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The Value of One Office Day a Month*

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Abstract

Remote work has expanded rapidly, but the value of regular in-person contact remains unclear. We report a randomized controlled trial in which a large multinational randomly assigned 248 customer-service employees to remain fully remote or to work at the office together one day a month. Monthly office days gradually increased productivity, with treated employees handling 7.8% more calls per hour in the post-intervention period. Office days also strengthened workplace communication: treated employees spent 36 additional minutes communicating with colleagues in the week after an office visit, were more likely to report receiving manager feedback, and employee pairs randomly assigned as desk neighbors were 11 percentage points more likely to communicate afterward. In addition, monthly office days reduced attrition by a third. The resulting gains in productivity and retention generated a benefit–cost ratio of about 5:1. These findings show that limited, coordinated in-person contact can improve communication, performance, and retention in remote teams.

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1 Introduction

Remote and hybrid work have transformed a central feature of economic organization: where workers, managers, and teams interact. What began as an emergency response to the COVID-19 pandemic has become a durable feature of modern labor markets. By 2026, roughly 100 million employees in Europe and North America worked schedules that combine home and office days (Barrero et al., 2023; Aksoy et al., 2025a; Buckman et al., 2025). This shift creates large potential gains: remote work can reduce commuting costs, expand worker flexibility, and widen firms' access to talent, with particular benefits for workers facing commuting, disability, care, or scheduling constraints (Mas and Pallais, 2017; Pabilonia and Vernon, 2022; Aksoy et al., 2025b; Bloom et al., 2026).

At the same time, remote work changes how firms create and sustain organizational capital. Many interactions that support coordination, learning, mentoring, problem solving, and attachment to the firm occur informally and repeatedly through face-to-face contact. When employees are no longer routinely together, these interactions may weaken. Existing evidence highlights this design problem: hybrid arrangements can reduce attrition without harming performance in some settings, while productivity effects in fully remote operations appear to depend on task structure, monitoring technologies, managerial practices, and adaptation over time.[1] The central question is therefore whether firms can preserve the flexibility of remote work while sustaining the workplace interactions that support performance and retention.

We study this question using a nine-month randomized controlled trial in a large multinational service firm. The study involves 248 employees in a fully remote inbound customer-service team. Employees were randomly assigned either to remain fully remote or to work together in person one fixed day each month.[2] The intervention was deliberately minimal. It did not change pay, workload, working hours, production technology, incentives, or formal training. Instead, it reintroduced regular opportunities for co-workers and team leaders to spend time together, allowing us to test whether occasional coordinated in-person contact can restore peer learning, team communication, and informal manager feedback while preserving the firm's remote operating model.

The setting is well suited to isolating the effect of periodic coordinated co-location. Employees perform standardized tasks, calls are routed centrally rather than chosen by workers, and the firm records detailed, high-frequency administrative data on productivity, time use, service quality, and retention. Because treatment was assigned at the employee level, the employee is the unit of randomization and inference. The repeated administrative observations improve precision and allow us to trace how effects evolve before, during, and after the intervention.

We find that coordinated co-location led to improvements in several workplace outcomes. In the five-month post-intervention period, treated employees handled 7.8% more calls per hour than their fully remote peers. This raw gap combines within-worker productivity gains and changes in the composition of employees who remain with the firm. Employee fixed-effects estimates imply a within-worker gain of 0.63 calls per hour, or 5.8% of the pre-treatment mean. At the same

[1] See Yang et al. (2022), Brucks and Levav (2022), and Emanuel et al. (2023) on collaboration, creativity, and proximity; Bloom et al. (2024) and Choudhury et al. (2023) on hybrid work; and Battiston et al. (2021), Choudhury et al. (2021), Deole et al. (2023), Gibbs et al. (2023), Burdett et al. (2024), and Emanuel and Harrington (2024) on remote work and productivity.

[2] We attribute the firm's receptivity to this intervention at least partly to its history. Specifically, the firm shifted from fully on-site to fully remote work for its customer-service employees in reaction to the pandemic. That shift brought productivity gains but raised turnover and turnover costs (Aksoy et al., 2025b).

time, cumulative attrition fell from 21.0% in the control group to 13.7% in the treatment group. We find no evidence of a decline in service quality, indicating that higher output reflects greater efficiency rather than a speed–quality trade-off. For the firm, the value of productivity gains and lower turnover outweighed the costs of extra spending on transport and meals by a factor of five.

The timing and pattern of the effects are difficult to reconcile with a transitory morale response or a simple Hawthorne effect. If the gains reflected novelty, excitement about being selected, or disappointment among control workers, we would expect them to appear immediately and fade. Instead, productivity gains emerged gradually, after repeated in-person encounters, and persisted after mandated office attendance ended. The results are also robust to alternative aggregation levels and to adding team-leader and date fixed effects. We find similar patterns when comparing treated employees with non-volunteers in the same province, and no evidence that the gains are concentrated among a narrow subset of employees.

Additional evidence points to interaction, learning, and feedback. Coordinated co-location strengthened subsequent communication among co-workers after they returned to remote work, and randomized seating assignments generated new peer links within the treated group. These communication-network results complement survey evidence that treated employees reported more regular manager feedback, stronger team communication, and greater cultural fit. Taken together, the results suggest that the benefits of proximity do not require a full return to the office: even limited but coordinated in-person contact can improve communication, retention, and performance in fully remote teams.

2 Setting and experimental design

Our intervention was implemented in the customer service division of TEMPO BPO, in collaboration with one of its clients, a publicly listed multinational telecommunications company.[3] TEMPO BPO is one of Turkey's largest business-process-outsourcing providers, delivering customer service and back-office services to domestic and international clients. As of 2025, the firm employed over 3,500 employees.

The experiment takes place in the firm's inbound customer service operations. This setting is well suited to studying workplace practices because production is standardized, routinely monitored, and organized around a clearly defined task: handling incoming customer calls under fixed schedules and stable team structures.

Customer service employees are paid the national minimum wage and do not receive performance bonuses or other variable pay. Career progression operates through promotions, with strong performers eligible for team-leader roles that involve managerial responsibilities and higher pay. Each employee is assigned to a team of roughly 20 employees overseen by a team leader, who manages day-to-day operations and monitors individual performance. The firm records detailed, high-frequency performance data through an in-house system, including call volume, talk time, customer-satisfaction measures, and call-audit scores.

Work is organized in standard five-day, eight-hour shifts, with two 15-minute breaks and a 30-minute lunch. Calls are routed centrally to the first available customer service employee, so

[3]For confidentiality reasons, we do not disclose the client's name.

employees cannot choose which calls to handle. Employees handle queries using firm-provided equipment and standardized client protocols.

In March 2020, following Turkey's first national lockdown, the firm moved its entire workforce to remote work within two weeks. It provided laptops and internet subsidies, while keeping team structures, schedules, monitoring, and evaluation metrics unchanged. After restrictions eased in late 2021, the firm maintained remote work as its default operating model. By mid-2024, more than 90% of employees remained fully remote.

While remote work delivered operational benefits, including access to a wider talent pool and lower office-related costs, internal feedback pointed to weaker team cohesion, fewer opportunities for real-time coaching, and persistent retention problems. In the fully remote setting, most day-to-day interactions took place through messaging applications, leaving limited scope for team leaders or peers to observe problems as they emerged, provide immediate feedback, or exchange advice informally. These concerns motivated the randomized trial, in which we test whether occasional coordinated in-person contact could restore some benefits of office-based interaction without sacrificing the advantages of the firm's remote work model.

2.1 Study population and randomization

The study population consists of employees assigned to the telecommunications client account in inbound customer-service roles, handling calls related to billing, packages, upgrades, and service issues. To implement the intervention, the firm used its Şanlıurfa office, one of several regional hubs it operated before the pandemic. The office is accessible by shuttle and had sufficient spare capacity to host treatment-group employees once per month without disrupting other operations.

In March 2024, the firm informed 661 employees in the province about a program requiring one office day per month. Participation was voluntary. Over March–April, 476 employees, about 72%, expressed interest, while 185 did not volunteer, most commonly citing family or care responsibilities.

From the volunteer pool, the firm applied pre-specified eligibility criteria to ensure that monthly office days were feasible and that participants had sufficient job experience. Employees were eligible if, at the start of the experiment, they (i) had at least three months of tenure and had not handed in notice to leave, and (ii) lived within roughly 45 minutes of the office, approximately a 16-kilometer / 10-mile radius. These criteria yielded 248 eligible volunteers, who constitute the experimental sample.[4] Relative to non-volunteers, eligible employees were less likely to be married or have children, consistent with the family and care constraints reported by some non-volunteers. However, they did not differ systematically in baseline productivity or service-quality measures.[5] This pattern suggests that selection into the experimental sample primarily reflected feasibility and willingness to attend monthly office days, rather than baseline performance differences.

