The Value of Some Office Time*

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Abstract

Can regular office time improve employee performance? A large multinational firm randomized 248 customer-service employees to remain fully remote (control) or to work from the office together one day per month (treatment), finding three key results. First, monthly office days gradually raise productivity, increasing this by +7.8% after 9 months. Second, monthly office days strengthen workplace communication networks. Treated employees spend 36 additional minutes communicating with colleagues in the week after an office visit, are more likely to receive frequent manager feedback, and employee pairs randomly assigned as desk neighbours are 11 percentage points more likely to communicate afterward. Third, monthly office days reduced attrition by a third, from 21.0% in the control group to 13.7% in the treatment group. Overall, monthly office days increased productivity and reduced turnover by improving employee connectivity, generating a benefit–cost ratio of 3.9 for the firm.

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1 Introduction

Remote and hybrid work surged during the COVID-19 pandemic and remains common in many labour markets. By 2025, roughly 100 million employees in Europe and North America worked schedules that combine home and office days (Barrero et al., 2021, 2023; Aksoy et al., 2025a; Buckman et al., 2025; Luca et al., 2025). Remote work can expand flexibility for workers and widen firms' access to talent, with potential inclusion gains for groups facing commuting or scheduling constraints (Mas and Pallais, 2017; Pabilonia and Vernon, 2022; Aksoy et al., 2025b; Bloom et al., 2026). Yet a recurring concern is that sustained physical separation may weaken collaboration, mentoring, slow learning, and erode cohesion (Yang et al., 2022; Brucks and Levav, 2022; Emanuel et al., 2023).

These concerns sit alongside an evidence base that is still mixed and highly context dependent. Hybrid arrangements can reduce attrition without harming performance in some settings (Bloom et al., 2024; Choudhury et al., 2024), but in fully remote operations productivity effects appear to hinge on task structure, monitoring technologies, managerial practices, and the scope for adaptation over time (Battiston et al., 2021; Choudhury et al., 2021; Deole et al., 2023; Gibbs et al., 2023; Burdett et al., 2024; Emanuel and Harrington, 2024). As a result, the central question for many organisations is not whether remote work is feasible, but how to design it: when day-to-day work is remote, what in-person contact, if any, is needed to sustain coordination, learning, and attachment to the firm?

We address this question by studying a simple policy that many remote-first organisations can implement: employees attend the office on one fixed day per month and otherwise remain fully remote. We evaluate this policy in a nine-month randomised controlled trial in the customer service division of a large multinational firm, in partnership with its global telecommunications client. The study sample comprises 248 fully remote employees working on the same client account. Employees were randomly assigned either to remain fully remote (control) or to attend the office one day per month (treatment). The intervention was intentionally light-touch: it held pay, shift schedules, tasks, call routing, and performance evaluation constant. Office days provided co-location with peers and in-person access to supervisors, but introduced no formal training curriculum.

Three findings summarise our results. First, monthly office days raise productivity, but only gradually. We detect little change in the first four months after the policy begins; effects emerge after about five months, when treated employees handle more calls per hour than their fully remote peers. By the end of the 9-month intervention calls per hour are about 7.8% higher on average, and the gains persist into the post-intervention period.[1] The increase is driven by shorter calls: average call duration falls by roughly 4% by Months 6–9 (and by about 5% in Months 10–14), with the largest change coming from talk time rather than administrative or hold components. There is no evidence of a speed–quality trade-off: customer satisfaction ratings and internal audit scores remain stable, and break time does not decline.

Second, monthly office days increase employees' communication with colleagues on their home days. At the employee level, treated employees spend about 36 additional minutes communicating

[1] This 7.8% figure refers to the raw treatment–control gap in calls per hour at the end of the intervention period. Our employee fixed-effects difference-in-differences estimates imply smaller average treatment effects over broader windows: about 4.0% in Months 6–9 and 5.8% in Months 10–14. We report the 7.8% figure here to describe the end-of-intervention difference, not as the headline regression-based treatment effect.

with their three most frequent contacts in the week after an office visit, roughly a 22% increase relative to the mean. Complementing this result, pair-level evidence shows that co-workers randomly assigned to adjacent seats on office days are more likely to communicate afterward. At the same time, treated employees' productivity gains are unrelated to the productivity of their randomly assigned neighbours. Third, retention improves. Cumulative attrition is lower in the treatment group (13.7%) than in the control group (21.0%), the gap persists beyond the final office visit, and the policy reduces the selective loss of high performers: in the control group, quitters are more productive than stayers, whereas in the treatment group, stayers are substantially more productive than quitters.

We provide evidence on mechanisms consistent with richer interaction, stronger managerial support, and greater organisational attachment. Randomised seating assignments during the first office visits generate exogenous variation in peer proximity, and assigned desk neighbours are more likely to communicate afterward. At the same time, exposure to higher-performing neighbours does not predict larger productivity gains, suggesting that the benefits of office days are unlikely to operate primarily through direct spillovers from more productive peers. Endline surveys point in the same direction: treated employees report more frequent managerial feedback and a stronger perceived cultural fit, while broader measures of general well-being move little. Taken together, the evidence is most consistent with periodic co-location strengthening communication, coordination, and manager, employee interaction rather than with direct learning from higher-performing desk neighbours alone. From the firm's perspective, the policy is inexpensive and appears cost-effective. The per-visit cost of transport and meals is modest, and a conservative cost–benefit calculation yields a benefit–cost ratio of 3.9, with gains arising from both higher productivity and reduced turnover.

Related literature and our contributions. Our paper speaks to two strands of research. First, we relate to work on structured managerial practices, in particular feedback, coaching, and performance conversations, as determinants of engagement and performance. Bloom et al. (2013) and Lazear et al. (2015) emphasise the productivity returns to better management, while Manthei et al. (2023) show that regular performance conversations can improve both profits and retention in a retail setting. Closest to our mechanisms, Choudhury et al. (2023) show that structured virtual interactions can improve newcomer performance, Emanuel et al. (2023) highlight the role of coworker proximity for training and productivity, and Choudhury et al. (2025) documents how temporary co-location shapes communication patterns in an all-remote organisation. Taken together, this literature suggests that performance in remote settings depends not only on where work is done, but also on how opportunities for interaction, feedback, and learning are organised. We contribute by providing experimental evidence that even infrequent, coordinated co-location can strengthen managerial feedback and communication in a fully remote service operation.

Second, we contribute to the growing evidence on the effects of hybrid and fully remote work on worker performance. Experimental and quasi-experimental studies of hybrid arrangements typically find positive or null impacts on performance (Bloom et al., 2015; Battiston et al., 2021; Choudhury et al., 2021, 2024; Bloom et al., 2024). A complementary literature shows that remote work can reshape collaboration, idea generation, and cognitive performance (Brucks and Levav, 2022; Yang et al., 2022; Künn et al., 2022). By contrast, evidence on fully remote work spans a wider range of estimates, from negative to positive, reflecting heterogeneity in tasks, monitoring, organisational practices, and firms' ability to adapt (Gibbs et al., 2023; Atkin et al., 2023; Emanuel and Harrington, 2024; Aksoy et al., 2025b; Gupta et al., 2026). The recent evidence in Gupta et al. (2026), for example, suggests that the productivity consequences of remote work can differ sharply across firms depending on their stage of development and hiring constraints. Our contribution is to study a remote-first setting, where remote work is already the default, and to evaluate a minimal hybrid policy that many remote organisations can implement: one coordinated office day per month. In a semi-routinised service context with tight performance measurement, we show that this policy raises productivity without reducing service quality and improves retention, including by mitigating the selective loss of high performers. More broadly, our results highlight that managerial and peer-interaction structures are central to sustaining performance in fully remote operations.

