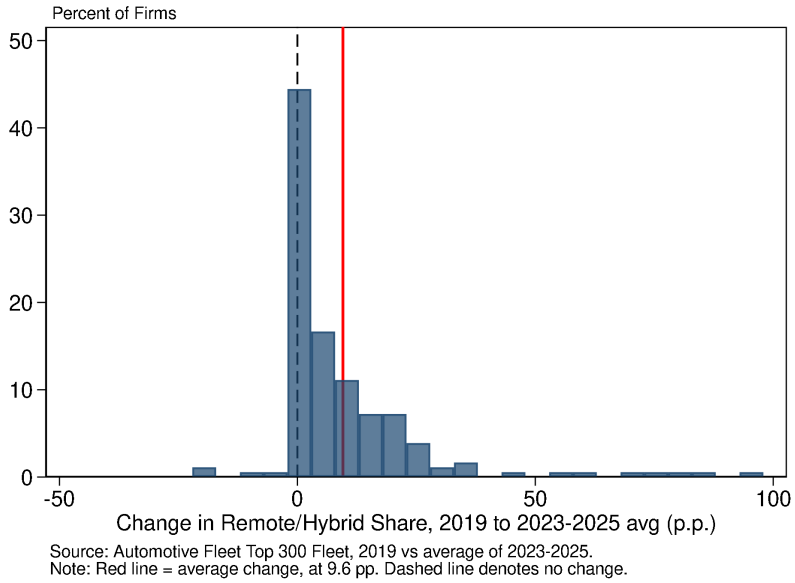
4.3 Firm Fleets

The county-level analysis documented significant effects on firm fleets, the other segment where registration declines were concentrated. To examine this channel more directly, we

turn to organization-level fleet data from Automotive Fleet's Top 300 Fleet Survey, which reports vehicle inventory for the largest corporate fleets in the United States and point to some modest declines in reported fleet sizes over the post-pandemic period.

To assess whether these changes relate to remote work, we match firm names to Lightcast job postings and construct the change in each firm's remote work share between 2019 and the 2023–2025 average. We use a post-pandemic three-year average to maximize firm coverage given the survey's year-to-year variation in reporting firms. Figure 6 presents the distribution of changes. The shift is substantial: on average, the remote/hybrid share of postings rose by almost 10 percentage points, with nearly the entire distribution concentrated above zero.

Figure 6: Remote Work Demand: 2019 vs. 2023-2025



Note: The observed bar is the national decline in registrations from 2019 to 2024. The predicted bars apply the county-level OLS and IV coefficients to the national 4.3 percentage point increase in the remote work share. Source: authors' calculations.

Table 8 links firms' remote postings to their fleet sizes. The cross-sectional pattern is clear: firms with higher shares of remote/hybrid postings maintain substantially smaller fleets. In columns 1 and 3, each percentage point increase in a firm's remote work share is associated with approximately 44 to 50 fewer vehicles in inventory, with both estimates

statistically significant at the 1 percent level.

However, this relationship largely reflects differences across firms rather than adjustments within them over time. When we include firm fixed effects (columns 2 and 4) to isolate within-firm changes, the coefficients drop sharply to between -6 and -4 vehicles and lose statistical significance. The muted within-firm response likely reflects sample limitations. The survey captures only 165 to 180 firms per specification, a small sample for panel estimation, and the five-year window may be too short to capture fleet downsizing decisions that unfold gradually through natural attrition and deferred replacement. The cross-sectional evidence nonetheless corroborates the county-level findings.

Table 8: Remote Work and Commercial Fleets

	(1)	(2)	(3)	(4)
	2019 vs. 2024		2019 vs. 2023-2025	
Variables	Total Inventory			
Share of Remote Postings	-44.297***	-6.076	-50.066***	-3.752
	(16.208)	(4.467)	(17.457)	(5.033)
Year FE	y	n	y	n
Firm FE	n	y	n	y
Observations	330	330	360	360
R-squared	0.005	0.003	0.005	0.001
Number of Firm IDs		165		180

Note: Each cell is a separate regression of the total firm fleet inventory on that firm's share of postings with remote/hybrid. Columns 1 and 3 report cross-sectional regressions which control for year fixed effects, while columns 2 and 4 report fixed-effects regressions. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

5 Conclusion

Remote work has fundamentally reshaped U.S. vehicle demand through channels that differ sharply from conventional durable goods cycles. Counties experiencing larger increases in remote work adoption saw vehicle registrations decline by 3 to 8 percent per ten percentage point rise in remote work share, accounting for roughly 15 to 30 percent of the persistent shortfall in national vehicle sales—approximately 200,000 to 400,000 fewer an-

nual purchases at 2019 levels. Crucially, this decline operates entirely through institutional fleets rather than household purchases, revealing that the relevant margin is not personal vehicle ownership but the broader commercial infrastructure organized around office-based commuting.

The fleet channel manifests across multiple margins of adjustment. Government registrations fall by roughly 20 percent per ten percentage point increase in remote work, while corporate fleet registrations decline by 6 to 10 percent. Federal fleet data illuminate the underlying mechanism: between 2019 and 2024, average vehicle age increased 9 percent, miles per vehicle fell 12 percent, and disposals dropped 19 percent, even as inventory rose modestly. Organizations responded to reduced commuting intensity not only by acquiring fewer vehicles but by extending vehicle lifespans and reducing utilization—a shift in the replacement cycle rather than merely the level of the stock.

The absence of a household retail response presents a puzzle, particularly when juxtaposed against aggregate trends. The post-pandemic recovery in retail sales is markedly more muted compared with the post-Great Recession period. Four years into recovery, retail sales have stalled at pre-pandemic levels versus 125 percent in the comparable post-Great Recession window. While our county-level analysis finds no relationship between local remote work and retail purchases, the failure of retail sales to return to their historical growth trajectory suggests that remote work's aggregate footprint may extend beyond what cross-sectional comparisons can capture. The household null result may also reflect offsetting forces that net to zero: some households reducing ownership while others in newly suburban locations increase it, or reduced commuting value offset by increased non-work travel needs in dispersed residential locations.

Several research priorities emerge from these findings. First, household panel data linking individual remote work status to vehicle ownership over time could reveal whether the aggregate null masks heterogeneous responses across demographic groups or remote work intensity. Second, extending the analysis to other categories of business spending that depend on worker concentration—office supplies, commercial cleaning services, food service equipment, business travel—would quantify remote work's full commercial foot-

print beyond vehicles alone. Third, employer return-to-office mandates create sharp quasi-experimental variation in local remote work shares; difference-in-differences analysis around mandate announcements could identify fleet adjustment dynamics and test whether these effects are reversible or represent permanent capital reallocation.

As remote work matures and potentially expands further with advancing digital collaboration tools and artificial intelligence, understanding these institutional channels becomes increasingly critical for forecasting durable goods demand, urban spatial structure, and environmental outcomes. The vehicle market adjustments we document—delayed replacement cycles, fleet relocation, and institutional downsizing—represent the leading edge of a broader reconfiguration of the capital stock organized around a half-century of office-centered work. Whether this reorganization proves durable or partially reverses with future return-to-office pressures remains an open question, but the magnitude and persistence of the effects through 2025 suggest that remote work has already induced structural changes in how organizations provision and utilize vehicle capital. The challenge for researchers and policymakers is to understand how these adjustments propagate through interconnected markets for real estate, transportation infrastructure, and the full array of goods and services whose demand was predicated on the daily spatial concentration of workers that remote work has permanently diminished.

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A Robustness of the Aggregate Estimates

This appendix subjects the aggregate estimates of Section 3 to a battery of robustness checks. We confirm the results hold under an independent, survey-based measure of remote work, and we explore supply-side controls for the pandemic-era semiconductor shortage and pre-period economic structure. Throughout, the negative effect of remote work on vehicle demand remains stable.

A.1 An Alternative Remote Work Measure

Our primary measure of remote work prevalence comes from job postings, which capture newly advertised remote positions rather than the stock of workers currently performing their duties from home. To validate our main results, we compare it against survey-based evidence on remote adoption from the American Community Survey. The ACS asks respondents about their primary means of transportation to work, with "worked from home" as one of the response categories (table B08006). We construct the share of workers reporting work from home for each county using the 2015–2019 and 2019–2023 five-year estimates, and compute the change over the two periods.[20] The posting-based measure and the ACS work-from-home share are positively correlated in both levels and changes, with Spearman rank correlations of 0.50 in 2023 levels and 0.43 in changes. The moderate rather than high correlation is consistent with the two measures capturing related but distinct concepts.

The key test is whether substituting the ACS measure for the posting-based measure in our main specification alters the results. Table A.1 reports this comparison. Column 1 reproduces the baseline estimate from Table 2 after including the full set of controls (including changes in the number of establishments, employment, and average weekly pay). Column 2 replaces ΔRemote_c with $\Delta\text{WFH}_c^{\text{ACS}}$, the change in ACS work-from-home share,

[20] We use five-year estimates to maintain coverage of all 3,014 counties in our main sample.

using the identical specification with state fixed effects and population weights. The coefficient is −0.40 and it is statistically indistinguishable from the posting-based measure. Columns 3-6 present the IV estimates, with columns 3-4 referring to the baseline model, while columns 5-6 introduce the ACS-based measure. We continue to instrument using measures of remote ability and the 2018 county occupation structure. Even for the IV estimates, the resulting coefficients are little affected by how we measure the shift to remote work.

