

Working from Home and Consumption in Cities

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Abstract

We estimate the impact of the Covid-induced shift to working from home (WFH) on offline consumer spending within cities. The analysis builds on a postcode-level panel (2019–2023) of novel cellphone mobility and payment card transaction data for 50 German metropolitan areas (MAs). Identification uses local differences in the exposure to the WFH shock, measured by neighbourhoods' WFH potential: the fraction of residents with a teleworkable job. Difference-in-differences estimates show that higher WFH potential leads to persistent declines in morning mobility and higher local spending within MAs. We estimate an elasticity of spending with respect to WFH-induced mobility changes of -3.7%, driven by large MAs. Unlike in the US, the WFH shock did not spur business turnover, urban outmigration, or a sustained shift to e-commerce in Germany. We conclude that WFH primarily reshapes urban consumption by reallocating where people spend time.

Keywords: Remote Work; Consumer Spending; Urban Agglomerations; Cities; Spatial Analysis; Cellphone Mobility

JEL Codes: D1; J0; R1

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1 Introduction

The Covid-19 pandemic has disrupted traditional work organisation, inducing a sudden and lasting shift to working from home (WFH) worldwide (Hansen *et al.*, 2023; Barrero *et al.*, 2023). In Germany, WFH surged from below 10% before stabilising at 25% of employees after Covid restrictions were lifted in 2022 (Panel A, Figure 1). This shift has changed workplace mobility: Panel B of Figure 1 shows that the morning and evening rush hours register about 6% fewer trips. This reduction is consistent with the adoption of hybrid work schedules as the prevalent WFH model in most advanced economies (Aksoy *et al.*, 2022, 2023). Studies have linked the WFH shock to flattening house price gradients, lower neighbourhood crime, or the urban exodus of high-skilled individuals (Matheson *et al.*, 2024; Ramani *et al.*, 2024; Gupta *et al.*, 2022; Mondragon and Wieland, 2022; Althoff *et al.*, 2022; Coven *et al.*, 2023). These rapid changes sparked speculations that remote work may fundamentally alter the spatial distribution of economic activity in cities and even challenge their “survival” (Glaeser, 2022; Florida *et al.*, 2023; Glaeser and Cutler, 2021).

This paper presents new evidence on the geography of cities after the big shift to remote work: Using novel data covering geo-coded mobility and payment card transactions from 2019 to 2023, we estimate WFH’s impact on local consumer spending *within* 50 German metropolitan areas (MAs). Our setting allows us to go beyond the existing literature by establishing a causal link between WFH and shifts in consumer spending within agglomerations. The identifying variation comes from postcode-level differences in the exposure to the WFH shock. Specifically, we use a difference-in-differences (DiD) design that compares residential areas at a similar distance from the city centre but with different levels of *WFH potential*, i.e., the fraction of employed residents with a teleworkable job. Our key finding is that offline spending persistently shifts into residential neighbourhoods with higher WFH potential, away from central areas with high job density.

We contribute by providing the first causal evidence that links the predictions of recent spatial equilibrium models to observed changes in the geography of urban consumption. Monte *et al.* (2023) predict that large cities are particularly prone to persistent transitions to low-commuting equilibria after a coordinated shift to remote work. We corroborate this by showing that WFH-induced spending shifts are concentrated in larger metro areas. At the same time, our findings add nuance to the theoretical narrative. Structural models typically emphasize population resorting as the primary margin of spatial adjustment (Delventhal and Parkhomenko, 2023; Brueckner, 2024; Brueckner *et al.*, 2023; Gokan *et al.*, 2024). Instead, we demonstrate that WFH-driven

spending shifts cannot be explained by internal migration or business turnover between 2019 and 2023 in Germany. In this sense, our findings make an empirical contribution to the structural frameworks of [Davis et al. \(2024\)](#) and [De Fraja et al. \(2025\)](#), by offering causal evidence that WFH primarily reshapes urban consumption by reallocating where people spend their time.

Our analysis also underscores the role of institutional context. Unlike the US, where the WFH shock was followed by a lasting rise in e-commerce and an urban exodus ([Alcedo et al., 2024](#); [Coven et al., 2023](#); [Ramani et al., 2024](#)), we document that both phenomena were limited in Germany: online shopping reverted to pre-pandemic levels and population growth from 2019 to 2023 was parallel across high- and low-WFH potential areas. This aligns with [Monras \(2020\)](#), who emphasizes that internal migration responds slowly to (persistent) local shocks, particularly in regulated housing markets like Germany's. Further, European cities' concentrated and accessible amenities reduce remote workers' incentives to move compared to the US. Taken together, we highlight that urban adjustment under WFH may follow different paths across countries, depending on housing market frictions, city structure, or labour mobility.

We first establish that higher local WFH potential predicts stronger uptake of WFH (both intensive and extensive margin), as measured by a 2022 survey. Using newly available, geo-coded cellphone mobility data from Germany's largest carrier *Deutsche Telekom*, we also document a stronger and persistent decline in morning mobility in postcodes with a higher WFH potential, consistent with reduced commuting: A one percentage point (p.p.) higher WFH potential is associated with 0.16% fewer outbound trips on weekdays in 2023.

Second, we estimate intention-to-treat (ITT) effects of WFH on local offline consumer spending. Spending data are supplied by *Mastercard* and comprise daily transaction volumes aggregated from merchants' locations to the postcode level. The DiD estimates suggest that spending trends sharply diverged after the pandemic outbreak: During lockdown periods, spending declined in areas with higher relative to lower WFH potential. Lockdowns were marked by transitory spikes in online commerce due to mandatory business closures and a temporary "urban flight" coming from high-WFH potential areas. However, the pattern reversed as the economy recovered, with high-WFH areas persistently attracting more offline spending, and online shopping returning to the pre-pandemic level. Our estimates suggest a 0.77% increase in offline spending levels in 2023 for a one p.p. higher WFH potential.

Third, we estimate the elasticity of spending with respect to WFH-induced changes in mobility. To this end, we instrument 2019–23 changes in mobility with postcode-level WFH potential. Across different estimators (PPML versus OLS) and model specifications, we find that spending increases by 3.31% to 3.79% in response to a 1% decline in morning mobility. Our most demanding specification controls for postcodes’ distance to the city centre, changes in population size (2019–23), and 2019 measures of mobility, spending per capita, industry composition, and mobility in neighbouring postcodes. In a back-of-the-envelope calculation for Berlin, our estimate implies an annual spending increase of EUR 700 per capita in postcodes at the 75th relative to the 25th percentile of the WFH potential distribution.

Heterogeneity analyses reveal that the effects are driven by larger MAs, which are characterized by a higher share of teleworkable jobs. We find insignificant effects of WFH potential on mobility changes among the 35 smallest MA. This is consistent with [Monte et al. \(2023\)](#), who propose that weaker agglomeration relative to congestion forces leads smaller cities to revert to pre-Covid commuting patterns. Breaking down effects by spending category shows that WFH-induced spending shifts are strongest in the food service industry and grocery stores. We find null effects for spending in apparel stores, likely due to limited shopping opportunities in residential neighbourhoods.

We find no evidence that spending differences are driven by WFH-induced migration: Excess cross-county migration (2020–22) totalled 119 thousand, which is quantitatively negligible compared to 6 million employees transitioning to WFH after 2019.¹ Germany, like many advanced economies, saw a net urban population loss during the crisis. However, this is explained by a sharp decline in population inflows rather than an acceleration of outflows, consistent with models of internal migration ([Monras, 2020](#)). Migration patterns are bolstered by DiD results showing that relative mobility between high versus low WFH areas returned to pre-Covid levels on Saturdays. These results reinforce that weekday gaps are driven by work-related mobility and that the population has not systematically changed across areas. Similarly, we find that the number of initial (first-morning) pings only temporarily dropped in higher-WFH-areas during lockdowns, suggesting no persistent relocations. This is confirmed by administrative data at the municipality level. Together, the evidence does not support the popular narrative that remote work causes an urban exodus in Germany.

¹Counties (*Kreise and kreisfreie Städte*) correspond to the NUTS-3 level, which is geographically coarser than postcodes or municipalities (LAU-1 / LAU-2 level).

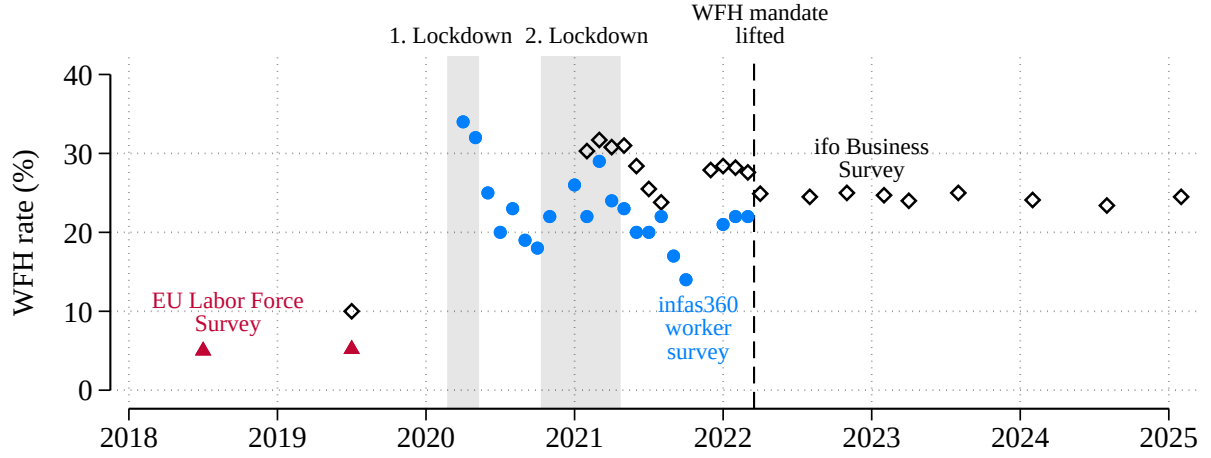
We explore whether the supply-side responds to the WFH shock using information from Bureau van Dijk’s Orbis database and administrative data on business notifications and insolvencies (*Gewerbeanzeigenstatistik*). Adopting our DiD framework, we find that firm entries and exits in non-tradable industries evolved similarly across areas with different WFH potential throughout our observation period. Consequently, changes in purchase opportunities are unlikely to explain WFH impact estimates. Instead, the effect is fully driven by remote workers’ shifting demand. These findings contrast with [Duguid et al. \(2023\)](#), who find that the number of establishments in large US cities grew in peripheral areas where remote workers tend to move to. Unlike the U.S., Germany saw no measurable remote-worker outmigration, no sustained shift to e-commerce, and prioritized direct business subsidies that effectively averted firm defaults during the pandemic ([BMWK, 2022](#)). These factors likely contribute to the contrasting patterns.

The final section focuses on workplace areas, defined as postcodes with high job density in 2019. These areas are closer to city centres, likely consumption hubs, and register higher evening rush hour outbound mobility. We find persistent spending declines in workplace areas mirrored by a decline in evening mobility. This is consistent with WFH shifting consumer spending away from workplaces into residential areas. These changes imply a reduction in spatial spending inequality within metro areas and relative spending gains in the city’s periphery.

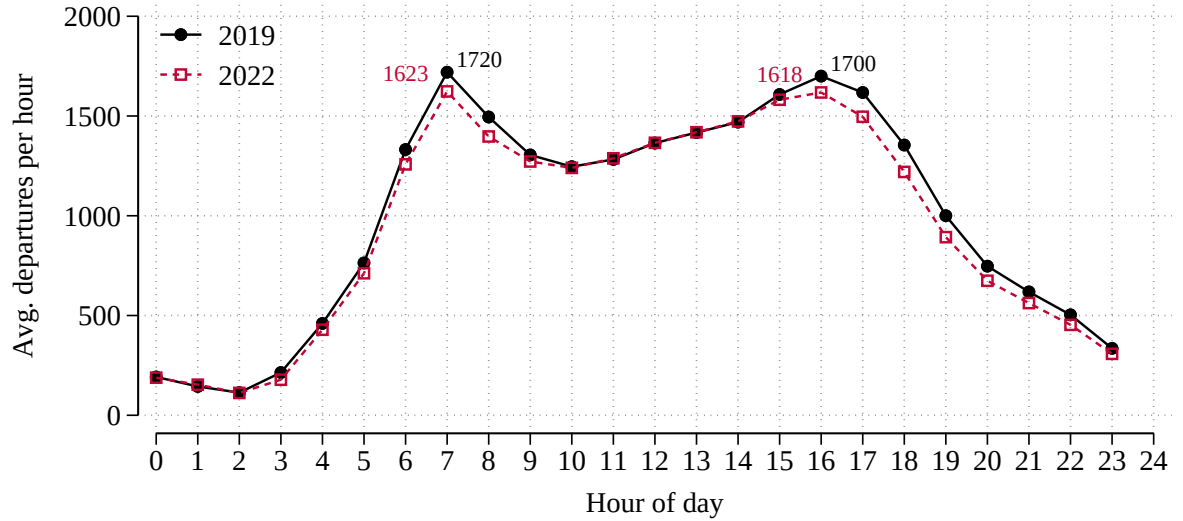
The article proceeds with a description of the main data sources in [Section 2](#). [Section 3](#) lays out our empirical strategy and its identifying assumptions. [Section 4](#) presents our main results. [Section 5](#) concludes.

Figure 1: The WFH shock in Germany

Panel A. % employees working from home (at least partly)



Panel B. Avg. number of outbound trips by postcode throughout the day (Mo-Fr)



Notes: Panel A reports the fraction of employees in Germany working from home at least one day per week based on surveys from the EU Labour Force Survey, the infas360 Casa Monitor Survey, and the ifo Business Survey. Panel B shows the average number of outbound trips by postcode and hour of day in 2019 and 2022. The data are based on cellphone pings from T-Systems by Deutsche Telekom (see [Section 2](#) for details).

2 Data on Consumer Spending, WFH, and Mobility

Sample. Our sample includes postcode-level observations covering all 50 German metropolitan areas and about 72% of the population. Metro areas are comparable to US Commuting Zones. They are centred around at least one urban core (i.e., a municipality with a population above 100,000) and extend to boundaries determined by 2019 commuting linkages with the core. MAs can have multiple cores if they are close

to each other and interconnected (e.g., MA Berlin-Potsdam or MA Cologne-Bonn).² We define a “city centre” as the postcode in the administrative centre of an urban core and calculate the Euclidean distance between each postcode and the (closest) city centre. [Figure A.1](#) maps the postcodes in Germany and highlights the MAs. The average postcode hosts 14 thousand residents. The smallest MA is Salzgitter with a total population of 160 thousand; the largest MA is Berlin-Potsdam with more than 5 million residents.

Card transaction data. We measure local offline consumer spending using anonymised and aggregated card transactions from *Mastercard Location Insights*. Offline spending refers to all card payments at brick-and-mortar establishments, such as restaurants and retail stores, cleared through the Mastercard network. This includes all payments at a physical point-of-sale (POS) with physical Mastercard credit, debit or Maestro cards as well as with their virtual counterparts (e.g., Apple Pay or Google Pay used with a smartphone or a smartwatch and a virtual Mastercard card in the background).³ Transactions are grouped by industry using the merchant category codes (MCC) classification. We observe daily spending volumes, aggregated from the POS to the postcode $\times$ industry level from January 2019 to September 2023.⁴ We exclude transactions from foreign cards to avoid distortions related to travel bans and tourism. Confidentiality restrictions mask values in day $\times$ industry $\times$ postcode cells with fewer than four transactions across less than four merchants. To minimise missing values, we focus on the “total spending” category in the main analysis, which contains the transaction volume across all MCC industries, and assume zero spending when values are masked. All our results are robust to excluding postcodes with missing values.

We also use data from *Mastercard Spending Pulse*, which provide daily national aggregates of spending volumes in brick-and-mortar stores versus e-commerce. The data cover all payment modes (including cash), providing a high-frequency measure of private consumption that aligns closely with official statistics ([Fourné and Lehmann, 2023](#)).⁵ The data can reveal shifts between offline and online spending and offer insight into how shopping behaviour changed through the crisis.

²The formal definition of German metropolitan areas (*Großstadtregionen*) is determined by the [Federal Institute for Research on Building, Urban Affairs, Spatial Development \(BBSR\)](#).

³Similar data at the US county level are used by [Mian et al. \(2013\)](#) to study the elasticity of consumption with respect to housing net worth.

⁴Absolute spending volume is divided by an unknown constant, preserving relative differences (over time and across space) within an industry but masking monetary values for data privacy reasons.

⁵Mastercard uses survey-based estimates for certain payment forms, such as cash and checks, to improve representativeness.

WFH survey. WFH measures come from an employee survey conducted by *infas360*, a company specialised in micro-geographic survey and data collection methods. We introduced WFH-related questions into the spring 2022 wave of the [infas360 CASA Monitor](#), a recurring representative survey of roughly 11,000 adults with internet access in Germany. We elicit whether respondents’ primary job *could* be done at home at least one day per week, as well as their current and pre-Covid WFH status. To break down WFH measures by postcode, *infas360* collapses WFH rates to the occupation level and then extrapolates to postcodes based on the local composition of employees’ occupations. This approach is akin to the procedures used by [Dingel and Neiman \(2020\)](#) and [Matheson et al. \(2024\)](#) for the US and the UK, respectively. We define the fraction of employed residents with a job that can (at least partly) be done from home as a postcode’s *WFH potential*. In [Appendix D.1](#), we show that county-level aggregates of this index closely align with alternative measures of regional WFH potential from [Alipour et al. \(2023\)](#) and [Dingel and Neiman \(2020\)](#) for Germany.

Cellphone mobility data. We obtain access to newly-available cellphone “ping” data provided by *T-Systems by Deutsche Telekom*, Germany’s largest telecommunications company. Telekom uses a proprietary algorithm to identify user movements based on mobile phone pings to cell towers. We observe hourly aggregates of the number of *outbound* trips, defined as movements crossing a postcode’s boundaries, from January 2019 to October 2023. To capture commuting flows, we focus on the number of outbound trips between 6–9 am, covering typical morning mobility peaks ([Figure 1](#), Panel B). Additionally, we observe the average number of initial (first-morning) pings in each postcode by month from 2020 onwards. These data allow us to track changes in local population at a high frequency, including shifts that are not driven by formal relocations.

Migration data. We complement the ping data with administrative statistics on population and internal migration from the Federal Statistical Office (*Destatis*). First, we use annual origin-destination matrices of moves across Germany’s 401 counties from 2013 to 2022. A limitation of the dataset is that it reports only net flows for each county pair. Second, we use annual county-level migration flows that distinguish inbound and outbound migration but lack information on specific origins and destinations. Third, we compile an annual panel of municipalities to track formal population changes within MAs.

Business turnover data. We examine supply-side responses to the WFH shock using two datasets to trace business turnover in relevant non-tradable industries: Bureau

van Dijk’s Orbis database and the Statistics of Business Notifications (SBN) (*Gewerbeanzeigenstatistik*), which compiles registration data mandated by the Trade Regulation Code (*Gewerbeanzeigeordnung*).⁶ Orbis provides postcode-level data on firm entries and exits, defined by dates of incorporation and termination through insolvency, liquidation, or dissolution. The panel records turnover at monthly intervals from 2018 to 2023. The SBN collects municipality-level notifications of establishments at a quarterly interval (2016–2023).⁷

3 Empirical Strategy

We aim to estimate the causal impact of WFH on the geography of offline spending within metro areas. The first challenge is that an upsurge in WFH affects spending in multiple locations simultaneously: We expect that consumption relocates from the vicinity of workplaces to residential areas. Additionally, locations along commuting routes are likely affected, as non-commuting trips often occur along these paths (Miyachi *et al.*, 2022; Oh and Seo, 2023). A sensible approach is to focus on “at-home” consumption: Our thought experiment compares changes in local spending between “similar” residential postcodes that differ with respect to the severity of the WFH shock while (for now) neglecting the origin of potential spending relocations.

The second challenge is that *observed* WFH rates may be driven by other determinants of spending changes, creating an endogeneity issue. Indeed, the crisis prompted disruptions ranging from behavioural changes to policy interventions, which are difficult to disentangle and potentially correlated with WFH. We address this by comparing postcodes with different levels of WFH potential: The idea is to approximate the pre-existing local capacity to expand WFH after Covid forced the economy into the WFH experiment. As such, the measure is unaffected by other sources of spending disruptions after the outbreak. We use our mobility data to verify whether WFH capacity constraints have bite and explain changes in morning commuting. We expect that a higher WFH potential relates to lower morning mobility and more local spending.

⁶We restrict attention to NACE Rev. 2 two-digit industries in retail trade (47), food and beverage services (56), accommodation (55), and personal services (96).