The remainder of the paper is organised as follows. Section 2 describes the setting and experimental design. Section 3 presents the data and sample characteristics. Section 4 outlines the empirical strategy. Section 5 reports the main results on productivity, service quality, retention, and mechanisms. Section 6 presents the cost–benefit analysis. Section 7 concludes.

2 Setting and experimental design

The firm and setting. 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).[2] 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 more than 3,500 workers.

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 standardised and routinely monitored. Employees perform a clearly defined task (handling incoming customer calls) under fixed schedules and stable team structures.

Customer service agents 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. Organisationally, 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 organised 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 agent. Employees handle consumer queries using a headset and a firm-provided computer, following standard protocols set by the client account.

Remote-work transition and motivation for the intervention. 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.

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

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 the shift delivered operational benefits, internal feedback increasingly highlighted organisational costs, including weaker cultural cohesion, fewer opportunities for real-time coaching, and persistent retention problems. In practice, day-to-day interactions between employees and team leaders often occurred via messaging applications, with limited informal contact and no in-person interaction. These concerns motivated the firm to collaborate on the randomised controlled trial evaluated in this paper, testing whether structured monthly office visits can strengthen managerial support and improve retention and performance.

Client account and implementation site. 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.

Study population and recruitment. In March 2024, the firm informed 661 employees 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.

Eligibility and analysis sample. 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 they (i) had at least three months of tenure at the time of recruitment and (ii) lived within roughly 45 minutes of the office (approximately a 16-kilometre / 10-mile radius). These criteria yielded 248 eligible volunteers, who constitute the experimental sample.[3] 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.

Treatment assignment. Assignment was implemented in late April 2024 by the firm's HR department using employees' pre-existing company ID numbers: workers with odd-numbered IDs were assigned to treatment and those with even-numbered IDs to control, yielding two groups of 124 employees each. Although company IDs reflect hiring sequence, assignment depended only on whether an employee's ID number was odd or even. Both groups continued working fully remotely during June and July 2024, which provides a clean pre-intervention baseline for measuring outcomes and assessing balance, as discussed in the next section.

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). Because of office-capacity constraints the monthly office day was implemented over two separate days each month. Treatment

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

employees were assigned to two groups, one group attended visit Day 1 and the other attended visit Day 2.

On office days, treatment employees travelled 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. 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.

What changes on office days. Relative to a standard remote day, the monthly office visit shifted three 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. Third, *commuting and the work environment*: treated employees incurred travel time (up to roughly 45 minutes each way), while control employees continued working from home in their usual setting.

To measure interaction patterns and facilitate the study of potential peer spillovers, we also implemented randomised seating assignments during office visits. The office-day schedule (Figure A.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.

Hypotheses. Our pre-analysis plan specifies three primary hypotheses about the effects of monthly in-office visits on workers and the organisation:

H1. **Productivity and service quality.** Monthly office visits improve performance. Through more effective real-time feedback and monitoring on office days, treated employees are expected to handle calls more efficiently (e.g., more calls per hour and/or shorter call duration) and improve service-quality metrics (e.g., customer satisfaction and call-audit scores).

H2. **Communication and team cohesion.** Monthly office visits strengthen communication between colleagues and team leaders. Face-to-face interaction is expected to facilitate knowledge sharing, and team cohesion, which may improve both performance and the broader work climate.

H3. **Retention.** Monthly office visits increase retention (lower quits/turnover) among treated employees.

These hypotheses reflect the idea that even infrequent in-person contact can "refresh" social ties and support the exchange of tacit knowledge in an otherwise remote work arrangement. At the same time, the magnitude of any effects is ex ante ambiguous: a single day per month may be too limited to generate large changes in behaviour or outcomes. The experiment is designed to quantify these effects.

3 Data, sample characteristics and balance

Data sources and linkage. We combine administrative and survey data to evaluate the effects of the monthly office-day intervention. The analysis draws on three sources: (i) high-frequency operational data from the firm's internal performance dashboard, (ii) HR records on employment status and office-day attendance, and (iii) surveys fielded by us to measure worker experiences and interaction patterns.

All datasets are linked using anonymised identifiers and cover the full experimental sample of 248 employees over a nine-month intervention window (August 2024–April 2025) and five-month post-intervention period (May–September 2025). The raw data are recorded at high frequency and can be aggregated to daily, weekly, and monthly panels.

Administrative outomes. 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*. 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). These are the same metrics used by the firm for routine monitoring and evaluation of employees.

Retention outcomes are constructed from internal HR records that track employment status and start/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.

Surveys and randomised seating arrangements. To complement the administrative outcomes, we administered two online surveys during working hours. A baseline survey was fielded in July 2024, before the intervention began (and after the study sample was finalised), and an endline survey was fielded in April 2025, at the end of the intervention period. We collected demographics and background information (including prior fully remote work experience), along with attitudes toward remote 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 organisational 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 employee 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, afternoon; shared meals or coffee breaks) and whether they communicated with colleagues in the subsequent week, naming counterparts and indicating the communication channel (e.g., phone or messaging). These data are designed to characterise how in-person contact maps into subsequent communication and potential peer spillovers.

Survey responses are complemented with administrative logistics for office days. We observe seating locations inside the office which allow us to construct measures of physical proximity. As mentioned above, seating was randomised and enforced during the first three office visits; from the fourth visit onward, employees chose their seats. Taken together, these data provide multiple measures of co-location, interaction, and follow-up communication.

Main sample. Our preferred specifications use the full administrative sample (all 248 employees). Survey-based analyses use the corresponding respondent samples, and when required we restrict attention to balanced panels (e.g., respondents observed at both baseline and endline), which we specify in each table and figure.

3.1 Sample characteristics and balance

Sample description. Table 1 reports baseline summary statistics on worker characteristics, productivity, and service-quality measures. At baseline, the average age is about 23. Women comprise the majority of the workforce (81% in control and 74% in treatment). 70% of employees hold a bachelor's degree and 27% hold an associate degree. Around 22% are married, 12% have children, and baseline tenure averages just under half a year.

Balance on observables and baseline outcomes. The treatment and control groups are similar across all observed covariates: gender, age, education, marital status, parental status, and tenure are closely aligned, and none of the differences are statistically significant. Baseline productivity measures are broadly similar across treatment and control groups. We detect no meaningful baseline differences in calls per hour, call duration, break time, talk time, or administrative time. The only exception is hold time, which is modestly higher in the treatment group at baseline. Baseline customer ratings and random audit scores are likewise statistically indistinguishable across groups.

Figure A.2 provides a complementary balance check using a linear probability model, plotting the association between treatment assignment and a selected set of baseline characteristics and baseline outcomes. For the variables shown in the figure, the estimated coefficients are close to zero and statistically insignificant, indicating that these observables do not systematically predict treatment status. Taken together, Table 1 and Figure A.2 suggest that the treatment and control groups are similar prior to the intervention.

4 Empirical Strategy

4.1 Main specification

Our empirical strategy exploits the assignment of customer service agents to the one-office-day-per-month policy and follows outcomes over time to capture both the timing and persistence of the intervention's effects. We estimate difference-in-differences specifications that compare treated and control employees before and during specific post-intervention periods. We also complement this approach with daily specifications that allow treatment effects to evolve flexibly over time, rather than imposing a constant effect within each treatment window.

