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The Value of One Office Day a Month*

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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 assigned 248 customer-service employees either to remain fully remote or to work from the office together one day per month. Monthly office days gradually increased productivity, with treated employees handling 7.8% more calls per hour at the end of the nine-month intervention. 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 5:1. These findings show that even limited but coordinated in-person contact can improve communication, performance, and retention in remote teams.
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1 Introduction

Remote work has become a durable feature of modern labor markets. It expands hiring pools, reduces commuting burdens, and gives workers greater flexibility. But it also weakens the repeated face-to-face interactions through which employees learn from one another, receive feedback, solve problems, and build workplace ties (Sandvik et al., 2020; Yang et al., 2022; Emanuel et al., 2023; Emanuel and Harrington, 2024). Whether organizations can recover some of these benefits without giving up the advantages of remote work has become a central question.

Existing evidence provides only a partial answer. Research on hybrid work often finds little harm to performance and, in some settings, improvements in retention (Battiston et al., 2021; Bloom et al., 2024), whereas evidence on fully remote work is more mixed (Brucks and Levav, 2022; Atkin et al., 2023; Choudhury et al., 2021, 2024; Gibbs et al., 2023; Aksoy et al., 2025). This suggests that the effects of remote work depend not only on the tasks being performed, but also on how interaction is structured once workers are no longer routinely together.

This paper addresses that question in two key ways. First, it uses a randomized controlled trial to examine the causal effect of infrequent but coordinated in-person contact in an otherwise fully remote setting. Second, it studies employees in a large multinational service firm whose pay, schedules, tasks, and formal training remained unchanged throughout the intervention, allowing us to isolate the effect of periodic coordinated co-location.

Our study examines a nine-month randomized controlled trial involving 248 employees in a fully remote team. Employees were randomly assigned either to remain fully remote or to work together in person one fixed day each month. The intervention was deliberately minimal. It did not alter compensation, workload, working hours, or formal training, but instead reintroduced regular opportunities for co-workers and team leaders to spend time together. It was designed to restore limited opportunities for peer learning, team communication, and informal manager feedback without materially changing the firm's remote operating model. Because treatment was assigned at the employee level, the employee is the unit of randomization and the relevant unit of inference for the main treatment effects. The high-frequency administrative data provide repeated objective measures of the same workers over time, improving precision and allowing us to trace how treatment effects evolve before, during, and after the intervention.

We find that coordinated co-location generated substantial and persistent benefits. By the end of the intervention, treated employees handled 7.8% more calls per hour than their fully remote peers, and this gain persisted for at least five months after the experiment ended. At the same 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. From the firm's perspective, the policy is highly profitable. The per-visit cost of transport and meals is modest, and the firm estimates a benefit–cost ratio of 5:1, with gains arising from both higher productivity and lower turnover.

The timing and pattern of the effects suggest that these gains did not reflect a transitory morale response. They emerged only after repeated in-person encounters and persisted after mandated attendance had ended, consistent with accumulated learning and stronger workplace ties. Additional evidence shows that coordinated co-location strengthened subsequent interaction among co-workers even once they returned to remote work, and that randomized seating assignments during office visits generated persistent peer links within the treated group. These communication-network

results provide supporting evidence on mechanism and complement the survey findings that treated employees reported more regular manager feedback, stronger team communication, and a greater sense of 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).[1] The firm is one of Turkey's largest business-process-outsourcing (BPO) providers. Founded in 2002, it delivers customer service and back-office services to a mix of domestic and international clients in several sectors. As of 2025, the firm employed over 3,500 employees.

The experiment takes place in the firm's inbound customer service operations. This environment is well suited to studying workplace practices because production is highly standardized and routinely monitored. Employees perform 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 instead operates through promotions: strong performers can be promoted to team-leader positions, which involve managerial responsibilities and higher pay. Organizationally, each employee is assigned to a team of roughly 20 employees overseen by a team leader. Team leaders manage day-to-day operations and monitor individual performance. The firm tracks outcomes continuously through an in-house dashboard that records, among other metrics, 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. Employees handle consumer queries using a headset and a firm-provided computer, following standard protocols set by the client account.

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 chose to maintain remote work as its default operating model. By mid-2024, more than 90% of employees remained fully remote. While this shift delivered important operational benefits (such as access to a wider talent pool and lower office-related costs), internal feedback increasingly pointed to organizational costs, including weaker team cohesion, fewer opportunities for real-time coaching, and persistent retention problems. In the fully remote setting, most day-to-day interactions between employees and team leaders took place through messaging applications, with limited informal contact and little scope for team leaders or co-workers to observe in real time how employees handled difficult customer issues.