A.2 Supply-Side Controls

Light Vehicle Production by Manufacturer. For supply-side robustness checks, we obtain manufacturer-level vehicle production data for 2019–2022 from WARDS. We combined these data with county-level manufacturer sale shares from the 2019 Polk registration data, and we construct a Bartik-style shift-share measure of county-level exposure to the 2021–2022 semiconductor shortage.

A key concern is that our results might reflect lingering supply-side disruptions from the 2021–2022 semiconductor chip shortage. In a nutshell, motor vehicles rely on hundreds of semiconductor chips to operate everything from engine controls to infotainment systems. When pandemic-era demand for consumer electronics collided with COVID-related factory closures, automakers found themselves unable to source enough chips to sustain production. The resulting cuts idled auto assembly lines worldwide and reduced U.S. light-vehicle output well into 2022, constraining the supply of new vehicles independently of any change in demand. Because these production cuts fell unevenly across manufacturers, and counties differ in the mix of brands they buy, the shortage could in principle generate cross-county variation in registration declines that is correlated with remote work.[21] To address this, we

[21] The production data reveal substantial heterogeneity in manufacturer-level impacts. For instance, Nissan experienced the largest decline at −41 percent in 2021, with −35 percent cumulatively over 2021–22. Honda, Subaru, and Ford followed at −29, −27, and −23 percent, respectively. In contrast, Tesla and Volkswagen increased production, while Toyota experienced only a modest decline.

Table A.1: Validation: Posting-Based vs. ACS Work-from-Home Measure

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS		IV			
	Postings	ACS	Postings		ACS	
			Rem. Comm.	Telew.	Rem. Comm.	Telew.
Δ Remote	-0.350***		-0.925***	-0.694***		
	(0.118)		(0.228)	(0.181)		
Δ WFH^{ACS}		-0.398***			-0.715***	-0.618***
		(0.142)			(0.177)	(0.151)
Δ Population	0.369***	0.425***	0.325***	0.343***	0.448***	0.441***
	(0.111)	(0.107)	(0.115)	(0.111)	(0.110)	(0.110)
Δ Income	0.289**	0.333***	0.305***	0.299***	0.376***	0.363***
	(0.111)	(0.109)	(0.107)	(0.107)	(0.106)	(0.111)
Δ Num. Estab.	0.147**	0.161**	0.228***	0.195***	0.212***	0.196***
	(0.066)	(0.066)	(0.076)	(0.068)	(0.076)	(0.070)
Δ Empl.	0.151**	0.134*	0.095	0.118*	0.094	0.106
	(0.065)	(0.069)	(0.065)	(0.064)	(0.081)	(0.080)
Δ Pay	0.229**	0.233**	0.270***	0.254***	0.256**	0.249**
	(0.097)	(0.099)	(0.091)	(0.091)	(0.100)	(0.099)
R^2	0.369	0.368	0.128	0.149	0.153	0.156
State FE	Yes	Yes	Yes	Yes	Yes	Yes
N	3,014	3,014	3,014	3,014	3,014	3,014

Note: The dependent variable in all columns is the log change in vehicle registrations from 2019 to the 2024 average. The remote-work measure is the posting-based share in Columns 1, 3, and 4 and the ACS work-from-home share (five-year estimates, 2015–2019 to 2019–2023) in Columns 2, 5, and 6, as indicated in the column headers. Columns 1, 3-4 reproduce the baseline OLS and IV estimates from Table 2. Within each IV block the first column instruments with the remote-communication measure (Rem. Comm., Montenovo et al., 2022) and the second with teleworkability (Telew., Dingel and Neiman, 2020). Both specifications include state fixed effects and population weights. Standard errors clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

construct a shift-share control measuring each county's exposure based on its pre-pandemic manufacturer composition:

$$\text{ChipExposure}_c = \sum_m \theta_{cm}^{2019} \times g_m \tag{A.1}$$

where θ_{cm}^{2019} is manufacturer m's share of county c's 2019 registrations and g_m is manufacturer m's production shock relative to 2019. We measure g_m as the percent change in manufacturer m's North American light-vehicle output between 2019 and the shortage period, using annual production-by-manufacturer data from WARDS, and average the resulting county exposure over 2021–2022.

Table A.2 presents results. The OLS remote work coefficient attenuates modestly from -0.35 to -0.29 when adding the cumulative chip exposure, while the changes in the IV coefficients are mixed, with one slightly larger and one slightly smaller. None of these differences is statistically significant. The chip exposure coefficients themselves are small and add no explanatory power conditional on state fixed effects.

These results provide supportive evidence that the baseline relationship between remote work and vehicle demand is not a consequence of lingering supply-side disruptions. As a further check, extending the long difference to a 2019 to 2025 endpoint, well after semiconductor supply had normalized, leaves the estimates if anything somewhat larger in magnitude (IV coefficients of -0.96 and -0.77, compared with -0.78 and -0.59 for the 2024 endpoint). Because a transitory supply-constraint story would predict the effect fading as production normalized, this strengthening rather than reversion indicates that the effect reflects a durable shift in demand rather than transitory supply constraints.

Finally, two further checks bear on the validity of the shift-share design and are reported in Appendix F. First, conditioning on a county's pre-pandemic (2019) level of median household income leaves both IV coefficients essentially unchanged, indicating that the estimates are not an artifact of counties' pre-existing income levels (Table F.3). Second, computing exposure-robust standard errors for the shift-share instrument, following Borusyak et al. (2022), if anything sharpens statistical significance, confirming that inference is not sensitive to the shift-share standard-error critique (Table F.4).

Table A.2: Robustness: Supply-Side Control for Chip Shortage Exposure

	(1)	(2)	(3)
		$\Delta \ln$ Regs	
	OLS	IV	
Δ Remote	-0.287**	-1.086**	-0.637*
	(0.129)	(0.432)	(0.331)
Chip exposure	-0.521	0.369	-0.131
	(0.323)	(0.578)	(0.493)
Δ Population	0.357***	0.325***	0.343***
	(0.115)	(0.113)	(0.112)
Δ Income	0.307***	0.296**	0.302***
	(0.113)	(0.112)	(0.111)
Δ Num. Estab.	0.162**	0.233***	0.193***
	(0.066)	(0.078)	(0.070)
Δ Empl.	0.140**	0.092	0.119*
	(0.068)	(0.065)	(0.061)
Δ Pay	0.224**	0.282***	0.249***
	(0.092)	(0.091)	(0.084)
State FE	Yes	Yes	Yes
Population weights	Yes	Yes	Yes
R^2	0.371	0.108	0.153
N	3,014	3,014	3,014

Note: Dependent variable is change in log vehicle registrations, 2019 to 2024 average. We use the average 2021–2022 exposure. Chip exposure is $\sum_m \theta_{cm}^{2019} \times g_m$. All specifications include state fixed effects, population weights, and state-clustered standard errors. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

B Tracing the Evolution: Year-by-Year Effects on Fleet and Retail Registrations

To trace how the relationship between remote work and registration has evolved over time, we estimate the cross-sectional specification separately for each year from 2020 through 2025. Motivated by the evidence in section 4.1, we look at results for fleet and retail reg-

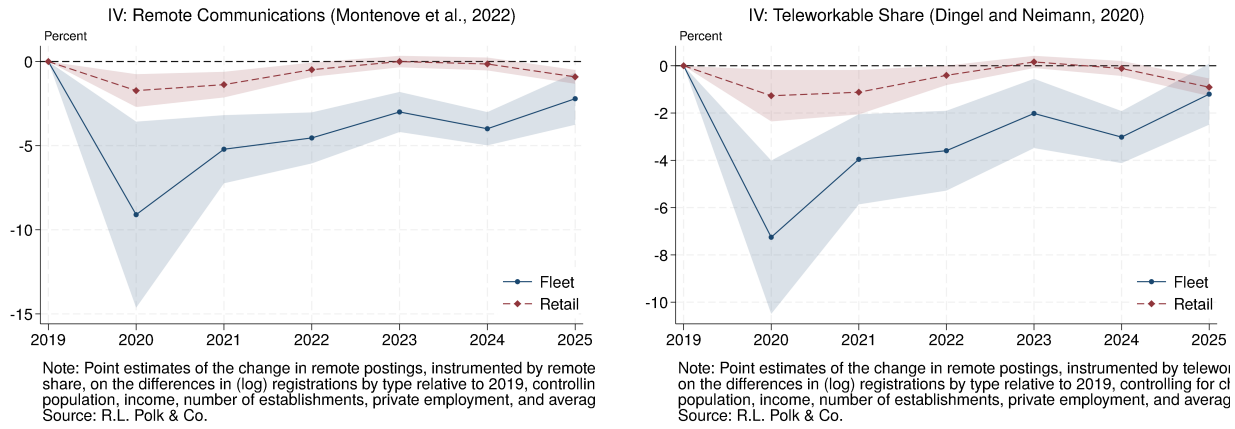
istrations and, within the fleet, at the split between the corporate and rental subchannels. For each year t, we regress the log change in registrations from 2019 to year t on the change in remote work share over the same period, instrumented with our two measures of remote ability. The regressions also control for the usual set of controls (changes in population, income, number of establishments, employment, average pay) and for state fixed effects.