Assignment was implemented in late April 2024 by the firm's HR department using employees' pre-existing company ID numbers, which had been assigned for administrative purposes before the experiment. Among eligible volunteers, employees with odd-numbered IDs were allocated to treatment and those with even-numbered IDs to control, yielding two groups of 124 employees each.

[4] During the intervention, there was no replacement hiring in the experimental sample.

[5] Table A.1 compares eligible volunteers with non-volunteers in the same province. Eligible volunteers were less likely to be married or have children, but had similar baseline calls per hour, call duration, break time, customer ratings, and audit scores.

Because the rule depended only on ID parity, HR had no discretion once the eligible sample had been defined, and adjacent employees in the hiring sequence were mechanically split across treatment and control.

In June 2024, two months before the intervention began, the firm finalized team assignments and fixed team leaders for the study. These assignments were made without reference to treatment status, and employees could not sort across teams or choose their shifts.[6] This kept employees' managerial and team environments constant during and after the experiment.

The setting limits interference, especially across teams. Outside assigned office days, employees worked fully remotely and had limited reason to interact outside their own team. Production was also standardized: calls were routed centrally to the first available employee, tasks and protocols were uniform, and performance was monitored through the same system. These features make cross-team spillovers unlikely. We study within-team interactions using the randomized seating plan. Both groups remained fully remote during June and July 2024, which we use as the pre-intervention baseline.

2.2 Intervention: monthly office day

The intervention ran for nine months, from August 2024 to April 2025. Treated employees were scheduled to work from the office on one fixed day each month, while control employees remained fully remote throughout (Figure A.1a). The once-per-month schedule was designed as a minimal intervention that reintroduced coordinated in-person contact without changing the firm's remote operating model.

On office days, treated employees traveled together on company shuttles and arrived before their regular shift (Figure A.1b). They performed their usual tasks on their regular company laptops, using the same software systems and protocols as on remote days. Production technology and incentives were unchanged: shift length and pay were fixed, calls were routed in the same way, performance-evaluation criteria were identical, and no overtime was permitted.

Relative to a standard remote day, the monthly office visit shifted two margins central to the intervention's hypothesis. First, *managerial input*: office days created opportunities for real-time feedback and mentoring by team leaders. Second, *peer interaction*: the office facilitated informal social contact, peer support, and on-the-job learning. The intervention was therefore designed to restore forms of workplace interaction that had become limited in the fully remote environment, especially rapid problem solving, informal peer learning, and face-to-face coaching.

To measure communication patterns and potential peer spillovers, we implemented randomized seating assignments within teams. The office-day schedule also created shared time for informal contact, including breakfast, coffee breaks, and lunch (Figure A.1c). We did not introduce any formal training or curriculum, so the treatment captures in-person co-location and interaction rather than structured instruction. The firm covered all direct participation costs, including transport, meals, and refreshments. After each office day, treated employees returned to fully remote work until the next scheduled visit. Compliance was high: approximately 95% of assigned office days were attended as planned.

[6] Because team assignments and team leaders were fixed before the intervention began, both treatment and control employees are observed under the post-assignment managerial structure during the two-month pre-intervention period. The results are robust to controlling for team-leader fixed effects.

3 Data and estimation strategy

3.1 Data sources and outcome measures

We combine administrative and survey data from three sources: (i) high-frequency operational data from the firm's internal performance system, (ii) HR records on employment status and office-day attendance, and (iii) surveys fielded by us to measure employee experiences and interaction patterns. All datasets are linked using anonymized identifiers.

The administrative data cover the full experimental sample of 248 employees during the pre-intervention window, the nine-month intervention window (August 2024–April 2025), and the five-month post-intervention period (May–September 2025), when treated employees had returned to fully remote work. Survey coverage differs by instrument: the pre-intervention survey covers the full sample, the endline survey covers 223 of 248 employees, and the post-visit interaction surveys are available for treated employees after office days. The raw data are recorded at high frequency and can be aggregated to daily, weekly, and monthly panels.

For each employee, we observe time-use measures recorded at the employee-by-hour-by-day level and aggregate them to the employee-day level for the main analysis. Our main productivity outcome is *calls per hour*, the firm's standard operational measure of output in this setting.[7] We separately examine customer ratings and audit scores to assess service quality. To understand the margins behind changes in call volume, we use *average call duration* and its components: *talk time*, *hold time*, and *administrative time*, all measured in seconds per call. We also use *break time*, measured in minutes per day.

Service-quality outcomes come from the firm's monitoring data. Customer rating is the average rating an employee receives from customers and ranges from 1 to 100. Random audit rating is a dummy variable equal to 1 if a manager deems an employee to be performing well after evaluating ten randomly recorded calls from a given month, and 0 otherwise. Performance information is communicated privately to each employee at the beginning of the month. These metrics are not shared publicly, and the firm does not use performance bonuses, awards, prizes, or employee-of-the-month schemes.

Retention outcomes come from HR records that track employment status and start and end dates. We define attrition as a permanent exit from the firm. We also observe baseline characteristics and job attributes, including gender, age, tenure, location, team assignment, and shift schedules, which we use for balance checks, heterogeneity analysis, and robustness.

To complement the administrative data, we administered two online surveys during working hours. Pre-intervention survey was fielded in July 2024 before the first office visit. An endline survey was fielded in April 2025, at the end of the intervention period. Response rates were 100% in the pre-intervention survey (248/248) and 90% at endline (223/248). We also fielded short post-visit interaction surveys for treated employees after each office day, asking whom they interacted with during the office day and whether they communicated with colleagues in the subsequent week.

[7] Calls per hour is defined as the total number of calls handled by employee i on date t divided by total logged working hours on that date.

3.2 Sample characteristics and balance

Employees are young, mostly women, and relatively well educated, and treatment and control employees are balanced on demographics, tenure, productivity, and service-quality measures (details in Table A.2). Before the first office visit, control and treatment employees handled nearly identical numbers of calls per hour, 10.79 and 10.82, respectively, and had very similar average call duration, 349.34 and 349.11 seconds. The only statistically significant difference is hold time, which is modestly higher in the treatment group and small relative to total call duration. Our preferred specifications include employee fixed effects, which absorb baseline level differences across employees.

To check randomization balance we regress treatment status on pre-intervention characteristics, performance measures, and survey responses. The estimated coefficients are generally close to zero and all statistically insignificant, suggesting that observable characteristics and outcomes do not systematically predict treatment assignment before the first office visit (details in Figure A.2)

3.3 Main specification

Our empirical strategy exploits the random assignment of 248 customer service employees to the one-office-day-per-month policy or to the fully remote control group. The main experimental estimand is the intent-to-treat effect of assignment among eligible volunteers who lived within commuting distance of the office.[8] The employee is the unit of randomization and the primary unit of inference. For productivity outcomes, we use the employee–day panel to compare treated and control employees before, during, and after the intervention.

We estimate difference-in-differences specifications of the following form:

$$\begin{aligned} y_{it} = {} & \gamma_E D_t^E + \gamma_L D_t^L + \gamma_P D_t^P \\ & + \delta_E \left(\mathit{OneOfficeDayPerMonth}_i \times D_t^E \right) \\ & + \delta_L \left(\mathit{OneOfficeDayPerMonth}_i \times D_t^L \right) \\ & + \delta_P \left(\mathit{OneOfficeDayPerMonth}_i \times D_t^P \right) \\ & + \mu_i + \varepsilon_{it}, \end{aligned} \tag{1}$$

where i indexes employees and t indexes calendar dates. The outcome y_{it} is measured at the employee–day level and includes calls per hour, call duration and its subcomponents, and break time. $\mathit{OneOfficeDayPerMonth}_i$ equals 1 for employees assigned to monthly office days and 0 for employees assigned to remain fully remote. Because assignment is time-invariant, its main effect is absorbed by employee fixed effects.

The reference period is June–July 2024, before any treated employees attended the office. D_t^E indicates the early experimental window (Months 1–5), D_t^L indicates the later experimental window (Months 6–9), and D_t^P indicates the post-intervention window (Months 10–14), when treated employees had returned to fully remote work. The coefficients δ_E, δ_L, and δ_P are the treatment effects of interest for each time window. To show that the results are not driven by these broad cutoffs, Appendix Figure A.4 reports an event-study version of equation (1) with monthly relative-time indicators showing gradual treatment effects.

[8] Compliance with assigned office days was approximately 95%, so treatment-on-the-treated estimates obtained by scaling the intent-to-treat estimates by compliance are very similar.

Our preferred specifications include employee fixed effects (μ_i), which absorb time-invariant differences across employees such as baseline ability, experience, and work style. Standard errors are clustered at the employee level to allow for serial correlation in repeated daily observations and to align inference with the level of randomization. Repeated observations improve precision but do not change the number of experimental units. Because attrition is itself affected by treatment, we analyze retention separately using the original randomized assignment.