Before the shift to fully remote work, by contrast, in-person office days had allowed team leaders and peers to observe problems as they emerged, provide immediate feedback, exchange advice in real time, and continue those interactions informally throughout the day. Such interactions were

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

especially important in a production environment where coaching, communication, and problem solving often depend on immediate context and are difficult to replicate through asynchronous remote communication. In response to these concerns, the firm collaborated on the randomized controlled trial evaluated in this paper, testing whether occasional coordinated in-person contact could restore some of the benefits of office-based interaction without sacrificing the advantages of a fully remote work model.

2.1 Study population and recruitment

The study population consists of employees assigned to the telecommunications client account, working in inbound customer-service roles (handling calls related to billing, packages, upgrades, and service issues). To implement the intervention, the firm relied on its Şanlıurfa office, one of several regional hubs the firm operated before the pandemic. The office is accessible by shuttle and has sufficient capacity to host treatment-group employees once per month. This allowed the firm to run the intervention using existing infrastructure and without disrupting other ongoing operations.

In March 2024, the firm informed 661 employees in the province where the office is located about a pilot program that would require working from the office one day per month. Participation in the study was voluntary. Over March–April, 476 employees (about 72%) expressed interest, while 185 did not volunteer, most commonly citing family or care responsibilities and the burden of commuting.

From the volunteer pool, the firm applied pre-specified criteria to ensure that monthly office days were feasible and that participants had sufficient experience in the role. 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.[2] Relative to non-volunteers, eligible employees tended to have shorter commutes and were more likely to be single, but they did not differ systematically in baseline performance measures. This pattern suggests that selection into the experimental sample primarily reflected feasibility and willingness to attend monthly office days, rather than baseline productivity 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 previously for administrative purposes rather than for 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. 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 rather than assigned in blocks.

In June 2024, two months before the intervention began, the firm finalized team assignments and fixed team leaders for the study. Employees could not sort across teams or choose their shifts. This design was intended to keep employees' managerial and team environments constant during and after the experiment. The setting also helps limit the scope for interference, especially between treatment and control employees across teams. Outside the assigned office day, work was fully remote and employees worked from home. In practice, fully remote employees did not meet in person and

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

had little reason to interact with employees outside their own team. Production was also highly standardized: calls were routed centrally to the first available employee, tasks and protocols were uniform, and performance was monitored through the same system. For these reasons, spillovers between treatment and control employees across teams are unlikely. At the same time, we do not rule out within-team spillovers entirely. We use the randomized seating plan, discussed below, to study within-team interactions more directly. Both groups continued working fully remotely during June and July 2024. We use these months as a pre-office-visit baseline for measuring outcomes and assessing balance.

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 1a). The once-per-month schedule was designed as a minimal and scalable intervention that reintroduced coordinated in-person contact without changing the firm's underlying remote model.

On office days, treatment employees traveled together on company shuttles and arrived before their regular shift (Figure 1b). They performed their usual tasks on their regular company laptops, using the same software systems and protocols as on remote days. Importantly, the production technology and incentives were unchanged: shift length and pay were fixed, calls were routed 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 that are central to the intervention's hypothesis. First, *managerial input*: office days created opportunities for real-time feedback and mentoring by team leaders that are difficult to replicate through asynchronous messaging. Second, *peer interaction*: the office facilitated informal social contact and on-the-job learning through conversations and peer support. More broadly, the intervention was designed to restore aspects 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 communications patterns and facilitate the study of potential peer spillovers, we also implemented randomized seating assignments within teams. The office-day schedule (Figure 1c) also created shared time for informal contact (e.g., breakfast, coffee breaks, and lunch). Importantly, we did not introduce any formal training or curriculum on office days, so the treatment captures the effect of in-person co-location and interaction rather than structured instruction. All direct costs of participation, transport, meals, and refreshments were borne by the firm. After each office day, treated employees returned to fully remote work until the next scheduled visit. Compliance was high: the firm sent multiple reminders to treated employees ahead of each scheduled office day, and approximately 95% of assigned office days were attended as planned.

3 Data and estimation strategy

3.1 Data sources and outcome measures

We combine administrative and survey data to evaluate the effects of the monthly office-day intervention. [3] The analysis draws on 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 over a nine-month intervention window (August 2024–April 2025) and a five-month post-intervention period (May–September 2025), during which employees returned to fully remote work, allowing us to evaluate the persistence of treatment effects. Survey coverage differs by instrument: the baseline 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 customer service employee, we observe time-use measures recorded at the employee-by-hour-by-day level. We aggregate these records to the employee-day level for the main analysis. In particular, we observe the number of calls handled and our main productivity outcome, *calls per hour*. We focus on *calls per hour* because it is the firm's standard operational measure of output in this setting, while separately examining customer ratings and audit scores to assess whether higher output came at the expense of service quality. To understand the mechanisms behind changes in call volume, we use *average call duration* (seconds per call) and its components: *talk time* (time speaking with the customer), *hold time* (time the customer is placed on hold), and *administrative time* (after-call/on-screen processing), all measured in seconds per call. We also use *break time*, which is constructed by aggregating time away from active call handling across the workday and is measured in minutes per day.