Figure B.1 presents the year-by-year coefficients. The pattern reveals three distinct phases. The first phase was around 2021 when the coefficient estimates for fleet registrations remain elevated and somewhat larger than our baseline specification, likely reflecting the compounding effects of ongoing supply chain shortages that imposed additional negative pressures on vehicle procurement. In the second phase, as restrictions lifted and supply chains began normalizing in 2022, the coefficients moderate. Finally, by 2023 through 2025, the effects diverge sharply by registration type. The coefficients for fleet registrations stabilize at levels closely matching the long-difference estimate from Table 4, remaining large throughout (ranging from roughly -2.2 to -4.0) with no evidence of reversion toward pre-pandemic patterns. The retail coefficients, by contrast, fall to economically small magnitudes in 2023 and 2024, indicating that the transitory retail response observed during the acute pandemic period had largely dissipated.

The divergence we observe in Figure B.1 confirms that the fleet registration relationship had reached its new steady-state level, with no evidence of reversion toward pre-pandemic patterns, while the retail response proved largely transient. This trajectory accords with standard theories of durable goods adjustment to a permanent shock, with fleet registrations bearing the persistent impact of remote work adoption.

Within the fleet, the persistent response is a corporate-fleet phenomenon. Appendix C traces the corporate and rental subchannels year by year: the firm-ex-rental series settles at a stable, precisely estimated level of roughly -1.1 from 2022 onward, tracking the persistence of remote work, while the rental series is a statistical non-event dominated by the transitory travel and supply shocks of 2020.

Figure B.1: Dynamic Effect of Remote Work on Vehicle Registrations
Differences relative to 2019



Note: Each point shows the coefficient from a cross-sectional regression of the log change in registrations (2019 to year t) on the change in remote work share (2019 to year t), instrumented with measures of remote ability (remote communications in the left panel and teleworkability score in the right panel), controlling for changes in population, income, number of establishments, employment, average pay, and state fixed effects. All regressions are weighted by 2010 population. The shaded area represents 95 percent confidence intervals.

C Is the Fleet Effect a Rental-Car Story?

The firm results in Table 5 exclude rental registrations, but the raw data group rental together with firm transactions, and rental fleets are large enough that this grouping matters a great deal. This subsection makes the case for treating rental separately and shows that doing so leaves the commuting interpretation of the corporate- and government-fleet results intact, while the rental channel itself behaves in a way that is unrelated to remote work.

Table C.1 documents the levels that motivate the concern. In 2019, rental accounted for more than two million registrations so roughly three-quarters of the combined firm-plus-rental bucket and nearly half of all fleet registrations. By 2024, rental registrations fell 34 percent. Firm purchases, by contrast, were essentially flat over the same period (up 3.6 percent), and government registrations actually rose. A "firm fleet" category that lumps rental together with firm lease and purchase would therefore be dominated in both

level and change by rent-a-car fleets. Because rental fleets contracted for reasons plausibly distinct from commuting—the collapse in business and leisure travel, a major operator's 2020 bankruptcy and fleet liquidation, and the semiconductor shortage that fell heavily on fleet buyers—attributing that decline to remote work would conflate the commuting mechanism with a set of transitory travel and supply shocks.

Table C.1: Registrations by Institutional Category: Levels and Change, 2019–2024

Category	2019	2024	% change
Firm lease	135,123	107,768	−20.2
Firm purchase	535,413	554,599	+3.6
Rental	**2,050,335**	**1,352,015**	−34.1
Government	256,141	309,914	+21.0
Commercial	1,019,743	1,074,896	+5.4
Dealer/manufacturer	373,912	372,476	−0.4
Retail	13,132,146	12,352,444	−5.9

Note: National registration counts by transaction type, summed across counties, for calendar years 2019 and 2024. Rental accounts for 75 percent of the combined firm-plus-rental bucket and 47 percent of all fleet (non-retail) registrations in 2019. *Source:* R.L. Polk & Co.

Table C.2 addresses the concern directly by separating rental into its own category, so that true corporate fleets (firm lease and purchase, excluding rental) and rental companies can be read side by side in the same specification used for Table 5. The government and commercial channels are unaffected by the split (neither contains rental) and are omitted here to avoid repeating Table 5. Two results are decisive. First, the true corporate-fleet effect survives: firm-ex-rental registrations fall significantly, with an OLS coefficient of −0.58 and IV estimates of −1.01 to −1.05, negative, precisely estimated, and comparable in magnitude to the government channel. Second, the rental channel shows no commuting relationship at all: its coefficients are statistically insignificant and, if anything, wrong-signed, and they are estimated on only 477 counties in which rental registrations are concentrated. The remote-work mechanism operates through employer and government fleet decisions, not through rent-a-car demand.

The same point holds at the aggregate level. Table C.3 re-estimates the fleet-versus-

Table C.2: Fleet Sub-Decomposition Separating Rental

	(1)	(2)	(3)	(4)	(5)	(6)
	Firm (ex-rental)			Rental		
	OLS	IV		OLS	IV	
		Rem. Comm.	Telew.		Rem. Comm.	Telew.
Δ Remote	-0.579***	-1.010**	-1.048**	4.822	1.918	11.383*
	(0.168)	(0.387)	(0.413)	(3.499)	(7.863)	(5.825)
Controls	✓	✓	✓	✓	✓	✓
First-stage F		138.9	213.5		53.1	89.2
R^2	0.426	0.107	0.105	0.186	0.051	0.034
N	3,005	3,005	3,005	477	477	477

Note: The dependent variable in all columns is the log change in registrations (2019 to 2024 average). Each column reports a separate regression of registrations on the change in remote work share, restricted to the indicated transaction type. The firm (ex-rental) columns replicate Table 5; the rental columns isolate rent-a-car (RENTAL) registrations, which are observed in only 477 counties. The government and commercial channels are omitted here as they are unaffected by the rental split and appear in Table 5. Columns 1 and 4 report OLS estimates; the remaining columns report IV estimates, instrumented within each pair with the remote-communication measure (Rem. Comm., Montenovo et al., 2022) in the first column and teleworkability (Telew., Dingel and Neiman, 2020) in the second. First-stage F is the Kleibergen-Paap Wald rk statistic. All specifications include state fixed effects, population weights, and state-clustered standard errors. Controls (the ✓ row) are the changes in population, the number of establishments, employment, average weekly pay, income, and heating- and cooling-degree days. In the firm (ex-rental) columns, population, establishments, average weekly pay, and income all enter positively and significantly, heating-degree days enters negatively and significantly, and employment and cooling-degree days are insignificant. In the rental columns, employment enters positively and significantly and cooling-degree days negatively and significantly; the remaining controls are insignificant, except heating-degree days, which is marginally significant in one IV column. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

retail decomposition of Table 4, adding a column that removes rental from the fleet definition. Excluding rental reduces the fleet IV coefficient from -3.73 / -2.85 (rental included, reproducing Table 4) to -1.23 / -0.80, while retail remains a precise zero. The fleet channel therefore continues to carry the entire aggregate response after rental is removed, but the magnitude of the fleet response is materially smaller once the mechanical contribution of the shrinking rental fleet is stripped out. We treat the rental-inclusive fleet estimate as the headline aggregate result, since it corresponds to the full institutional fleet, and read Table C.3 as bounding how much of that aggregate is attributable to the commuting-linked corporate and government fleets versus the separately-driven rental fleet.

Table C.3: Aggregate Fleet Response Excluding Rental

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Fleet (incl. rental)			Fleet (ex-rental)			Retail		
	OLS	IV		OLS	IV		OLS	IV	
		Rem. Comm.	Telew.		Rem. Comm.	Telew.		Rem. Comm.	Telew.
Δ Remote	-1.322***	-3.732***	-2.852***	-0.253	-1.230***	-0.804**	-0.055	0.012	0.011
	(0.341)	(0.449)	(0.507)	(0.231)	(0.389)	(0.332)	(0.064)	(0.189)	(0.153)
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓
First-stage F		163.3	271.7		163.3	271.7		163.4	271.7
R^2	0.117	-0.036	0.016	0.164	0.000	0.018	0.625	0.299	0.299
N	3,025	3,025	3,025	3,025	3,025	3,025	3,028	3,028	3,028

Note: The dependent variable in all columns is the log change in registrations (2019 to 2024). Columns 1–3 (fleet including rental) and columns 7–9 (retail) reproduce Table 4. Columns 4–6 redefine the fleet outcome to exclude rental (rent-a-car) registrations. Columns 1, 4, and 7 report OLS estimates; the remaining columns report IV estimates, instrumented within each pair with the remote-communication measure (Rem. Comm., Montenovo et al., 2022) in the first column and teleworkability (Telew., Dingel and Neiman, 2020) in the second. First-stage F is the Kleibergen-Paap Wald rk statistic. All specifications include state fixed effects, population weights, and state-clustered standard errors. Controls (the ✓ row) are the changes in population, income, and heating- and cooling-degree days. Among these, income enters positively and significantly in every column; the change in population is positive and significant only in the retail columns; cooling-degree days is positive and marginally significant in the fleet-ex-rental columns; heating-degree days is insignificant throughout. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Finally, Table C.4 asks directly what the cross-sectional rental decline actually loads on. On the rental sample we regress the log change in rental registrations on candidate drivers: teleworkability (the commuting mechanism), business-travel geography (metropolitan status and transit ridership), and chip-shortage exposure. Entered alone, teleworkability is marginally significant and, consistent with the null commuting relationship in Table C.2, wrong-signed (column 1). But it loses significance once business-travel geography and chip exposure are included (column 4), while chip exposure enters positively and significantly (column 4) and dominates the remote-postings measure when the two are raced directly (column 6). In short, the rental decline tracks supply-side and travel shocks, not the commuting margin that drives the corporate-fleet result.[22]

[22] We proxy business-travel geography with metropolitan status and transit ridership. The horse-race specifications are reduced-form (registration changes regressed on exposure measures directly) and estimated on the small rental sample, so the coefficients are imprecise; we read them as indicating which driver survives conditioning rather than as precise elasticities.