3.4 Communication and spillovers

Employees completed short post-visit interaction surveys after each office day. We link these responses to administrative records on office logistics, especially the seating plan, to measure communication and potential spillovers. Identification in these analyses comes from randomized seating assignments during office visits, which generate exogenous variation in proximity within the treated group. These results provide mechanism-supporting evidence rather than the basis for the main treatment effects.

We estimate:

$$c_{ij} = \alpha + \beta \, SeatingPlanNeighbour_{ij} + \varepsilon_{ij}. \tag{2}$$

where c_{ij} captures follow-up communication between reference employee i and potential colleague j. The coefficient β captures the difference in follow-up communication between randomly assigned seating-neighbor pairs and other potential within-group pairs.

The sample consists of 124 treated employees who completed post-visit interaction surveys. For logistical reasons, treated employees were split into two visiting groups: Group 1 (50 employees) and Group 2 (74 employees). Because the groups visited on different days, in-person interactions could occur only within group.[9]

We enumerate all potential within-group pairs, that is, pairs of employees physically present in the office on the same day. This yields $(50 \times 49) + (74 \times 73) = 7{,}852$ potential within-group colleague pairs. We also aggregate follow-up communication with individuals outside the employee's visiting group or outside the treatment sample into a single "outside" option for each reference employee. Adding 124 outside-option observations, one per reference employee, gives 7,976 possible reference-employee pairs.

In the final employee-pair data, $SeatingPlanNeighbour_{ij}$ equals 1 if employee i was seated next to colleague j during the office visit and 0 otherwise. The outcome c_{ij} captures follow-up communication between employee i and potential colleague j on the extensive margin and, where relevant, the intensive margin.

4 Results

4.1 Productivity

Figure 1 plots weekly averages of calls per hour for treated and control employees over the pre-intervention period, the nine-month intervention period, and the five-month post-intervention pe-

[9] For the seating-neighbor analysis, identification comes from within-visiting-group variation, since only employees present on the same office day could be randomly assigned as desk neighbors.

riod, together with 95% confidence intervals. Formal inference comes from the employee-level randomized design, with standard errors clustered at the employee level, as reported in Table 1. In the months before the intervention begins in August 2024, the two series move closely together, and both groups average 10.8 calls per hour.

After monthly office days begin, the treatment group gradually rises above the control group. The divergence is not immediate: in the first few months of the intervention, weekly calls per hour remain similar across groups. Over time, however, treated employees increasingly outperform their fully remote peers, and the gap widens as exposure to repeated in-person contact accumulates. After the final office visit in April 2025, the difference remains sizable and clearly visible at the start of the post-intervention period.

For the five months after the experiment ended, treated employees continue to exhibit higher calls per hour. The post-period mean is 12.4 calls per hour for the treatment group versus 11.5 for the control group, implying a raw post-intervention productivity gap of 7.8% relative to the control group. Figure A.3 shows that the productivity gain is not driven by a few outliers. During Months 6–14, when the productivity effects are largest, the full distribution of average employee productivity shifts to the right for the treatment group relative to the control group.

Regression estimates. Table 1 evaluates the experiment in a regression framework. Panel A reports changes over time that are common to both treatment and control employees, relative to the pre-intervention baseline. Panel B reports the treatment effects of interest: the interaction between assignment to *One Office Day Per Month* and each time window. Column 1 estimates these effects without individual fixed effects and therefore captures the overall treatment-control difference in each window, including both within-worker productivity changes and any changes in the composition of employees who remain in the sample. The results show a clear build-up over time. In the first five months of the experiment, the effect on calls per hour is close to zero (0.07). The effect rises to 0.67 in Months 6–9 and to 0.87 in the post-intervention window, Months 10–14.[10] This timing is less consistent with a purely immediate Hawthorne or "perk" interpretation, since the gains do not appear right away.

Column 2 adds individual fixed effects, so the estimates compare changes over time within the same employee relative to the corresponding changes among control employees. The post-intervention estimate falls from 0.87 to 0.63 calls per hour. This suggests that most of the post-intervention productivity gain reflects within-worker improvement, while the remaining difference is consistent with changes in worker composition. In proportional terms, the within-worker component accounts for about 72% of the post-intervention regression estimate (0.63/0.87). This pattern is consistent with higher-productivity workers being relatively more likely to remain at the firm in the treatment group than in the control group, a selection margin we examine further in Section 4.5.

The increase in calls per hour is driven by shorter calls, as shown in Column 3. Average call duration is essentially unchanged in Months 1–5 (−0.04 seconds), but declines by 13.21 seconds in Months 6–9 and by 17.60 seconds in Months 10–14. Column 4 shows no significant change in the combined measure of admin, hold, and break time; separate regressions for each component are also statistically insignificant.

[10] Results are qualitatively the same when estimating this specification using a balanced panel. Point estimates for the early, late and post-intervention phases are 0.07, 0.55 and 0.71, respectively.

Event-study evidence. The month-by-month event-study estimates in Figure A.4 show a similar pattern. Treatment effects are close to zero before the first office visit and during the early months of the experiment, then rise gradually after repeated monthly office visits. This dynamic pattern supports the interpretation that the productivity gains built over time rather than appearing immediately after treatment assignment.

Comparison with non-volunteers. Table A.3 replaces the randomized control group with the 185 non-volunteers in the same province. The purpose of this table is to assess whether the main productivity pattern is specific to the randomized control group, or whether treated employees also improve relative to a broader set of employees in the same location who remained outside the intervention. The results show a qualitatively similar dynamic pattern, with small early differences and larger gaps emerging later, which helps to rule out the impact was because of disappointment in the control gr oup over not being randomized into treatment.

Robustness to aggregation level, team-leader fixed effects, and date fixed effects. Appendix Tables A.4 and A.5 show that the productivity results are robust at both the employee-day and employee-week levels. In each case, the estimates remain qualitatively unchanged when we add team-leader and calendar fixed effects (day of week and month of year).

Heterogeneity. Figure A.5 shows no statistically significant heterogeneity in productivity effects by gender, age, baseline performance, marital status, or parental status. The point estimates are broadly similar across groups, indicating that the gains are broad-based.

4.2 Service quality

A natural concern is that the productivity gains reflect a speed–quality trade-off, with treated employees handling more calls by cutting corners, reducing the quality of customer interactions, or deviating from protocols. We examine treatment impacts on two service-quality outcomes: customer satisfaction ratings and internal random audit scores. We find no evidence that monthly office days affected service quality on average after the start of the intervention (Table A.6).

4.3 Peer connections, communication, and managerial input

Table 2 shows that monthly office days increased reported communication among employees. Column (1) uses the endline survey, which was administered to both treatment and control employees. The outcome is the total time an employee reports spending over the past week communicating with their three most frequent contacts. Treated employees report spending 36 more minutes communicating with these colleagues than control employees.

Columns (2)–(8) examine communication at the employee-pair level using the treated-group post-visit interaction surveys and exploit randomized seating within the treated group. Column (2) shows that pairs randomly assigned as desk neighbors are 11 percentage points more likely to communicate in the following week, relative to a mean of 9%. Columns (3)–(5) show similarly large associations for pairs who report interacting during the office day, during the morning, lunch, or afternoon breaks, with increases of 18–19 percentage points. Because these break-interaction measures are not randomly assigned, we interpret them as correlations rather than causal estimates. Columns (6)–(8) show that these associations remain strong when the seating-neighbor indicator

is included alongside each interaction measure, indicating that shared breaks provide an additional channel of connectivity beyond adjacent desk seating.

The evidence from the end-of-the-survey points in the same direction. Figure 2 plots treatment–control differences, in percentage points, for a set of endline survey outcomes, with statistical significance assessed using Romano–Wolf stepdown-adjusted p-values to account for multiple hypothesis testing.[11] Relative to control employees, treated employees are more likely to support the once-per-month office policy, to report receiving very regular performance feedback from their team leader, to report a stronger perceived fit with the firm's culture, and to report that team communication is effective. In contrast, we do not detect statistically significant effects on broader measures of well-being, including life satisfaction and work–life balance.

4.4 Interpreting the mechanisms

Taken together, the evidence is consistent with a combination of more frequent managerial feedback, richer peer interaction, and stronger subsequent team communication. The design does not allow us to separately identify the relative contribution of each channel, but the results consistently indicate that monthly in-person contact restored margins of interaction that were constrained in the fully remote setting.

Several findings also support this interpretation. Productivity gains accrue gradually over time and persist beyond the end of the intervention, which is less consistent with a purely immediate response and more consistent with slower-building channels such as learning, feedback, stronger workplace ties, or attachment to the firm. Survey responses indicate increased post-office-day communication, more regular managerial feedback, and stronger perceived cultural fit. The randomized seating results further show that exogenously induced in-person proximity made employees more likely to communicate in the following remote-work week.[12] Consistent with this interpretation, qualitative feedback from employees suggests that office days were helpful for learning from colleagues handling customers and for in-person managerial coaching. Employees also reported that office visits made it easier to ask colleagues for help on subsequent work-from-home days because they felt "closer to" their co-workers.