Service-quality outcomes are observed at the monthly level and include customer satisfaction ratings, which customers provide voluntarily at the end of calls, as well as internal call-audit scores. Audit scores are based on 10 randomly selected calls per employee-month and are evaluated by the firm's independent quality-assurance department to assess procedural accuracy (e.g., greeting the customer and addressing them by name) and informational accuracy (e.g., providing correct information and appropriate solutions).

Retention outcomes are constructed from internal HR records that track employment status and start and end dates at the employee level. We define attrition as a permanent exit from the firm. In addition, we observe baseline characteristics and job attributes (e.g., gender, age, tenure, location, team assignment, and shift schedules), which we use for balance checks, heterogeneity analysis, and robustness.

To complement the administrative outcomes, we administered two online surveys during working hours. A baseline survey was fielded in July 2024, after the study sample was finalized and before the intervention began, and an endline survey was fielded in April 2025, at the end of the intervention period. We collected demographics and background information, along with attitudes toward remote

[3] Replication file for figures and tables at https://www.dropbox.com/scl/fi/0sr29h72ijs6ixfkjv3xs/Rep.zip?rlkey=mu9xceap41smg9oh33ht0lh79&dl=0

work and office attendance. The endline survey focused on experiences during the trial, with modules on perceived team cohesion, feedback frequency, satisfaction with management, work–life balance, job satisfaction, and organizational attachment, as well as support for the monthly office-day policy. Response rates were 100% at baseline (248/248) and 90% at endline (223/248).

We also fielded short post-visit interaction surveys for treated employees after each office day. In the week following a visit, employees reported whom they interacted with during the office day (e.g., morning, around lunch, or afternoon; shared meals or coffee breaks) and whether they communicated with colleagues in the subsequent week, naming counterparts. These data are designed to characterize how in-person contact maps into subsequent communication and potential peer spillovers.

At baseline, the average age is about 23. Women comprise the majority of the workforce (81% in the control group and 74% in the treatment group), and employees are well educated, with 70% holding a bachelor's degree and 27% holding an associate degree.[4] The treatment and control groups do not differ significantly or materially on any baseline covariates.[5]

3.2 Methods and statistical inference

The experiment randomized 248 employees, with 124 assigned to the monthly office-day treatment and 124 assigned to remain fully remote. The employee is therefore the unit of randomization and the primary unit of inference for the main treatment effects. The large number of employee-day observations does not increase the number of experimental units. Rather, repeated daily observations improve precision by providing objective, high-frequency measures of performance for each randomized employee over extended pre-intervention, intervention, and post-intervention periods.

Repeated measurement is valuable in this setting for two reasons. First, daily administrative records provide objective performance measures that are less affected by recall error or selective reporting than self-reported outcomes. Second, repeated observation reduces noise from day-to-day fluctuations in call volume and allows us to estimate treatment effects from within-employee changes over time while preserving the original randomized comparison between treated and control employees.

Our main productivity specifications are estimated on employee-day panels using treatment-by-period interactions. Standard errors are clustered at the employee level to account for serial correlation in outcomes within employees over time. We also report specifications with employee fixed effects, so that identification comes from differential changes over time between treated and control employees. Thus, statistical significance does not rely on treating employee-days as independent realizations, but on the combination of employee-level randomization, repeated objective measurement, and treatment effects that emerge and accumulate over time.

Retention, by contrast, is inherently an employee-level outcome and is analyzed at that level using the original randomized assignment. Service-quality outcomes are observed at the employee-month level and are analyzed at that frequency. More generally, the role of the high-frequency panel is to improve measurement and precision, not to redefine the experimental unit. This distinction is especially important in our setting because the intervention is assigned once at the employee level, but its effects may develop gradually as monthly in-person contact accumulates. Observing

[4] Sample statistics are reported in Table A.1.

[5] Figure A.1 provides a graphical balance check.

employees repeatedly before, during, and after the intervention allows us to trace that dynamic adjustment while maintaining inference based on the underlying employee-level experiment.

For the communication-network analyses, identification comes from randomized seating assignments during office visits, which generate exogenous variation in proximity within the treated group. These analyses therefore provide additional evidence on mechanism rather than serving as the basis for the main treatment claims.

4 Results

4.1 Productivity

Figure 2 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 period, together with 95% confidence bands. 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. By the end of the experimental period in May 2025, the difference is sizable and clearly visible.

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 post-period productivity gain of 7.8% relative to the control group. Figure A.2 shows that this post-experiment gain is visible across the employee-level distribution of average productivity, rather than being driven by a small number of outliers.