At the employee–day level, we estimate the following specification:

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

where indices i and t denote the customer service agent and calendar date, respectively. The outcome y_{it} is measured at the employee–day level and captures individual performance, including calls per hour, call duration and its subcomponents (talk time, hold time, and administrative time), as well as break time. The variable $\text{OneOfficeDayPerMonth}_i$ is an indicator equal to 1 for employees assigned to the monthly office-day group and 0 for employees assigned to the fully remote control group. Because this assignment is time-invariant, its main effect is absorbed by the employee fixed effects.

Three time-period dummies summarize post-intervention dynamics over broad windows. D_t^E is an indicator equal to 1 if calendar date t falls in the early experimental phase (Months 1–5) and 0 otherwise. D_t^L is an indicator equal to 1 if calendar date t falls in the late experimental phase (Months 6–9) and 0 otherwise. D_t^P is an indicator equal to 1 if calendar date t falls in the post-experimental phase (Months 10–14), when treated employees had returned to a fully remote setting, and 0 otherwise. The omitted period is the pre-intervention baseline.

This grouping is motivated by the gradual patterns observed in the descriptive evidence and allows us to assess whether the effects of monthly office days emerge immediately or build up over time with repeated in-person interaction. The coefficients reported in the tables correspond to average treatment effects over these respective periods and provide a compact summary of the underlying dynamic effects.

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 throughout to allow for serial correlation in individual outcomes.

4.2 Communication and spillover

Employees completed short post-visit interaction surveys after each office day. We link these responses to administrative records on office logistics (in particular, the seating plan) to measure communication and potential spillovers. We estimate the following specification:

$$c_{ij} = \beta \, \text{SeatingPlanNeighbour}_{ij} + \varepsilon_{ij}. \tag{2}$$

where, c_{ij} captures follow-up communication between the reference employee i and a potential colleague j. Indices i and j denote the reference employee and the set of all colleagues with whom the reference employee could plausibly have interacted during the office visit, respectively.

The sample comprises 124 treated employees who completed a short post-visit interaction survey after each office day. For logistical reasons, treated employees were split into two mutually exclusive groups that visited the office on different days: Group 1 (50 employees) always visited on one designated day, and Group 2 (74 employees) always visited on another. Because the two groups were never in the office on the same day, employees could only have in-person interactions with colleagues in their own group.

To construct the employee-pair dataset, we first enumerate all potential within-group pairs (that is, pairs of employees who were physically present in the office on the same day and therefore had an opportunity to interact in person). This yields $(50 \times 49) + (74 \times 73) = 7{,}852$ potential within-group colleague pairs.

Our analysis also considers follow-up communication after the office visit (e.g., subsequent messages or calls). Such follow-up can occur not only with colleagues in the same visiting group but also with individuals outside the employee's visiting group and outside the treatment sample. We capture all potential follow-up interactions with these outside individuals by aggregating them into a single "outside" option for each reference employee. Adding this outside option increases the total number of possible reference-employee pairs to 7,976.

In the final employee-pair data, we observe (i) $\text{SeatingPlanNeighbour}_{ij}$, an indicator equal to 1 if employee i was seated next to colleague j during the office visit and 0 otherwise, and (ii) c_{ij}, which captures follow-up communication between employee i and potential colleague j on both the extensive margin (any follow-up) and the intensive margin (the amount of follow-up).

To test whether there are learning spillovers we augment the production data with measures of neighbouring employees' productivity. Each reference employee has two adjacent neighbours and these are identified using administrative records on (randomised) seating plan. We estimate equation 3 and then obtain predictions along the distribution of neighbouring employees' productivity.

$$
\begin{aligned}
y_{it} = \gamma_E D_t^E + \gamma_L D_t^L + \gamma_P D_t^P \\
+\delta_E \left(\overline{Neighbours}_i \times D_t^E\right) \\
+\delta_L \left(\overline{Neighbours}_i \times D_t^L\right) \\
+\delta_P \left(\overline{Neighbours}_i \times D_t^P\right) \\
+\mu_i + \varepsilon_{it},
\end{aligned}
\tag{3}
$$

where where y_{it} denotes calls per hour of employee i at time t, and $\overline{Neighbours}_i$ denotes the average baseline calls per hour of employee i's two adjacent neighbours.

The baseline productivity measure $\overline{Neighbours}_i$ is constructed in two steps. First, for each neighbour, we compute average calls per hour using only odd calendar dates during the pre-experiment period. Second, we take the average of these two neighbour-specific averages. To avoid mechanical mean reversion, all odd calendar dates from the pre-experiment period are excluded from the production data prior to estimating equation 3.

5 Results

5.1 Productivity

Figure 1 plots weekly averages of calls per hour (our primary measure of productivity) for treated and control employees over the pre-intervention, intervention, and post-intervention periods, together with 95% confidence bands. While purely descriptive, the figure is informative about both the timing and persistence of the intervention's effects. In the months prior to the start of the experiment (August 2024), the two series move closely together and have essentially identical pre-period means (10.8 calls per hour in both groups), providing evidence of no differential pre-trends.

After monthly office days begin, the treatment group gradually departs from the control group. The divergence is not immediate: in the first 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 (May 2025), the difference is sizeable and clearly visible.

The post-intervention period provides evidence on persistence. Following the end of monthly office days, treated employees continue to exhibit higher calls per hour than the control group, with no sharp convergence in the months immediately after the intervention ends. Consistent with this visual pattern, 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-period productivity gap of 0.90 calls per hour, or 7.8% relative to the control group's post-period mean.

Next, we present these patterns using our regression specifications. Figure A.3 complements Figure 1 by reporting daily estimates: it plots fitted values from a regression of calls per hour on a treatment indicator, daily time dummies, and their full set of interactions, with 95% confidence intervals. The daily series corroborates the weekly evidence: there are no meaningful differential pre-trends, the treatment-control gap emerges gradually during the experimental period, and the higher productivity of treated employees persists into the post-intervention months.

Table A.2 quantifies these patterns by estimating equation 1. The results 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 (-0.02) (Column 2 of Table A.2). In the later phase (Months 6–9), calls per hour increases by 0.43 (about 4.0% of the pre-treatment mean). In the post-experiment period (Months 10–14), the effect rises further to 0.63 (about 5.8% of the pre-treatment mean), consistent with the visual evidence that the gains persist rather than dissipate immediately once office days end.

To further interpret the post-period productivity gap, we provide a simple accounting decomposition of the treatment-control difference into a "within" (behavioural) component and a "between" (composition/selection) component.[4] This decomposition is descriptive rather than causal, but it is useful for organizing the evidence. It shows that the total post-period productivity advantage

[4] The total post-period productivity gap is 0.90 calls per hour, given by the difference in post-period means shown in Figure 1 ($12.4 - 11.5 = 0.90$), which corresponds to 7.8% relative to the control group's post-period mean ($12.4/11.5 - 1 = 0.078$). The estimated "within" component from Column 2 of Table A.2 is 0.63 calls per hour, or 5.5% of the control post-period mean ($0.63/11.5 = 0.055$). The remaining 0.27 calls per hour is the residual "between" component, defined mechanically as the difference between the total and within components ($0.90 - 0.63 = 0.27$), corresponding to 2.3% of the control post-period mean ($0.27/11.5 = 0.023$). Mechanically, the two components add up to the full gap: $7.8\% = 5.5\% + 2.3\%$.

is 7.8%, of which 5.5 percentage points are accounted for by the "within" component and 2.3 percentage points by the "between" component. Put differently, most of the observed post-period gap reflects productivity increase among employees who remain in the sample, while a smaller but still meaningful share reflects composition effects arising from differential selection.