4.5 Attrition

Figure 3 plots cumulative attrition for the treatment and control groups. Attrition is flat in the pre-experiment period and for the first two months by construction, as only employees with at least three months of experience at baseline and who had not handed in notice to leave were included in the experiment. Attrition is an employee-level outcome, and the figure tracks how cumulative exits evolve over time across the two randomized groups.

Once monthly office days begin, attrition increases in both groups, but more slowly in the treatment group. By the end of the sample, cumulative attrition reaches 21.0% in the control group, compared with 13.7% in the treatment group. The gap remains visible after the final office

[11] The figure reports linear probability model estimates for each outcome separately.

[12] At the same time, we do not observe the content of remote communications directly, in part because much day-to-day communication occurred through informal channels outside the firm's administrative systems. Our evidence on mechanism therefore comes from randomized proximity, follow-up communication surveys, and reports of manager feedback rather than message-level communication data.

visit, indicating that the policy does more than simply delay separations. Employees reported feeling more connected to the firm and their colleagues after the monthly office visits, and they valued the improved learning from in-person time.

Table A.7 provides descriptive evidence on the productivity of employees who remain with the firm ("stayers") and those who ultimately leave ("quitters"). Among employees observed in the post period, quitters in the control group are more productive than stayers (11.744 vs. 11.481 calls per hour; difference $= -0.262$, $p < 0.001$), whereas in the treatment group the pattern is reversed: stayers are much more productive than quitters (12.149 vs. 10.955; difference $= 1.194$, $p < 0.001$). The corresponding difference-in-differences estimate is 1.456 calls per hour. These descriptive patterns are consistent with selective retention of relatively more productive workers in the treatment group, with managers better able to retain high performing workers when they see them once a month in person.

5 Cost–benefit analysis

We provide a back-of-the-envelope benefit–cost calculation from the firm's perspective over a nine-month operating horizon. The calculation summarizes the economic magnitude of the effects rather than providing a full welfare analysis.

Program costs. The intervention required treated employees to attend the office one day per month. The incremental cost per visit was \$9.63 per employee for transport, lunch, and refreshments, plus \$250 per month for office space. Over nine visits and 124 treated customer service agents, total program expenditure was about \$13,000.

Turnover savings. The nine-month intervention reduced attrition by 7 percentage points. Applied to the 124 treated employees, this implies 9 fewer separations. Valuing each separation at \$714 (an estimate provided by the firm that covers recruitment, initial training, and the productivity ramp-up of new hires) yields turnover savings of \$6,400.

Productivity gains. Using the raw post-intervention productivity gap of 7.8% as the steady-state productivity gain, we value the gain using the employer labor cost of 130 TRY per hour (approximately \$4). Assuming each employee worked 1,560 hours over a nine-month operating horizon, the implied productivity benefit is \$60,300.[13]

Total monetized benefits equal \$66,700, which is more than five times program costs. Hence, not only did employees appear to benefit from the policy, through reduced attrition and positive survey responses, but the firm also found it profitable.

6 Conclusion

This paper asks whether occasional, coordinated in-person contact improves outcomes in an otherwise fully remote workplace. We study a nine-month randomized controlled trial in which treated employees work from the office together one fixed day per month and otherwise remain fully remote.

[13] This valuation assumes that higher calls per hour allow the firm to handle the same call volume with fewer paid labor hours, so that productivity gains can be valued using employer labor costs.

Three findings stand out. First, working together in the office one day a month raises productivity. The effects build gradually, remain visible after the final office visit, and are not accompanied by a deterioration in service quality. Customer ratings and internal audit scores remain stable, suggesting that higher output reflects greater efficiency rather than a speed–quality trade-off.

Second, the evidence points to interaction, learning, and feedback as likely mechanisms. Randomized seating assignments generate new peer links that persist beyond the office day, and treated employees report more regular managerial feedback, more effective team communication, and stronger cultural fit. While we cannot separately identify the contribution of each channel, the evidence suggests that monthly office days partially restored forms of workplace learning and support that were otherwise limited in the fully remote setting.

Third, monthly office days improve retention. Attrition accumulates more slowly in the treatment group, and the gap remains visible through the end of the observation window. These gains are economically meaningful because the policy reduces the selective loss of higher-performing employees, an important margin in settings where onboarding is costly and service delivery depends on experience. From the firm's perspective, the policy is cost effective.

The broader message of our paper is that remote work need not imply isolated work. This issue extends beyond our setting: prominent remote-first or location-flexible firms, including Airbnb and Dropbox, already combine remote work with periodic in-person gatherings. Yet there is little causal evidence on whether such light-touch co-location policies improve workplace outcomes. Our results suggest that even limited, well-coordinated in-person contact can strengthen communication, productivity, and retention without requiring a return to a traditional office model.

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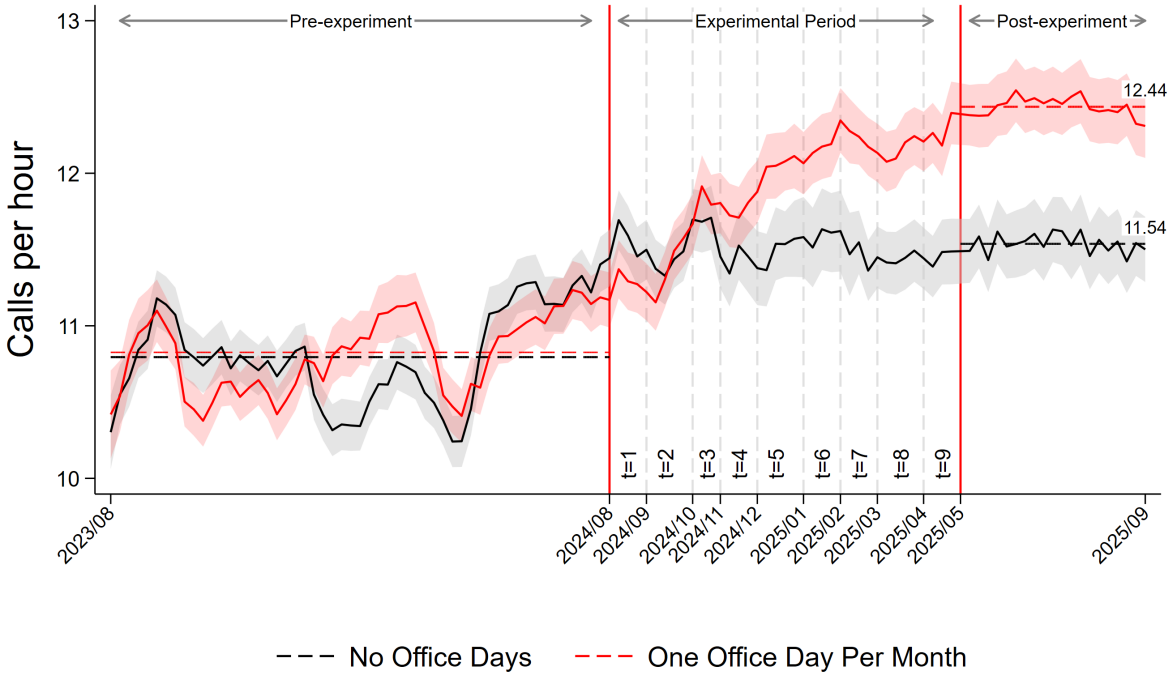
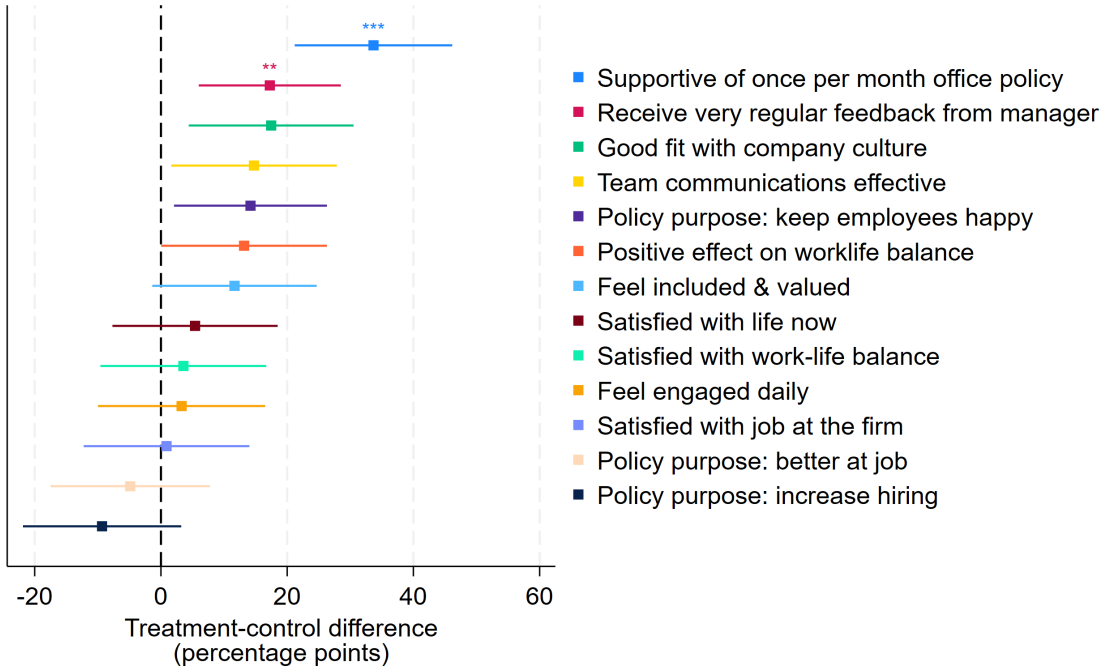
Figures and Tables