⁷Exemptions include freelancers, businesses in the primary sector, and managers of own assets (Sec. 14, Trade Regulation Code, *Gewerbeanzeigeordnung*). Registration is required for the establishment of a new business, relocation from another jurisdiction, a merger or demerger, a change in legal form, the admission of new partners, or the acquisition of the business. Reasons for de-registration include business closure, relocation to another district, closure in connection with a merger or demerger, withdrawal of partners, a change in legal form, or the transfer of the business to successors. We focus on new registrations (*Neugründungen*) and complete closures (*vollständige Aufgabe*), which account for approximately 80% of all business notifications.

Difference-in-differences approach. The reasoning motivates a DiD design that compares high vs. low-WFH-potential postcodes over time. As WFH effects are likely to vary through different stages of the Covid crisis and the recovery, we consider a dynamic DiD that tracks differences across postcodes over time:

$$y_{it} = \sum_{k \neq 2020m2} \mathbb{1}(k = t) \times [\beta^k WFHPOT_i + \mu^k dist_i] + \gamma_i + \gamma_{r(i)t} + \epsilon_{it}, \quad (1)$$

where y_{it} denotes an outcome (spending or mobility) for postcode i in month t . $WFHPOT_i$ denotes postcode i 's WFH potential (0-100), and $dist_i$ is the log distance to the city centre. Both variables are interacted with time dummies to capture differences by month-year. Controlling for proximity to the city centre is important to account for possible confounding due to the location of postcodes along commuting routes. γ_i denotes postcode fixed effects. To ensure that we compare changes among postcodes located in the *same* metro area r , we interact month-year with MA fixed effects $\gamma_{r(i)t}$. The error term ϵ_{it} captures unobserved shocks, which are assumed to be uncorrelated with the regressors of interest. Then, the coefficients β^k trace outcome differences associated with a one p.p. higher WFH potential over time, conditional on trend differences by distance to the city centre. February 2020 is the reference period.

As the left-hand side of [Equation 1](#) is a non-negative (count) variable, we estimate the model using the Poisson Pseudo Maximum Likelihood (PPML) estimator ([Santos Silva and Tenreiro, 2006](#)). PPML imposes proportional effects and can handle zeros in the dependent variables, which is occasionally the case for spending (especially during lockdowns) ([Chen and Roth, 2024](#)).⁸ We also report OLS estimates for models with log-transformed dependent variables as a robustness check. For these specifications, we drop all postcodes that record zero spending from the sample. The findings are qualitatively and quantitatively consistent.

In the baseline, we cluster standard errors by postcode, which corresponds to the level of treatment “assignment” ([Abadie et al., 2023](#)). Inference is robust to alternative assumptions about the variance-covariance matrix; in particular, to two-way clustering at the postcode level (allowing for serial correlation) and the MA $\times$ month-year level, allowing for arbitrary error correlation across postcodes of the same MA in each period ([Cameron et al., 2011](#)). We also consider corrections for spatial correlation by [Conley \(1999\)](#) and [Müller and Watson \(2022\)](#) ([Appendix D.3](#)).

⁸Using OLS requires a $\log(y + 1)$ transformation or excluding zeros from the sample. Both approaches can be problematic; see [Chen and Roth \(2024\)](#) for a thorough discussion.

Our setting involves a continuous treatment where all postcodes are treated at varying intensities. Consequently, the validity of the DiD design rests on the *strong* parallel trends (SPT) assumption (Callaway *et al.*, 2024): We assume that the observed trend of postcodes with a given WFH potential corresponds to the (unobserved) average trend across all postcodes had they been assigned the same WFH potential, conditional on trend differences by distance to the city centre. Strong parallel trends ensure that lower-WFH-potential areas are a valid counterfactual for higher-WFH-potential areas (and vice versa). The assumption is met, for instance, if we rule out that postcodes select into levels of WFH potential based on expected treatment effects. Like standard parallel trends, this assumption cannot be tested directly. We provide visual verification of parallel pre-trends while acknowledging that these do not necessarily inform about postcodes' treated potential outcomes.

A concern for the validity of SPT is other determinants of spending shifts that are correlated with WFH potential. For instance, our estimates may pick up differences in the severity of supply-side disruptions if local WFH potential and industry composition are co-determined. We address this by successively adding controls that may explain diverging spending trends after the outbreak. Our most demanding specification controls for postcodes' distance to the city centre, changes in population size (2019–23), and 2019 measures of mobility, spending per capita, industry composition, and mobility in neighbouring postcodes. As we do not observe cash payments, another potential concern is heterogeneity in the shift from cash to card. Payment card usage in Germany has steadily increased from 37% to 54% of all domestic payments between 2017 and 2023, without disruption from the pandemic (see [Appendix C](#)). In contrast, the number of POS terminals remained stable, indicating a steady card payment infrastructure. By including MA×time-period fixed effects, we control for MA-specific time shocks, including heterogeneous shifts in payment methods across MAs. Additionally, by controlling for changes in spending trends by proximity to the city centre, we capture potential trend breaks related to different degrees of urbanisation within MAs. Finally, additional controls account for differential shifts driven by industry composition.

Instrumental variable approach. The DiD estimates deliver intention-to-treat effects because of non-compliance in the sense that WFH potential is not fully realised. For example, varying local conditions and incentives to realise WFH opportunities can mediate how WFH rates actually change in regions that have similar WFH potential. To account for this, we estimate an IV model. Specifically, we instrument changes in morning outbound mobility with WFH potential, yielding an estimate of the elasticity

of spending with respect to WFH-induced mobility changes. The IV approach is akin to the strategy by [Glaeser et al. \(2022\)](#) and delivers local effects for regions that adjust their mobility *because* they have a higher scope to work from home.

Focusing on the changes between 2019 and 2023 (the post-Covid economy), our relationship of interest is given by

$$spending_{i\tau} = \alpha \times \log mobility_{i\tau} + Post_{\tau} \times \mathbf{X}_i' \rho + \lambda_i + \lambda_{r(i)\tau} + \epsilon_{i\tau}, \quad (2)$$

with the corresponding first stage:

$$\log mobility_{i\tau} = \pi \times Post_{\tau} \times WFHPOT_i + Post_{\tau} \times \mathbf{X}_i' \delta + \gamma_i + \gamma_{r(i)\tau} + \vartheta_{i\tau}, \quad (3)$$

where $\log mobility_{i\tau}$ is a postcode's log morning mobility in year $\tau \in \{2019, 2023\}$ and $Post_{\tau}$ is a dummy equal to one in 2023.⁹ $\mathbf{X}_i$ is a vector of controls interacted with the post dummy (excluded instruments). As before, we include postcode and time $\times$ MA fixed effects. The validity of this approach hinges on a strong first stage, i.e., that a higher scope for WFH reduces morning mobility ($\hat{\pi} < 0$).

We show results using a control function approach (CFA) and the 2SLS estimator to estimate the IV model. The CFA is appropriate for non-linear models with non-negative outcomes such as spending ([Wooldridge, 2015](#)). Specifically, the CF corresponds to the first-stage residuals $\widehat{\vartheta}_{it}$ from estimating [Equation 3](#) by OLS. [Equation 2](#) is estimated via PPML and introduces the control function as an additional covariate ([Lin and Wooldridge, 2019](#)).¹⁰ Both approaches deliver an estimate of the spending elasticity with respect to WFH-induced changes in morning mobility ($\hat{\alpha}$). To account for the two-step procedure, the standard errors of the CF estimates are cluster-bootstrapped by postcode.

4 Results

4.1 Higher WFH potential boosts remote work and lowers mobility

We first probe the premise that WFH potential properly captures spatial differences in the exposure to the WFH shock induced by Covid-19.

⁹With two time periods, [Equation 2](#) is equivalent to a “long differences” specification that uses $\Delta spending_i$ on the left-hand side.

¹⁰Note that the CFA is numerically equivalent to 2SLS in the linear case.

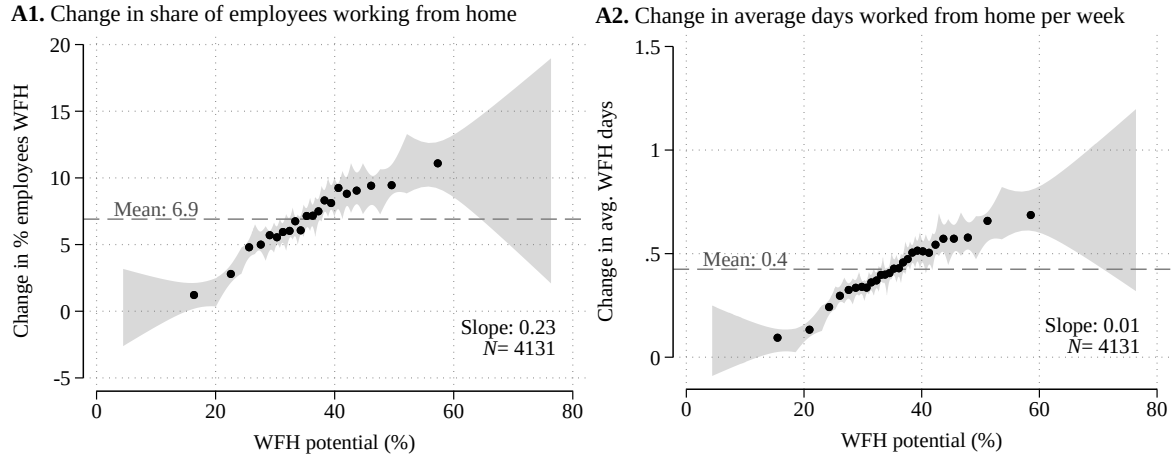
Changes in actual WFH 2019–22. Panel A of [Figure 2](#) plots extensive and intensive margin changes in local WFH prevalence against WFH potential. We use the methodology by [Cattaneo et al. \(2024\)](#) to group observations into equal-sized bins and construct 95% confidence bands for the conditional mean functions, controlling for MA fixed effects. Panel A1 shows a positive relationship between postcodes’ WFH potential and the 2019–22 change in the share of residents working 1+ days per week from home. The horizontal line, which marks the average change of (6.9 p.p.), clearly lies outside the confidence band. Panel A2 shows that average days worked remotely increased by 0.4 days and that WFH intensified more in high-WFH potential areas.

Changes in morning mobility. Panel B of [Figure 2](#) reports DiD-PPML results based on [Equation 1](#). The dependent variable is the average number of outbound trips between 6 and 9 am per month. We plot results separately for weekdays (Mo-Fr) and Saturdays. We exclude Sundays as stores are generally closed in Germany. PPML estimates are transformed to reflect proportional changes associated with higher WFH potential.¹¹ The estimates show parallel trends in 2019, followed by a sharp mobility drop in areas with higher WFH potential after the crisis reached Germany. The mobility gap narrowed as the economy recovered and the federal WFH mandate ended in April 2022. Yet, a sizeable difference persists: In 2023, morning mobility on weekdays is about 0.16% lower for a one p.p. increase in WFH potential. This is consistent with reduced commuting due to stabilising WFH levels in the economy. By contrast, average mobility differences on Saturdays returned to their pre-Covid level, corroborating the notion that weekday changes are driven by work-related mobility.

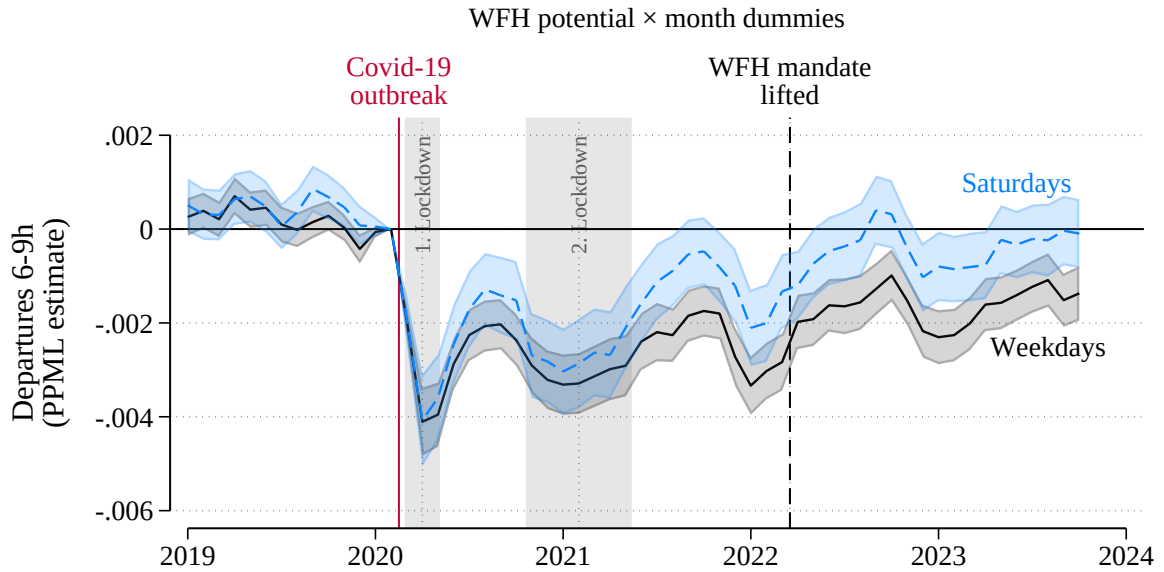
¹¹Specifically, we report $\exp(\hat{\beta}^k) - 1$.

Figure 2: The link between WFH potential and WFH uptake

Panel A. WFH potential and WFH growth during the pandemic (2019-2022)



Panel B. WFH potential and change in morning mobility



Notes: Panel A reports binned scatterplots conditional on MA fixed effects using the methodology by Cattaneo *et al.* (2024). The shaded areas highlight 95% confidence bands of the conditional mean functions. Horizontal lines correspond to the mean of the dependent variable. Survey data on WFH practices are from the infas360 Casa Monitor. Panel B shows DiD results based on Equation 1 estimated by PPML. The dependent variable is the number of outbound trips between 6-9 am at the postcode level. Estimates are transformed by $\exp(\hat{\beta}^k) - 1$ to reflect proportional changes. Confidence bands are drawn at the 95% level based on standard errors clustered by postcode.

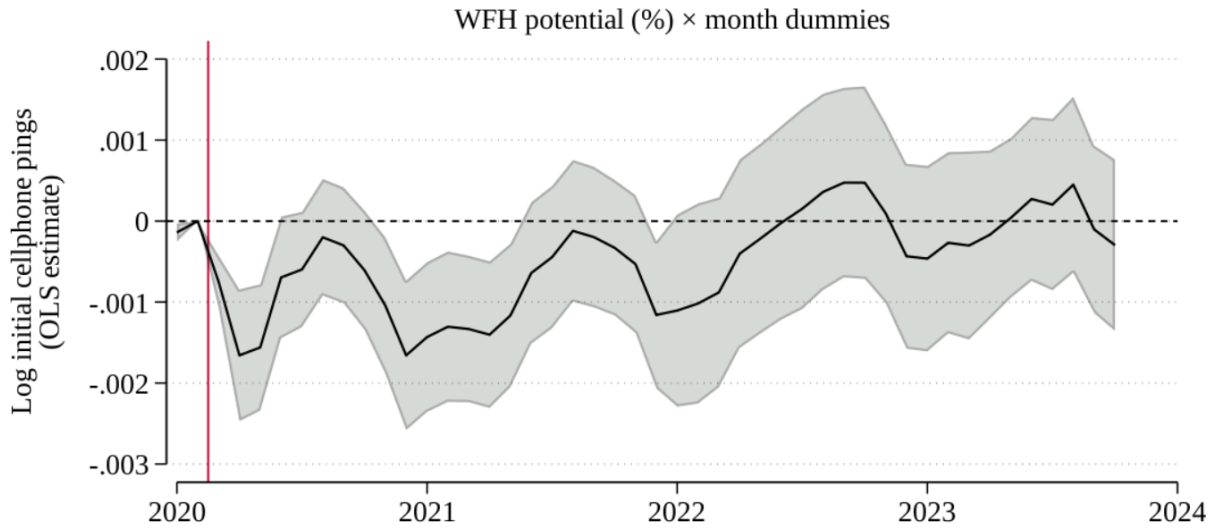
WFH-induced migration. Closing mobility gaps on Saturdays is also revealing because we would expect differences to persist if WFH systematically increased the likelihood of moving. Perhaps surprisingly, the data do not substantiate this channel. We examine Saturday mobility gaps throughout the day and find that they closed at all times (Appendix [Figure E.12](#)). Additionally, we investigate whether WFH potential predicts changes in local population measured by the log of initial (first-morning) cellphone pings. The DiD results shown in [Figure 3](#) reveal temporary population declines linked to higher WFH potential, evident only during lockdowns. By 2022, the coefficients are close to zero and insignificant. This finding contradicts the popular narrative that remote workers are causing urban decline in Germany. Instead, it corroborates anecdotal evidence of temporary relocations to family or vacation residences during pandemic peaks ([Coven et al., 2023](#); [Althoff et al., 2022](#)).

We further investigate the link between WFH and internal migration using administrative data in [Appendix E](#). The evidence bolsters the case that WFH-induced migration played a negligible role at best: First, population outflow from cities *slowed* during the crisis. The observed urban decline is explained by the even sharper decline in population inflow. Indeed, these patterns are anticipated by models of internal migration responses to local economic shocks ([Monras, 2020](#)). Second, origin-destination data on internal migration reveal that excess net migration from more to less central counties between 2020–22 totalled only 59 thousand. This amounts to less than one percent of the 6 million employees who transitioned to remote work after 2019. Third, using panel data on employees’ municipality of residence, we estimate a version of the DiD in [Equation 1](#): We find no significant link between WFH potential and changes in the log number of employed residents. Again, this conflicts with the view that remote worker outmigration drives urban decline. Our findings corroborate evidence based on administrative information about employees’ home and workplace municipality: [Coskun et al. \(2024\)](#) report no association between changes in commuting distances among workers in high versus low WFH potential occupations in 2020. Although higher WFH potential is linked to increased commuting distance in 2021, this change is almost entirely due to job transitions, not relocations. Overall, the evidence indicates that migration dynamics during this period are influenced by factors other than WFH.

4.2 Higher WFH potential increases local spending

Next, we turn to the ITT effects of WFH potential on local spending. [Figure 4](#) reports PPML estimates of trend differences in weekday spending associated with differences in WFH potential ([Equation 1](#)). Postcodes are on similar spending trends before the

Figure 3: WFH potential and change in initial cellphone pings



Notes: The figure shows DiD results based on Equation 1 estimated by OLS. The dependent variable is the number of initial (first-morning) cellphone pings at the postcode level. Confidence bands are drawn at the 95% level based on standard errors clustered by postcode.

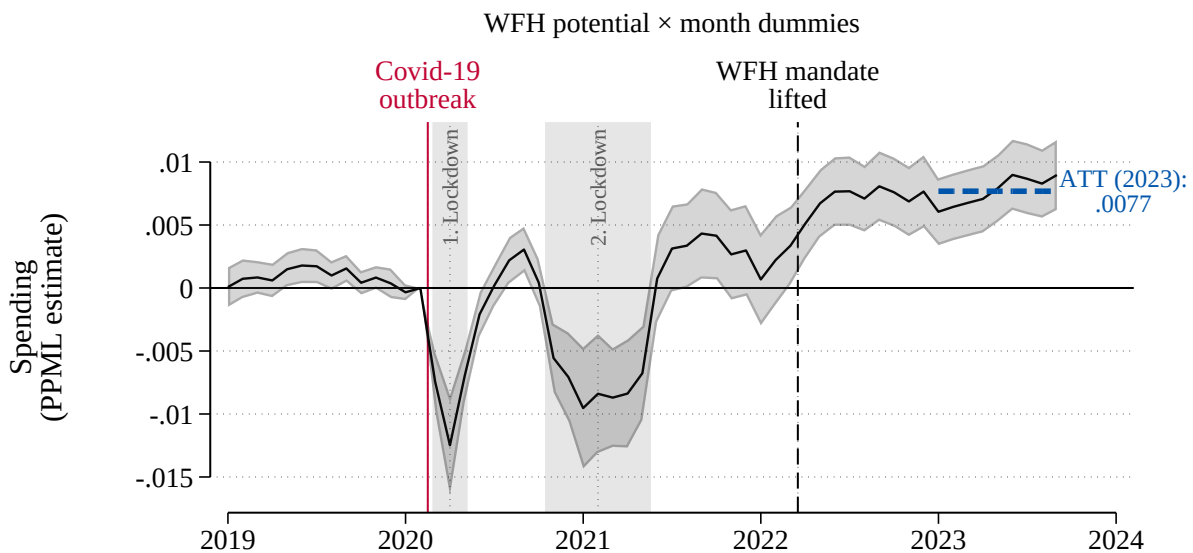
shock and sharply diverge during the crisis: spending in postcodes with higher WFH potential dropped relatively more during the two lockdown periods, with a brief recovery in between. The pattern reversed as the economy recovered. In 2023, the average effect among treated postcodes amounts to a 0.77% increase in local spending for a one p.p. higher WFH potential.