Table 1 evaluates the experiment in a regression framework. The results in Column 1 confirm that productivity gains emerge only with repeated exposure to monthly office days. In the early phase of the experiment (Months 1–5), the effect on calls per hour is essentially zero (0.07). In the later phase (Months 6–9), calls per hour increase by 0.67. In the post-experiment period (Months 10–14), the effect rises further to 0.87.[6] A simple Hawthorne or "perk" interpretation is difficult to reconcile with this timing, because the productivity gains emerge only after repeated exposure and persist after mandated office visits end.

Column 2 includes individual fixed effects to examine the treatment impact on individual workers. This reveals that the long-run treatment effect drops from 0.87 to 0.63 calls per hour. This suggests that 72% (0.63/0.87) of the increase in productivity occurs at the worker level, while the remaining 28% arises from selection. Higher-productivity workers are relatively more likely to remain at the firm in the office treatment group than in the control group, which we examine further in Section 4.5 below.

The increase in calls per hour is driven by shorter calls, as shown in Column 3. This again shows a building treatment effect, with average call duration unchanged in Months 1–5 (−0.04 seconds),

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

declining by 13.21 seconds in Months 6–9, and by 17.60 seconds in Months 10–14. Column 4 shows that there is no significant change in the other time components: admin time, hold time, and break time.

We also examine heterogeneity in treatment effects across observable worker characteristics, finding similar effects by gender, age, baseline performance, marital status, and parental status (see Figure A.3), suggesting that productivity gains are broad-based.

4.2 Service quality

A natural concern is that the productivity gains reflect a speed–quality trade-off: treated employees may handle more calls by cutting corners, reducing the quality of customer interactions, or deviating from protocols. We examine treatment impacts on two service-quality outcomes measured at the employee–month level: customer satisfaction ratings and internal random audit scores. We find no evidence that monthly office days affected service quality over any period (details in Table A.2).

4.3 Peer connections, communication, and managerial input

Table 3 shows that the monthly office-day policy increased overall communication among employees. In Column (1), the outcome is the total time an employee spent over the past week communicating with their three most frequent contacts. Treated employees spent 36 more minutes communicating with these colleagues in the week after an office visit.

The remaining columns examine communication at the employee-pair level. 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. Columns (6)–(8) show that these associations remain strong when the seating-neighbor indicator is included alongside each interaction measure, suggesting that shared breaks provide an additional channel of connectivity beyond adjacent desk seating.

The survey evidence points in the same direction. Figure 4 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.[7] 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 Mechanism

Taken together, the evidence is consistent with a combination of more frequent manager feedback, richer peer interaction, and stronger subsequent team communication. The design does not allow us to separately identify the relative importance of these channels but the results consistently indicate

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

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 experiment, which is more consistent with learning and relationship-building than with a purely transitory response. 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.[8] 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 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 2 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 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. They help interpret why the long-run treatment effect is smaller in specifications with employee fixed effects, but we do not interpret Table 2 itself as a standalone causal estimate of selective retention.

5 Cost–benefit analysis

We calculate the steady-state benefit–cost ratio for the firm.

[8] 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.

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, refreshments, and $250 per month for office space. Over nine visits and 124 treated customer service agents, total program expenditure was $12,950.

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. The estimated long-run treatment effect on productivity is 7.8%. We value this using the employer labor cost of 130 TRY per hour (approximately $4). Assuming each employee worked 1,560 hours over the nine-month period, the implied productivity benefit is $60,300.

Total monetized benefits equal $66,700, which is more than five times the $12,950 cost. 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 can improve outcomes in an otherwise fully remote workplace. We study a nine-month randomized controlled trial in which treated employees worked from the office one fixed day per month and otherwise remained fully remote.

Three findings stand out. First, monthly office days raise productivity, and the effects build gradually over time. Calls per hour change little in the first months of the intervention, increase meaningfully only after repeated exposure, and remain elevated after the final office visit. The increase in output is driven by shorter calls, primarily through lower talk time, rather than by compressing breaks or shifting administrative or hold time. We also find no evidence of 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 on mechanisms points to interaction, learning, and feedback. The results are most consistent with a combination of more frequent manager feedback, richer peer interaction, and stronger subsequent team communication. Randomized seating assignments generate new peer links that persist beyond the office day, and treated employees are more likely to report more regular managerial feedback, more effective team communication, and stronger cultural fit. While we cannot cleanly separate the relative importance of these channels, the evidence indicates that monthly office days partially restored forms of learning and support that were more limited in the fully remote setting.

Third, monthly office days improve retention. Attrition accumulates more slowly in the treatment group, and the gap persists beyond the intervention window, indicating that the policy does more than postpone exits. Employees also report feeling more connected to the firm and their colleagues after the monthly office visits. These retention gains are economically meaningful: the policy reduces the selective loss of higher-performing employees, a margin that is especially important in settings where onboarding is costly and service delivery depends on experienced workers.