Figure 1: Calls per hour before, during and after the RCT



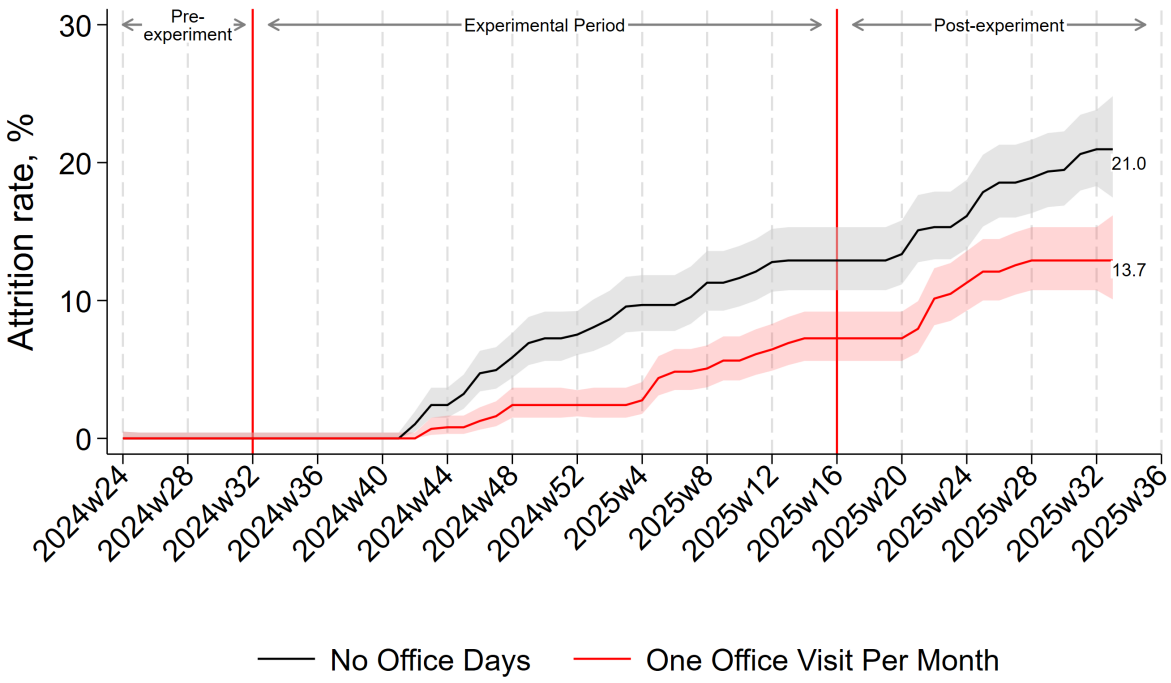
Notes: This figure shows weekly mean *calls per hour* for the control (No Office Days) and treatment (One Office Day Per Month) groups. Weekly means are calculated using employee-day-level data. Horizontal dashed lines represent mean values in the *pre-experiment* and *post-intervention* periods for the *No Office Days* (control) group (black dashed line) and the *One Office Day Per Month* (treatment) group (red dashed line). Mean *calls per hour* in the pre-treatment period are 10.8 for both the control and treatment groups, while the corresponding means in the post-intervention period are 11.5 and 12.4, respectively. The vertical red lines at August 2024 and May 2025 indicate the start of the treatment period and the start of the post-intervention period, respectively. Week-to-week fluctuations partly reflect common seasonal and calendar factors, including holiday periods. Shaded red and gray areas depict 95% confidence intervals around the mean.

Figure 2: At endline, treated employees were more supportive of the policy and reported stronger workplace connections



Notes: This figure reports estimates from linear probability regressions, where each coefficient corresponds to a separate regression of the outcome listed in the legend on a treatment indicator. All outcomes are binary variables. Coefficients are rescaled to percentage points. The sample corresponds to the endline survey sample ($N = 223$). Whiskers depict 95% confidence intervals, and standard errors are robust to heteroskedasticity. ***, **, and * denote significance at the 1, 5, and 10 percent levels based on Romano–Wolf stepdown p-values (1,000 bootstrap replications) for multiple hypothesis testing.

Figure 3: Attrition is lower among customer service employees who went to the office



Notes: This figure shows cumulative attrition separately for employees in the treatment group (red line), who were assigned to one office day per month, and employees in the control group (black line), who remained fully remote. The lines plot the cumulative share of employees who exited the firm over time in each group. The vertical red lines mark the weeks of the first and last office visits for the treatment group. Shaded areas represent 95% exact binomial confidence intervals for proportions. Employees were eligible for the study only if they had at least three months of tenure at the start of the experiment and had not handed in notice to leave, which explains the absence of attrition in the early part of the sample.

Table 1: Treatment effects in the early, late, and post-intervention periods

	Calls per hour		Average call duration (seconds per call)	Admin, hold & break time (minutes per day)
	(1)	(2)	(3)	(4)
Treatment group difference before office days	0.03			
	(0.27)			
A. Changes over time common to both groups				
Experimental window, Months 1–5	0.71***	0.78***	-21.37***	9.16***
	(0.13)	(0.12)	(3.60)	(0.76)
Experimental window, Months 6–9	0.69***	0.85***	-23.77***	11.36***
	(0.17)	(0.14)	(4.14)	(0.87)
Post-intervention window, Months 10–14	0.74***	0.85***	-24.10***	13.08***
	(0.17)	(0.14)	(4.12)	(0.94)
B. Treatment effects by time window				
One Office Day Per Month × Experimental window, Months 1–5	0.07	-0.02	-0.04	0.59
	(0.19)	(0.18)	(5.07)	(1.18)
One Office Day Per Month × Experimental window, Months 6–9	0.67***	0.43**	-13.21**	-0.03
	(0.23)	(0.20)	(5.64)	(1.33)
One Office Day Per Month × Post-intervention window, Months 10–14	0.87***	0.63***	-17.60***	0.81
	(0.23)	(0.20)	(5.53)	(1.48)
Adj. R-squared	0.05	0.81	0.81	0.48
Number of observations	144,769	144,769	144,769	144,769
Outcome pre-treatment mean	10.81	10.81	349.21	38.32
Agent FE	No	Yes	Yes	Yes

Notes: This table reports estimates from linear regressions. Dependent variables are shown in the column headings. Panel A reports changes over time that are common to treatment and control employees, relative to the pre-intervention baseline. Panel B reports the treatment effects of interest: the interaction between assignment to *One Office Day Per Month* and each time window. *Treatment group difference before office days* reports the baseline treatment-control difference in Column (1); the time-invariant treatment indicator is absorbed by employee fixed effects in Columns (2)–(4). *Admin, hold & break time*, measured in minutes per day, is the sum of admin time, hold time, and break time. Admin and hold time are converted to minutes per day before being combined with break time. In analogous separate regressions for each of these three outcomes, the interaction terms between *One Office Day Per Month* and the time-window indicators are statistically insignificant. The regressions are estimated using employee-day-level observations, but the employee is the unit of randomization, so repeated daily observations improve precision without changing the number of experimental units. Standard errors are clustered at the employee level. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

Table 2: Office time increased employee communication, especially among employees seated next to each other

	Minutes spent communicating	Dummy equal to 1 if an employee pair communicated in the week after the office visit and 0 if they did not communicate						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
One Office Day Per Month	36.28***							
	(0.99)							
Seating plan neighbor		0.11***				0.08***	0.07***	0.07***
		(0.02)				(0.02)	(0.02)	(0.02)
Morning interaction			0.18***			0.18***		
			(0.01)			(0.01)		
Lunch interaction				0.19***			0.18***	
				(0.01)			(0.01)	
Afternoon interaction					0.18***			0.17***
					(0.01)			(0.01)
Number of observations	223	7,976	7,976	7,976	7,976	7,976	7,976	7,976
Outcome mean	165.58	0.09	0.09	0.09	0.09	0.09	0.09	0.09