At first glance, the negative DiD estimates during the pandemic seem inconsistent with the WFH story, given that WFH rates peaked during lockdowns (Figure 1). Three mechanisms may explain this pattern. First, mandatory closures of non-essential industries temporarily prevented most offline transactions. The estimates reflect this supply-side disruption. Indeed, we document notable spikes in e-commerce as demand shifted online during these periods: Appendix Figure F.19 plots aggregate trends in offline and online spending since 2018 using data from Mastercard Spending Pulse. During lockdowns, the fraction of online spending nearly doubled from 14% to 26%. Remarkably, e-commerce returned to its pre-Covid level in both absolute and relative terms by 2023, suggesting that these supply-side disruptions were largely transitory. Second, the spending declines in high-WFH areas align with the transitory urban flight documented in Figure 3. This is consistent with remote workers temporarily relocating during strict social distancing periods, when fully remote work was strongly encouraged (Coven *et al.*, 2023). Third, the crisis caused unprecedented

spikes in short-time work (STW).¹² The number of short-time workers peaked at 6 million (18% of employees) in April 2020 and spiked again in February 2021, before returning to near zero by 2022 (Appendix Figure F.18). Because WFH demonstrably shielded workers from STW and the associated wage losses (Adams-Prassl *et al.*, 2020; Alipour *et al.*, 2021; Ben Yahmed *et al.*, 2024), the estimates may also reflect a spurious negative correlation between WFH and spending, driven by more STW in areas where fewer people could work remotely.

Importantly, mandatory closures, online spikes, urban flight, and STW were transitory. WFH has stuck. The positive DiD estimates following the removal of Covid restrictions suggest that WFH redirected spending into remote workers' neighbourhoods. The effect stabilises alongside the recovering economy and steadying WFH rates.

Figure 4: WFH potential and change in local spending



Notes: The figure shows DiD results based on Equation 1 estimated by PPML. The dependent variable is average spending on weekdays (Mo-Fr) at the postcode level. Estimates are transformed by $\exp(\hat{\beta}^k) - 1$ to reflect proportional changes. Confidence bands are drawn at the 95% level based on standard errors clustered by postcode.

Supply-side responses. A key question is whether the supply-side adaptations could explain these effects. For instance, mandatory closures may have permanently scarred regions by boosting firm exits. Then, our WFH impact estimates would be smaller

¹²The short-time work scheme (*Kurzarbeit*) is designed to prevent layoffs while allowing companies to reduce labour costs during an economic downturn. The government partially compensates for wage losses due to reductions in working hours. The German STW scheme was generously expanded during the Covid crisis such that unemployment barely increased (Ben Yahmed *et al.*, 2024).

compared to a scenario without temporary closures. For US cities, [Delventhal and Parkhomenko \(2023\)](#) show theoretically and [Duguid et al. \(2023\)](#) find empirically that retail establishments follow population growth caused by remote worker migration.¹³ The authors find that areas with higher residential WFH potential experience a *decline* in the number of establishments due to WFH-induced outmigration. Because WFH did not boost migration out of German cities, this channel is likely negligible. Remarkably, [Duguid et al. \(2023\)](#) find that WFH potential still predicts establishment losses, conditional on population changes. A possible explanation is the changing shopping behaviour of remote workers, including a shift to online commerce ([Alcedo et al., 2024](#)). Indeed, the US shows a lasting increase in the online share of retail spending ([Alcedo et al., 2024](#)), in contrast to the trend reversal observed in Germany and other advanced economies ([Auer et al., 2023](#)).

We examine supply-side responses to the WFH shock in Germany using the firm turnover panel from the Orbis database and the establishment turnover panel from the SBN. [Figure 5](#) plots monthly firm entry, exit, and net entry rates (as a share of active firms) from the Orbis data. Trends are net of 2018–19 month fixed effects to remove seasonality. Thus, deviations from zero reflect departures from the pre-Covid month-specific average. Following the Covid outbreak, net entry declined sharply and remained below pre-crisis levels through 2023, primarily due to fewer entries rather than rising exits. This pattern coincides with substantial government support measures—loans, grants, and recapitalizations totalling 130 billion Euros—in addition to the short-time work scheme ([BMWK, 2022](#)). Together, these interventions demonstrably averted a wave of firm defaults ([Grimm et al., 2021](#)). In other European countries, targeted consumption subsidies for food services stabilized business activity ([Gonzalez-Pampillon et al., 2024](#)). These institutional features differ from the US, where direct firm subsidies were rare and support focused on worker subsidies.

To test for differential trends by WFH exposure, we estimate the DiD model in [Equation 1](#) using OLS with firm entry and exit rates as outcomes ([Figure 6](#)). Across specifications, nearly all estimates are close to zero and statistically insignificant, indicating no systematic divergence in firm dynamics across high- and low-WFH areas within MAs.¹⁴

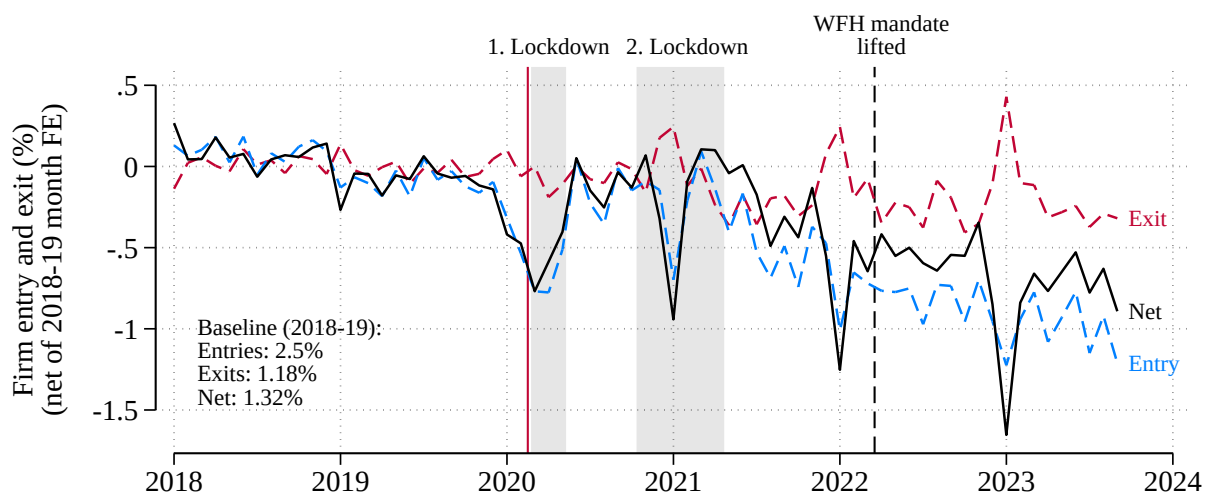
¹³The authors examine 16 large US cities and find moderate effects overall: average establishment growth (2019Q4–2021Q4) was -2% in city centres mirrored by 2% growth in outer suburban rings.

¹⁴Omitting the distance-to-centre interaction in the DiD or using PPML instead of OLS for entry and exit counts yields the same conclusions (see Appendix [Figure A.3](#)).

We replicate the analysis using the SBN data for 4,695 municipalities in Appendix Figure A.4. Panel A reports effects on new registrations; Panel B on closures. Again, treatment effects are near zero and nearly all insignificant, corroborating common trends in firm turnover by WFH potential.

Taken together, the evidence suggests that the WFH shock did not trigger differential business turnover in Germany. The economic and institutional differences likely contribute to the contrasting pattern with the US. Additionally, the WFH-driven spending shifts are possibly insufficient to affect business turnover (details in the next section). Adjustments may also be delayed by initial uncertainty about the permanence of WFH and the process involved in establishing new businesses.

Figure 5: Firm turnover in non-tradable industries

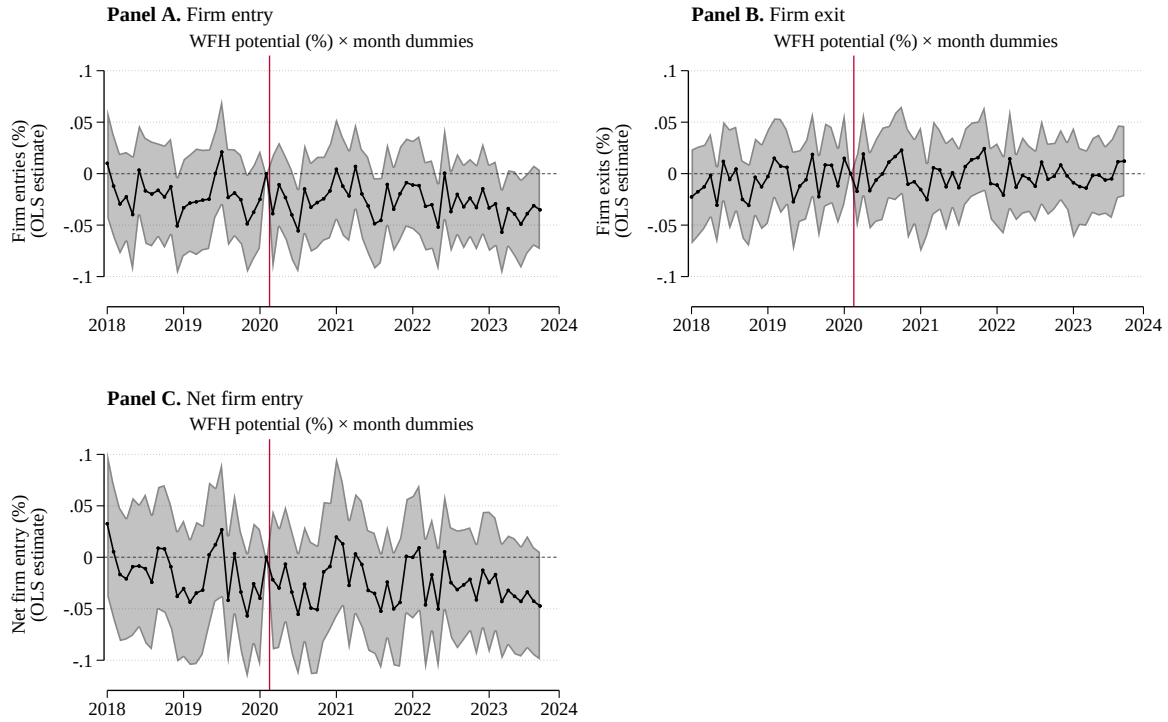


Notes: The figure plots aggregate monthly firm entry and exit rates (in percent of active firms) for non-tradable industries. Values equal the month-specific deviation from the corresponding 2018-19 average. The sample is restricted to the following NACE Rev. 2 two-digit industries: 47 (Retail trade, excl. motor), 56 (Food & beverage services), 55 (Accommodation), 96 (Personal care services). Data are from Bureau van Dijk's Orbis database.

4.3 IV results: Declining mobility boosts spending

We turn to our IV estimates of WFH-induced mobility changes on spending in Table 1. We report results for the non-linear model estimated by PPML and the control function estimator. Results for the linear model estimated by OLS/ 2SLS are presented in Appendix Table D.3. Column 1 gives the reduced-form estimates of WFH potential on the 2019–23 change in weekday spending, conditional on log distance to the city centre. The PPML and OLS estimates are close and significant: a one p.p. increase

Figure 6: DiD estimates of WFH potential on firm turnover, Postcode level (OLS results)



Notes: The figure shows DiD results based on Equation 1 estimated by OLS. Confidence bands are drawn at the 95% level based on standard errors clustered by postcode. Firm turnover data are from Bureau van Dijk's Orbis Database.

in WFH potential implies a 0.66% (0.80%) increase in spending.¹⁵ The coefficient on log distance is positive, indicating that peripheral postcodes saw increased spending relative to closer areas. In Column 2, we add pre-determined controls for morning mobility, spending per capita, and industry composition (spending share in 'food services', 'grocery & food stores', and 'apparel', respectively). We also include 2019 mobility in neighbouring postcodes, measured as inverse-distance-weighted mobility in *other* postcodes of the same MA. The idea is to account for postcodes' location along commuting routes.¹⁶ Finally, we include the 2019–23 change in population as a proxy for net migration. Reassuringly, adding the controls does not affect the reduced-form estimates.

¹⁵Recall that the proportional changes implied by PPML and OLS carry different interpretations. As spending is log-transformed in the OLS regression, the proportional change is unit-specific. PPML delivers the average level effect rescaled by the outcome mean. Thus, OLS places higher weight on effects for postcodes with lower initial spending (Chen and Roth, 2024).

¹⁶Mobility in neighbouring postcodes corresponds to the number of trips in other postcodes of the same MA, weighted by the inverse distance between postcodes' centroids.

Columns 3 and 4 report the results of [Equation 2](#). The estimated elasticity of spending with respect to morning mobility is negative but insignificant without additional controls. Including controls severely impacts the point estimate, indicating that mobility is correlated with observable and likely unobservable determinants of spending.

Columns 5 and 6 address endogeneity by instrumenting mobility by WFH potential. The coefficient on the control function is statistically significant, implying that we can reject that mobility is exogenous ([Lin and Wooldridge, 2019](#)). The elasticity of spending to mobility implied by the IV estimates is negative 3.31–3.74 and consistent with the 2SLS results (negative 3.41–3.79). The first-stage F -statistic is above 50, suggesting that WFH potential is a strong instrument.¹⁷

The estimates imply that a WFH-induced reduction in morning outbound mobility leads to a disproportionate increase in local spending. This finding is consistent with the observation that WFH adoption is driven by those at the top of the wage distribution: We document an hourly wage premium of 7.6% for jobs that can (at least partly) be worked remotely based on 2018 data. The share of teleworkable jobs is increasing almost monotonically through the wage distribution (see [Appendix F.1](#) for details).¹⁸ Thus, as WFH is adopted disproportionately by higher-income workers in areas with a higher WFH potential, spending appears highly elastic to mobility.

It is worth noting that the inclusion of population change (2019–23) does not affect the reduced-form or IV estimates, nor is it correlated with spending changes (conditional on the other covariates). Indeed, we find that the regression function of population change (2019–23) over WFH potential is flat ([Appendix Figure E.16](#)). This corroborates our results of no systematic outmigration from high-WFH areas following the Covid shock (see [Appendix E](#)).¹⁹ In [Appendix D.3](#), we show that inference in the linear and non-linear models is robust to different approaches correcting spatial correlation in model errors.

¹⁷[Lee et al. \(2022, 2023\)](#) show that confidence intervals of the 2SLS estimate need to be adjusted in case the first-stage F -statistic is below 104.67. We report their proposed VtF -95% confidence intervals, which smoothly translate the value of the F -statistic into appropriate interval length, in square brackets. The coefficients remain significant.

¹⁸Pre-Covid data for the US also show that workers who can work remotely enjoy a wage premium on average ([Dingel and Neiman, 2020](#); [Delventhal and Parkhomenko, 2023](#)).

¹⁹Note that, in principle, population change should be considered a “bad control” if we expect WFH to affect spending via migration. As the point estimates are insensitive to the inclusion of this variable, we report the even columns with all covariates for expositional brevity.

Back-of-the-envelope (BoE) calculation. A BoE calculation illustrates the magnitude of our estimated effects for the city of Berlin. The first-stage estimate implies that moving from the 25th to the 75th percentile in the distribution of WFH potential (13.7 p.p.) reduces morning mobility by $13.7 \times -0.0018 = 0.025$ log points in 2023 (Column 6). Using our elasticity estimate and the average spending of EUR 133.6 million per postcode in 2019, this translates into an annual spending increase of EUR 13.2mn, or EUR 700 per capita, relative to a postcode at the 25th percentile.²⁰

Table 1: Main results (non-linear model)

	<i>Spending (Mo-Fr)</i>					
	Reduced form		Main equation		IV	
	(PPML)		(PPML)		(CFA)	
	(1)	(2)	(3)	(4)	(5)	(6)
WFH potential (%)	0.007*** (0.001)	0.007*** (0.001)				
Log departures 6-9h			-0.141 (0.103)	-0.718*** (0.100)	-3.369*** (0.672)	-3.816*** (0.666)
Control function					3.303*** (0.686)	3.152*** (0.667)
Log distance to city centre	0.113*** (0.013)	-0.030** (0.015)	0.083*** (0.010)	-0.047*** (0.013)	0.168*** (0.022)	-0.002 (0.014)
2019 log departures 6-9h		-0.401*** (0.019)		-0.421*** (0.019)		-0.461*** (0.023)
Net migration (2019-23)		0.125 (0.237)		0.118 (0.229)		0.140 (0.229)
2019 log departures (6-9h) of neighbours		-0.320** (0.130)		-0.394*** (0.127)		-0.433*** (0.127)
2019 log spending p.c.		-0.334*** (0.020)		-0.344*** (0.020)		-0.349*** (0.020)
2019 spending share Food Services		0.066*** (0.020)		0.075*** (0.018)		-0.017 (0.029)
2019 spending share Grocery and Food Stores		-0.025*** (0.009)		-0.023*** (0.009)		-0.007 (0.010)
2019 spending share Apparel		0.141*** (0.042)		0.150*** (0.039)		0.133*** (0.039)
First stage coef					-0.0019 <i>F</i> = 62.04	-0.0018 <i>F</i> = 51.82
Implied prop. effect (%)	0.66	0.71	-0.14	-0.72	-3.31	-3.74
Tot. obs.	8,124	8,124	8,124	8,124	8,124	8,124
#Postcodes	4,062	4,062	4,062	4,062	4,062	4,062

Notes: The table presents results based on Equations 2 and 3. All columns include postcode and metro area×post-dummy fixed effects. The implied proportional effect (IPE) corresponds to the percentage change in spending associated with a percentage-point change in WFH potential or a percent change in departures 6-9h, respectively, and is calculated as $100 \times [\exp(\cdot) - 1]$. Standard errors are clustered by postcode and reported in parentheses. Columns 5 and 6 report cluster-bootstrapped standard errors (1,000 repetitions). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

²⁰Spending *levels* are not directly observed in the Mastercard data. We approximate them by allocating Berlin's total offline spending in 2019 (EUR 30.7bn ([Amt für Statistik, 2021](#))) to postcodes using their observed spending shares, yielding an average of EUR 134mn per postcode in 2019. Applying the IV-PPML estimate from Column 6 (-3.816) gives an annual increase of $134 \times [\exp(-3.816 \times -0.025) - 1] \approx 13.2$ mn EUR.

4.4 Heterogeneity Analysis

Heterogeneity by MA size. Theory motivates exploring heterogeneity by metro area size. For instance, MA size positively correlates with the concentration of teleworkable jobs in the core city (Appendix Figure A.5), which can enhance network effects and thus increase incentives to realise WFH opportunities (Monte *et al.*, 2023).²¹ Thus, we compare impact estimates among small (bottom 35), medium (mid 10), and large (top 5) MAs to verify if the WFH shock hits larger MAs more severely. We find that it does. Table 2 presents the results from the non-linear model by MA size. The reduced-form effects are significant only among the largest 5 MA (Column 1) and the 10 mid-sized MAs (Column 3). By contrast, the reduced-form estimate is near zero and barely significant among the smallest 35 MAs (Column 5). Moreover, the first-stage results suggest that WFH potential is a poor predictor of morning mobility changes ($F < 10$), suggesting weak incentives or capabilities to realise WFH opportunities in smaller MAs (Column 6). The estimated mobility elasticity of spending is greater in the largest 5 MAs compared to the mid-sized MAs (-3.66 versus -3.14). This result is consistent with theoretical arguments by Monte *et al.* (2023), who suggest that, given productivity spillovers among individuals working in person, a coordinated switch to remote work is more likely to be permanent in larger cities. In their model, smaller cities lack conditions for multiple equilibria due to weak agglomeration forces relative to congestion forces. Consequently, these cities return to pre-existing commuting patterns after the temporary shift to remote work.

We report results from a linear model that excludes all postcodes with zero-valued spending for robustness in Appendix Table B.1. The conclusions are the same; however, the gap between the estimated elasticities in large versus mid-sized MAs is somewhat larger (-4.88 versus -2.46).

Heterogeneity by industry. Finally, we assess whether WFH impacts industries differently. Postcodes with fewer than four merchants in a given category drop out of the sample due to data privacy restrictions. Thus, to ensure enough power, we focus on the largest industries: food services, grocery stores, and apparel stores. Table 3 presents the results from the non-linear model, which closely mirror those from the linear model (Appendix Table B.2).

Despite the smaller sample, the first-stage effects of WFH potential on mobility remain strong for all categories (Columns 2, 4, and 6). The reduced-form effects of

²¹The concentration of teleworkable jobs corresponds to the 2019 fraction of jobs that could be done from home at least partly, as measured by Alipour *et al.* (2023).