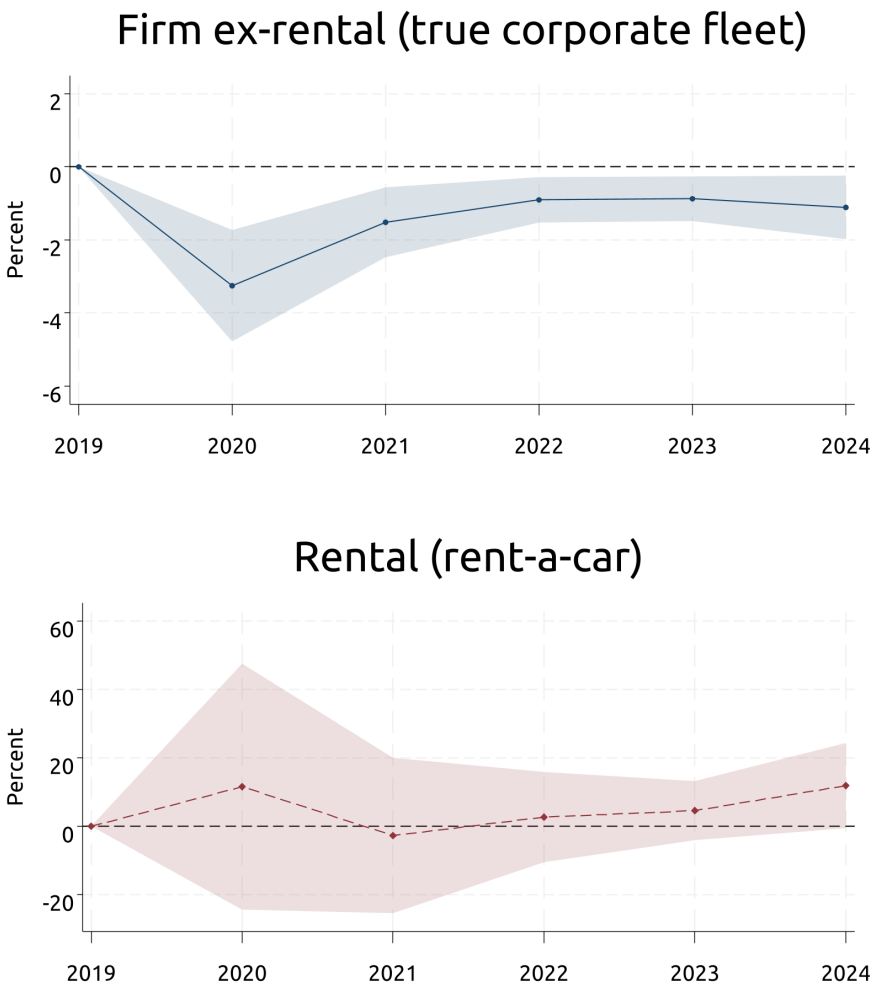
Table C.4: What Drives the Rental Decline? A Horse Race

	(1) Telework only	(2) Geography only	(3) Chip only	(4) Horse race	(5) Postings only	(6) Postings + chip
Teleworkability	6.695*			5.612		
	(3.467)			(3.812)		
Metro		-0.740		-0.821		
		(0.735)		(0.756)		
Transit (log)		0.011		-0.048		
		(0.032)		(0.036)		
Chip exposure			12.796	14.354*		9.509
			(7.977)	(8.426)		(6.976)
Δ Remote postings					4.527	3.221
					(2.726)	(2.265)
R^2	0.152	0.149	0.151	0.173	0.150	0.155
N	480	345	480	345	480	480

Note: Reduced-form regressions of the log change in rental (rent-a-car) registrations, 2019 to 2024, on candidate drivers, estimated on the rental sample. Teleworkability is the county Dingel-Neiman share; Metro is an indicator for metropolitan counties (rural-urban continuum code ≤ 3); Transit is log transit ridership; Chip exposure is the county's cumulative semiconductor-shortage exposure. All columns control for changes in population and income and include state-clustered standard errors. The sample falls to 345 in specifications that include transit ridership, which is missing for non-metropolitan counties. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

The year-by-year lens of Section B sharpens the same distinction: within the fleet, the persistent response is entirely a corporate-fleet phenomenon. Figure C.1 traces the two firm subchannels separately. The true corporate-fleet series (firm ex-rental, top panel) settles at a stable, precisely estimated level of roughly -1.1 from 2022 onward, tracking the persistence of remote work. The rental series (bottom panel), by contrast, is a statistical non-event: a large, wrong-signed spike in 2020 with enormous confidence intervals that reverts toward zero thereafter, the signature of the transitory travel and supply shocks that hit rental fleets, not of a durable commuting channel.

Figure C.1: Dynamic Effects: Corporate Fleet (ex-rental) versus Rental
Differences relative to 2019



Note: Year-by-year Teleworkability-IV coefficients from regressions of the log change in registrations relative to 2019 on the change in remote work share, estimated separately for firm-ex-rental (top panel, own vertical scale) and rental (bottom panel, own vertical scale). Each series is plotted on its own scale because the rental confidence intervals are an order of magnitude wider than those for the corporate fleet. Shaded bands are 95 percent confidence intervals from state-clustered standard errors. The corresponding figure using the remote-communication instrument is nearly identical and is reported in Appendix Figure F.1. *Source:* R.L. Polk & Co.

D The Industry-Composition Channel

Data. To probe the industry-composition channel behind the fleet decline (Section 4.1 in the main text), we assemble two additional pieces of data. First, we measure each county's pre-pandemic industry mix using 2019 county-by-industry employment from the Census Bureau's County Business Patterns (CBP), which reports employment by three-digit NAICS industry for essentially all U.S. counties; we cross-check these shares against the Bureau of Labor Statistics' Quarterly Census of Employment and Wages (QCEW) by-area files, which cover the same universe of private establishments. Second, we construct an industry-level teleworkability measure by aggregating the Dingel and Neiman (2020) occupation (SOC) teleworkability scores to the industry level, weighting occupations by their national industry employment from the 2019 Occupational Employment and Wage Statistics (OEWS) industry-by-occupation matrix. This procedure reproduces Dingel and Neiman's own industry aggregation and yields, for each three-digit NAICS industry, the employment-weighted share of jobs that can be performed at home. Mirroring our treatment of rental registrations, we exclude the vehicle rental and leasing industry (NAICS 532) from this industry measure.

If the fleet decline reflects firms and public agencies optimizing vehicle assets in response to remote work, it should be concentrated in places whose industry mix is more amenable to working from home, not simply in places that happen to have adopted remote work for idiosyncratic reasons. We test this implication directly with a shift-share design that isolates the industry-composition channel. For each county we construct a predicted exposure,

$$\text{Exposure}_c = \sum_i s_{ic}^{2019} \times T_i, \tag{D.1}$$

where s_{ic}^{2019} is the share of county c's 2019 employment in three-digit NAICS industry i (from County Business Patterns) and T_i is the industry's teleworkability, obtained by aggregating the Dingel and Neiman (2020) occupation scores to the industry level as described

above.[23] This exposure is an industry-based analogue of the paper's occupation-based instrument. It varies across counties only through their pre-pandemic industrial composition, and it is high where local employment is concentrated in industries whose work can be done from home.

Table D.1 reports the results. Column 1 shows that predicted telework exposure strongly predicts fleet registration declines. A one-standard-deviation increase in exposure is associated with a 6.5 percent larger fleet decline from 2019 to 2024, conditioning on the full control set and state fixed effects. Counties whose 2019 employment sat in more teleworkable industries saw systematically larger fleet contractions which is an independent confirmation of the mechanism using a completely different source of variation than the registration-type cuts above.

Columns 2 and 3 assess how much of the headline remote-work effect operates through this industry-composition channel. Entering the remote-work measure on its own in column 2 reproduces the large negative fleet coefficient. Adding predicted telework exposure (column 3) attenuates that coefficient from -1.79 to -1.00, a reduction of roughly 44 percent, with both terms remaining statistically significant. A large share of the reduced-form relationship between remote work and fleet registrations thus reflects counties being concentrated in teleworkable industries, precisely the margin along which the asset-optimization mechanism should operate. The result is robust to measuring industry shares from QCEW rather than County Business Patterns, which yields an essentially identical exposure coefficient and mediation share.[24]

[23] The shifter excludes the vehicle rental and leasing industry (NAICS 532), paralleling our treatment of rental registrations.