From the firm's perspective, the policy is inexpensive and appears cost-effective. Modest spending on transport and meals is outweighed by monetized gains from higher productivity and lower turnover, yielding a benefit–cost ratio of 5:1. More broadly, our results show that the performance of remote work depends not only on where work is done, but also on how interaction is organized. In fully remote settings, even limited but well-coordinated in-person contact can deliver durable gains in communication, productivity, and retention without compromising service quality.

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Figures and Tables

Figure 1: During the intervention

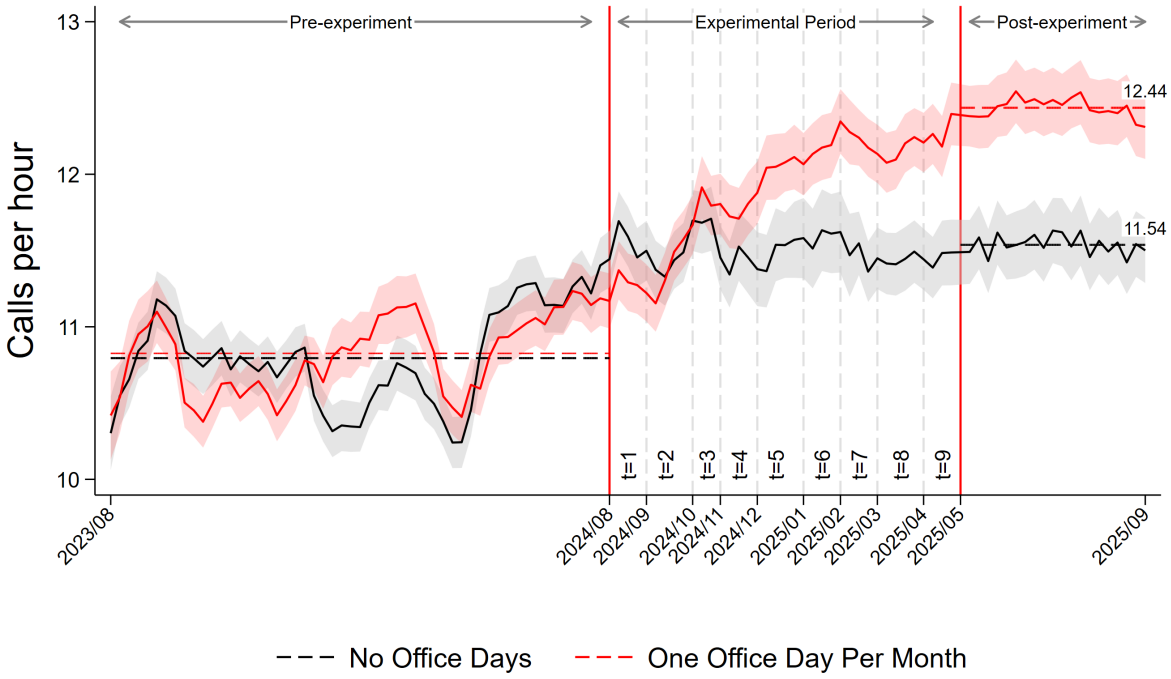
(a) Working at home environment

(b) Transport to and from the office

(c) Office-day activities

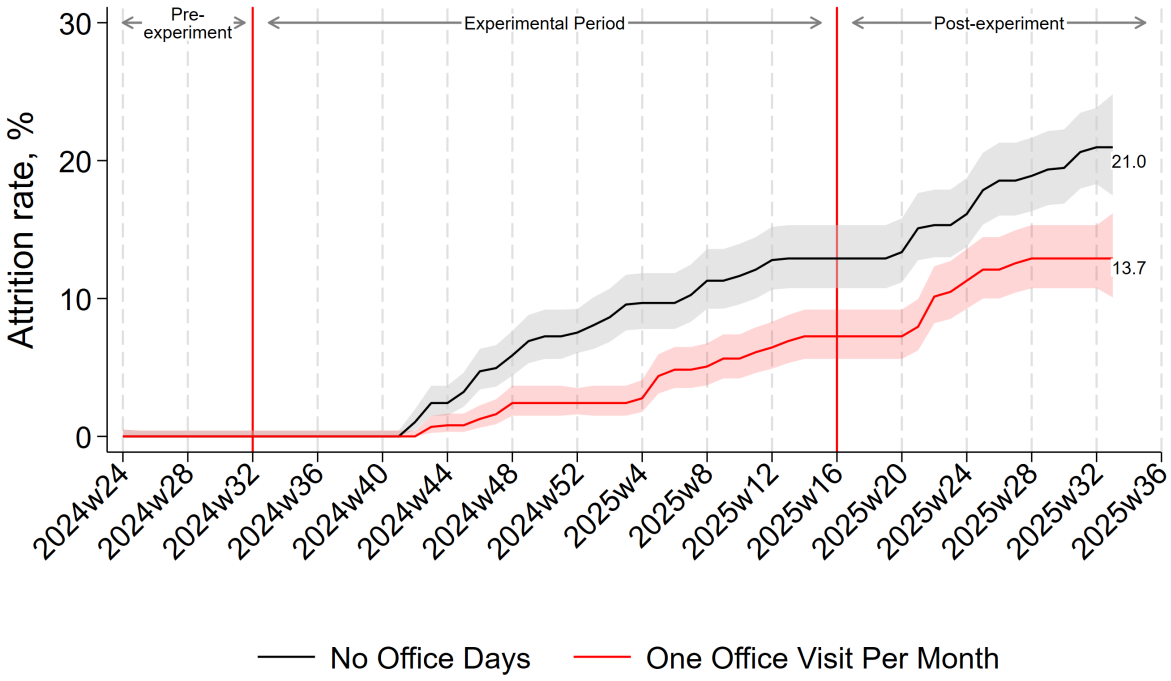
Notes: This figure presents illustrative photos from the intervention period, showing employees working from home (Panel (a)); transportation arranged by the company and group activities during monthly office days (Panel (b)); and the office-day work environment and team social activities (Panel (c)).