Table 2: Heterogeneity by metro area size (non-linear model)

	<i>Spending (Mo-Fr)</i>					
	Top 5 MAs		Mid 10 MAs		Bottom 35 MAs	
	RF (PPML)	IV-CFA	RF (PPML)	IV-CFA	RF (PPML)	IV-CFA
	(1)	(2)	(3)	(4)	(5)	(6)
WFH potential (%)	0.007*** (0.002)		0.007*** (0.002)		0.003* (0.002)	
Log departures 6-9h		-3.730*** (1.149)		-3.186*** (0.987)		-2.508 (1.505)
Control function		2.886*** (1.119)		2.815*** (1.007)		2.119 (1.513)
Log distance to city centre	-0.004 (0.026)	0.025 (0.025)	-0.082*** (0.027)	0.011 (0.040)	-0.016 (0.017)	-0.052** (0.024)
2019 log departures 6-9h	-0.334*** (0.030)	-0.411*** (0.041)	-0.425*** (0.041)	-0.481*** (0.047)	-0.469*** (0.023)	-0.502*** (0.028)
Net migration (2019-23)	-0.275 (0.393)	0.268 (0.402)	0.590** (0.289)	0.465 (0.321)	0.175 (0.321)	-0.117 (0.361)
2019 log departures (6-9h) of neighbours	-0.599*** (0.191)	0.010 (0.247)	-0.308 (0.197)	-0.524** (0.207)	-0.155 (0.127)	-0.315* (0.164)
2019 log spending p.c.	-0.281*** (0.025)	-0.308*** (0.026)	-0.356*** (0.038)	-0.360*** (0.040)	-0.418*** (0.017)	-0.418*** (0.018)
2019 spending share Food Services	0.093*** (0.025)	0.026 (0.035)	0.045 (0.035)	0.004 (0.044)	-0.019 (0.036)	-0.055 (0.048)
2019 spending share Grocery and Food Stores	-0.011 (0.014)	0.024 (0.017)	-0.029* (0.017)	-0.021 (0.018)	-0.020 (0.014)	-0.027* (0.015)
2019 spending share Apparel	0.142*** (0.043)	0.065 (0.051)	0.146* (0.083)	0.157* (0.087)	0.176*** (0.048)	0.182*** (0.055)
First stage coef.		-0.0019 $F=18.46$		-0.0021 $F=26.95$		-0.0010 $F=5.34$
Implied prop. effect (%)	0.71	-3.66	0.67	-3.14	0.28	-2.48
Tot. obs.	2,496	2,496	2,726	2,726	2,902	2,902
#Postcodes	1,248	1,248	1,363	1,363	1,451	1,451

Notes: The table presents results based on Equations 2 and 3. The sample is split into three by metro area population. All columns include postcode and metro area $\times$ post-dummy fixed effects. The implied proportional effect (IPE) corresponds to the percentage change in spending associated with a percentage-point change in WFH potential or a percent change in departures 6-9h, respectively, and is calculated as $100 \times [\exp(\cdot) - 1]$. Standard errors are reported in parentheses and clustered by postcode in odd columns. Even columns use cluster-bootstrapped standard errors (1,000 repetitions). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

WFH potential on spending are significant only for food services and grocery stores (Columns 1 and 3). The impact on apparel spending is zero and insignificant, suggesting that remote workers do not shift spending on clothing and accessory products to the vicinity of their homes (Column 5). The elasticity estimates show that spending on food services reacts more strongly to WFH than grocery spending: A percent decline in morning mobility increases restaurant spending by 3.92% compared to 2.65% in grocery stores. This is unsurprising, given that eating out for lunch is a common feature of working at the office. Transactions are not time-stamped in our data. But it is plausible that some of the saved lunch money is redirected towards restaurant visits during the evenings or even spent on groceries near workers' homes instead. Thus, the positive elasticity estimate for grocery stores may also reflect substitution among spending categories after workers transition to remote work.

Table 3: Heterogeneity by spending category (non-linear model)

	<i>Spending (Mo-Fr)</i>					
	Food services		Grocery stores		Apparel stores	
	RF (PPML)	IV-CFA	RF (PPML)	IV-CFA	RF (PPML)	IV-CFA
	(1)	(2)	(3)	(4)	(5)	(6)
WFH potential (%)	0.007*** (0.002)		0.005*** (0.001)		-0.001 (0.004)	
log departures 6-9h		-3.995*** (1.425)		-2.690*** (0.669)		0.527 (2.182)
Control function		3.108** (1.487)		2.320*** (0.662)		-0.618 (2.152)
Log distance to city centre	-0.064*** (0.020)	-0.021 (0.029)	-0.003 (0.016)	0.017 (0.020)	-0.063* (0.034)	-0.066 (0.053)
2019 log departures 6-9h	-0.439*** (0.036)	-0.495*** (0.045)	-0.448*** (0.022)	-0.482*** (0.025)	-0.206*** (0.065)	-0.202*** (0.077)
2019-23 Population change (%)	0.635 (0.400)	0.798** (0.345)	-0.535* (0.306)	-0.448 (0.310)	-1.091* (0.636)	-1.107 (0.698)
2019 log departures (6-9h) of neighbours	-0.444** (0.174)	-0.622*** (0.180)	0.139 (0.118)	0.033 (0.126)	-0.180 (0.323)	-0.154 (0.364)
2019 log spending p.c.	-0.421*** (0.020)	-0.443*** (0.021)	-0.519*** (0.013)	-0.524*** (0.013)	-0.280*** (0.030)	-0.281*** (0.034)
2019 spending share Food Services	-0.218*** (0.038)	-0.246*** (0.043)	-0.068*** (0.019)	-0.120*** (0.025)	-0.283*** (0.093)	-0.273** (0.115)
2019 spending share Grocery and Food Stores	-0.099*** (0.016)	-0.082*** (0.019)	-0.104*** (0.019)	-0.089*** (0.021)	-0.151*** (0.049)	-0.155*** (0.057)
2019 spending share Apparel	-0.020 (0.037)	-0.016 (0.040)	-0.151*** (0.039)	-0.161*** (0.041)	-0.287** (0.129)	-0.284** (0.142)
First stage coef.		-0.0017 <i>F</i> = 36.56		-0.0020 <i>F</i> = 59.17		-0.0022 <i>F</i> = 45.18
Implied prop. effect (%)	0.73	-3.92	0.54	-2.65	-0.11	0.53
Tot. obs.	5,240	5,240	6,700	6,700	3,460	3,460
#Postcodes	2,620	2,620	3,350	3,350	1,730	1,730

Notes: The table presents results based on Equations 2 and 3. All columns include postcode and metro area×post-dummy fixed effects. The implied proportional effect (IPE) corresponds to the percentage change in spending associated with a percentage-point change in WFH potential or a percent change in departures 6-9h, respectively, and is calculated as $100 \times [\exp(\cdot) - 1]$. Standard errors are reported in parentheses and clustered by postcode in odd columns. Even columns use cluster-bootstrapped standard errors (1,000 repetitions). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Besides substitution across spending categories, the estimates may also reflect income effects. Cheaper lunches on home days, but also different wage growth dynamics by WFH status may directly influence spending constraints. For instance, [Barrero *et al.* \(2022\)](#) suggest that WFH reduces wage growth pressure because employers share its amenity value with employees. Wages may also respond to the changing competitive landscape for teleworkable jobs after employers realise their WFH potential. Similarly, wages will likely respond to WFH-induced productivity changes. Recent evidence suggests that hybrid WFH models tend to deliver neutral or positive productivity effects ([Bloom *et al.*, 2024](#); [Choudhury *et al.*, 2024](#); [Angelici and Profeta, 2024](#)), whereas shifting to fully remote generates losses ([Emanuel and Harrington, 2024](#); [Gibbs *et al.*, 2023](#)). Thus, income effects could go different ways and potentially bear significant distributional consequences at the macro level ([Autor *et al.*, 2023](#)). We cannot disentangle spending shifts from income effects with our data and leave this question to future research.

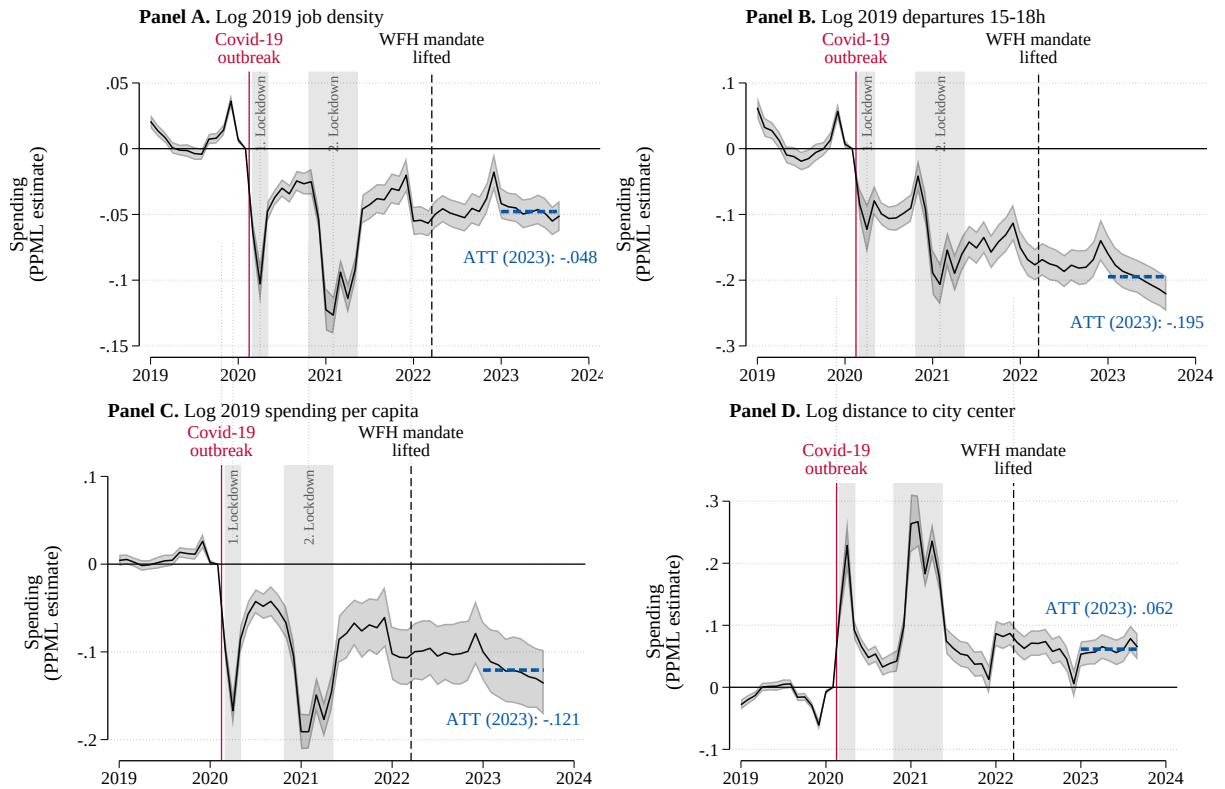
4.5 Consequences for workplace areas

Our estimates exploit variation in WFH-induced changes in morning commuting to estimate spending changes across home areas. By design, this approach abstracts from the locations from which spending is redirected to remote workers' neighbourhoods. While compelling for causally identifying spending effects, it ignores a crucial aspect of WFH's impact on the geography of consumption in cities: the consequences for workplace areas. We turn to these in this section.

We explore spending trends since 2019 by adopting the DiD framework introduced in [Section 3](#). Here, we do not include any controls but explore trend differences across several dimensions separately. Note that WFH does not necessarily fully mediate spending effects. Thus, we are cautious about causal claims. We define workplace areas as postcodes with a high job density, defined as employment per square kilometre in 2019. Thus, the DiD estimates reflect the (relative) spending shift from workplace to residential areas. Binscatter regressions show that a higher job density is associated with nearly monotonic increases in the proximity to the city centre, outbound mobility in 2019 (a proxy for home commutes), and 2019 spending per capita ([Appendix Figure F.20](#)).

Panel A of [Figure 7](#) plots the PPML-DiD estimates from the interaction of log 2019 job density with month dummies. The results show that job sites were severely hit by the crisis. Weekday spending plummeted during the lockdowns and only partially recovered in the post-pandemic economy. On average, spending is 4.8% lower for

Figure 7: Consequences for workplace areas



Notes: Panels A–D report DiD estimates from a PPML regression of monthly weekday spending on DiD interaction terms of month dummies with a time-invariant variable Z , month $\times$ MA fixed effects, and postcode fixed effects. Z corresponds to log 2019 job density in Panel A, log 2019 departures between 3 and 6 pm (Panel B), log 2019 spending per capita (Panel C), and log distance to the city centre (Panel D). Estimates are transformed by $\exp(\cdot) - 1$ to reflect proportional changes. Shaded areas highlight 95% confidence bands using standard errors clustered at the postcode level.

a one percent increase in the 2019 job density. Spending trends similarly diverged when comparing areas by their pre-crisis evening outbound mobility (Panel B): In 2023, local spending is 19.5% lower among areas with one percent more evening commuting in 2019. Panel C shows that these changes affect the regional inequality in economic activity: Areas with a higher spending per capita in 2019 persistently attract less spending in the post-Covid economy. We estimate an elasticity of spending with respect to 2019 per capita spending of -12.1%. Finally, we explore the spatial implications of the shock. Panel D reports trend differences across postcodes' distance to the city centre. We find persistent gains in less versus more central areas. On average, spending in 2023 is 6.2% higher for a percent distance increase. Appendix [Figure F.21](#) plots analogue DiD estimates using outbound mobility between 3 and 6 pm as the dependent variable. The results confirm that workplace areas persistently experience reduced evening mobility, as expected with higher levels of remote work.

Breaking down the results by industry reveals that spending in all categories (Food Services, Grocery Stores, Apparel Stores) declined in areas with higher versus lower job concentration between 2019 and 2023 (Appendix [Table F.6](#)). Spending losses in apparel stores seem at odds with the result from [Section 4.3](#) that variation in WFH potential does not explain different spending trends in this category. Note that this is not necessarily a contradiction as the causal WFH estimates capture differences among residential postcodes, whereas these estimates capture relative changes between workplace and residential areas. Thus, the loss could be explained by increases in WFH that are not driven by WFH potential.

5 Conclusion

We study the consequences of the big shift to remote work for local consumer spending within German agglomerations. The analysis builds on a postcode-level panel (2019–23) of novel cellphone mobility and payment card transactions in brick-and-mortar establishments. Identification uses postcode-level differences in WFH potential, i.e., the ability of the resident population to transition to WFH after February 2020.

DiD estimates suggest persistent declines in mobility, paralleled by increases in spending, in areas with higher WFH potential by 2023. Instrumenting 2019–23 mobility changes by WFH potential, we estimate an elasticity of spending with respect to WFH-induced mobility changes of -3.31% to -3.79%. The effects are driven by larger metro areas and spending in food services and grocery stores. We do not find evidence that WFH-induced migration, business turnover, or persistent shifts to online commerce

can explain these effects. This contrasts with findings for the US ([Alcedo et al., 2024](#); [Coven et al., 2023](#); [Ramani et al., 2024](#); [Duguid et al., 2023](#)), emphasising that institutional context and urban geography matter for the impact of WFH.

Finally, we show that workplace areas (high job density postcodes) saw persistent spending losses. These areas are more likely consumption hubs and close to the city centre. Consequently, the WFH shock reduced spatial inequality in economic activity by generating relative gains in residential and more peripheral areas of the city.

Our findings inform at least three areas: First, recent research on determinants of agglomerations reinforced the notion that serendipitous exchange among workers from different firms is crucial for cluster success ([Atkin et al., 2022](#)). Thus, companies must learn how to sustain such interactions in a hybrid work environment. Second, for municipalities that levy local business taxes and receive federal funds per resident, understanding spatial changes in economic activity and resident population is essential to anticipate fiscal revenues and plan infrastructure. Third, urban planning for public transport and zoning policies relies on micro-geographic evidence on shifts in economic activity and how people allocate time within the city. Indeed, a debate has emerged over interventions to reinvigorate urban spaces for leisure, as millions of workers no longer commute regularly ([Glaeser and Ratti, 2023](#)). The high-resolution and near real-time data used in this paper can support policymakers in promptly evaluating local policy experiments and developing effective strategies.

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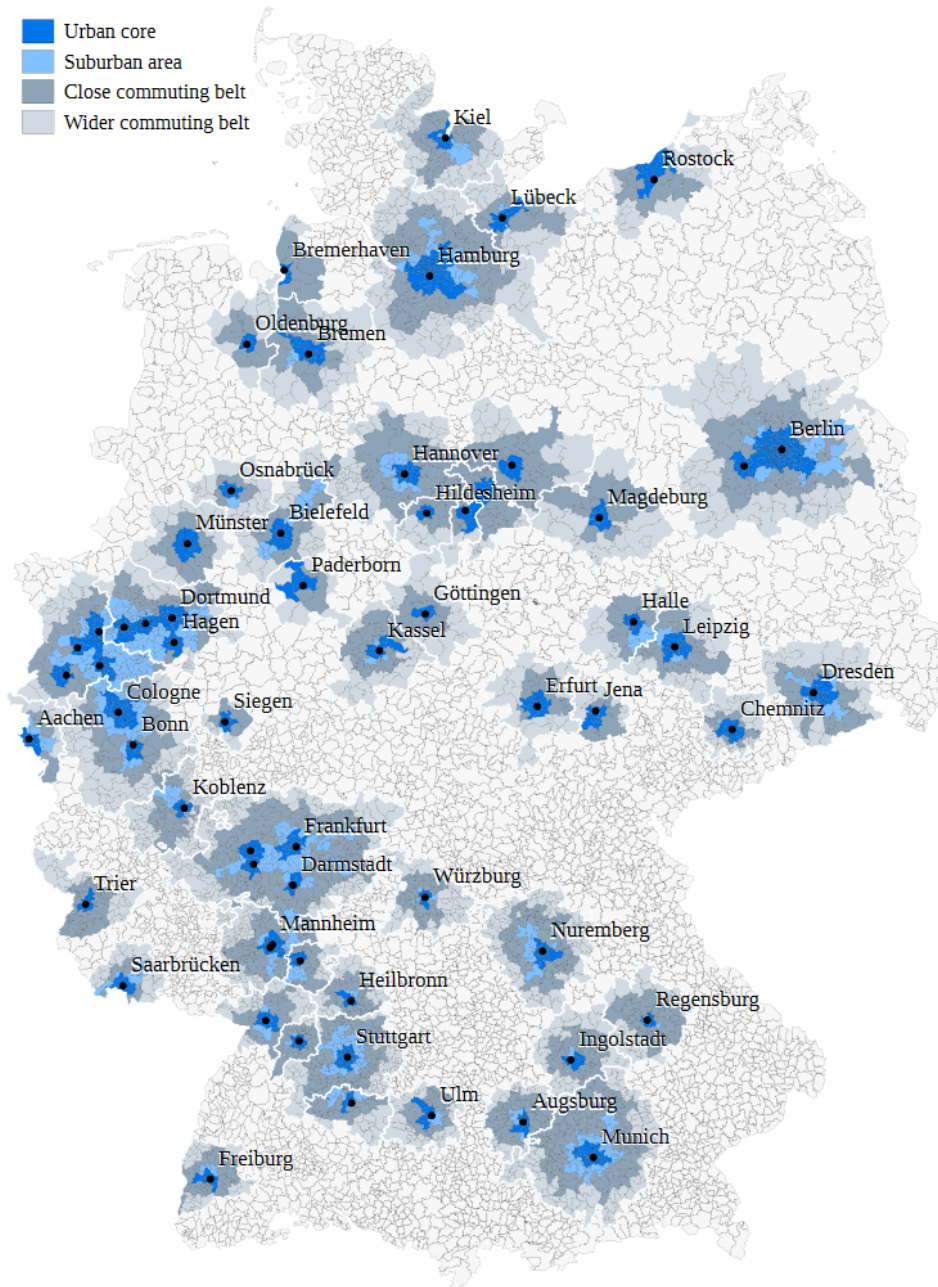
Online Appendix

Working from Home and Consumption in Cities

by *Jean-Victor Alipour, Oliver Falck, Simon Krause, Carla Krolage, and Sebastian Wichert*