[24] The QCEW-based exposure coefficient is -0.068 and the remote-work coefficient attenuates to -1.06 when exposure is added, versus -1.00 in the CBP-based specification, both signidicant. County-level exposures built from the two sources are constructed on different establishment universes yet deliver the same conclusion.

Table D.1: The Industry-Composition Channel: Shift-Share Exposure and Fleet Registrations

	(1)	(2)	(3)
	Δ ln Fleet registrations, 2019–2024		
Predicted telework exposure (1 SD)	-0.065***		-0.047***
	(0.012)		(0.015)
Δ Remote		-1.792***	-0.999***
		(0.287)	(0.345)
Controls	Yes	Yes	Yes
State FE	Yes	Yes	Yes
R^2	0.145	0.138	0.152
N	3,006	3,011	3,006

Note: The dependent variable is the log change in fleet (all non-retail) registrations from 2019 to 2024. Predicted telework exposure is the county's 2019 employment-share-weighted average of three-digit-industry teleworkability, standardized to unit standard deviation; the industry shifter excludes the vehicle rental and leasing industry (NAICS 532). Column 1 is the reduced form on exposure; column 2 reports the fleet response to the remote-work measure alone; column 3 adds exposure to gauge how much of the remote-work coefficient operates through the industry-composition channel. All specifications include state fixed effects, 2010 population weights, state-clustered standard errors, and the full control set (changes in population, establishments, employment, pay, income, and heating- and cooling-degree days). Industry shares are from County Business Patterns 2019; a QCEW-based version delivers nearly identical estimates (see footnote). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

E Federal Fleet Segment Detail

This appendix reports the segment-level detail behind the aggregate federal fleet facts in Section 4.3. Within the federal fleet, the slowdown follows the gradient the mechanism predicts. Sedans, the vehicles most tied to office attendance, age three times as fast and shed five times the mileage of light trucks, whose operational use is largely unaffected. Table E.1 reports the full segment split and Figure E.1 traces the paths.

A noteworthy composition shift occurs such that the sedan inventory falls sharply while the passenger subtotal holds steady, reflecting a within-fleet substitution from sedans toward passenger SUVs that predates the pandemic and continues through it. The aging and mileage results are measured within segment and so survive the shift, and the passenger subtotal, which absorbs it, shows the same qualitative pattern as sedans alone.

Table E.1: Federal Fleet by Segment: Stock, Age, and Utilization, 2019–2024

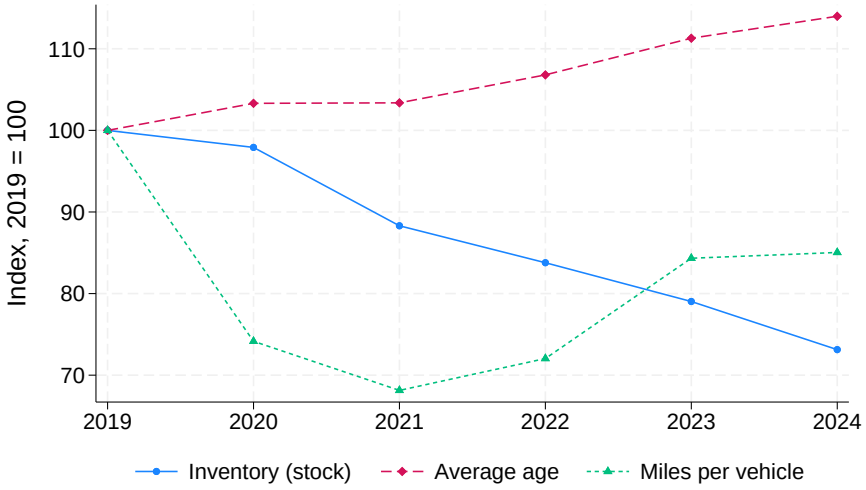
Quantity	Segment	2019	2024	% change
Average age (years)	Sedan	6.8	7.8	+14.0
	Passenger sub.	6.3	6.8	+8.9
	Light truck	6.2	6.3	+3.1
Miles per vehicle	Sedan	9,125	7,760	−15.0
	Passenger sub.	8,910	7,824	−12.2
	Light truck	8,239	7,990	−3.0
Inventory (stock)	Total Civ. Agencies	245,950	248,155	+0.9
	Passenger sub.	146,694	144,517	−1.5
	Light truck	51,270	53,529	+4.4
	Sedan	50,741	37,108	−26.9
Disposals	Total Civ. Agencies	29,927	24,337	−18.7
Acquisitions	Total Civ. Agencies	29,006	28,422	−2.0

Note: All rows cover the Total Civilian Agencies aggregate of the GSA Federal Fleet Report for fiscal years 2019 and 2024, which excludes the Department of Defense and other non-civilian agencies. Inventory is the year-end stock from Table 2-1, average age from Table 2-9, miles per vehicle from Table 4-2, and acquisitions and disposals from Tables 5-4 and 5-5. The acquisition and disposal counts are single-year flows and are not expected to reconcile arithmetically with the five-year change in inventory, because the report records agency-owned acquisitions and disposals while the inventory also reflects GSA-leased and commercially leased vehicles that enter and leave without an agency purchase or disposal, as well as reclassifications across years. Cycle lengthening predicts a roughly flat stock, rising average age, falling miles per vehicle, and falling disposals, with passenger segments moving more than light trucks, which is the pattern observed here. The decline in sedan inventory alongside a flat passenger subtotal reflects a within-fleet shift from sedans toward passenger SUVs rather than a shrinking passenger fleet. These are descriptive time series and not estimated coefficients. They corroborate the county instrumental-variable estimates rather than replacing them.

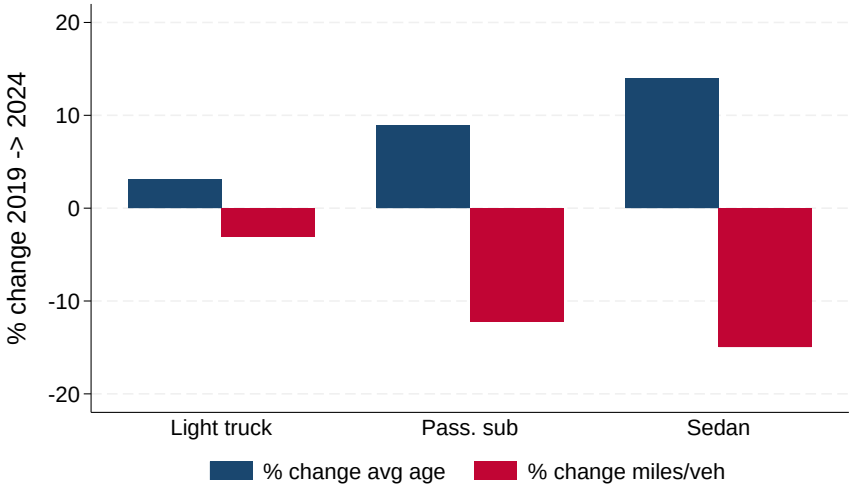
The OPM roster is the first wide enough to estimate the relationship separately by segment, and the cross-agency gradient runs opposite to the within-fleet gradient of Table 6. Light trucks carry the largest coefficients rather than the smallest, on both inventory and miles per vehicle. This is composition rather than contradiction. High-telework agencies are desk agencies such as Social Security, Treasury, and Education, which operate structurally small fleets of every type, while low-telework agencies are field agencies such as Interior and Agriculture, whose fleets are truck-heavy by mission. Across agencies, telework correlates with few trucks because of what those agencies do, not because remote

Figure E.1: Federal Fleet Ages and Slows

Panel A. Federal sedans: stock flat, age rising, miles falling



Panel B. Passenger vehicles age faster and lose more miles than trucks



Note: Total Civilian Agencies, GSA Federal Fleet Report. Panel A shows inventory, average age, and miles per vehicle for federal sedans, indexed to 2019 = 100. Panel B shows the percent change from 2019 to 2024 in average age and miles per vehicle by segment. Fiscal year 2025 is excluded, as described in Section 2.

work idles trucks. The commuting-versus-operational gradient the mechanism predicts is identified within a single fleet over time, where Table 6 shows it cleanly. We therefore read

the total-fleet rows as the corroboration and the segment split as descriptive of agency composition. The county registration data agree with the within-fleet ranking. Restricting the Polk file to government fleet transactions, the post-2020 decline is largest for sedans and smallest for pickups and vans, the same ordering as the federal aging and mileage series. We do not report those estimates, since government registrations are a small share of county totals and the segment cells are thin.