Notes: This table reports estimates from linear regressions. The dependent variable in Column (1) is the total time, in minutes, that an employee reports in the endline survey spending over the past week communicating with the three colleagues with whom they communicated most frequently. The dependent variable in Columns (2)–(8) is an indicator equal to 1 if an employee pair communicated in the week after the office visit and 0 otherwise. *Seating plan neighbor* is an indicator equal to 1 if an employee pair was randomly assigned to be seated next to each other in the office during the first three visits and 0 otherwise. *Morning interaction* is an indicator equal to 1 if an employee pair interacted during a morning coffee break. *Lunch interaction* is an indicator equal to 1 if an employee pair interacted during lunch. *Afternoon interaction* is an indicator equal to 1 if an employee pair interacted during an afternoon coffee break. Follow-up communication, as well as morning, lunch, and afternoon interactions, are self-reported in surveys completed at the end of each week following an office visit. The data for Column (1) come from the endline survey, while the data for Columns (2)–(8) include all possible employee pairs and focus on the first three office visits and the subsequent weeks, when the seating plan was randomized and enforced. The employee-pair data includes all within-group pairs physically present on the same office day, yielding $(50 \times 49) + (74 \times 73) = 7,852$ potential pairs plus 124 outside-option observations, one per reference employee. Standard errors are robust to heteroskedasticity in Column (1) and clustered at the employee-pair level in Columns (2)–(8). ***, **, and * denote significance at the 1, 5, and 10 percent levels.

Appendix

Figure A.1: During the intervention

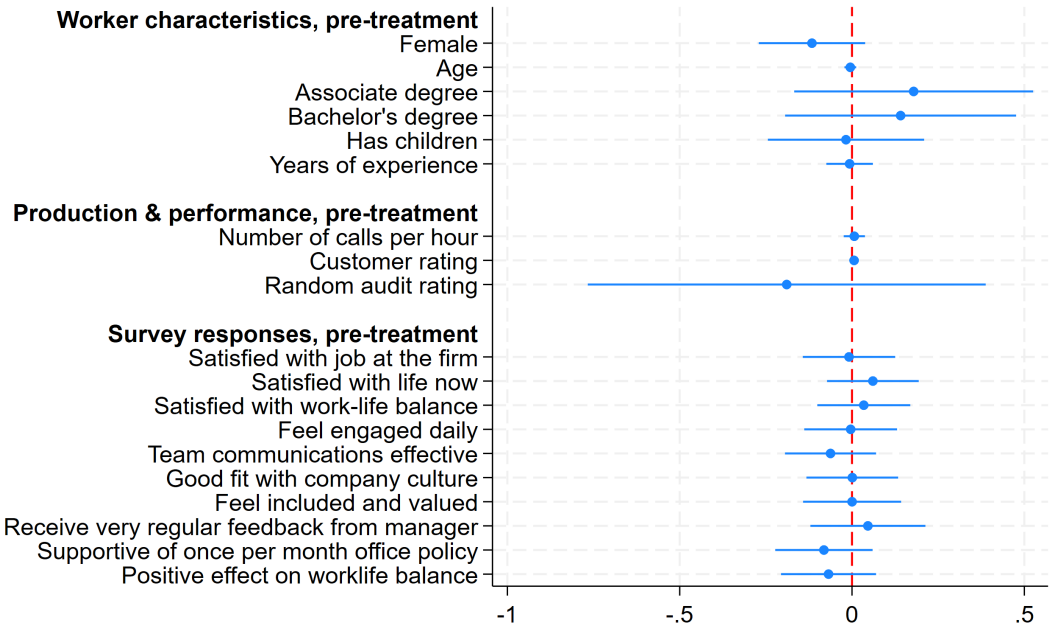
(a) Working at home environment

(b) Transport to and from the office

(c) Office-day activities

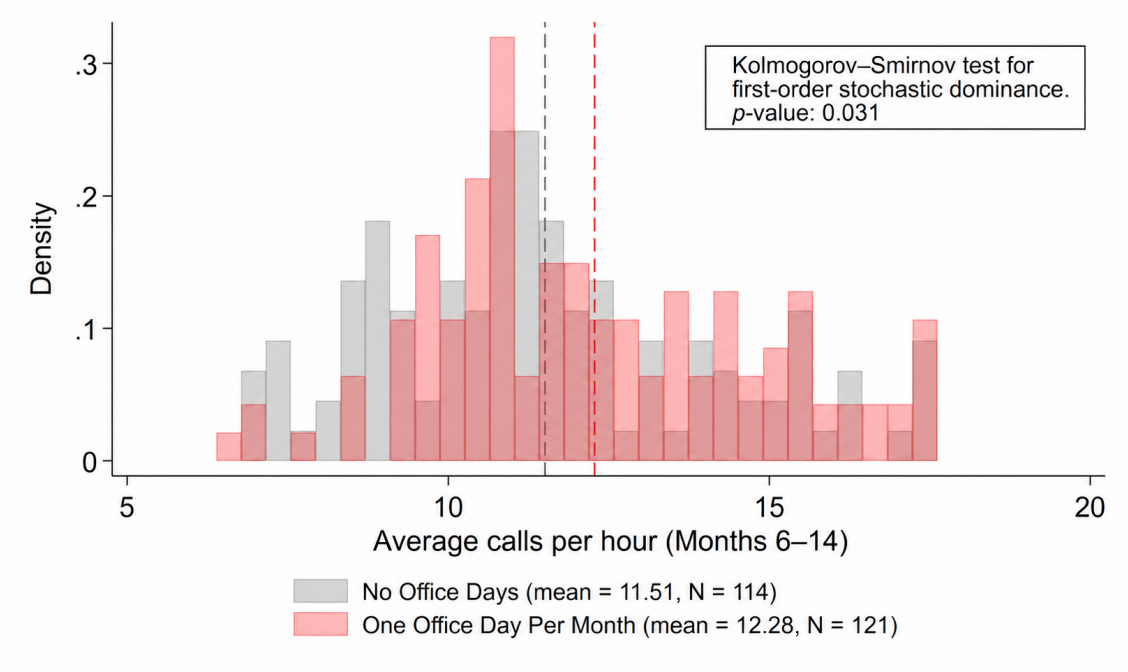
Notes: This figure presents illustrative photos from the intervention period, showing employees working from home (Panel (a)); transportation arranged by the company and group activities during monthly office days (Panel (b)); and the office-day work environment and team social activities (Panel (c)).

Figure A.2: Balance test for treatment assignment



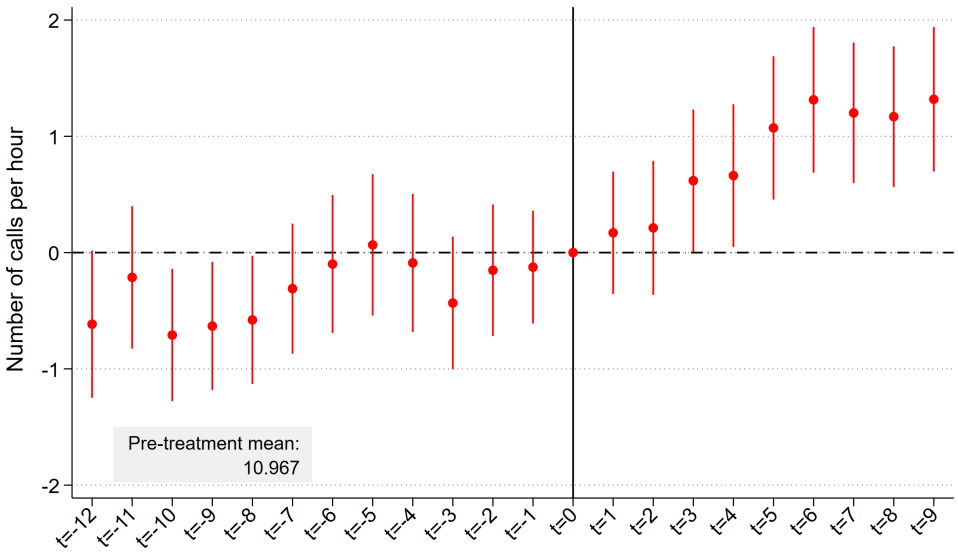
Notes: This figure shows coefficient estimates from a linear probability model. The dependent variable equals 1 if an employee is assigned to the treatment group. The data include one observation per employee ($N = 248$). Standard errors are robust to heteroskedasticity, and whiskers show 95% confidence intervals.