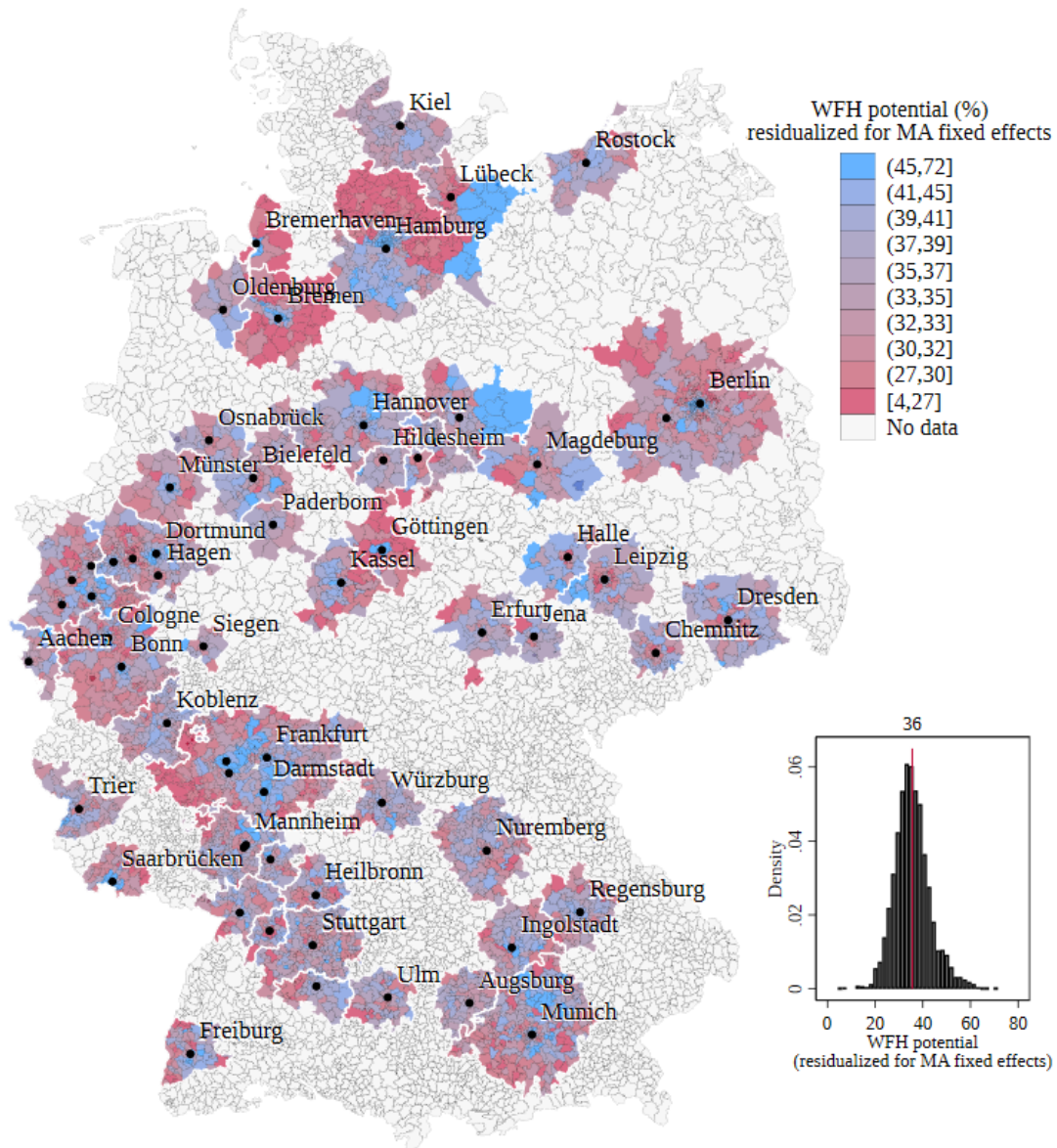
Appendix A Figures

Figure A.1: German Metropolitan Areas



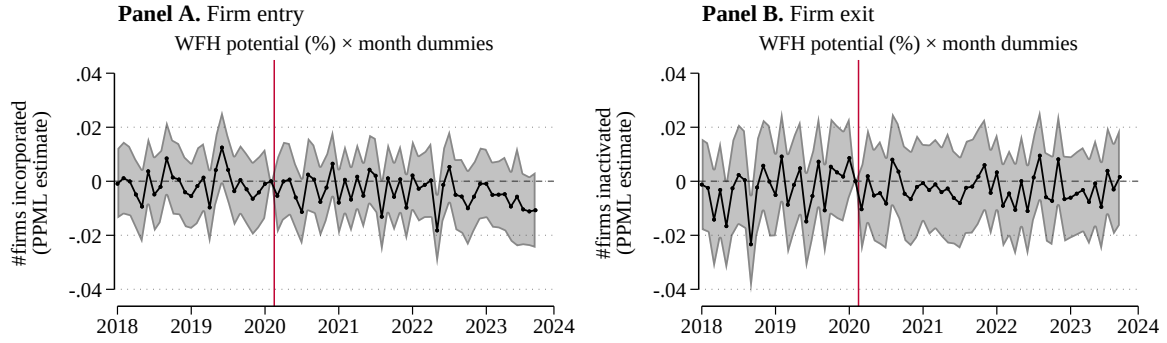
Notes: The map highlights postcodes that are part of a metropolitan area (MA) and thus included in our sample. The Federal Office for Building and Regional Planning (BBSR) defines 50 MAs (as of 2019). An MA is centred around at least one urban core (i.e., a municipality with a population above 100,000) and extends to boundaries determined by commuting linkages. MAs can have multiple cores if they are close to each other and interconnected.

Figure A.2: Spatial distribution of WFH potential within metro areas



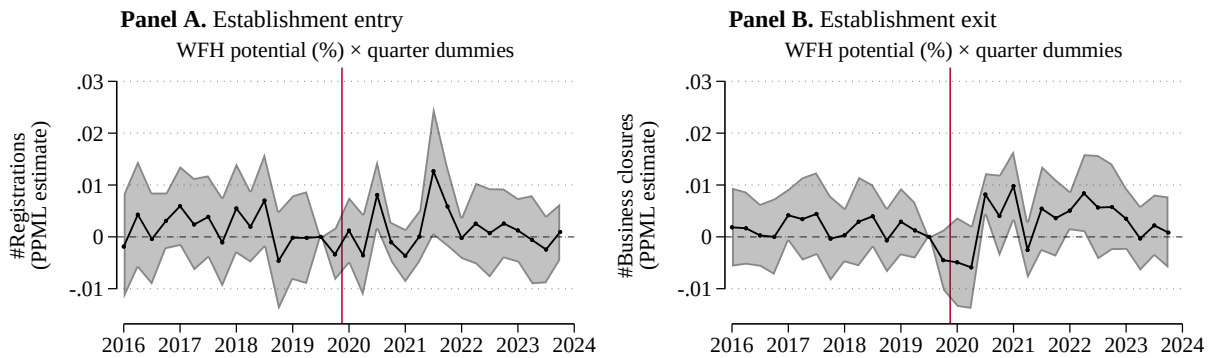
Notes: The map shows the spatial distribution of WFH potential, defined as the percentage of residents whose job can be done at home at least one day per week in a postcode. WFH potential is residualized for metro area fixed effects to illustrate the identifying variation. Colours correspond to deciles. The vertical line highlights the mean of the distribution (36%).

Figure A.3: DiD estimates of WFH potential on firm turnover, Postcode level (PPML results)



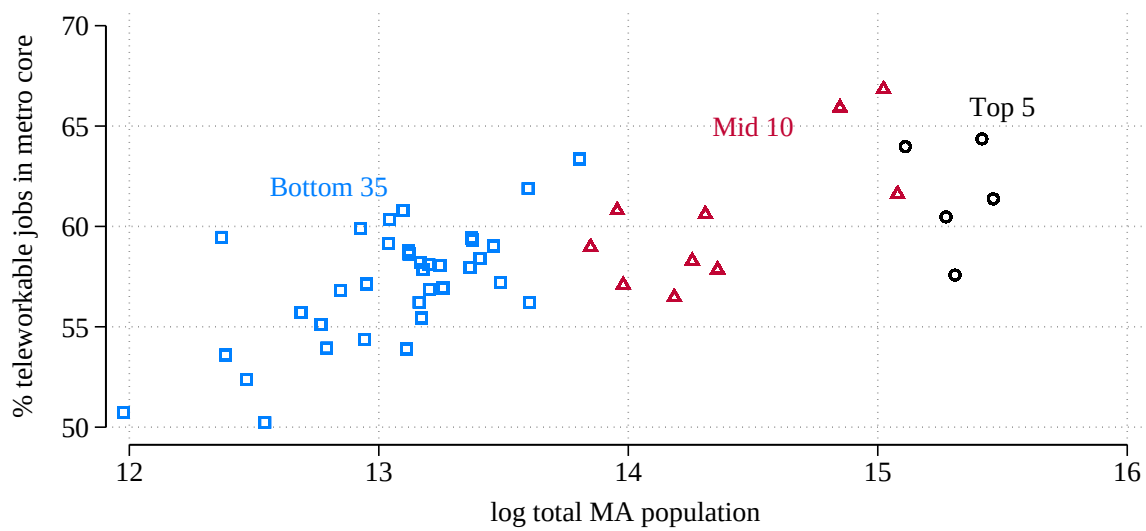
Notes: The figure shows DiD results based on Equation 1 estimated by PPML. Estimates are transformed by $\exp(\hat{\beta}^k) - 1$ to reflect proportional changes. Confidence bands are drawn at the 95% level based on standard errors clustered by postcode. Firm turnover data are from Bureau van Dijk's Orbis Database.

Figure A.4: DiD estimates of WFH potential on establishment turnover, Municipality level (PPML results)



Notes: The figure shows DiD results based on a version of Equation 1 adapted to the municipality-level panel and estimated by PPML. Estimates are transformed by $\exp(\hat{\beta}^k) - 1$ to reflect proportional changes. Confidence bands are drawn at the 95% level based on standard errors clustered by municipality. Establishment turnover data are from the Statistics of Business Notifications (*Gewerbeanzeigenstatistik*) by the German Federal Statistical Office (Destatis).

Figure A.5: MA size and concentration of teleworkable jobs in MA core



Notes: The figure plots the percent of jobs that can at least partly be done from home in the MA core(s) against the log of MA population. The measure of WFH jobs concentration comes from [Alipour et al. \(2023\)](#) and is based on the 2018 BIBB-BAuA Employment Survey and 2019 employment statistics by county from the Federal Employment Agency. For MAs with multiple cores, the measure corresponds to a population-weighted average.

Appendix B Tables

Table B.1: Heterogeneity by metro area size (linear model)

	<i>Log Spending (Mo-Fr)</i>					
	Top 5 MAs		Mid 10 MAs		Bottom 35 MAs	
	RF (OLS)	2SLS	RF (OLS)	2SLS	RF (OLS)	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
WFH potential (%)	0.010*** (0.002)		0.006*** (0.002)		0.000 (0.002)	
log departures 6-9h		-4.878*** (1.221) [-7.52,-2.63]		-2.461*** (0.865) [-4.11,-0.87]		-0.577 (2.616) [...]
Log distance to city centre	0.003 (0.021)	0.017 (0.029)	-0.032 (0.028)	0.039 (0.043)	-0.023 (0.021)	-0.030 (0.032)
2019 log departures 6-9h	-0.528*** (0.045)	-0.624*** (0.067)	-0.681*** (0.031)	-0.720*** (0.035)	-0.657*** (0.029)	-0.662*** (0.034)
Net migration (2019-23)	-0.703** (0.275)	0.098 (0.461)	0.141 (0.433)	0.012 (0.441)	0.187 (0.267)	0.136 (0.349)
2019 log departures (6-9h) of neighbours	-0.361** (0.182)	0.409 (0.308)	-0.032 (0.192)	-0.183 (0.208)	0.114 (0.163)	0.081 (0.218)
2019 log spending p.c.	-0.614*** (0.030)	-0.658*** (0.036)	-0.676*** (0.020)	-0.686*** (0.021)	-0.681*** (0.020)	-0.679*** (0.021)
2019 spending share Food Services	0.062*** (0.024)	-0.027 (0.046)	0.047 (0.029)	0.018 (0.036)	-0.013 (0.041)	-0.019 (0.053)
2019 spending share Grocery and Food Stores	-0.046*** (0.014)	-0.005 (0.024)	-0.030 (0.019)	-0.028 (0.021)	-0.029* (0.017)	-0.031 (0.019)
2019 spending share Apparel	0.349*** (0.048)	0.264*** (0.070)	0.359*** (0.059)	0.371*** (0.058)	0.302*** (0.074)	0.302*** (0.073)
First stage coef.		-0.0021 <i>F</i> = 24.64		-0.0023 <i>F</i> = 30.36		-0.0008 <i>F</i> = 2.73
Implied prop. effect (%)	1.04	-4.88	0.56	-2.46	0.04	-0.58
Tot. obs.	2,410	2,410	2,398	2,398	2,466	2,466
#Postcodes	1,205	1,205	1,199	1,199	1,233	1,233

Notes: The table presents results based on Equations 2 and 3. The sample is split into three by metro area population and excludes postcodes with zero-valued spending. All columns include postcode and metro area×post-dummy fixed effects. The implied proportional effect (IPE) corresponds to the percentage change in spending associated with a percentage-point change in WFH potential or a percent change in departures 6-9h, respectively. Standard errors are reported in parentheses and clustered by postcode. *VtF*-95% confidence intervals due to [Lee et al. \(2022, 2023\)](#) are reported in brackets in even columns. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B.2: Heterogeneity by spending category (linear model)

	<i>Log Spending (Mo-Fr)</i>					
	Food services		Grocery stores		Apparel stores	
	RF (OLS)	2SLS	RF (OLS)	2SLS	RF (OLS)	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
WFH potential (%)	0.009*** (0.002)		0.005*** (0.002)		0.004 (0.004)	
log departures 6-9h		-3.678*** (1.060)		-2.558*** (0.761)		-1.359 (1.464)
		[-5.66,-1.70]		[-4.02,-1.12]		[-4.09,1.36]
Log distance to city centre	-0.106*** (0.026)	-0.078** (0.033)	-0.008 (0.016)	0.009 (0.019)	-0.104** (0.042)	-0.098** (0.045)
2019 log departures 6-9h	-0.566*** (0.033)	-0.628*** (0.040)	-0.617*** (0.029)	-0.648*** (0.031)	-0.327*** (0.052)	-0.344*** (0.057)
2019-23 Population change (%)	0.434 (0.415)	0.590 (0.477)	-0.587* (0.326)	-0.440 (0.287)	-1.282 (0.807)	-1.242 (0.804)
2019 log departures (6-9h) of neighbours	-0.588*** (0.190)	-0.769*** (0.204)	0.096 (0.128)	-0.068 (0.142)	-0.245 (0.324)	-0.355 (0.333)
2019 log spending p.c.	-0.711*** (0.016)	-0.740*** (0.019)	-0.816*** (0.014)	-0.835*** (0.016)	-0.531*** (0.022)	-0.537*** (0.023)
2019 spending share Food Services	-0.006 (0.037)	-0.001 (0.038)	-0.069*** (0.021)	-0.113*** (0.028)	-0.131** (0.063)	-0.150** (0.069)
2019 spending share Grocery and Food Stores	-0.030 (0.021)	-0.020 (0.023)	-0.012 (0.020)	0.014 (0.023)	-0.049 (0.037)	-0.035 (0.041)
2019 spending share Apparel	0.153*** (0.040)	0.175*** (0.047)	-0.081** (0.036)	-0.074* (0.040)	0.321*** (0.110)	0.345*** (0.113)
First stage coef.		-0.0024 <i>F</i> = 43.41		-0.0020 <i>F</i> = 56.59		-0.0027 <i>F</i> = 43.79
Implied prop. effect (%)	0.88	-3.68	0.52	-2.56	0.36	-1.36
Tot. obs.	3,548	3,548	4,826	4,826	2,580	2,580
#Postcodes	1,774	1,774	2,413	2,413	1,290	1,290

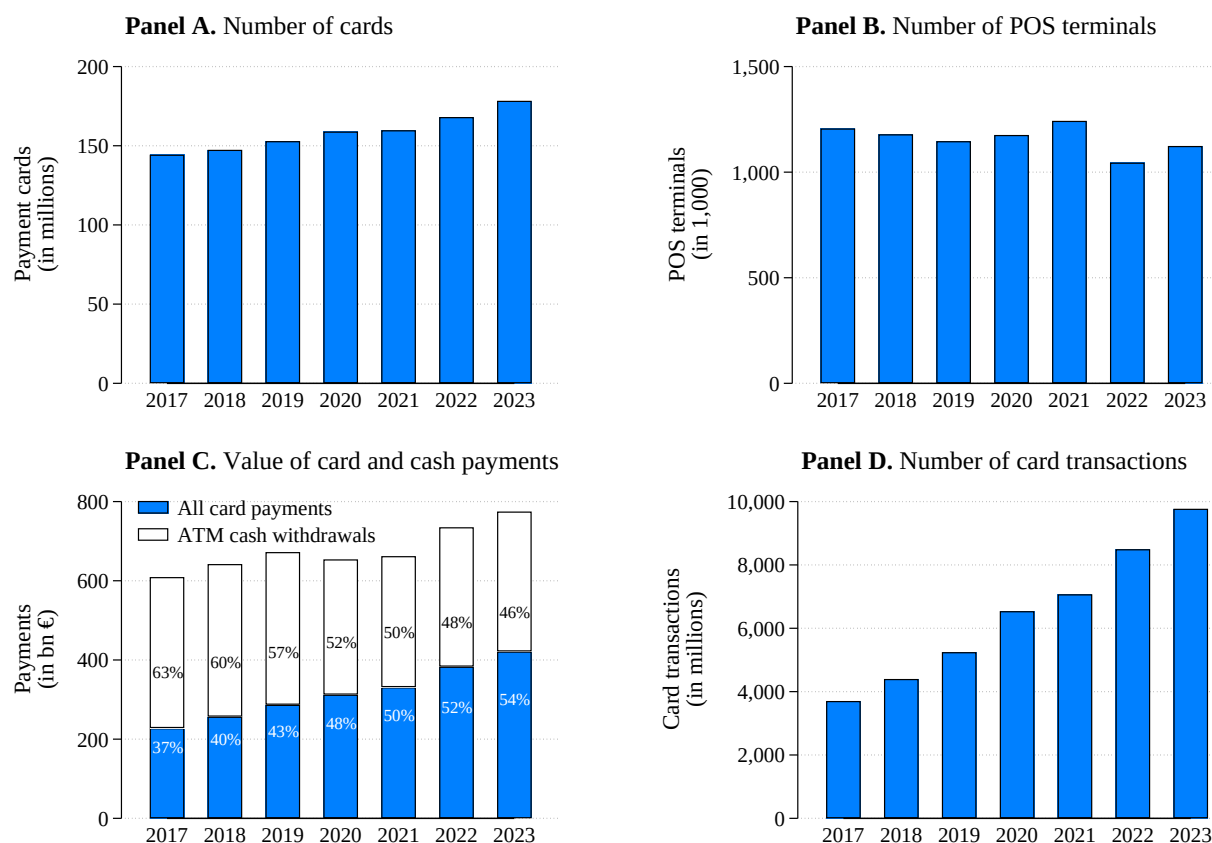
Notes: The table presents results based on Equations 2 and 3. All columns include postcode and metro area×post-dummy fixed effects. The implied proportional effect (IPE) corresponds to the percentage change in spending associated with a percentage-point change in WFH potential or a percent change in departures 6-9h, respectively. Standard errors are reported in parentheses and clustered by postcode. *VtF*-95% confidence intervals due to Lee *et al.* (2022, 2023) are reported in brackets in even columns. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix C Aggregate Trends in Card and Cash Payments

A shortcoming of the geo-referenced Mastercard payment data is the lack of information on cash payments. As noted in [Section 3](#), heterogeneous shifts in the payment technology from cash to card within metro areas constitute a potential threat to identification in our DiD framework if these are correlated with WFH potential. Unfortunately, spatially fine-grained information on the adoption of cash versus card payments is unavailable.

In [Figure C.6](#), we present aggregate data on cash and card payments from the European Central Bank (ECB) Payment Statistics. Reassuringly, the data suggest no discernable trend breaks since the pandemic. Panel A illustrates the consistent growth in the number of domestic payment cards issued (comprising debit, delayed debit, and credit cards) since 2017, reflecting the rising demand for card-based transactions. Panel B shows a relatively flat trend in the number of point-of-sale (POS) terminals, indicating a stable supply of card acceptance infrastructure. Although we cannot rule out any regional disparities based on the aggregate data, the stable number of POS terminals does not suggest major spatial differences in card acceptance by merchants following the pandemic. Panels C and D depict the continued rise in both the value and volume of card payment transactions, respectively. According to [Bundesbank \(2024\)](#) (German Central Bank), the volume of card payments in 2023 is split into 89% for in-store purchases and 10% for online transactions. The share of card payments in all consumer payments increased from 37% to 54% between 2017 and 2023, with an average annual growth rate of about 3 percentage points. There is no evidence of a trend break caused by the pandemic. This may seem unexpected, as authorities encouraged the use of card payments for hygiene reasons; however, this has had no noticeable impact on the long-term trend of payment behaviour.

Figure C.6: Trends in Card Payments in Germany



Notes: The figure presents trends in the adoption of consumer card payments in Germany. Card payments comprise all types of consumer payment cards (debit, delayed credit, and credit cards) that were issued by resident payment service providers (PSP) and used for offline and online card payments in Germany, excluding cards with an e-money function only. Panel A shows the annual number of cards. Panel B presents the number of POS terminals that accept card payments. Panel C reports the annual volume of card and cash payments and highlights the annual share of each payment technology. Panel D depicts the annual number of card payment transactions. Data are from the European Central Bank (ECB) Payment Statistics.