The increase in calls per hour is driven by shorter calls (Column 4 of Table A.2). Average call duration is unchanged in Months 1–5 (-0.04 seconds), but declines by 13.21 seconds in Months 6–9 (about 3.8% of the pre-treatment mean) and by 17.60 seconds in Months 10–14 (about 5.0% of the pre-treatment mean). Columns 5–10 decompose call duration into talk time, administrative time, and hold time. The reduction is driven primarily by shorter talk time, with little movement in administrative or hold time. Finally, break time is broadly unchanged, indicating that higher output is not achieved by compressing breaks.

We also examine heterogeneity in treatment effects across observable worker characteristics. Figure A.4 shows broadly similar effects by gender, age, baseline performance, marital status, and parental status. Differences across subgroups are small and statistically indistinguishable from zero, suggesting that the productivity gains are not concentrated in a particular group.

5.2 Service quality

A natural concern is that the productivity gains documented above reflect a speed–quality tradeoff: treated employees may handle more calls by cutting corners, reducing the quality of customer interactions, or deviating from protocols. Table A.1 addresses this concern by reporting treatment effects on two service-quality outcomes measured at the employee–month level: customer satisfaction ratings and internal random audit scores. Across specifications, we find no evidence that monthly office days affected service quality. In the employee fixed-effects specifications (columns 2 and 4), the estimated treatment effects are small and statistically indistinguishable from zero in both the early and late phases of the intervention. For customer ratings, the treatment–post interactions are 0.93 (s.e. 1.01) in Months 1–5 and -0.76 (s.e. 1.40) in Months 6–9; for audit scores, the corresponding estimates are -0.03 (s.e. 0.02) and 0.01 (s.e. 0.03).

Taken together, these results suggest that the increase in calls per hour reflects greater efficiency in handling customer requests rather than a deterioration in customer experience or procedural compliance.

5.3 Mechanisms

The productivity gains (higher calls per hour driven by shorter calls, without deterioration in service quality) documented above point to channels operating through interaction, learning, and feedback rather than short-run increases in effort. In this section, we provide evidence on these mechanisms using (i) random variation in peer proximity generated by seating assignments on office days and (ii) endline survey measures of communication and managerial support.

Office days create new peer links and sustained communication. We start by testing whether in-person co-location translates into subsequent interaction. During the first three office visits, treated employees were assigned seats according to a randomized seating plan.

Table 3 shows that the monthly office-day policy increases overall communication. In column (1), the outcome is constructed from the endline survey as the total number of minutes an employee reports spending over the past week communicating with the three colleagues with whom they communicated most frequently. The estimates indicate that treated employees spend 36 additional minutes communicating with these contacts in the week after an office visit. Turning to employee pairs, column (2) shows that pairs randomly assigned as desk neighbours are 11 percentage points more likely to communicate in the following week, relative to a mean of 9%. Columns (3)–(5) show similarly large effects 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-neighbour indicator is included alongside each interaction measure. Because desk neighbours were randomly assigned rather than chosen, these results isolate the causal role of physical proximity in creating new peer links that persist beyond the office day itself.

Peer exposure generates productivity spillovers, but is independent of neighbours performance. We next ask whether physical proximity translates into learning spillovers. Figure A.5 reports predictions from equation 3. Although the predictions are increasing across the distribution of their neighbours' productivity confidence intervals are overlapping. This indicates that productivity uplifts among treated employees are independent of their neighbours productivity.

Treated employees report more feedback and stronger organisational attachment. Survey evidence is consistent with the interaction-based channels documented above. Figure 3 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.[5]

Treated employees are more likely to support the once-per-month office policy, to report receiving very regular performance feedback from their manager, to report a stronger perceived fit with the firm's culture, to report that team communications are effective, and to report that the purpose of the policy is to improve employee happiness. After adjusting for multiple hypothesis testing, support for the once-per-month office policy and regular manager feedback remain statistically significant at the 5% level, while stronger perceived cultural fit remains significant at the 10% level. In contrast, we do not detect statistically significant effects on broader measures of well-being, including life satisfaction, work–life balance, engagement, or overall job satisfaction.

Taken together, the survey results point to mechanisms operating through workplace relationships and professional support, particularly managerial feedback and cultural fit, rather than a general improvement in well-being. This evidence complements the earlier results showing that office days create new peer links (Table 3).

5.4 Attrition

Fully remote organisations often cite elevated turnover as a key operational challenge. We therefore examine whether occasional, coordinated in-person contact can reduce attrition in an otherwise fully remote setting.

Figure 2 plots cumulative attrition for the treatment and control groups. Attrition is essentially flat in the pre-experiment period. Once monthly office days begin, attrition increases in both groups, but more slowly in the treatment group, so the gap between the two groups widens over time. By

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

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.

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. 11.477 calls per hour; difference = -0.267, $p < 0.001$), whereas in the treatment group the pattern is reversed: stayers are more productive than quitters (12.118 vs. 10.955; difference = 1.163, $p < 0.001$). The corresponding difference-in-differences estimate is 1.430 calls per hour. We view this pattern as consistent with monthly office days helping retain relatively more productive employees, while recognizing that Table 2 is descriptive and based on observed post-period outcomes.

Overall, the evidence indicates that monthly office visits reduce turnover. The lower cumulative attrition in the treatment group, together with the productivity patterns in Table 2, suggests that the policy helped retain relatively stronger employees. This is an economically important margin in settings where onboarding is costly and service delivery depends on experienced agents.

6 Cost–benefit analysis

This section provides a back-of-the-envelope cost–benefit calculation from the firm's perspective. We focus on the two margins where we observe clear treatment effects (productivity and retention) and monetise benefits using conservative assumptions. All figures are expressed in USD.

Programme costs. The intervention required treated employees to attend the office one day per month. The incremental cost per visit was $9.63 per employee, covering transport, lunch, refreshments, and use of existing office space. Over nine visits and 124 treated customer service agents, total programme expenditure was $10,749. The programme ends after Month 9, so we incur no additional programme costs thereafter.

Turnover savings. During the nine-month intervention window, the intervention reduced attrition by 7 percentage points relative to the control group. Applied to the 124 treated employees, this implies 8.7 fewer separations. We extend the calculation to Months 10–14 using observed headcount changes by arm over the post window, which implies a further two avoided separations. We value each avoided separation at $714, an estimate provided by the firm that covers recruitment, initial training, and the productivity ramp-up of new hires. This yields turnover savings of $7,626.

Productivity gains. By the end of the intervention period, the estimated treatment effect is about 4.0% in Months 6–9. For the cost–benefit exercise, however, we adopt a more conservative assumption to avoid overstating benefits given the gradual ramp-up in effects. Specifically, we assume that productivity gains increase linearly from zero at the start of the intervention to 3.4% by the end of the nine-month treatment window, implying an average gain of 1.7% over the intervention period. We value this gain using an employer labour cost of 130 TRY per hour (approximately $4) and assume each treated employee worked 1,560 hours over the nine-month period. Under these assumptions, the implied productivity benefit is $12,786. We then extend the horizon to Months 10–14 and apply the estimated post-period treatment effect of 5.8%, which yields an additional $21,521. Total productivity gains therefore equal $34,307.