Figure A.3: Distribution of calls per hour in the later part of the sample (months 6–14)



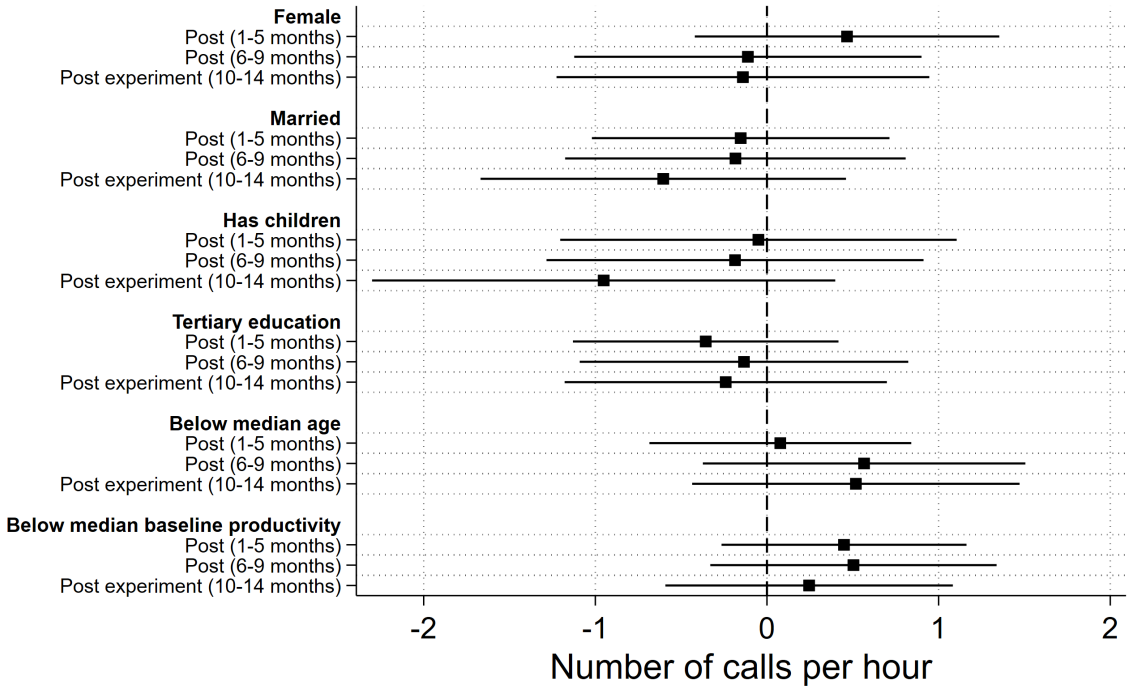
Notes: This figure plots histograms of average *calls per hour* separately for the treatment group (One Office Day Per Month, red) and the control group (No Office Days, gray) during the late-experiment and post-intervention periods (Months 6–14, January–September 2025). The sample includes all employees with at least five observed working days in Months 6–14 ($N = 121$ treatment, $N = 114$ control). Each employee contributes one observation, equal to their average *calls per hour* over all observed workdays in Months 6–14. Dashed vertical lines indicate group means. A two-sample Kolmogorov–Smirnov test rejects the null of identical distributions in favor of first-order stochastic dominance of the treatment distribution over the control distribution ($p < 0.05$).

Figure A.4: Event-study estimates for calls per hour



Notes: This figure reports event-study estimates for *calls per hour*. The coefficients are estimated from a dynamic version of equation (1) that replaces the three broad time-window interactions with monthly relative-time indicators. The vertical line at relative month 0 marks the first office visit, which took place on August 12, 2024. Negative values on the horizontal axis refer to months before the first office visit, and positive values refer to months after the first office visit. Standard errors are clustered at the employee level, and whiskers represent 95 percent confidence intervals.

Figure A.5: No statistically significant evidence of heterogeneity in productivity effects



Notes: This figure shows point estimates from six separate linear regressions. The estimates correspond to the triple interaction between *One Office Day Per Month*, a post-period indicator, and the individual characteristic listed on the vertical axis. *Below-median productivity* is calculated using odd calendar days in the pre-experiment period; those observations are then dropped before estimating the regression to rule out mean reversion. Standard errors are clustered at the employee level, and whiskers represent 95% confidence intervals.

Table A.1: Baseline characteristics of eligible volunteers and non-volunteers

	Eligible volunteers		Non-volunteers		Difference	p-value
	N	Mean	N	Mean		
A. Worker characteristics						
Female	248	0.78	185	0.80	-0.02	0.63
Age	248	23.46	185	24.10	-0.64	0.14
High school	248	0.04	185	0.05	-0.01	0.48
Associate degree	248	0.27	185	0.29	-0.02	0.67
Bachelor's degree	248	0.70	185	0.66	0.04	0.39
Married	248	0.22	185	0.31	-0.09	0.03
Has children	248	0.12	185	0.20	-0.08	0.02
Years of experience	248	0.45	185	0.53	-0.08	0.45
B. Productivity variables at baseline						
Number of calls per hour	71,281	10.81	53,166	10.76	0.05	0.72
Call duration in seconds	71,281	349.23	53,166	350.11	-0.88	0.81
Break time in minutes per day	71,281	34.23	53,166	34.71	-0.48	0.58
Talk time in seconds per call	71,281	344.48	53,166	345.29	-0.81	0.84
Admin time in seconds per call	71,281	2.91	53,166	2.94	-0.03	0.61
Hold time in seconds per call	71,281	1.63	53,166	1.71	-0.08	0.69
C. Service-quality variables at baseline						
Customer rating	71,476	44.30	53,315	44.08	0.22	0.77
Random audit rating	71,476	0.18	53,315	0.18	0.00	0.91

Notes: This table compares eligible volunteers in the experimental sample with employees in the same province who did not volunteer for the monthly office-day. Eligible volunteers are the 248 employees who satisfied the pre-specified eligibility criteria and were randomized into treatment and control groups. Non-volunteers are the 185 employees who were informed about the monthly office visit policy but did not express interest in participating. The difference column reports the mean for eligible volunteers minus the mean for non-volunteers. The p-value column reports descriptive tests of equality between eligible volunteers and non-volunteers. For Panels B and C, p-values are based on regressions using pre-intervention observations with standard errors clustered at the employee level. These comparisons are descriptive and are intended to assess selection into the experimental sample, not treatment-control balance.

Table A.2: Observables are balanced at baseline

	(1)	(2)	(3)	(4)	(5)	(6)
	N	Mean	SD	Mean		Difference
	Control and Treatment			Control	Treatment	p-value
A. Worker characteristics						
Female	248	0.78	0.42	0.81	0.74	0.17
Age	248	23.46	4.60	23.60	23.31	0.62
High school	248	0.04	0.19	0.05	0.02	0.31
Associate degree	248	0.27	0.44	0.25	0.28	0.57
Bachelor's degree	248	0.70	0.46	0.70	0.69	0.89
Married	248	0.22	0.41	0.23	0.20	0.54
Has children	248	0.12	0.33	0.13	0.11	0.70
Years of experience	248	0.45	1.10	0.47	0.43	0.77
B. Productivity variables at baseline						
Number of calls per hour	71,281	10.81	2.36	10.79	10.82	0.91
Call duration in seconds per call	71,281	349.23	76.83	349.34	349.11	0.98
Break time in minutes per day	71,281	34.23	18.96	33.33	35.18	0.19
Talk time in seconds per call	71,281	344.48	76.68	345.05	343.89	0.90
Admin time in seconds per call	71,281	2.91	0.70	2.89	2.93	0.52
Hold time in seconds per call	71,281	1.63	3.65	1.22	2.06	0.03
C. Service quality variables at baseline						
Customer rating	71,476	44.30	16.17	43.98	44.63	0.27
Random audit rating	71,476	0.18	0.38	0.18	0.17	0.65
D. Worker responses at baseline survey						
Supportive of once per month office policy	248	0.31	0.47	0.35	0.27	0.17
Receive very regular feedback from manager	248	0.20	0.40	0.19	0.21	0.63
Good fit with company culture	248	0.43	0.50	0.42	0.44	0.80
Team communications effective	248	0.40	0.49	0.44	0.37	0.30
Positive effect on work-life balance	248	0.40	0.49	0.44	0.37	0.30
Feel included and valued	248	0.35	0.48	0.35	0.35	0.89
Feel engaged daily	248	0.43	0.50	0.43	0.44	0.90
Satisfied with life now	248	0.43	0.50	0.40	0.45	0.44
Satisfied with work-life balance	248	0.43	0.50	0.42	0.44	0.80
Satisfied with job at the firm	248	0.43	0.50	0.44	0.43	0.90

Notes: This table reports baseline summary statistics for worker characteristics (Panel A), productivity measures (Panel B), service-quality measures (Panel C), and worker responses to the survey (Panel D). Columns (1)–(3) report the number of observations, mean, and standard deviation pooling the control and treatment groups. Columns (4) and (5) report the corresponding means separately for the control and treatment groups. Column (6) reports the p-value for the test of equality of treatment and control means. Panel A reports worker-level characteristics measured at baseline. Panels B and C report pre-treatment productivity and service-quality measures at the employee-date level.