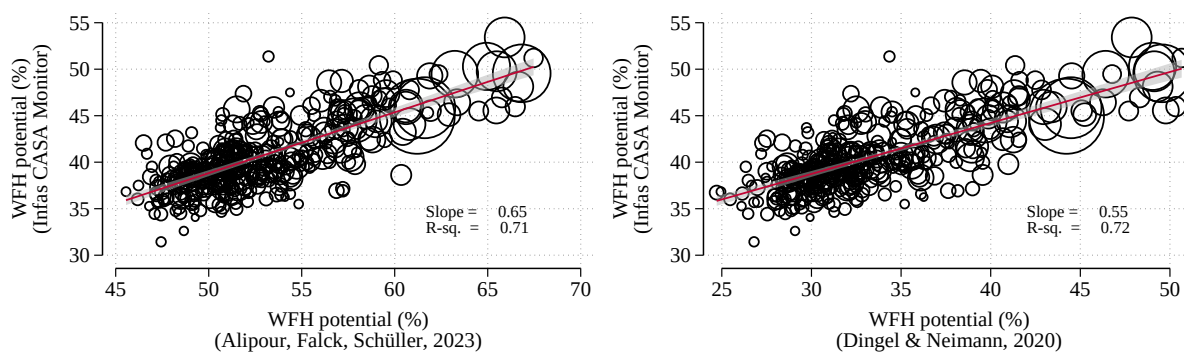
Appendix D Robustness

Appendix D.1 Validating WFH Potential

We check the plausibility of the WFH potential measure calculated from the infas360 Casa Monitor by comparing it to two other measures proposed by [Alipour, Falck and Schüller \(2023\)](#) (henceforth, AFS) and [Dingel and Neiman \(2020\)](#) (henceforth, DN), respectively. DN use information on the importance of various tasks from O*NET to classify US occupations as fully teleworkable or not. AFS draw on the BIBB-BAuA Employment Survey 2018, which asks about 17,000 employed persons in Germany whether their job could be done from home. If the respondent deems even occasional WFH “impossible”, their job is classified as incompatible with WFH. In the Casa Monitor, respondents are asked whether their job could be done from home at least one day per week. Thus, the three measures capture slightly different notions of WFH feasibility.

We compare the distribution of these measures across counties in [Figure D.7](#). The extrapolation of occupation-level WFH potential to regions is based on local 2019 occupation compositions. Specifically, a county’s WFH potential equals the weighted average of occupational WFH potentials, with weights equal to local employment shares. We plot the infas360 measure against the AFS and the DN measures, respectively. The linear fitted lines reveal that spatial differences in WFH potential are similar overall. The R^2 are high and virtually identical (0.7). The slopes are positive but below one, indicating that a marginal increase in either of the two alternative measures are matched by a less-than-proportionate increase in the infas360 measure.

Figure D.7: Comparison of WFH potential measures at the county level



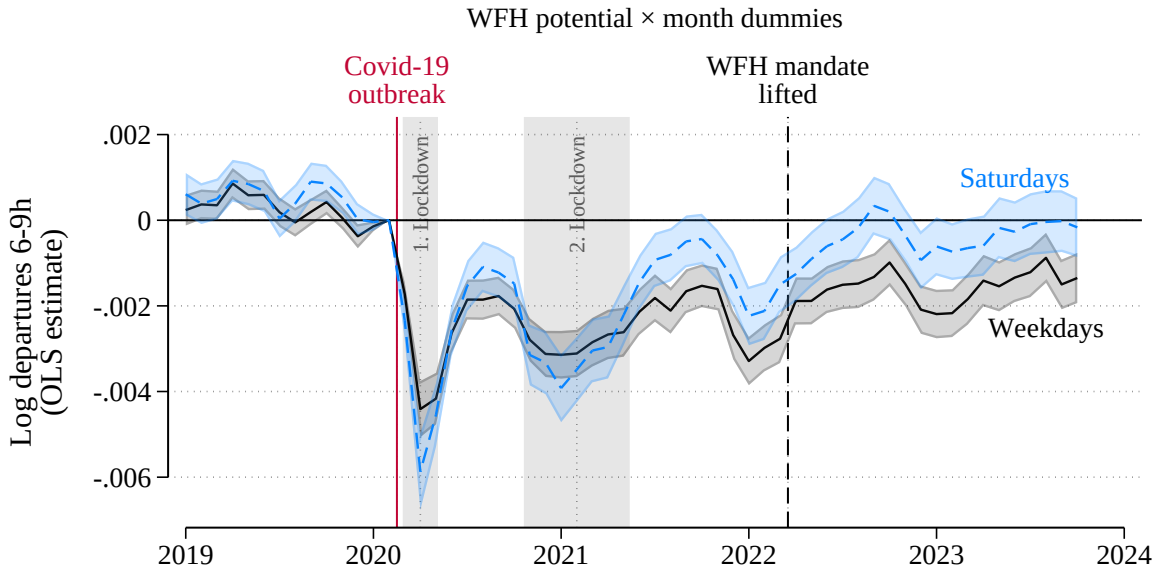
Notes: The figure shows scatterplots and linear fits of county-level WFH potential calculated from survey results of the infas360 Casa Monitor (see [Section 2](#)) against WFH potential computed by [Alipour, Falck and Schüller \(2023\)](#) based on the 2018 BIBB-BAuA Employment Survey (left) and [Dingel and Neiman \(2020\)](#) based on US O*NET data (right), respectively.

Appendix D.2 Robustness to alternative model assumptions

We test the robustness of the results delivered by the PPML estimator to estimating a linear model by OLS. Here, the dependent variables are log-transformed, and the sample excludes postcodes with zero-valued outcomes in any period. Recall that the log transformation means that the proportional effects of the explanatory variables implied by the OLS estimator are unit-specific. By contrast, the PPML estimates deliver proportional effects corresponding to an average level effect rescaled by the outcome mean. Thus, OLS effectively places higher weight on effects for postcodes with lower initial outcome level (Chen and Roth, 2024).

Dynamic DiD. First, we report the OLS results of the dynamic DiD specifications (Equation 1). Figure D.8 reports the first-stage estimates of WFH potential on log departures between 6 and 9 am. Figure D.9 plots the reduced-form estimates of WFH potential on log spending. In both cases, the patterns are virtually identical to the PPML results presented in Figure 2 and Figure 4, respectively.

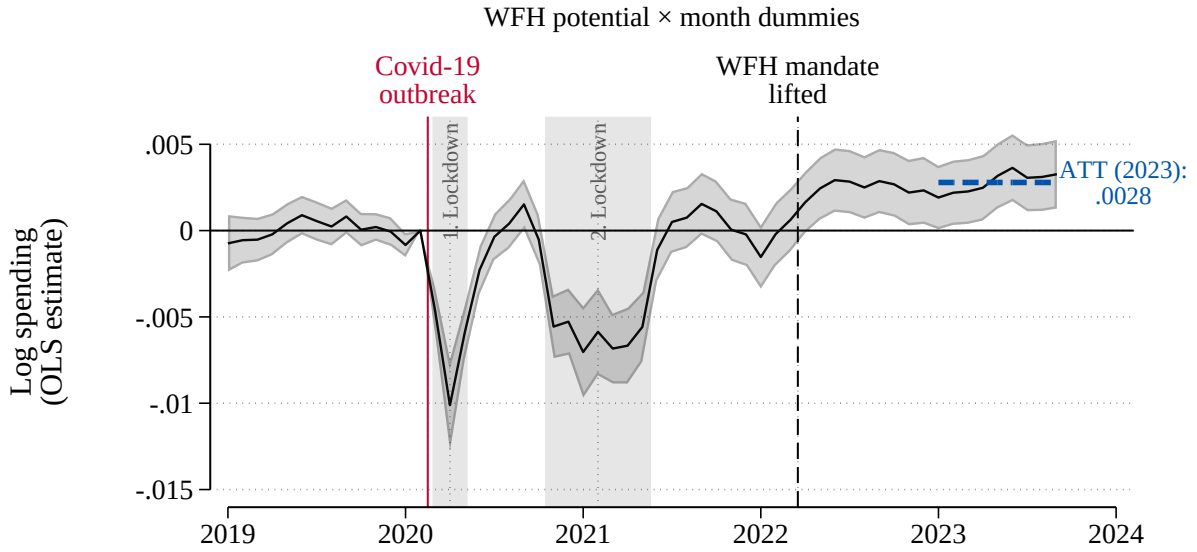
Figure D.8: First-stage DiD estimated by OLS



Notes: The figure shows DiD results based on Equation 1 estimated by OLS. The dependent variable is the log number of outbound trips between 6-9 am at the postcode level. Confidence bands are drawn at the 95% level based on standard errors clustered by postcode.

IV model. In Table D.3, we probe the robustness of the results from the non-linear model (Table 1) to estimating a linear model by OLS and 2SLS. Again, the dependent variables are log-transformed, and the sample excludes postcodes with zero-valued spending in 2019 or 2023. Nevertheless, the different models deliver virtually identical estimates of the ITT effects of WFH potential on spending (columns 1 and 2) and

Figure D.9: Reduced-form DiD estimated by OLS



Notes: The figure shows DiD results based on Equation 1 estimated by OLS. The dependent variable is log spending on weekdays (Mo-Fr) at the postcode level. Confidence bands are drawn at the 95% level based on standard errors clustered by postcode.

the elasticity of spending with respect to WFH-induced mobility changes (columns 5 and 6). Lee *et al.* (2022, 2023) show that confidence intervals of the 2SLS estimate need to be adjusted in case the first-stage F -statistic is below 104.67.²² Thus, square brackets report VtF -95% confidence intervals, which smoothly translate the value of the F -statistic into appropriate interval length. The IV estimates remain significant.

Appendix D.3 Correction for spatial correlation

This section addresses the potential concern that model errors are spatially correlated, delivering biased standard errors (SEs).

Dynamic DiD. To account for potential spatial correlation in the dynamic DiD specifications estimated by PPML, we use two-way clustering at the level of postcodes (to account for serial correlation) and at the level of MA×month-year, allowing for arbitrary correlation across postcodes in the same MA in each period (Cameron *et al.*, 2011). Figure D.10 reports the first-stage results of WFH potential on departures between 6 and 9 am. Figure D.11 plots the reduced-form results of WFH potential on spending. In both cases, the 95% confidence intervals are slightly larger compared to

²²More precisely, Lee *et al.* (2022) demonstrate that $F > 104.67$ ensures that the standard two-sided 5% t -test has a rejection rate of at most 5%.

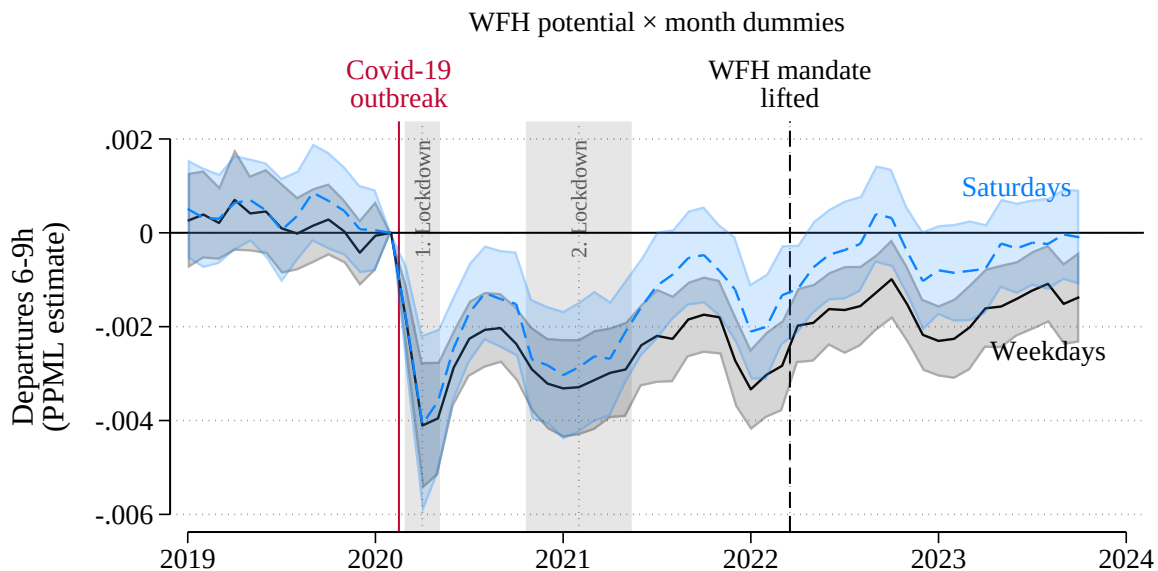
Table D.3: Main results (linear model)

	<i>Log Spending (Mo-Fr)</i>					
	Reduced form		Main equation		IV	
	(OLS)	(OLS)	(OLS)	(OLS)	(2SLS)	(2SLS)
	(1)	(2)	(3)	(4)	(5)	(6)
WFH potential (%)	0.008*** (0.002)	0.006*** (0.001)				
Log departures 6-9h			-0.006 (0.187)	-0.616*** (0.089)	-3.791*** (1.052) [-5.82,-1.79]	-3.412*** (0.679) [-4.69,-2.13]
Log distance to city centre	0.304*** (0.020)	-0.017 (0.013)	0.269*** (0.018)	-0.034*** (0.012)	0.361*** (0.033)	0.005 (0.016)
2019 log departures 6-9h		-0.634*** (0.020)		-0.644*** (0.020)		-0.676*** (0.024)
Net migration (2019-23)		-0.169 (0.190)		-0.144 (0.186)		-0.087 (0.231)
2019 log departures (6-9h) of neighbours		-0.075 (0.101)		-0.115 (0.101)		-0.174 (0.116)
2019 log spending p.c.		-0.662*** (0.013)		-0.664*** (0.013)		-0.674*** (0.013)
2019 spending share Food Services		0.060*** (0.017)		0.067*** (0.017)		-0.012 (0.028)
2019 spending share Grocery and Food Stores		-0.037*** (0.010)		-0.038*** (0.010)		-0.024** (0.012)
2019 spending share Apparel		0.333*** (0.035)		0.333*** (0.035)		0.325*** (0.037)
First stage coef.					-0.0021 <i>F</i> = 72.08	-0.0019 <i>F</i> = 56.62
Implied prop. effect (%)	0.80	0.65	-0.01	-0.62	-3.79	-3.41
Tot. obs.	7,274	7,274	7,274	7,274	7,274	7,274
#Postcodes	3,637	3,637	3,637	3,637	3,637	3,637

Notes: The table presents results based on Equations 2 and 3. The sample excludes all postcodes with zero-valued spending. All columns include postcode and metro area×post-dummy fixed effects. The implied proportional effect (IPE) corresponds to the percentage change in spending associated with a percentage-point change in WFH potential or a percent change in departures 6-9h, respectively. Standard errors are clustered by postcode and reported in parentheses. *VtF*-95% confidence intervals due to [Lee et al. \(2022, 2023\)](#) are reported in brackets in columns 5 and 6. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure 2 and Figure 4, respectively. Importantly, effects for weekdays remain significant and the same conclusions hold.

Figure D.10: First-stage DiD with two-way clustering of SE

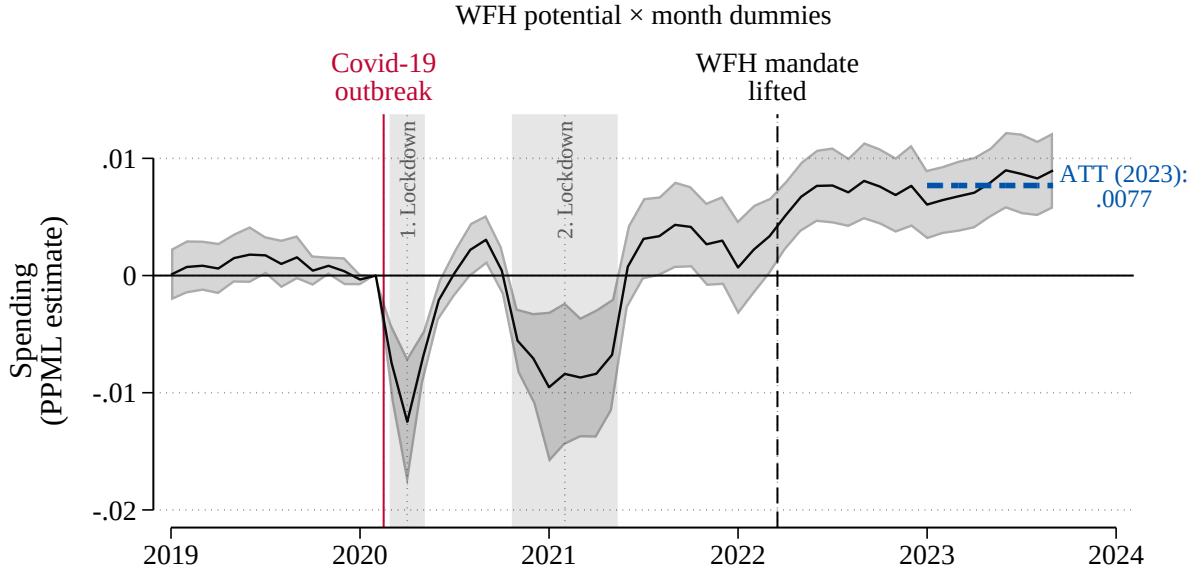


Notes: The figure shows DiD results based on Equation 1 estimated by PPML. The dependent variable is the number of outbound trips between 6-9 am at the postcode level. Estimates are transformed by $\exp(\beta^k) - 1$ to reflect proportional changes. Confidence bands are drawn at the 95% level based on standard errors clustered two-way by postcode and MA×month-year.

IV model. We test sensitivity of the IV results to correcting standard errors for spatial correlation in the model errors and alternative clustering in Table D.4. The first row reproduces the reduced-form and IV estimates from the linear model (columns 2 and 6 of Table D.3). Standard errors are clustered by postcode in the baseline. The corresponding 95% confidence intervals are reported in the second row for reference. First, we calculate Conley (1999) SEs to allow for spatial correlation of errors across neighbouring postcodes up to a given threshold (in addition to serial correlation). We set the threshold to 140km, which corresponds to twice the longest distance between a postcode and the city centre in our data, and assume a linear decay in the correlation structure (Bartlett kernel).²³ The confidence intervals are slightly larger than the baseline, but the estimates remain significant. Second, we cluster SEs by metro area to allow for serial correlation and arbitrary spatial correlation across postcodes in the same MA. This reduces the number of clusters to 50. To account for the small number of clusters, we use wild-cluster bootstrap SEs (5,000 repetitions) (MacKinnon

²³We compute Conley SEs using the acreg STATA command by Colella et al. (2020).

Figure D.11: Reduced-form DiD with two-way clustering of SE



Notes: The figure shows DiD results based on Equation 1 estimated by PPML. The dependent variable is average spending on weekdays (Mo-Fr) at the postcode level. Estimates are transformed by $\exp(\hat{\beta}^k) - 1$ to reflect proportional changes. Confidence bands are drawn at the 95% level based on standard errors clustered two-way by postcode and MA $\times$ month-year.

et al., 2022). The estimates remain significant at the 5% level. Finally, we adjust confidence intervals using the “spatial correlation principal components” (SCPC) method proposed by Müller and Watson (2022, 2023), which assumes a “worst-case” spatial correlation model.²⁴ Again, the 95% confidence intervals exclude zero, and our results are robust to all adjustments.

²⁴We implement the SCPC correction using the `scpc` STATA package.