Figure 2: Calls per hour before, during and after the RCT



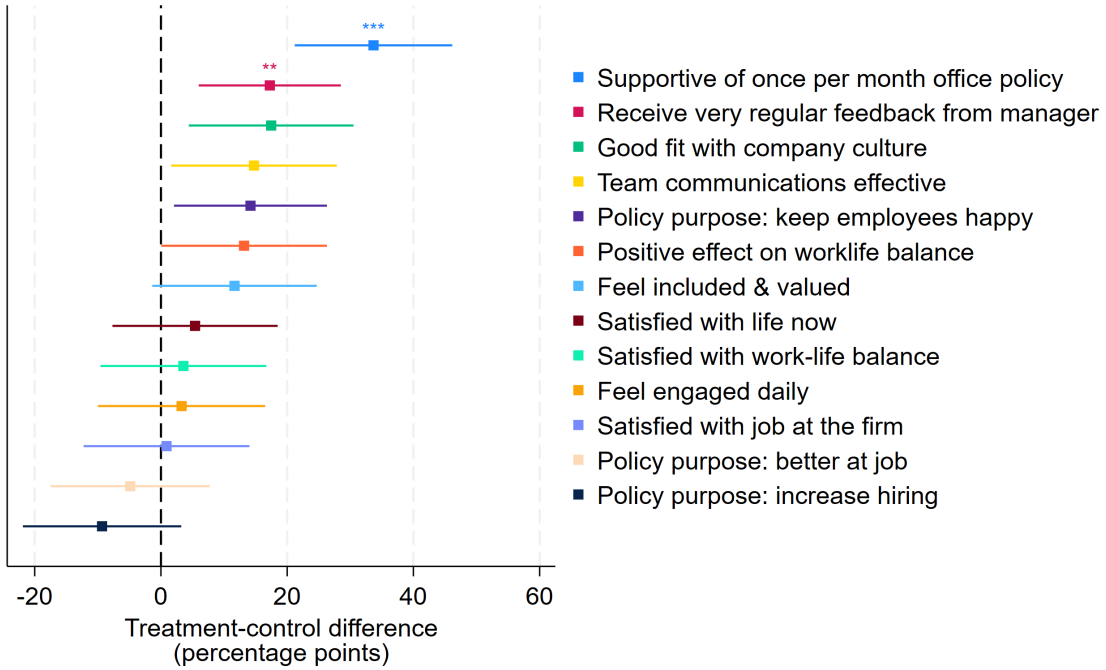
Notes: This figure shows weekly mean *calls per hour* for the control (No Office Days) and treatment (One Office Day Per Month) groups. Weekly means are calculated using employee-day-level data. Horizontal dashed lines represent mean values in the *pre-experiment* and *post-experiment* periods for the *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-experiment period are 11.5 and 12.4, respectively. The vertical red lines at August 2024 and May 2025 indicate the start and end of the treatment 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 3: Attrition is lower among customer service agents 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 visit 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.

Figure 4: At endline treated employees were more supportive of the policy and reported more frequent manager feedback, stronger cultural fit



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.