Table E.2: Remote Work and Fleet Quantity by Segment, OPM Telework Measure

	OPM
Panel A. Sedans	
Log inventory	-4.765^{***}
	(0.317)
Log miles per vehicle	-0.681^{***}
	(0.194)
Panel B. Light trucks	
Log inventory	-6.293^{***}
	(0.726)
Log miles per vehicle	-1.433^{***}
	(0.333)
Years	FY2019–24
Agencies	37
N	209

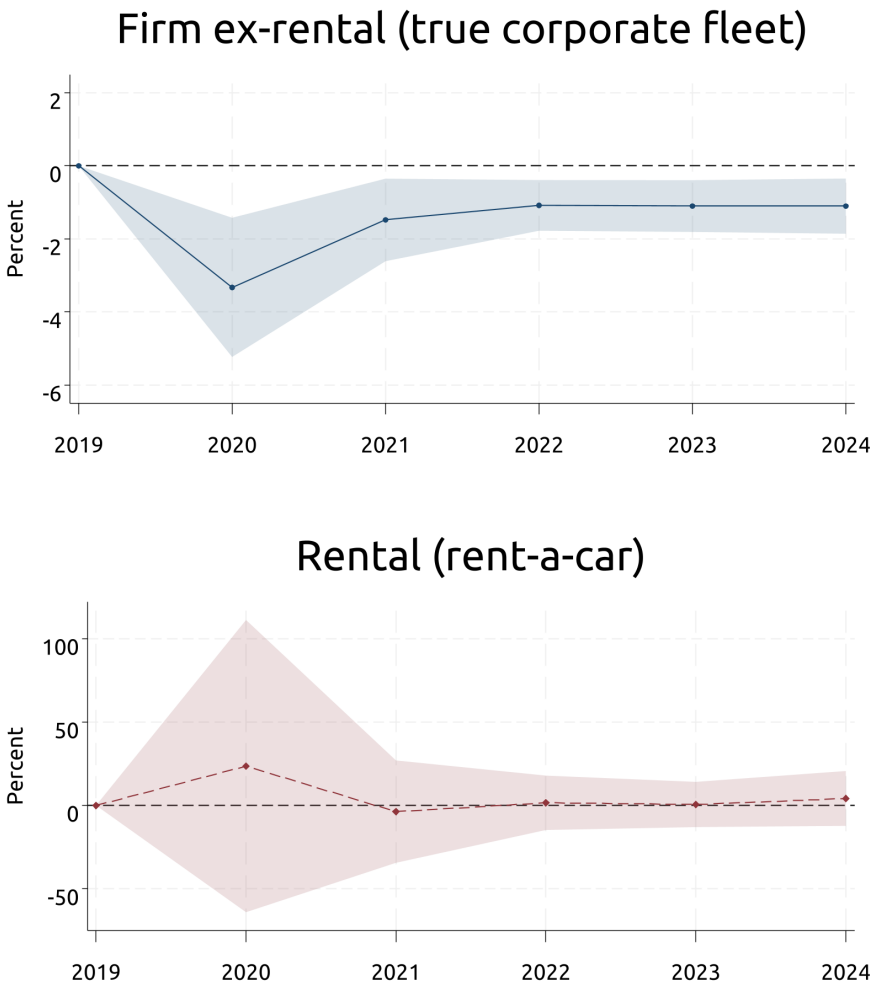
Note: Segment-level counterpart to Table 7 in the main text, estimated on the OPM administrative telework panel; the FEVS roster is too narrow to support the segment split. Each cell is a separate regression of the log of a federal agency's segment fleet outcome on that agency's telework share, pooling agency-years with year fixed effects, agency-employment weights, and heteroskedasticity-robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

F Additional Results

F.1 Full Control Coefficients for the Fleet Decomposition

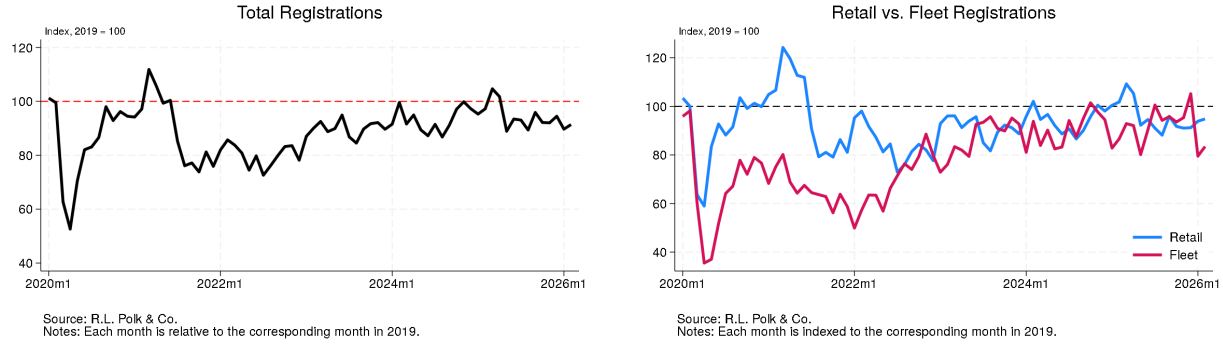
Table F.2 reports the complete set of control coefficients underlying Table 5 in the main text, which collapses the control block into a single "Controls" indicator for readability.

Figure F.1: Dynamic Effects: Corporate Fleet (ex-rental) versus Rental, Remote-Communication IV
Differences relative to 2019



Note: Remote-communication-IV counterpart to Figure C.1. Year-by-year IV coefficients from regressions of the log change in registrations relative to 2019 on the change in remote work share, estimated separately for firm-ex-rental (top panel, own vertical scale) and rental (bottom panel, own vertical scale). Each series is plotted on its own scale because the rental confidence intervals are an order of magnitude wider than those for the corporate fleet. Shaded bands are 95 percent confidence intervals from state-clustered standard errors. *Source:* R.L. Polk & Co.

Figure F.2: Outlook for Light Vehicle Registration



The remote-work coefficients, first-stage F statistics, R^2, and sample sizes are identical to those in the main-text table.

F.2 Pre-Period Economic Structure

A concern for the exclusion restriction is that our shift-share instruments, built from 2018 occupational composition, might proxy for pre-pandemic economic characteristics that independently shape vehicle demand growth. Counties concentrated in teleworkable occupations tend to be richer and more educated, and if such places were on differential registration trajectories for reasons unrelated to remote work, the instrument would be invalid. We probe this directly by conditioning on a county's pre-pandemic (2019) level of median household income, drawn from the 2015–2019 five-year American Community Survey (table B19013).[25]

Table F.3 reports the result. Columns 1 and 3 reproduce the baseline Rem. Comm. and Teleworkable IV estimates; columns 2 and 4 add the 2019 log income level. Both coefficients

[25] We focus on baseline income because it is the standard confounder of interest and preserves a strong first stage. Other candidate pre-period levels, such as the college-educated share, log population density, and the log 2019 registration level, are too collinear with the shift-share instruments to enter as controls: because the instrument's identifying variation is itself cross-sectional, partialling out these levels mechanically absorbs the instrument, collapsing the Kleibergen–Paap first-stage F from roughly 278 to between 12 and 30 individually (and to below 4 jointly) and rendering the resulting estimates uninterpretable weak-instrument artifacts rather than informative robustness checks.

survive: the Rem. Comm. estimate is essentially unchanged (-0.784 versus -0.779), and the Teleworkable estimate attenuates only modestly (-0.485 versus -0.591), remaining negative and well within the baseline confidence interval. The first stages stay strong, and the pre-period income level itself enters with a small coefficient. The remote-work effect is therefore not an artifact of counties' pre-existing income levels.

F.3 Exposure-Robust Standard Errors

Our baseline inference clusters standard errors at the state level. Because our instruments are shift-share (Bartik) measures, an alternative concern is that inference should account for the correlation structure induced by shared exposure to common occupation-level shocks (Adão et al., 2019; Borusyak et al., 2022). We therefore recompute the standard error on the Teleworkable 2SLS estimate using the exposure-robust procedure of Borusyak et al. (2022), which aggregates the county-level estimating equation to the level of the 415 underlying occupation shocks and conducts inference treating the shocks as the unit of quasi-random assignment.[26]

Table F.4 reports the comparison. The exposure-robust standard error (0.140) is, if anything, slightly smaller than the state-clustered standard error (0.155), so the t-statistic rises from 3.82 to 4.24. The estimate is thus robust to the shift-share inference critique: accounting for the shock-level correlation structure does not weaken, and marginally strengthens, the statistical significance of the effect.

[26] We implement this with the `ssaggregate` command. As a validation, the occupation-level 2SLS recovers the identical structural coefficient on remote work (-0.591), confirming that the shares and shocks exactly reproduce the county-level instrument; only the standard error differs.