Table D.4: Spatial-correlation robust inference

	Reduced form (OLS) (1)	IV-2SLS (2)
Estimate	0.006	-3.412
95%-confidence interval		
Cluster by postcode (baseline)	[0.0042, 0.0087]	[-4.7474, -2.0772]
Conley (1999) correction	[0.0030, 0.0099]	[-5.1604, -1.6641]
Wild cluster-bootstrap by MA	[0.00125, 0.01169]	[-6.369, -1.119]
Müller and Watson (2022) SCPC method	[0.0009, 0.0121]	[-6.1640, -0.6606]

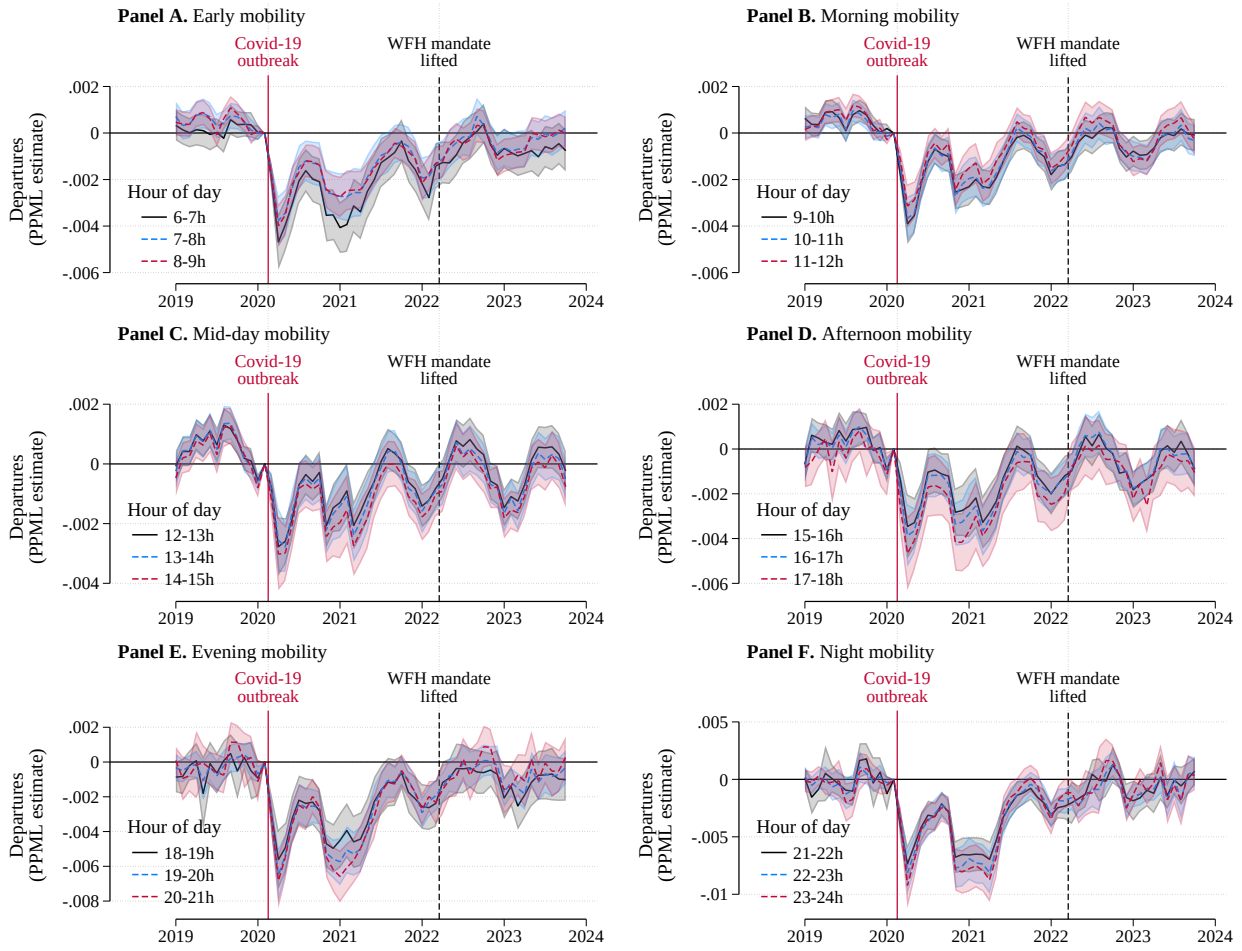
Notes: The table presents 95% confidence intervals constructed using different clustering of SEs and corrections for spatial error correlation. The first row reproduces the reduced-form and IV estimates from the linear model (columns 2 and 6 of [Table D.3](#)). The baseline uses clustered SEs by postcode. The subsequent rows use Conley SEs with a distance threshold of 140km and a Bartlett kernel, Wild cluster-bootstrapped SEs by metro area (5,000 repetitions), and the SCPC method by [Müller and Watson \(2022\)](#), respectively.

Appendix E WFH and Internal Migration

This section explores whether the realisation of WFH opportunities after the onset of Covid-19 spurred internal migration, particularly away from dense, urban areas.

Saturday mobility gaps. We extend the DiD results on mobility differences by WFH potential reported in [Figure 2](#), Panel B. We estimate DiD specifications for Saturday mobility, separately by the hour. The DiD plots presented in [Figure E.12](#) reveal that by 2023, average mobility differences across different levels of WFH potentials reverted to their pre-Covid levels throughout the day. These results do not support the hypothesis of systematic population loss driven by WFH-induced outmigration. Had this been the case, we would expect persistent mobility drops in areas subject to population outflows.

Figure E.12: WFH potential and Saturday mobility



Notes: Panels A–F show DiD results based on Equation 1 estimated by PPML. The dependent variables are the number of outbound trips by different hours of day at the postcode level. Estimates are transformed by $\exp(\hat{\beta}^k) - 1$ to reflect proportional changes. Confidence bands are drawn at the 95% level based on standard errors clustered by postcode.

Cross-county internal migration. We explore trends in internal migration in Germany using administrative data from the German Federal Statistical Office (Destatis). The data include annual origin-destination matrices of moves across the 401 counties from 2013–2022. One limitation is that only net flows are reported for each county pair; consequently, we cannot disentangle gross inflows from outflows.

Panel A of [Figure E.13](#) depicts the annual net internal migration of Germans versus foreigners. Net migration equals the sum of population gains over counties experiencing net gains, which, by definition, equals the aggregate loss in net-losing counties. A value of zero implies either that no migration occurred or that every population outflow was matched by an equal inflow, leaving the spatial distribution of the population unchanged. Net internal migration of foreigners spiked in 2015 due to the large influx of refugees. The data do not directly capture migration from abroad but reflect moves from the initial county of registration to another county in the same year. To avoid influences from such cross-country moves, we subsequently focus on the migration patterns of German citizens. From 2013 to 2019, German net migration across all counties remained steady at approximately 85,000 per year and accelerated in 2020. Note that an increase does not necessarily imply *more* moves; rather, it indicates that the migration that occurred resulted in a more pronounced change in the population distribution across counties.

Panel B breaks down migration flows by area type. We map counties into one of five categories following the MA catch area definitions (see [Figure A.1](#)): *City*, if the county is an urban core of an MA, *Suburb*, for counties primarily within the suburban areas of an MA, *Commuting belt*, for counties that are part of an MA but located further away, and *Remote*, if the county predominantly does not belong to a MA.²⁵ The chart shows that commuting belts have persistently gained population from cities. The trend accelerated in 2020 and 2021. By contrast, there has been net migration from remote counties into cities until 2019. This changed in 2020–22 when Germans moved from cities to remote counties on balance.

Panel C quantifies the excess net migration of Germans from 2020 to 2022. The linear trend line over the pre-Covid years (2013–19) shows that annual net migration remained stable until 2019. The dashed line traces cumulative deviations from the linear trend. Over 2020–22, excess migration totalled 119 thousand. For comparison, the extensive-margin increase in WFH among employed persons between 2019 and

²⁵ Ambiguous classifications are resolved based on a county's population majority.

2013 alone amounted to roughly 6 million.²⁶ Panel D plots excess migration among county types. The results indicate that excess migration was primarily driven by net flows from cities to remote counties and cities to commuting belts. Panel E calculates excess migration from more to less central counties. We observe an additional 59 thousand moves between 2020–22 above the pre-Covid trend. Even if we attribute all of these to WFH, this represents less than one percent of new remote workers. Hence, the consequences of WFH-induced migration for spatial changes in economic activity appear negligible at best.

Panel F documents that cities lost (German) population every year before 2020. But the urban population loss accelerated during the crisis. By contrast, commuting belts and remote counties saw even stronger population gains from other counties during the pandemic years.

Inward versus outward migration. Figure E.14 adds nuance to the net internal migration patterns by distinguishing between inflows and outflows. The data register annual flows by county but do not include information on origin or destination. Panel A plots the annual sum of outflows (dashed lines) and inflows (solid lines) by county type. In the years before Covid, cities saw greater outflows than inflows, while the opposite was true for commuting belts. Remarkably, both outflows from and inflows into cities dropped between 2019 and 2020. Thus, the accelerated urban population loss during the crisis is explained by the sharper drop in inflows. This finding contrasts with the narrative that workers transitioning to WFH were the source of a city exodus. Instead, the pattern is predictable by models of internal migration responses to local economic shocks and similar to previous crises (Monras, 2020). Panels B–C compute cumulative excess flows by county type for inward, outward, and net migration, respectively. 2020–22 excess migration is calculated as the sum of deviations from the linear 2013–19 trend. The charts confirm that migration in and out of all county types slowed during the crisis. Importantly, excess population gains in less central areas were driven by a stronger decline in moves to more central counties.

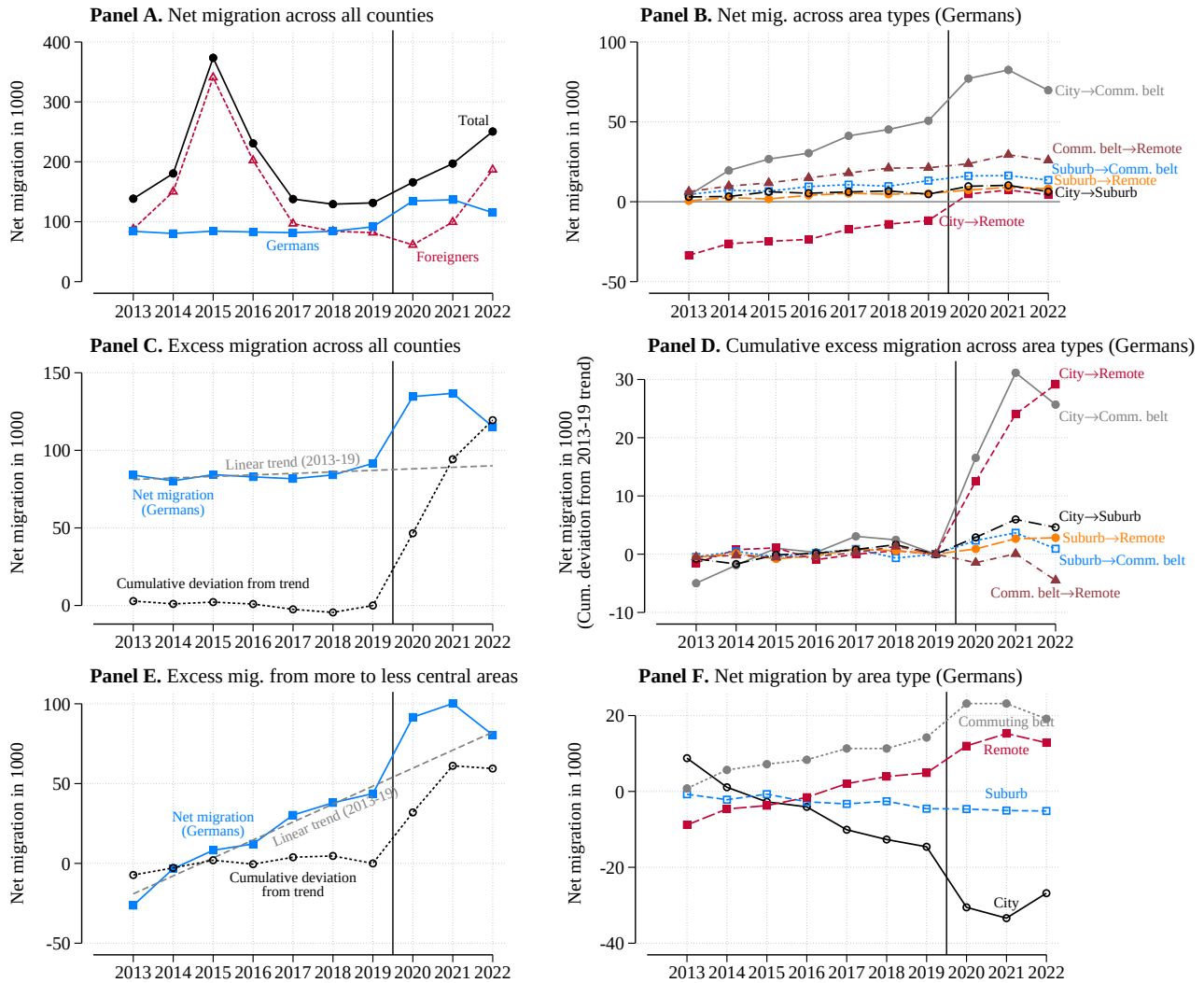
Municipality-level population changes. Next, we examine whether WFH potential is associated with diverging population trends using municipality-level data. To this end, we build a panel of 4,777 municipalities belonging to an MA, including information on each municipality’s distance to the MA city centre, its WFH potential (ag-

²⁶The value is based on WFH rates reported in Figure 1 and 33.5 million employed persons in 2019.

gregate the postcode level), and the annual number of employed German residents.²⁷ We adapt our DiD specification from [Equation 1](#) to the municipality panel. The dependent variable is the annual log number of employed German residents. Again, we include year-MA fixed effects to ensure comparisons within metro areas. Panel A of [Figure E.15](#) plots the DiD interaction terms with log distance to the city centre. The estimates suggest that more peripheral municipalities consistently grew more slowly than more central municipalities between 2016 and 2019. The trend reversed with the pandemic outbreak. Between 2019 and 2022, population growth was not different across more and less central municipalities. Panel B plots DiD estimates for WFH potential. The pattern reveals that higher-WFH-potential areas saw stronger population growth before Covid induced a trend break. Again, log employed population evolved roughly in parallel between 2019 and 2022 across areas with different WFH exposure. Finally, Panel C plots DiD estimates for WFH potential controlling for interaction terms of log distance with year dummies (matching our preferred specification in the main analysis). The results show that partialling out trend differences by distance renders population growth in high versus low WFH areas roughly parallel throughout the observation period. In particular, higher-WFH-potential areas did not experience a stronger population decline after 2019. Again, these findings challenge the view that realising WFH opportunities induced outmigration. More central areas missed out on population growth anticipated by the pre-crisis trends. This may be due to a new-found aversion to density stemming from contagion risks or a decreased appreciation for urban amenities, possibly subjective or influenced by Covid restrictions.

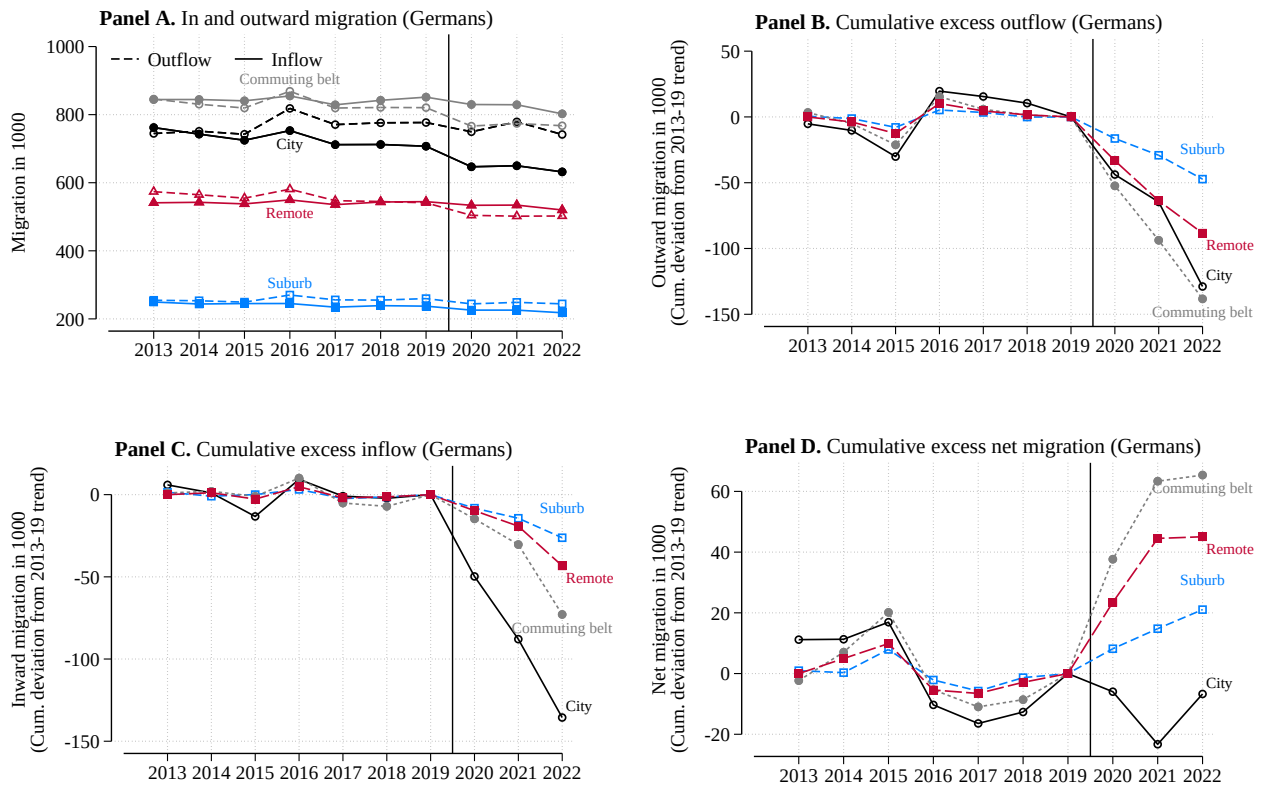
²⁷We focus on employed persons and German citizens to capture work-related population changes and avoid influences from the refugee waves of 2015 and 2022.

Figure E.13: Cross-county migration in Germany (origin-destination Matrix)



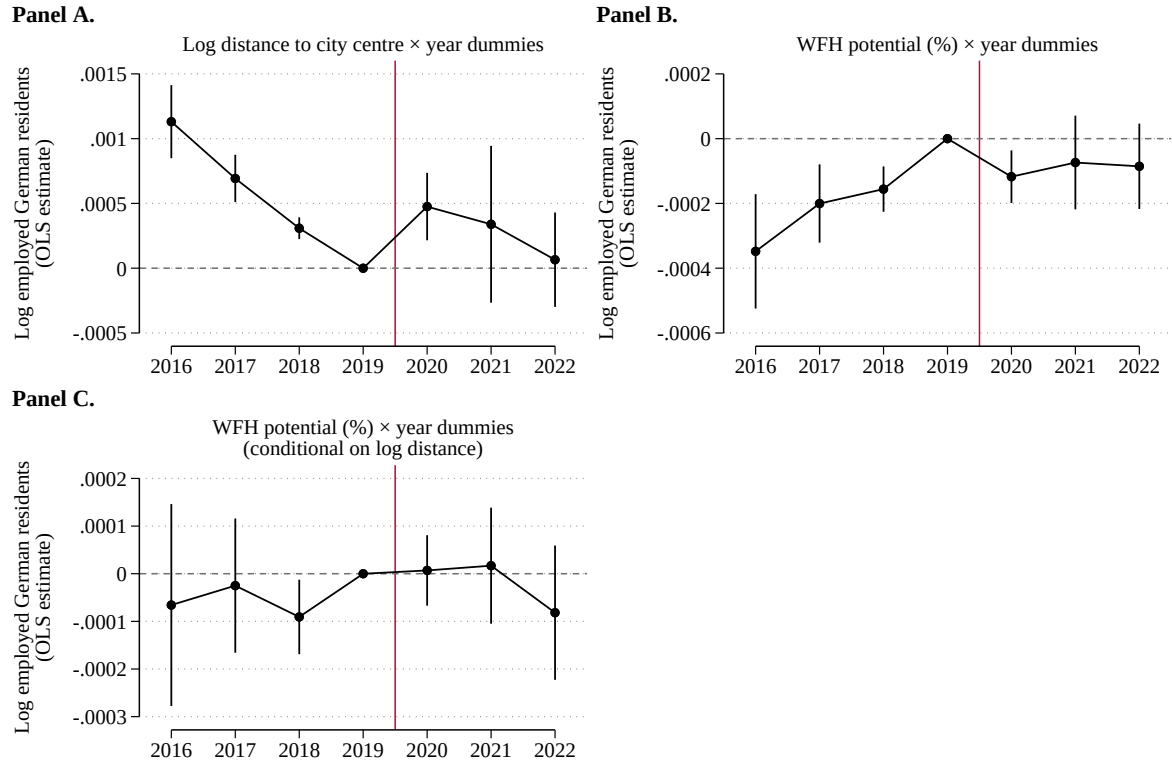
Notes: Panels A–F plot trends in internal migration across county boundaries (excluding flows to or from abroad) based on origin-destination matrices from the German Federal Statistical Office (*Destatis*). Panel A shows annual net migration across all 401 counties by citizenship. Net migration equals the sum of net population gains over counties with net gains (which is equivalent to the aggregate net loss across net-losing counties). Panel B plots the annual net migration of German citizens between different area types. Panel C shows the excess net migration of Germans across all counties, calculated as the cumulative deviation from the 2013-19 linear trend. Panel D reports excess migration for moves between area types, and Panel E shows this for moves from more to less central counties. Panel F plots the net migration of Germans by area type, where negative (positive) values correspond to a net loss (gain).