Net benefit and benefit–cost ratio. Total monetised benefits equal \$41,933 (= \$7,626 turnover savings + \$34,307 productivity gains). Netting out programme costs of \$10,749 yields a net benefit of \$31,184. The implied benefit–cost ratio is 3.90, meaning that each dollar of programme spending generated about \$3.9 in measurable benefits under conservative assumptions.[6]

Overall, the monthly office-day policy appears cost-effective. About one-fifth of the monetised gains come from lower turnover, with the remaining gains driven by higher output. Our turnover calculation is conservative because it excludes several relevant cost components (e.g., managerial time, vacancy costs, and other operational frictions), and we do not monetise potential benefits to service quality or team functioning. As a result, the programme's true return is probably higher. In summary, occasional in-person contact can generate meaningful value in fully remote settings, especially when firms can leverage existing office infrastructure.

7 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, but the effects build gradually. Calls per hour change little in the first months, increase meaningfully only after repeated exposure, and remain elevated in the months after the final visit. The increase in output is driven by shorter calls (primarily reduced 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, evidence on mechanisms points to interaction and learning. After adjusting for multiple hypothesis testing, treated employees are more likely to support the policy and report more frequent managerial feedback. Administrative and survey evidence on communication reinforces this interpretation: randomized seating assignments create new peer links that persist beyond the office day. At the same time, we do not find evidence that proximity to higher-performing assigned neighbours generates measurable productivity spillovers. We interpret this null result as narrowing, rather than undermining, the broader mechanism: the gains from monthly office days appear to operate through richer interaction, feedback, and coordination rather than through direct learning from higher-performing desk neighbours alone.

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. The retention gains are also economically meaningful: the policy reduces the selective loss of higher-performing employees, a margin that is particularly 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

[6] Using the same approach but restricting the horizon to the nine-month intervention window yields a benefit–cost ratio of 1.77.

turnover, yielding a conservative benefit–cost ratio of 3.9.

More broadly, our results emphasize that the performance of remote work depends not only on where work is done, but also on organizational design. In fully remote service settings, well-coordinated in-person contact can deliver durable gains without compromising service quality or sacrificing the flexibility that makes remote work attractive.

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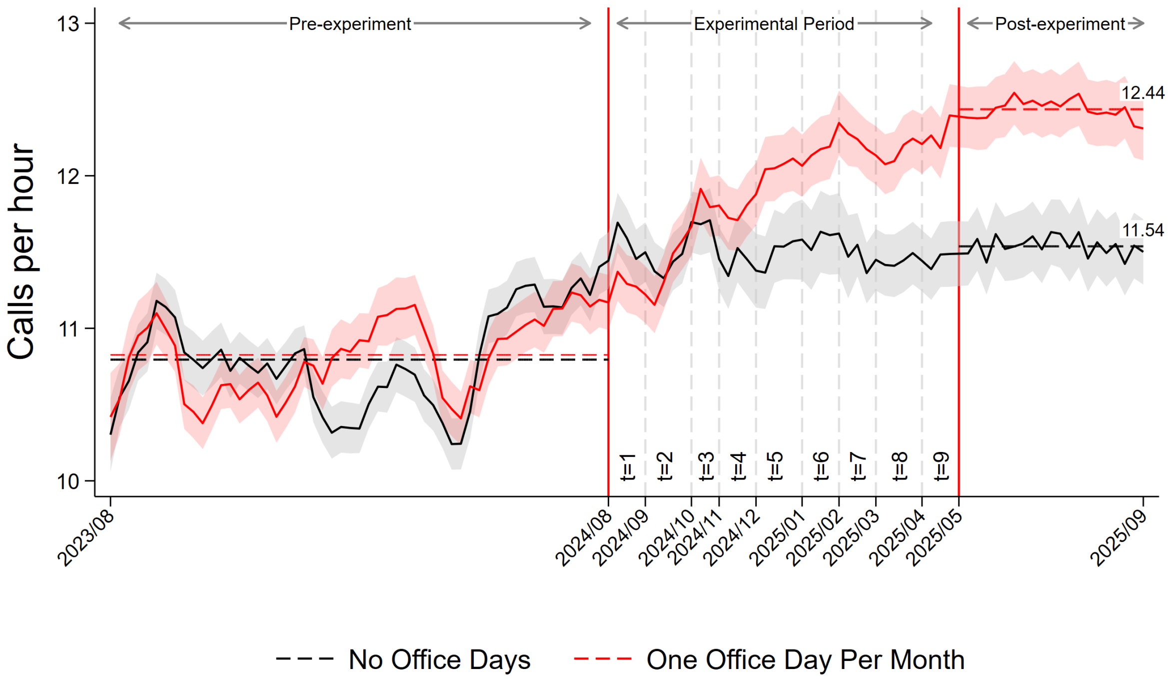
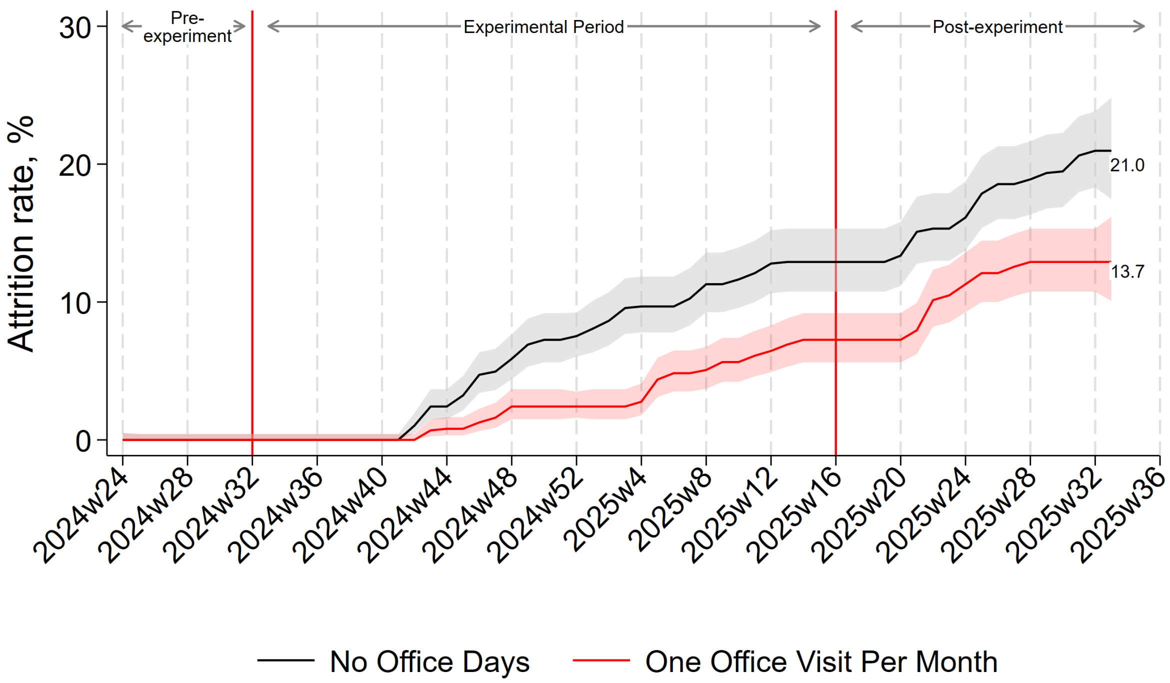
Figures and Tables