Table A.3: Comparison with non-volunteers as the control group

	Calls per hour		Average call duration (seconds per call)	Admin, hold & break time (minutes per day)
	(1)	(2)	(3)	(4)
Office-day group difference before office days	0.06			
	(0.25)			
A. Changes over time common to both groups				
Experimental window, Months 1–5	0.68***	0.75***	-20.84***	8.92***
	(0.12)	(0.11)	(3.42)	(0.72)
Experimental window, Months 6–9	0.66***	0.82***	-23.10***	10.95***
	(0.16)	(0.13)	(3.95)	(0.84)
Post-intervention window, Months 10–14	0.70***	0.83***	-23.72***	12.64***
	(0.16)	(0.13)	(3.98)	(0.91)
B. Office-day group differences by time window				
Office-day group × Experimental window, Months 1–5	0.09	0.00	-0.10	0.48
	(0.18)	(0.17)	(4.82)	(1.12)
Office-day group × Experimental window, Months 6–9	0.64***	0.40**	-12.60**	0.12
	(0.22)	(0.19)	(5.35)	(1.27)
Office-day group × Post-intervention window, Months 10–14	0.82***	0.59***	-16.50***	0.95
	(0.22)	(0.19)	(5.27)	(1.40)
Adj. R-squared	0.05	0.80	0.80	0.47
Number of observations	180,300	180,300	180,300	180,300
Outcome pre-treatment mean	10.78	10.78	349.80	38.80
Agent FE	No	Yes	Yes	Yes

Notes: This table reports estimates from linear regressions comparing employees assigned to the monthly office-day group with employees in the same province who did not volunteer for the office-day intervention. Dependent variables are shown in the column headings. Panel A reports changes over time that are common to both groups, relative to the pre-intervention baseline. Panel B reports differences between the office-day group and non-volunteers by time window. Unlike the main experimental estimates, these comparisons do not rely on random assignment to the comparison group and should therefore be interpreted as descriptive robustness checks. *Office-day group difference before office days* reports the baseline difference between treated employees and non-volunteers in Column (1); the time-invariant group indicator is absorbed by employee fixed effects in Columns (2)–(4). *Admin, hold & break time*, measured in minutes per day, is the sum of admin time, hold time, and break time. Admin and hold time are converted to minutes per day before being combined with break time. In analogous separate regressions for each of these three outcomes, the interaction terms between the office-day group and the time-window indicators are statistically insignificant. The regressions are estimated using employee-day-level observations. Standard errors are clustered at the employee level. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

Table A.4: Productivity and time-use effects at the employee-day level

	Calls per hour			Call duration (seconds per call)			Break time (minutes per day)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
One Office Day Per Month	0.03	0.00	-0.01	-0.19	0.70	1.08	1.81	1.60	1.49
	(0.27)	(0.26)	(0.26)	(8.88)	(8.62)	(8.63)	(1.40)	(1.35)	(1.35)
Post	0.69***	0.71***	0.84***	-18.93***	-19.76***	-23.58***	9.43***	9.54***	11.12***
	(0.14)	(0.14)	(0.15)	(4.10)	(4.13)	(4.52)	(0.79)	(0.83)	(0.92)
One Office Day Per Month × Post	0.33*	0.36*	0.37*	-11.36**	-12.21**	-12.47**	-0.34	-0.57	-0.50
	(0.19)	(0.20)	(0.20)	(5.73)	(5.82)	(5.82)	(1.21)	(1.22)	(1.22)
Adj. R-squared	0.03	0.05	0.05	0.03	0.05	0.05	0.06	0.09	0.10
Number of observations	117,461	117,461	117,461	117,461	117,461	117,461	116,133	116,133	116,133
Team leader FE		✓	✓		✓	✓		✓	✓
Day of week FE			✓			✓			✓
Month of year FE			✓			✓			✓

Notes: This table reports robustness estimates from linear regressions using employee-day-level observations. Dependent variables are shown in the column headings. *One Office Day Per Month* equals 1 for employees assigned to the monthly office-day group and 0 for fully remote employees. *Post* equals 1 for observations from the first office visit onward and 0 otherwise. The sample is the full employee-day panel. Pre-treatment means for *Calls per hour*, *Call duration*, and *Break time* are 11.32, 334.63, and 40.97, respectively, with standard deviations of 2.56, 76.98, and 18.19. Standard errors are clustered at the employee level. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

Table A.5: Productivity and time-use effects at the employee-week level

	Calls per hour			Call duration (seconds per call)			Break time (minutes per day)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
One Office Day Per Month	0.03	-0.00	-0.02	-0.09	0.86	1.42	1.72	1.52	1.36
	(0.27)	(0.26)	(0.26)	(8.88)	(8.63)	(8.64)	(1.40)	(1.35)	(1.35)
Post	0.69***	0.71***	0.86***	-18.99***	-19.79***	-24.17***	9.42***	9.53***	11.62***
	(0.14)	(0.14)	(0.16)	(4.13)	(4.15)	(4.63)	(0.79)	(0.84)	(0.94)
One Office Day Per Month × Post	0.36*	0.38*	0.39**	-12.05**	-12.91**	-13.36**	-0.26	-0.49	-0.39
	(0.19)	(0.20)	(0.20)	(5.75)	(5.85)	(5.85)	(1.22)	(1.23)	(1.23)
Adj. R-squared	0.03	0.05	0.05	0.03	0.05	0.05	0.09	0.13	0.15
Number of observations	20,322	20,322	20,322	20,322	20,322	20,322	20,127	20,127	20,127
Team leader FE		✓	✓		✓	✓		✓	✓
Day of week FE			✓			✓			✓
Month of year FE			✓			✓			✓

Notes: This table reports robustness estimates from linear regressions using employee-week-level observations. Dependent variables are shown in the column headings. *One Office Day Per Month* equals 1 for employees assigned to the monthly office-day group and 0 for fully remote employees. *Post* equals 1 for observations from the first office visit onward and 0 otherwise. The sample is the full employee-week panel. Pre-treatment means for *Calls per hour*, *Call duration*, and *Break time* are 11.32, 334.63, and 40.97, respectively, with standard deviations of 2.56, 76.98, and 18.19. Standard errors are clustered at the employee level. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

Table A.6: Effects on service quality outcomes

	Customer rating		Random audit rating	
	(1)	(2)	(3)	(4)
One Office Day Per Month	0.67		-0.01	
	(0.59)		(0.01)	
Post	-0.34	-0.23	0.01	0.00
	(0.57)	(0.58)	(0.01)	(0.01)
One Office Day Per Month × Post	0.35	0.41	-0.02	-0.02
	(0.90)	(0.90)	(0.02)	(0.02)
Adj. R-squared	0.00	0.01	-0.00	0.02
Number of observations	4,905	4,905	4,905	4,905
Pre-treatment mean	44.28	44.28	0.17	0.17
Agent FE	No	Yes	No	Yes

Notes: This table reports estimates from linear regressions. Dependent variables are shown in the column headings. Customer rating is the average rating an agent receives from customers and ranges from 1 to 100. Random audit rating is a dummy variable equal to 1 if a manager deems an agent to be performing well after evaluating ten randomly recorded calls from a given month, and 0 otherwise. *One Office Day Per Month* equals 1 for employees assigned to the monthly office-visit group and 0 for fully remote employees. Columns (2) and (4) include employee fixed effects. Standard errors are clustered at the employee level. ***, **, and * denote significance at the 1, 5, and 10 percent levels.

Table A.7: Monthly office days are associated with stronger retention of higher-performing employees

	Mean calls per hour		Difference (Stayer − Quitter)	
	(1) Stayer	(2) Quitter	(3) Diff.	(4) p-value
Fully remote (control)	11.481	11.744	-0.262	0.000
	(0.015)	(0.046)		
One office day per month (treatment)	12.149	10.955	1.194	0.000
	(0.014)	(0.040)		
Difference-in-differences (DiD)			1.456	0.000
			(0.064)	

Notes: This table compares post-period *calls per hour* between employees who remain with the firm ("stayers") and those who leave ("quitters"). Rows report results separately for the fully remote control group and the one-office-day-per-month treatment group. Columns (1)–(2) report group means, with standard errors (computed at the employee level) in parentheses, among employees with observed post-period productivity. Column (3) reports the within-row difference, defined as mean(Stayer) − mean(Quitter), and Column (4) reports the corresponding two-sample t-test p-value. The difference-in-differences estimate is obtained from an OLS regression of *calls per hour* on indicators for treatment status and stayer status, and their interaction; the reported coefficient is the interaction term (*Treatment* × *Stayer*), with standard errors in parentheses.