Figure E.14: Cross-county migration in Germany



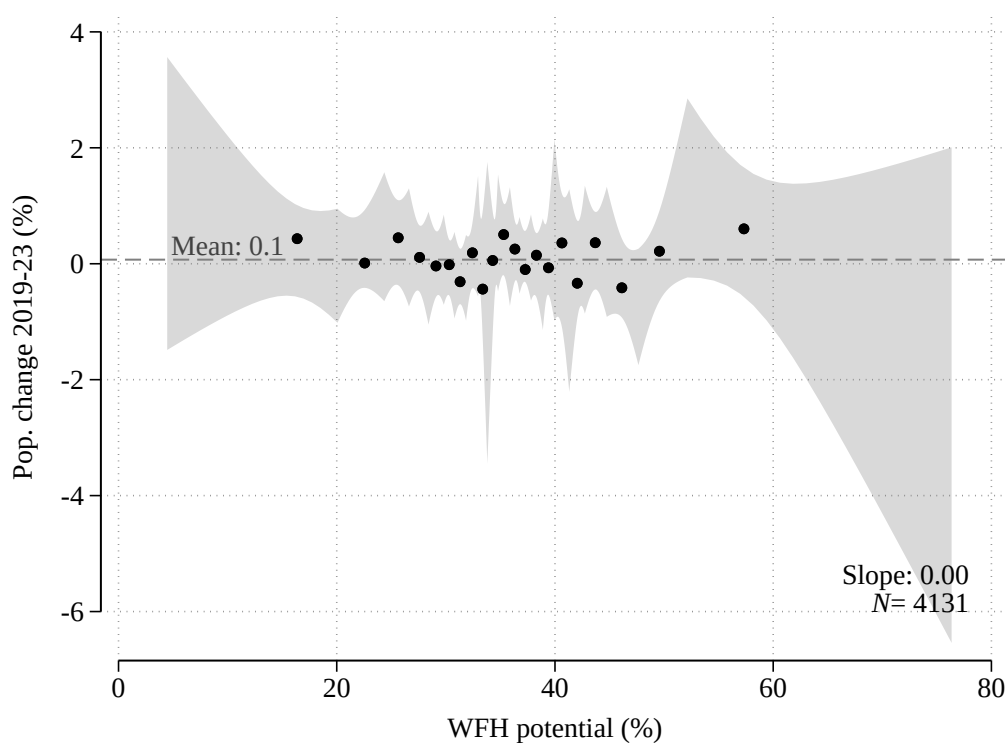
Notes: Panels A–D plot trends in internal migration of German citizens across county boundaries (including flows to or from abroad) based on data from the German Federal Statistical Office (*Destatis*). Panel A shows the annual sums of population inflows and outflows by area type. Panels B–D plot excess outflow, inflow, and net migration by area type, respectively, calculated as the cumulative deviation from the respective 2013–19 linear trend.

Figure E.15: DiD Results on changes in employed population (municipality-level)



Notes: Panels A–C report DiD estimates from OLS regressions of the log number of employed German residents on DiD interaction terms, year $\times$ MA fixed effects, and municipality fixed effects. DiD interactions equal year-dummies $\times$ log distance to the city centre in Panel A and year-dummies $\times$ WFH potential in Panel B. Panel C reports the DiD coefficients from WFH potential $\times$ year dummy interactions, conditional on log distance $\times$ year fixed effects. Confidence intervals are drawn at the 95% level using standard errors clustered by municipality. Employment data are from the Federal Employment Agency (*Bundesagentur für Arbeit*).

Figure E.16: WFH potential and 2019-23 population change (postcode-Level)



Notes: The figure shows a binned scatterplot of population changes between 2019 and 2023 in percent against WFH potential, conditional on MA fixed effects using the methodology by [Cattaneo et al. \(2024\)](#). Bins are of equal size using postcode-level observations. The shaded area highlights the 95% confidence band of the conditional mean function using standard errors clustered by postcode.

Appendix F Other Results

Appendix F.1 Estimating the wage premium on WFH feasibility

We quantify the wage premium on having a WFH feasible job using pre-Covid data from the 2018 BIBB-BAuA Employment Survey of the German workforce. We restrict the sample to employed persons aged 16–64. Following [Alipour, Falck and Schüller \(2023\)](#), we create a WFH feasibility dummy equal to one if the respondent does *not* indicate that (occasional) WFH is “impossible” with their job.

[Figure F.17](#) collapses the data into 50 hourly wage quantiles. The plot reveals that WFH potential increases nearly monotonically with wage. Employees at the top of the distribution are twice as likely to hold a teleworkable job compared to those at the bottom.

We quantify the average wage premium on WFH feasibility in [Table F.5](#). Column 1 reports the OLS estimate from regressing log hourly wage on a WFH feasibility dummy. The coefficient suggests that having a job that can be done from home comes with a 25.5% wage premium, on average. Column 2 adds broad occupation and sector fixed effects, reducing the estimate to 12.7%. Column 3 conditions on narrow occupation and industry fixed effects instead. This further reduces the premium to 9.9%. Finally, we add controls for demographic characteristics (gender, age, marital status, children, education, migrant background), firm size categories, and firm tenure. Accounting for these factors reduces the estimated premium to 7.6% (Column 4).

Table E.5: Estimating the wage premium on WFH feasibility

	<i>Log hourly wage</i>			
	(1)	(2)	(3)	(4)
WFH feasible job (0/1)	0.255*** (0.010)	0.127*** (0.011)	0.099*** (0.011)	0.076*** (0.010)
Female (0/1)				-0.135*** (0.010)
Migration background (0/1)				-0.011 (0.012)
Log age				0.093*** (0.019)
Married (0/1)				0.044*** (0.009)
Children under 18 (0/1)				0.040*** (0.009)
Academic degree (0/1)				0.217*** (0.010)
Log firm tenure				0.098*** (0.005)
Firm size category				
<10 workers (omitted)				
10-19 workers				0.068*** (0.018)
20-49 workers				0.065*** (0.016)
50-99 workers				0.093*** (0.017)
100-249 workers				0.111*** (0.016)
250-499 workers				0.160*** (0.017)
500-999 workers				0.173*** (0.019)
1000+ workers				0.241*** (0.017)
R ²	0.08	0.24	0.32	0.47
Observations	16,595	16,595	16,592	16,096
2-digit occupation fixed effects (36 cat.)		×		
Sector fixed effects (21 cat.)		×		
3-digit occupation fixed effects (139 cat.)			×	×
Industry fixed effects (84 cat.)			×	×

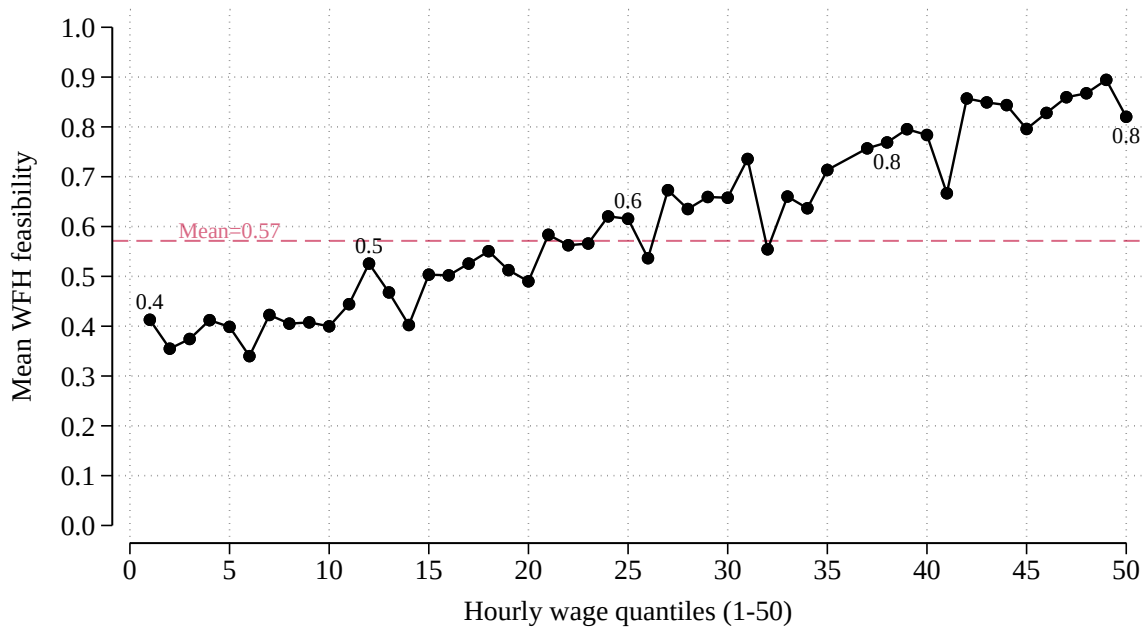
Notes: The table reports OLS regressions of log hourly wage on WFH feasibility and other characteristics at the employee level. WFH feasibility is a dummy equal to one if the respondent does not rule out that their job could be done at least partly from home (see [Alipour et al. \(2023\)](#) for details). Heteroskedasticity-robust standard errors are reported in parentheses. Data are from the 2018 BIBB-BAuA Employment Survey. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table F.6: Workplace effects by industry (PPML estimates, 2019–23 changes)

	<i>Spending (Mo-Fr)</i>							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Total spending				Grocery stores			
Log 2019 job density	-0.061*** (0.006)				-0.191*** (0.011)			
2019 log departures 15-18h		-0.226*** (0.016)				-0.609*** (0.030)		
2019 log spending p.c.			-0.224*** (0.021)				-0.535*** (0.010)	
Log distance to city centre				0.077*** (0.010)				0.228*** (0.022)
Implied prop. effect (%)	-0.06	-0.23	-0.22	0.08	-0.19	-0.61	-0.53	0.23
Tot. obs.	8,124	8,124	8,124	8,124	6,700	6,700	6,700	6,700
#Postcodes	4,062	4,062	4,062	4,062	3,350	3,350	3,350	3,350
	Food services				Apparel stores			
Log 2019 job density	-0.171*** (0.012)				-0.123*** (0.040)			
2019 log departures 15-18h		-0.281*** (0.034)				-0.302*** (0.107)		
2019 log spending p.c.			-0.337*** (0.018)				-0.277*** (0.027)	
Log distance to city centre				0.221*** (0.019)				0.136*** (0.047)
Implied prop. effect (%)	-0.17	-0.28	-0.34	0.22	-0.12	-0.30	-0.28	0.14
Tot. obs.	5,240	5,240	5,240	5,240	3,460	3,460	3,460	3,460
#Postcodes	2,620	2,620	2,620	2,620	1,730	1,730	1,730	1,730

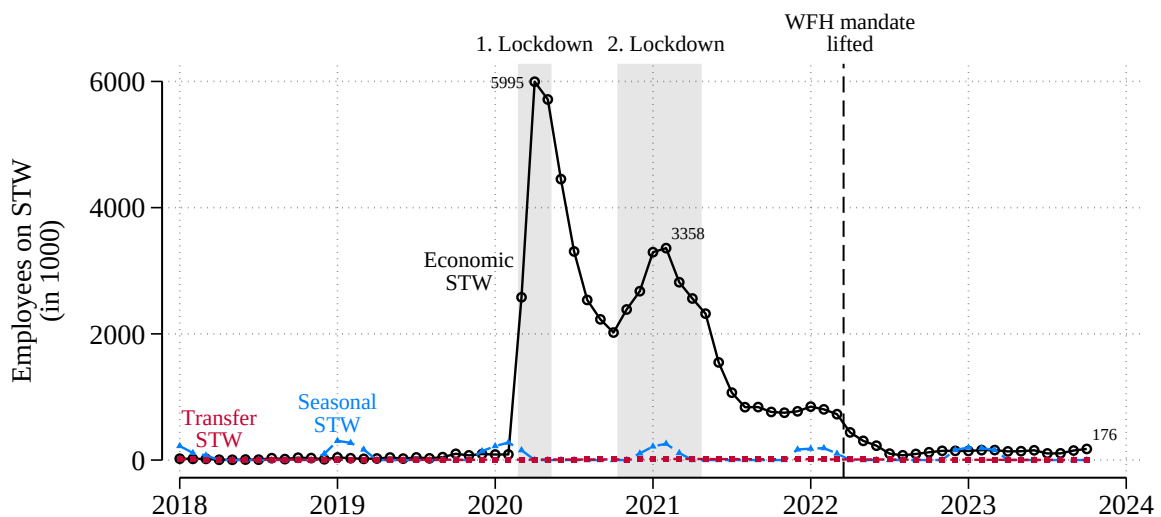
Notes: The table presents results based on a variation of [Equation 2](#), where average weekday spending is regressed on an interaction of post-dummy× explanatory variable of interest using the PPML estimator. All columns include postcode and metro area×post-dummy fixed effects. The implied proportional effect (IPE) corresponds to the percentage change in spending associated with percent change in the explanatory variable of interest, respectively, and is calculated as $100 \times [\exp(\cdot) - 1]$. Standard errors are clustered by postcode and reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure E.17: WFH potential across the wage distribution



Notes: The figure plots WFH potential by hourly wage quantile. WFH potential corresponds to the share of respondents in a wage quantile who do not rule out that their job could be done at least partly from home (see [Alipour et al. \(2023\)](#) for details). Data are from the 2018 BIBB-BAuA Employment Survey.

Figure E.18: Short-time work in Germany



Notes: The figure shows the number of employees on short-time work (STW) by month and STW type. Economic STW aims to mitigate unforeseen economic downturns such as the Covid-19 crisis. Seasonal STW addresses predictable demand fluctuations in sectors such as agriculture or construction. Transfer STW aims to support employees transitioning to new roles during employer restructuring. Data are from the Federal Employment Agency (*Bundesagentur für Arbeit*).

Panel A. Offline and online spending

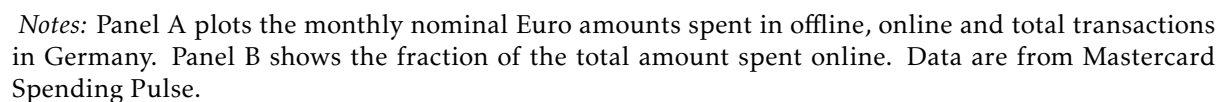
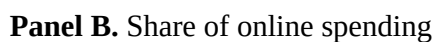
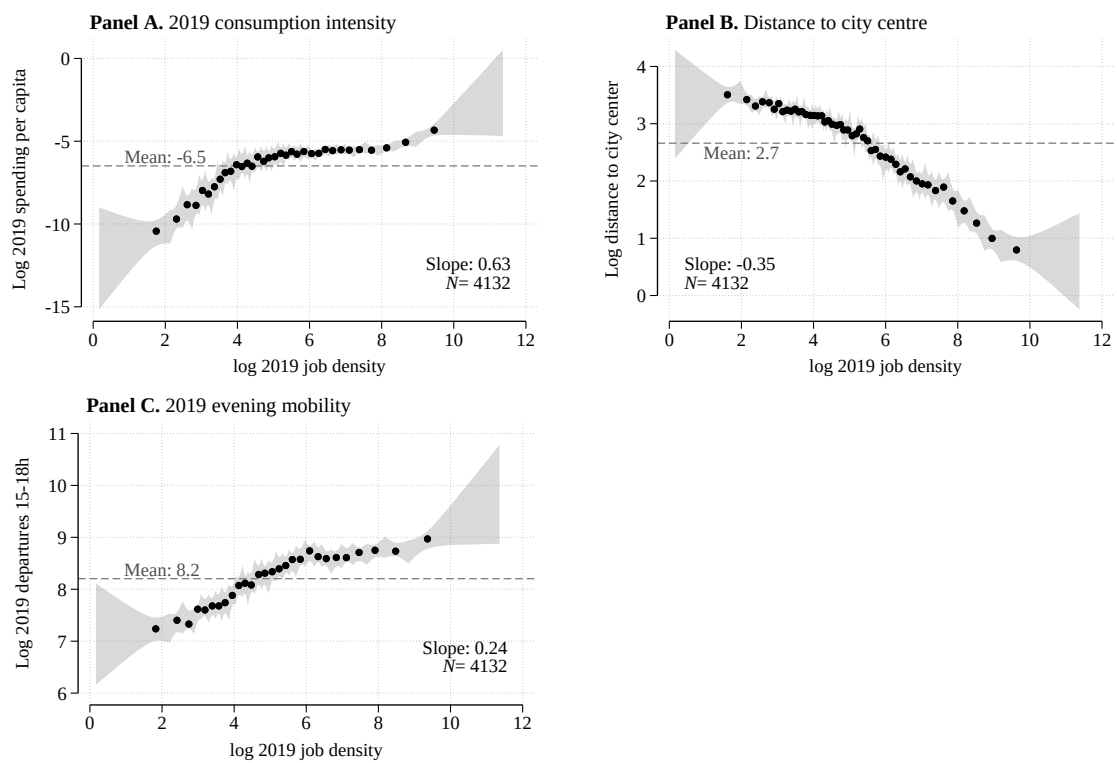
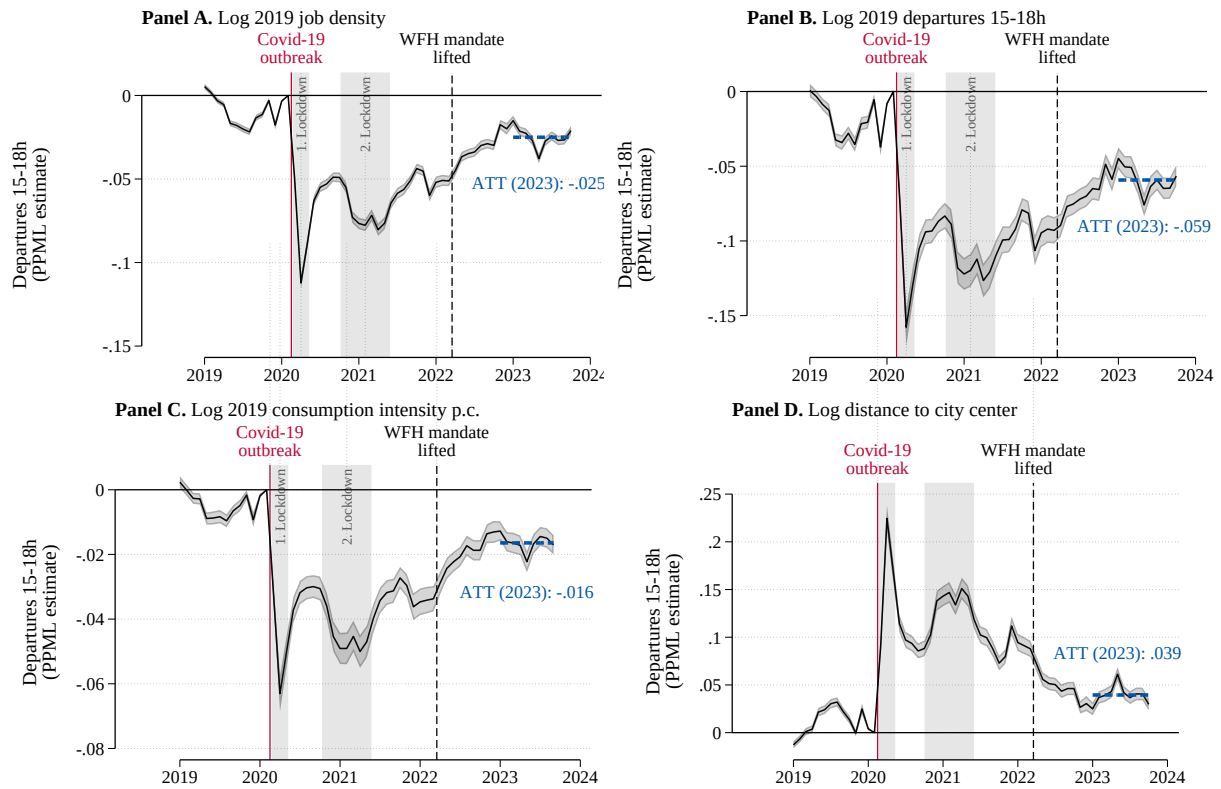


Figure E.20: Correlates of 2019 job density



Notes: Panels A–C show binned scatterplots conditional on MA fixed effects using the methodology by [Cattaneo *et al.* \(2024\)](#). Bins are of equal size using postcode-level observations. The shaded areas highlight 95% confidence bands of the conditional mean functions using standard errors clustered by postcode. Job density is defined as employment per square kilometre.

Figure E.21: Consequences for workplace areas



Notes: Panels A–D report DiD estimates from a PPML regression of monthly weekday evening departures on DiD interaction terms of month dummies with a time-invariant variable Z , month $\times$ MA fixed effects, and postcode fixed effects. Z corresponds to log 2019 job density in Panel A, log 2019 departures between 3 and 6 pm (Panel B), log 2019 spending per capita (Panel C), and log distance to the city centre (Panel D). Shaded areas highlight 95% confidence bands using standard errors clustered at the postcode level.