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



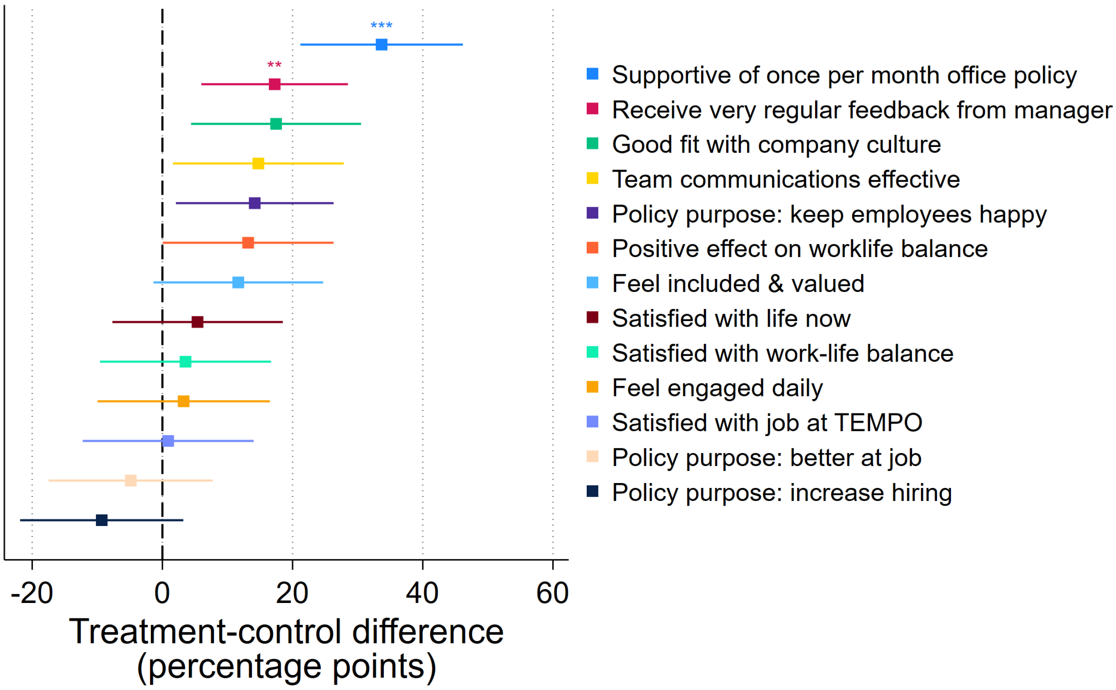
Notes: This figure shows weekly means of *Calls per hour* for control (No Office Days) and treatment (One Office Day Per Month) groups. Weekly means are calculated using employee-day level data. Horizontal dashed lines represent means in the *pre-experiment* and *post-experiment* periods among the *No Office Days* (black dashed line) and *One Office Day per Month* (red dashed line) groups. Means of *Number of calls, per hour* in pre-treatment period for control and treatment groups are 10.8 and 10.8, respectively, while the means in post-experimental period for control and treatment groups are 11.5 and 12.4, respectively. The vertical red lines at August 2024 and May 2025 indicates the start and end of the treatment period, respectively. Shaded red and grey areas depict 95% confidence intervals around the mean.

Figure 2: 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 weekly attrition rates calculated using employee-day level data. The vertical red lines mark the weeks of the first and last office visits for the treatment group. Shaded areas represent 95 percent exact binomial confidence intervals for proportions.

Figure 3: At endline treated employees were more supportive of the policy and reported more frequent manager feedback, stronger cultural fit



Notes: Figure showing linear probability regressions, each coefficient corresponds to a separate regression of the outcome (listed in the legend) on a treatment dummy. All outcomes are dummy variables. Coefficients have been rescaled to percentage points. The sample corresponds to the endline sample where N=223. Whiskers depict 95% confidence intervals and standard errors are heteroskedasticity robust. ***, **, 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: 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

Notes: This table reports baseline summary statistics for worker characteristics (Panel A), productivity measures (Panel B), and service quality measures (Panel C). 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 2: Monthly office days reduce attrition among higher-performing employees

	Mean calls per hour		Difference (Stayer − Quitter)	
	(1)	(2)	(3)	(4)
	Stayer	Quitter	Diff.	p-value
Fully remote (control)	11.477	11.744	-0.267	0.000
	(0.016)	(0.046)		
One office day per month (treatment)	12.118	10.955	1.163	0.000
	(0.015)	(0.040)		
Difference-in-differences (DiD)			1.430	0.000
			(0.069)	

Notes: The 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 (standard errors in parentheses) among employees with observed post-period productivity. Column (3) reports the within-row difference, defined as mean(Stayer) − mean(Quitter), with the corresponding two-sample t-test p-value reported in Column (4). The DiD estimate is obtained from an OLS regression of *calls per hour* on indicators for treatment status and being a stayer, and their interaction; the reported DiD 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 Day Per Month	36.28***							
	(0.99)							
Seating plan neighbour		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 a dummy equal to 1 if an employee pair communicated in the week after the office visit and 0 if they did not communicate. *Seating plan neighbour* is a dummy variable equal to 1 if an employee pair were (randomly allocated via a seating plan) to be neighbours in the office in the during the first three visits and 0 otherwise. *Morning interaction* is a dummy equal to 1 if an employee pair interacted during a morning coffee break together. *Lunch interaction* is a dummy equal to 1 if an employee pair interacted during a lunch break together. *Afternoon interaction* is a dummy equal to 1 if an employee pair interacted during an afternoon coffee break together. Follow-up communication, morning, lunch and afternoon interactions are self-reported in surveys at the end of each week following an office visit. The data for column (1) is from the endline survey while the data from columns (2) - (8) includes all possible employee pairs and focuses on the first three office visits and subsequent weeks where the seating plan was randomised and enforced. Standard errors are heteroscedasticity robust in column (1) and clustered at the employee level in columns (2) - (5). ***, **, and * denote significance at the 1, 5, and 10 percent levels.