Table F.1: Remote Work and Vehicle Demand: 2019 vs. 2023-2025

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS		Reduced Form		First Stage		2SLS	
	Δ ln Regs				Δ Remote Post.		Δ ln Regs	
Δ Remote	-0.323***	-0.300**					-0.748***	-0.523***
	(0.111)	(0.117)					(0.167)	(0.145)
Δ Population		0.520***	0.595***	0.572***	-0.135***	-0.125***	0.494***	0.507***
		(0.087)	(0.097)	(0.096)	(0.038)	(0.036)	(0.088)	(0.086)
Δ Income		0.441***	0.448***	0.448***	0.026	0.011	0.468***	0.454***
		(0.098)	(0.099)	(0.100)	(0.048)	(0.041)	(0.099)	(0.098)
Rem. Comm. IV			-0.370***		0.494***			
			(0.088)		(0.039)			
Teleworkable IV				-0.332***		0.635***		
				(0.095)		(0.039)		
First-stage F					165	267		
R^2	0.272	0.368	0.369	0.367	0.312	0.389	0.120	0.137
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
2010 Pop. W.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	3,015	3,015	3,015	3,015	3,015	3,015	3,015	3,015

Note: Dependent variable is the log change in average vehicle registrations between 2023-2025 and 2019, except in Columns 5-6 where it is the change in the share of remote job postings. Columns 1-2 report OLS estimates. Columns 3-4 present the reduced-form regression of log registrations directly on measures of remote work ability, the Bartik instruments, bypassing any measure of realized remote work. Columns 5-6 report the first-stage regression of actual remote work change on the two Bartik instruments. Columns 7-8 report two-stage least squares, instrumented with the remote-communication (Column 7) and teleworkability (Column 8) measures, respectively; within the reduced-form (Columns 3-4) and first-stage (Columns 5-6) pairs the instrument in use is the one whose coefficient row is populated. The Bartik instruments are the pre-pandemic county employment share in each occupation (from ACS 2014-2018 PUMS) weighted by occupation-level measures of remote ability (Rem. Comm. IV based on Montenovo et al. (2022) and Teleworkable IV based on Dingel and Neiman (2020)), using the characteristics from the 2018 O*NET survey. All columns also include the change in heating and cooling degree days to control for differences in weather patterns. All specifications use 2010 population weights. Standard errors clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table F.2: Remote Demand and Fleet Registrations: A Deeper Dive (Full Coefficients)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Δ ln Regs								
	Government			Firm (ex-rental)			Commercial		
	OLS	IV		OLS	IV		OLS	IV	
Δ Remote	-1.871***	-2.257***	-2.133***	-0.579***	-1.010**	-1.048**	0.075	-0.882	-0.113
	(0.592)	(0.804)	(0.789)	(0.168)	(0.387)	(0.413)	(0.283)	(0.578)	(0.537)
Δ Population	-0.977	-1.011	-1.000	0.486***	0.454**	0.451**	0.000	-0.075	-0.014
	(0.614)	(0.605)	(0.597)	(0.177)	(0.171)	(0.172)	(0.322)	(0.295)	(0.305)
Δ Income	0.706*	0.717*	0.714*	0.325**	0.337**	0.338**	-0.023	0.004	-0.018
	(0.417)	(0.409)	(0.412)	(0.147)	(0.152)	(0.154)	(0.228)	(0.246)	(0.233)
Δ Num. Estab.	0.472	0.528*	0.510*	0.257**	0.317**	0.323**	-0.155	-0.019	-0.128
	(0.300)	(0.305)	(0.281)	(0.106)	(0.139)	(0.132)	(0.218)	(0.229)	(0.221)
Δ Empl.	0.909*	0.870*	0.882*	0.132	0.091	0.087	0.518*	0.424	0.500
	(0.484)	(0.492)	(0.499)	(0.177)	(0.184)	(0.193)	(0.305)	(0.319)	(0.328)
Δ Pay	-0.473	-0.442	-0.452	0.296*	0.327**	0.329*	1.014***	1.085***	1.028***
	(0.393)	(0.406)	(0.402)	(0.155)	(0.159)	(0.167)	(0.361)	(0.372)	(0.369)
First-stage F		118.9	182.6		138.9	213.5		136.7	210.7
R^2	0.142	0.060	0.060	0.426	0.107	0.105	0.116	0.011	0.018
N	2,540	2,540	2,540	3,005	3,005	3,005	2,837	2,837	2,837

Note: Full-coefficient version of Table 5. Each column reports a separate regression of the log change in registrations (2019 to 2024 average) on the change in remote work share, where registrations in each column are restricted to the indicated transaction type. The firm category comprises firm lease and firm purchase registrations and excludes rental (rent-a-car) registrations. Columns 1, 4, and 7 report OLS estimates; the remaining columns report IV estimates using the two Bartik instruments. First-stage F is the Kleibergen-Paap Wald rk statistic. Sample sizes vary because some registration types are not present in all counties. All specifications include state fixed effects, population weights, controls for the change in the number of establishments, employment, average weekly pay, income, and heating- and cooling-degree days, and state-clustered standard errors. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table F.3: Robustness: Conditioning on Pre-Period (2019) Income

	(1)	(2)	(3)	(4)
	Δ ln Regs			
	Rem. Comm. IV		Teleworkable IV	
Δ Remote	-0.779***	-0.784***	-0.591***	-0.485**
	(0.186)	(0.262)	(0.155)	(0.206)
Δ Population	0.459***	0.458***	0.469***	0.499***
	(0.111)	(0.115)	(0.109)	(0.114)
Δ Income	0.495***	0.495***	0.484***	0.489***
	(0.100)	(0.101)	(0.100)	(0.101)
ln Income 2019		0.001		-0.016
		(0.022)		(0.020)
Weather controls	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Population weights	Yes	Yes	Yes	Yes
Kleibergen–Paap F	168.3	54.0	278.4	96.5
N	3,014	3,014	3,014	3,014

Note: Dependent variable is the change in log vehicle registrations, 2019 to 2024 average. Columns 1 and 3 reproduce the baseline Rem. Comm. and Teleworkable IV estimates from Table 2; columns 2 and 4 add the 2019 log median household income level (2015–2019 five-year ACS, table B19013). All specifications include changes in heating and cooling degree days, state fixed effects, population weights, and state-clustered standard errors. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table F.4: Exposure-Robust Inference for the Shift-Share IV

	Coefficient	Std. Error	t
State-clustered (baseline)	-0.591	0.155	-3.82
Exposure-robust (Borusyak et al., 2022)	-0.591	0.140	-4.24

Note: Coefficient on the change in remote work in the Teleworkable 2SLS specification (Table 2, column 8), instrumenting with the Dingel–Neiman teleworkability Bartik measure built from 415 occupation shocks and 2018 county employment shares. The state-clustered row is the baseline estimate; the exposure-robust row applies Borusyak et al. (2022) via occupation-level aggregation. The Kleibergen–Paap first-stage F for the canonical specification is 278.4. Both rows share the same point estimate by construction.

G Bartik Instrument Construction

This appendix provides a detailed description of the construction of the Bartik shift-share instrument used in our instrumental variable analysis. The instrument predicts county-level remote work adoption based on pre-pandemic occupational composition, exploiting the insight that counties with occupational structures concentrated in teleworkable jobs were better positioned to adopt remote work during and after the pandemic.

G.1 Instrument Definition

The Bartik instrument for county c is defined as:

$$Z_c = \sum_{o=1}^{O} s_{oc}^{\text{pre}} \times T_o \tag{G.1}$$

where s_{oc}^{pre} is the employment share of occupation o in county c from the ACS 2014–2018 five-year estimates (the "share" component), and T_o is the teleworkability score for occupation o (the "shift" component). The instrument measures the ex-ante remote work potential of a county based purely on its pre-pandemic occupational mix and national occupation-level teleworkability.

G.2 Data Sources

Occupational Employment Shares. We obtain county-level occupational employment from the U.S. Census Bureau's American Community Survey (ACS) Public Use Microdata Sample (PUMS) for the 2014–2018 five-year estimates. The PUMS provides individual-level records with detailed occupational codes and Public Use Microdata Area (PUMA) geography.

Remote work ability Scores. We use the occupation-level remote work ability classification developed by Dingel and Neiman (2020) and Montenovo et al. (2022). Both classifications rely on task requirements from the Department of Labor's O*NET database to determine which occupations have the ability to work at home. Dingel and Neiman (2020) focus on physical aspects of jobs, while Montenovo et al. (2022) identify remote communication tools. We construct remote ability scores at the six-digit Standard Occupational Classification (SOC) level from the 2018 O*NET survey, which predates our sample period.

G.3 Construction Steps

Step 1: Geographic Concordance. The ACS PUMS provides worker locations at the PUMA level, while our analysis requires county-level employment shares. PUMAs are statistical areas designed to contain approximately 100,000 people and do not always align with county boundaries. We construct a PUMA-to-county crosswalk using the Census Bureau's 2010 Census Tract-to-PUMA relationship file combined with 2010 ACS tract-level population data.

For each PUMA p, we compute the share of its 2010 population residing in each county c:

$$w_{pc} = \frac{\text{2010 population in tracts of PUMA } p \text{ located in county } c}{\text{Total 2010 population in PUMA } p} \tag{G.2}$$

We then allocate each worker in PUMA p with occupation o to counties using these 2010 population weights. If worker i resides in PUMA p and works in occupation o, we assign w_{pc} workers to county c for occupation o. Using 2010 population for allocation ensures the geographic weights are contemporaneous with the employment data while remaining pre-COVID and thus exogenous to pandemic outcomes.

Step 2: Occupation Coding. The ACS PUMS uses Census occupation codes, which we map to the SOC system used by Dingel and Neiman (2020). We implement a cascading

merge strategy to maximize coverage:

1. First, attempt to match at the six-digit SOC level (most detailed).
2. For unmatched observations, aggregate to the five-digit SOC level and match.
3. For remaining unmatched observations, aggregate to the two-digit SOC level (major occupation groups) and match.

This procedure yields successful matches for 80.2 percent of employment at the detailed (six- or five-digit) level. The remaining 19.8 percent are matched at the two-digit level or assigned the occupation-group average teleworkability score.

Step 3: Employment Share Calculation. For each county c, we compute the employment share of each occupation o using the 2015–2019 five-year ACS:

$$s_{oc}^{\text{pre}} = \frac{E_{oc}^{\text{pre}}}{\sum_{o'} E_{o'c}^{\text{pre}}} \tag{G.3}$$

where E_{oc}^{pre} is total employment in occupation o in county c from the ACS 2015–2019 PUMS, computed by summing across all workers (after geographic allocation from Step 1) and applying ACS person weights. This gives a vector of O employment shares for each county, where O ranges from 400 to 524 distinct occupations depending on the county's size and industrial diversity.