Table 1: Early, late and post experiment treatment effects

	Calls per hour		Average call duration (seconds per call)	Admin, hold & break time (minutes per day)
	(1)	(2)	(3)	(4)
One Office Day Per Month	0.03			
	(0.27)			
Post (1-5 months)	0.71***	0.78***	-21.37***	9.16***
	(0.13)	(0.12)	(3.60)	(0.76)
Post (6-9 months)	0.69***	0.85***	-23.77***	11.36***
	(0.17)	(0.14)	(4.14)	(0.87)
Post experiment (10-14 months)	0.74***	0.85***	-24.10***	13.08***
	(0.17)	(0.14)	(4.12)	(0.94)
One Office Day Per Month	0.07	-0.02	-0.04	0.59
× Post (1-5 months)	(0.19)	(0.18)	(5.07)	(1.18)
One Office Day Per Month	0.67***	0.43**	-13.21**	-0.03
× Post (6-9 months)	(0.23)	(0.20)	(5.64)	(1.33)
One Office Day Per Month	0.87***	0.63***	-17.60***	0.81
× Post experiment (10-14 months)	(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. *Admin, hold & break time*, measured in minutes per day, is the sum of admin time, hold time, and break time. In analogous separate regressions for each of these three outcomes, the interaction terms between *One Office Day Per Month* and the *Post* indicators are statistically insignificant. *One Office Day Per Month* equals 1 for employees assigned to monthly office visits and 0 for fully remote employees. 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: 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 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.

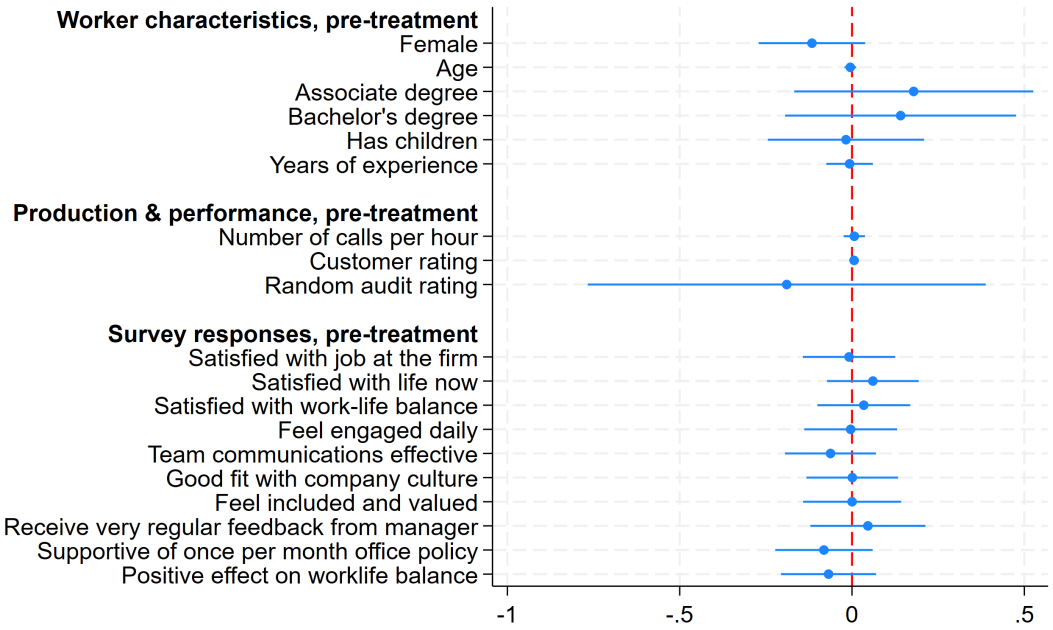
Table 3: Office time increased employee communication with colleagues and employees were more likely to communicate colleagues who were seated next to them in the office

	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 for each agent's outside option. 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.