Appendix

Figure A.1: During the intervention

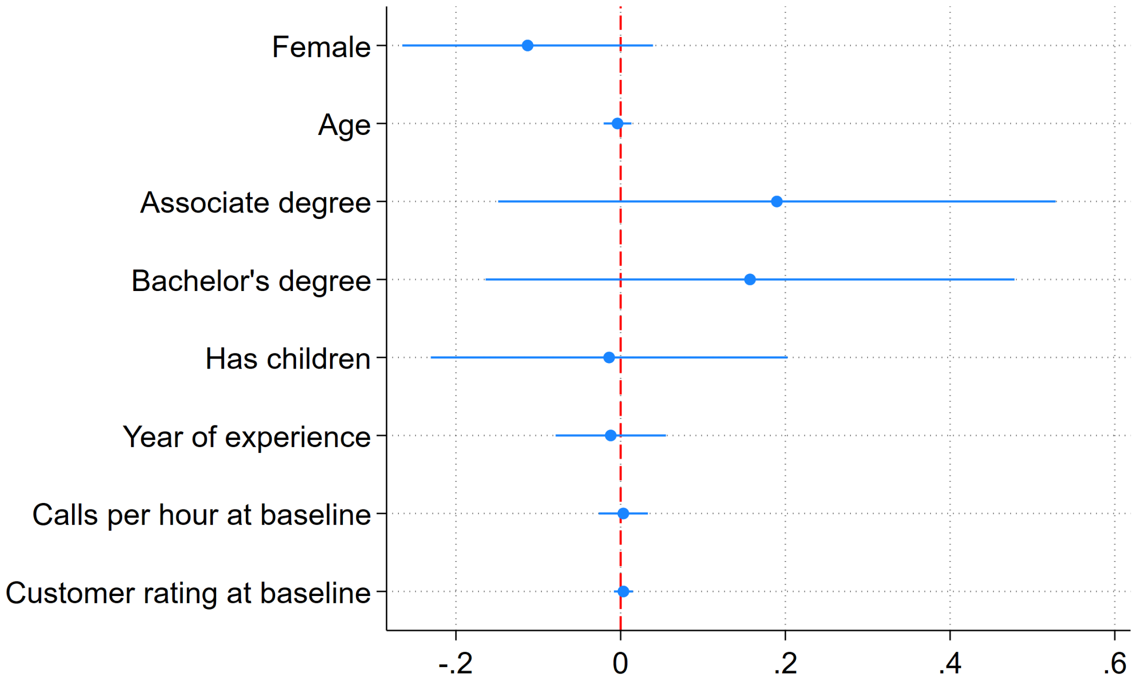
(a) Working at home environment

(b) Transport to and from the office

(c) Office-day activities

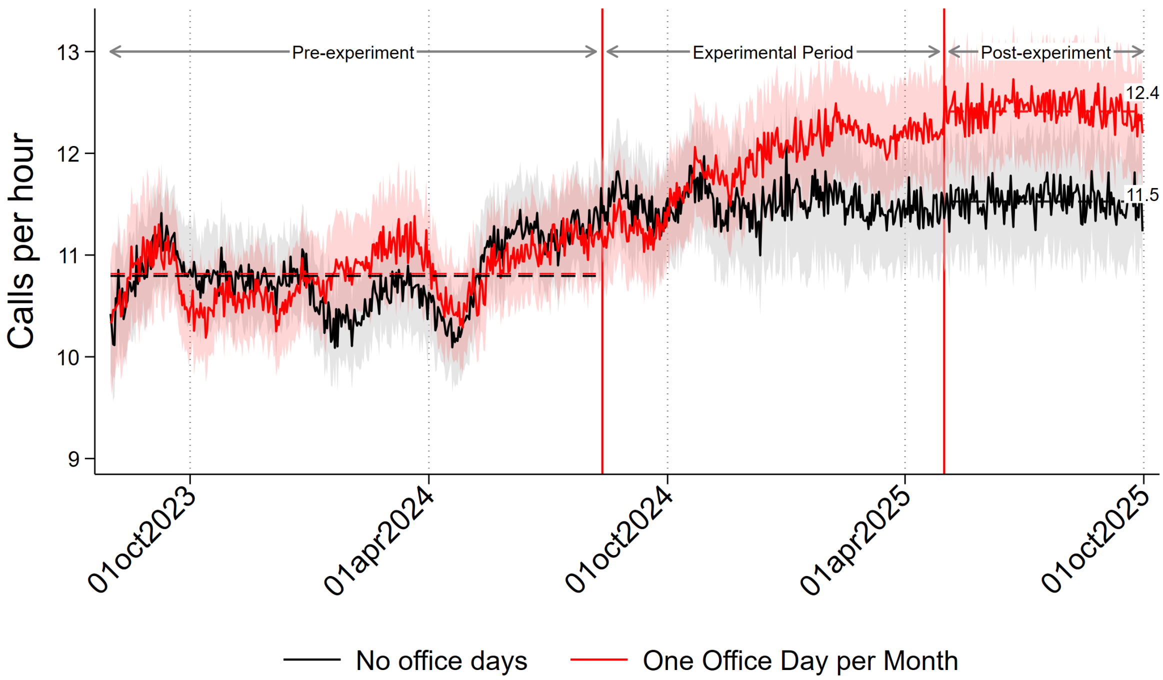
Notes: Illustrative photos from the intervention period showing: employees working from home (Panel (a)); transport arranged by the company and group activities held during monthly office days (Panel (b)); and the working environment on office days and team social activities (Panel (b)).

Figure A.2: Balance test for treatment assignment



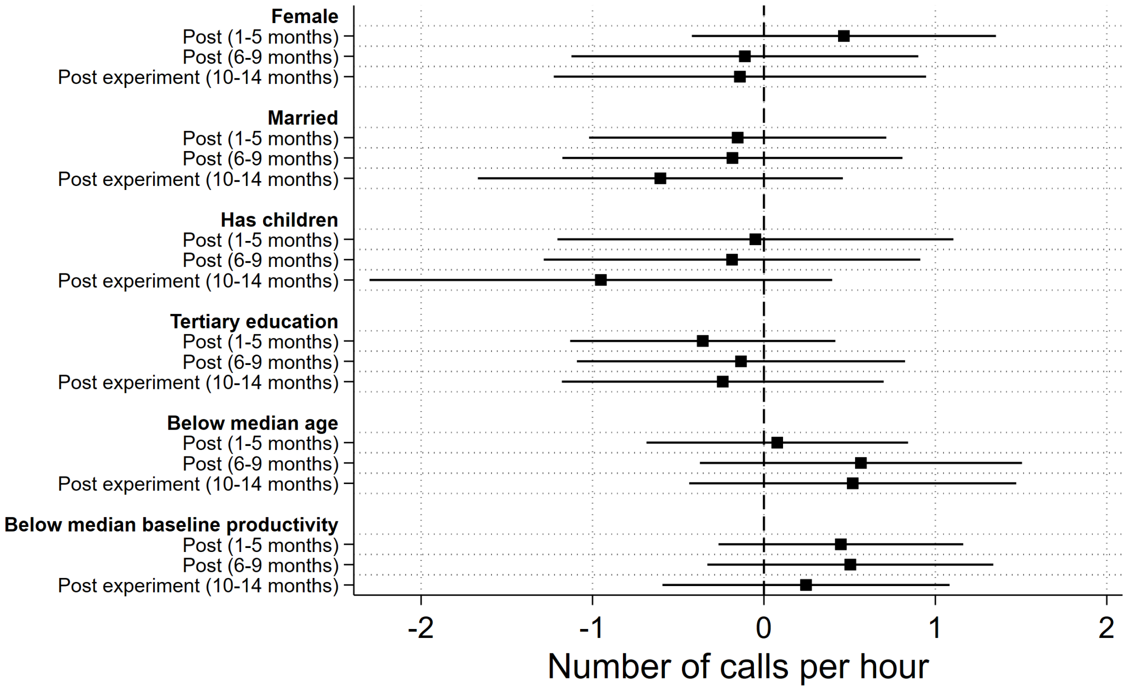
Notes: Figure shows coefficient estimates from a linear probability model. The dependent variable equals 1 if an employee is treated. The data include one observation per employee ($N = 248$). Standard errors are robust to heteroskedasticity, and whiskers show 95 percent confidence intervals.