Step 4: Instrument Construction. We compute the Bartik instrument by taking the inner product of the county's occupational employment share vector and the national vector of measures of remote work ability:

$$Z_c = \sum_{o=1}^{O} s_{oc}^{\text{pre}} \times A_o \tag{G.4}$$

This yields a county-level measure ranging from 0 to 1, representing the share of the county's pre-pandemic employment in occupations that can be performed remotely. The instrument varies across counties purely due to differences in pre-pandemic occupational composition, as the remote work ability scores A_o are constant across counties.

G.4 Interpretation and Variation

The Bartik instrument has a natural interpretation. It measures the fraction of a county's pre-pandemic (2014–2018) workforce in teleworkable occupations. Counties with high values have occupational structures concentrated in industries like professional services, information technology, and finance, where many jobs can be performed remotely. Counties with low values have structures concentrated in industries like manufacturing, retail trade, and food services, where physical presence is required.

In our sample, the Bartik instruments have a mean around 0.3, or around 30 percent of employment in occupations with the ability to work remotely. The instrument is positively correlated with urban status, education levels, and income, consistent with the concentration of knowledge-economy jobs in metropolitan areas.

G.5 Within-Metro Reallocation of Fleet

The decomposition by registration type tells us which vehicles account for the decline, but not what becomes of them. Registrations are recorded in the county of the owner, so a fleet vehicle re-based across a county line leaves one county's count and enters another's. We can therefore ask whether the vehicles that disappear from high-remote-work counties leave the road or simply move.

The question is not merely mechanical. Nationally, government registrations rose 21 percent between 2019 and 2024 and firm purchases were roughly flat (Appendix Table C.1), which sits uneasily with within-state estimates in which high-remote-work counties shed

fleet registrations. Spatial reallocation reconciles the two.

We test for it by replacing state fixed effects with Core-Based Statistical Area (CBSA) fixed effects, so that identification comes only from comparisons among counties in the same metropolitan area, and then by re-estimating the same relationship on metropolitan totals.[27] The two coefficients separate the competing interpretations. If fleets are relocated within a metropolitan area, the within-CBSA coefficient is negative while the metro-total coefficient is zero. If fleets are genuinely retired, both are negative. Table G.1 reports the comparison separately by registration type, and the contrast across types is the result.

Fleet registrations display the reallocation pattern. Within a metropolitan area, a county with higher remote-work exposure than its neighbors loses fleet registrations under both instruments, with a point estimate of -3.0 for the remote-communication measure that is close to the -3.5 state-fixed-effects benchmark estimated on the identical metropolitan sample.[28] Collapsing to metropolitan totals, however, the relationship vanishes. Metro-wide fleet changes are unrelated to metro-wide exposure under either measure. Fleets are re-registered away from emptying office cores toward lower-exposure counties in the same metropolitan area rather than being retired, a fleet analogue of the residential donut documented by Ramani and Bloom (2021).

Retail registrations show no donut, and if anything the reverse. High-remote-work counties gain household registrations relative to their metropolitan neighbors, and metropolitan totals rise with exposure as well. Because retail accounts for roughly three-quarters of all registrations, the all-registrations row inherits the household pattern and the fleet reallocation is invisible in the aggregate. The donut is only visible once fleet and retail are separated.

[27] A CBSA is the Census Bureau's official metropolitan or micropolitan area, a county or group of counties anchored by an urban core and tied together by commuting. Because each CBSA is built from whole counties, it maps cleanly onto our county-level data. The within-metro comparison is identified off the counties belonging to multi-county CBSAs.

[28] The teleworkability estimate carries the same sign and a smaller magnitude but is imprecise, so we read the within-metro fleet result as suggestive on its own and rely on the contrast with the metro totals for the substantive point.

Table G.1: Within-Metro Reallocation by Registration Type: A Fleet-Specific "Donut"

	(1) Within-CBSA (IV)	(2)	(3) Metro total
	Rem. Comm.	Telew.	(RC / Telew.)
All registrations	0.31	0.31	0.50^{***} / 0.57^{***}
	(0.60)	(0.38)	
Retail (household)	1.01^{***}	0.63^{***}	0.74^{***} / 0.85^{***}
	(0.30)	(0.22)	
Fleet (non-retail)	-3.00^{*}	-1.55	-0.38 / -0.24
	(1.70)	(1.38)	
Fixed effects	CBSA	CBSA	—
N (within-CBSA counties)	1,208	1,208	
N (CBSA totals)			909

Note: Columns 1–2 report within-CBSA IV estimates: the county log change in registrations of the indicated type (2019–2024) on the change in remote-work share, instrumented with the remote-communication (Montenovo et al., 2022) and teleworkability (Dingel and Neiman, 2020) Bartik measures respectively, absorbing CBSA fixed effects, weighted by 2010 population, with controls for changes in population, income, and heating- and cooling-degree days, and CBSA-clustered standard errors. A negative within-CBSA coefficient means the type reallocates away from high-remote-work counties within a metro. Column 3 collapses the metropolitan sample to CBSA totals and regresses the metro-total log change on the metro's population-weighted mean exposure (remote-communication / teleworkability), with heteroskedasticity-robust standard errors; a coefficient near zero means metro totals do not move. The "donut" pattern—negative within-CBSA, flat metro total—appears only for fleet. Estimates for the fleet row match the state-fixed-effects benchmark (-3.47^{***}) on the identical metropolitan sample. Replication: `30_donut_by_regtype.do`. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

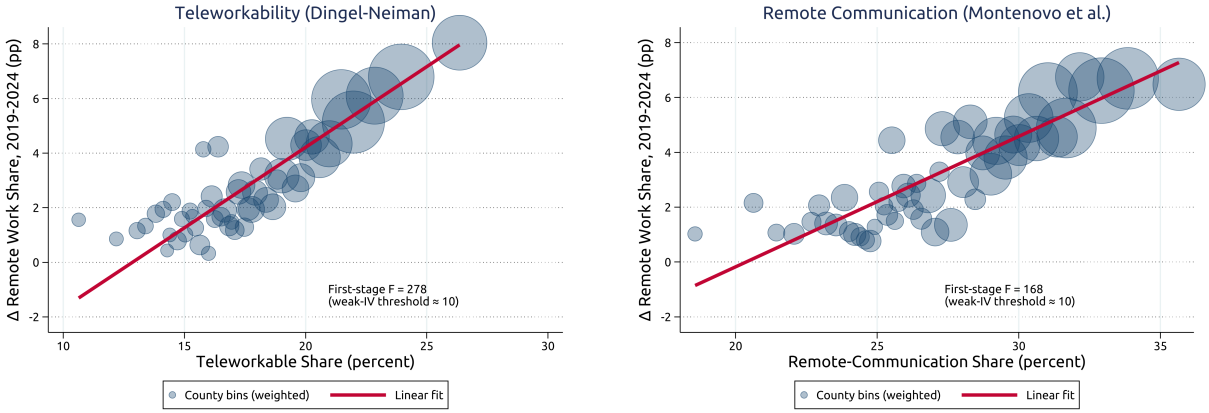
The positive retail pattern is not in tension with the residential donut. That literature concerns where households live, whereas registrations record whether households own vehicles. Remote work substitutes away from commuting rather than from car ownership, and to the extent that it pushes households toward less dense, more automobile-dependent locations it may raise ownership. The pattern also reflects composition. Remote-work exposure loads heavily on higher-income, more urban counties, with a population-weighted 2019 cross-sectional correlation between teleworkability exposure and log per-capita income of 0.75 (0.63 for the remote-communication measure), and exposure falls monotonically along the urban-rural continuum. These are precisely the counties in which households register more vehicles, so the household channel moves with exposure for reasons of income and

urban form rather than commuting.[29]

Taken together, the split refines the reading of the registration-type decomposition. Remote work reorganizes where firms and public agencies base commuting-oriented fleets, moving them within metropolitan areas without shrinking metropolitan totals, while household vehicle ownership is, if anything, slightly higher in the more exposed counties.

H Additional Figures and Tables

Figure H.1: First Stage: Occupational Remote-Work Ability Predicts Remote Work Adoption



Note: Each panel is a population-weighted binned scatter (50 equal-frequency bins) of the 2019–2024 change in the remote work share against a county's pre-pandemic remote-work-ability share: the Dingel and Neiman (2020) teleworkability measure (left) and the Montenovo et al. (2022) remote-communication measure (right). Marker area is proportional to total county population in each bin, and the line is the population-weighted linear fit. The annotated statistic is the Kleibergen–Paap first-stage F from the corresponding 2SLS estimating equation (Table 2). Both far exceed the conventional weak-instrument threshold of 10. Source: Lightcast remote work data, Dingel and Neiman (2020), and Montenovo et al. (2022).

[29] A one log-point increase in county per-capita income is associated with an 8.9 percentage-point higher teleworkable employment share. The most metropolitan counties average a 20.4 percent teleworkable share against roughly 16 percent in rural tiers. These are descriptive cross-sectional correlations rather than causal estimates. Source: authors' calculations from the county panel, replication file `31_telework_income_urban.do`.