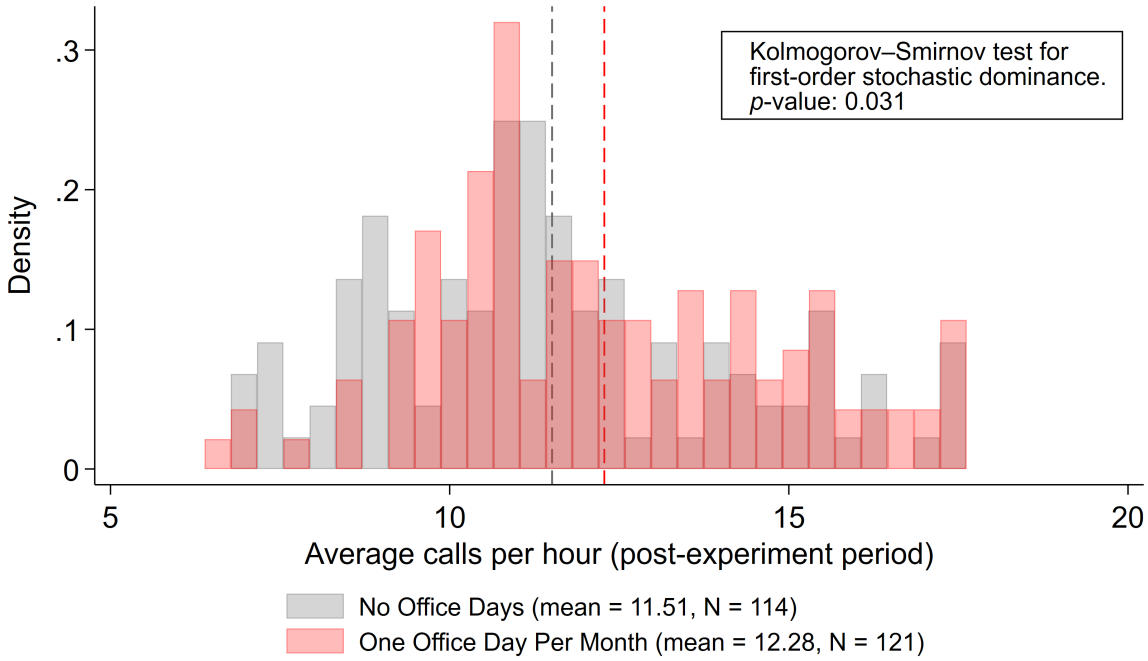
Appendix

Figure A.1: Balance test for treatment assignment



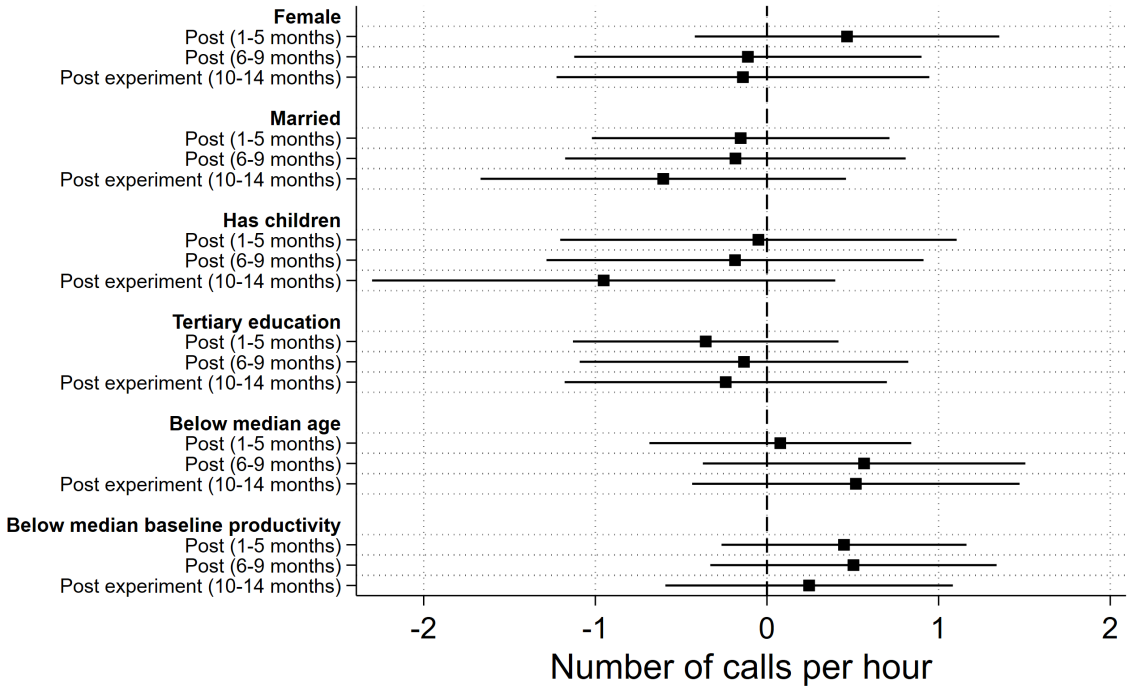
Notes: This figure shows coefficient estimates from a linear probability model. The dependent variable equals 1 if an employee is assigned to the treatment group. The data include one observation per employee ($N = 248$). Standard errors are robust to heteroskedasticity, and whiskers show 95% confidence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

Figure A.2: Distribution of post-experiment calls per hour



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 post-experiment period (Months 6–14, January–September 2025). The sample includes all employees with at least five observed working days in the post-experiment period ($N = 121$ treatment, $N = 114$ control). Each employee contributes one observation, equal to their average *calls per hour* over all observed post-experiment workdays. 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.3: 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: 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
Year 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.2: 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. *One Office Day Per Month* equals 1 for employees assigned to the monthly office-visit group and 0 for fully remote employees. Standard errors are clustered at the employee level. ***, **, and * denote significance at the 1, 5, and 10 percent levels.