Figure A.3: Calls per hour before, during and after the RCT — Daily estimates



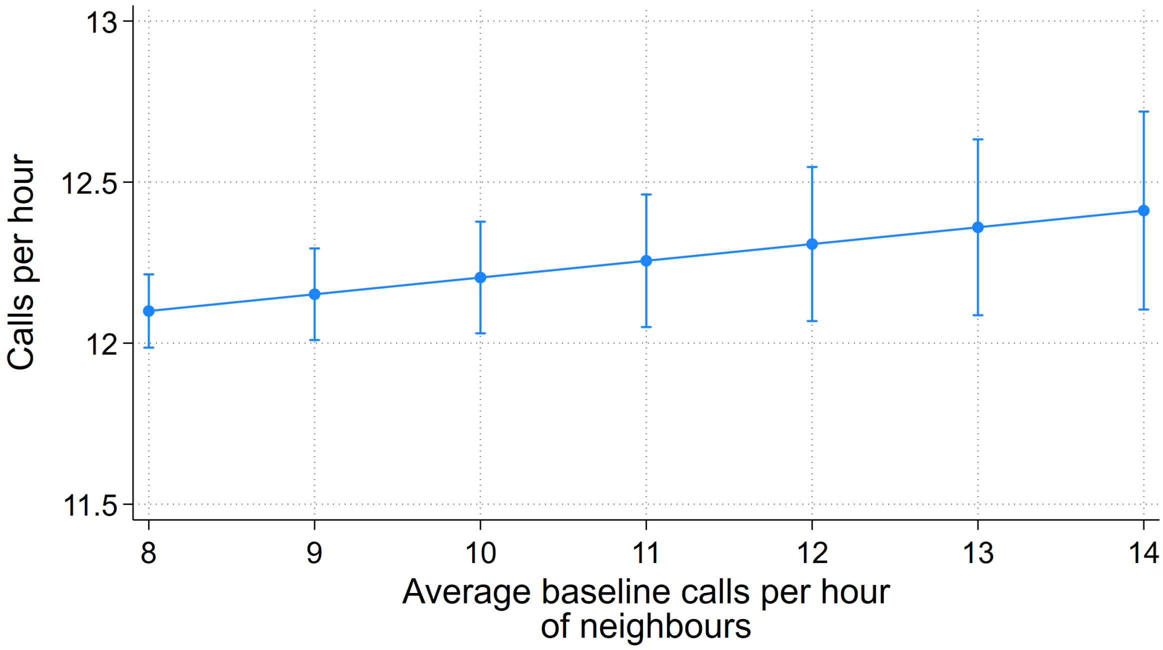
Notes: This figure shows predictions from a regression of calls per hour on treatment dummies, daily time dummies and a full set of interactions between the two. Standard errors are clustered at the employee level and whisker represent 95% confidence intervals.

Figure A.4: No statistically significant evidence of heterogeneity in productivity effects



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

Figure A.5: No correlation between improvements in own productivity and neighbours productivity



Notes: This figure shows linear predictions from a regression of calls per hour on period dummies for the *Post (1-5 months)*, *Post (6-9 months)* and *Post experiment (10-14 months)* phases, and their interactions with *Average baseline calls per hour of neighbours* and employee fixed effects. The chart plots predictions from the interaction between *Post experiment (10-14 months)* and *Average baseline calls per hour of neighbours*. Standard errors are clustered at the employee level and whisker represent 95% confidence intervals.

Table A.1: Effects on service quality outcomes

	Customer rating		Random audit rating	
	(1)	(2)	(3)	(4)
One Office Day Per Month	0.69		-0.01	
	(0.60)		(0.01)	
Post (1-5 months)	-0.67	-0.61	0.01	0.01
	(0.63)	(0.64)	(0.02)	(0.02)
Post (6-9 months)	0.55	0.76	-0.00	-0.00
	(0.98)	(0.99)	(0.02)	(0.02)
One Office Day Per Month × Post (1-5 months)	0.88	0.93	-0.03	-0.03
	(1.02)	(1.01)	(0.02)	(0.02)
One Office Day Per Month × Post (6-9 months)	-0.84	-0.76	0.01	0.01
	(1.39)	(1.40)	(0.03)	(0.03)
Adj. R-squared	-0.00	0.02	-0.00	0.02
Number of observations	4,919	4,919	4,919	4,919
Pre-treatment mean	44.24	44.24	0.17	0.17
Agent FE		✓		✓

Notes: Dependent variables appear 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.

Table A.2: Early, late and post experiment treatment effects

	Calls per hour		Average call duration (seconds per call)		Talk time (seconds per call)		Admin time (seconds per call)		Hold time (seconds per call)		Break time (minutes per day)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
One Office Day Per Month	0.03		-0.27		-1.19		0.04		0.84**		1.85	
	(0.27)		(8.83)		(8.82)		(0.06)		(0.39)		(1.40)	
Post (1-5 months)	0.71***	0.78***	-19.13***	-21.37***	-19.42***	-21.56***	0.08*	0.07*	0.13	0.06	8.84***	8.81***
	(0.13)	(0.12)	(3.82)	(3.60)	(3.80)	(3.58)	(0.04)	(0.04)	(0.15)	(0.13)	(0.76)	(0.75)
Post (6-9 months)	0.69***	0.85***	-18.62***	-23.77***	-19.04***	-23.98***	0.02	0.02	0.35	0.18	10.33***	10.87***
	(0.17)	(0.14)	(4.77)	(4.14)	(4.77)	(4.14)	(0.05)	(0.05)	(0.22)	(0.18)	(0.92)	(0.84)
Post experiment (10-14 months)	0.74***	0.85***	-20.66***	-24.10***	-21.12***	-24.40***	-0.03	-0.01	0.45**	0.28	11.58***	12.38***
	(0.17)	(0.14)	(4.85)	(4.12)	(4.84)	(4.11)	(0.06)	(0.05)	(0.21)	(0.19)	(1.02)	(0.91)
One Office Day Per Month × Post (1-5 months)	0.07	-0.02	-3.35	-0.04	-3.01	0.20	0.05	0.06	-0.29	-0.21	0.94	0.96
	(0.19)	(0.18)	(5.51)	(5.07)	(5.51)	(5.08)	(0.06)	(0.06)	(0.30)	(0.29)	(1.17)	(1.15)
One Office Day Per Month × Post (6-9 months)	0.67***	0.43**	-21.34***	-13.21**	-20.70***	-12.80**	-0.09	-0.09	-0.43	-0.26	0.83	0.36
	(0.23)	(0.20)	(6.51)	(5.64)	(6.53)	(5.68)	(0.06)	(0.06)	(0.36)	(0.33)	(1.34)	(1.28)
One Office Day Per Month × Post experiment (10-14 months)	0.87***	0.63***	-25.10***	-17.60***	-24.28***	-16.84***	-0.10	-0.10	-0.50	-0.47	2.07	1.16
	(0.23)	(0.20)	(6.51)	(5.53)	(6.52)	(5.54)	(0.07)	(0.07)	(0.37)	(0.36)	(1.51)	(1.42)
Adj. R-squared	0.05	0.81	0.04	0.81	0.04	0.82	0.01	0.45	0.01	0.71	0.13	0.46
Number of observations	144,769	144,769	144,769	144,769	144,769	144,769	144,769	144,769	144,769	144,769	144,769	144,769
Outcome pre-treatment mean	10.81	10.81	349.21	349.21	344.46	344.46	2.91	2.91	1.63	1.63	34.25	34.25
Agent FE		Yes		Yes		Yes		Yes		Yes		Yes

Notes: Table showing estimates from linear regressions. Dependent variables are shown in the column headings. *One Office Day Per Month* equals 1 for employees assigned to monthly office visits 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.