

Immediate Gratification and Delay Discounting: Reshaping Fragmented Physical Activity Habits in Sedentary Populations Using Micro-Instant Rewards

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Abstract

INTRODUCTION: Prolonged, uninterrupted occupational sitting is a major risk factor for adverse cardiometabolic outcomes. Although many office workers recognize these risks, an intention–behavior gap remains. Delay discounting—the tendency to devalue delayed outcomes relative to immediate costs or benefits—may contribute to this gap, but it is not the only plausible behavioral mechanism. Conventional workplace incentives often defer reinforcement and may offer low perceived attainability.

OBJECTIVES: We compared two mHealth reward packages for increasing hourly exercise snacks among sedentary office workers.

METHODS: In a randomized AB/BA crossover trial, 120 information-technology professionals completed two 8-week conditions separated by a 4-week washout; 115 were included in the per-protocol linear mixed-effects model. A smart seat cushion triggered a fixed prompt after 60 minutes of uninterrupted sitting. Condition A used delayed deterministic points with a high redemption threshold; Condition B delivered immediate randomized micro-rewards.

RESULTS: At Week 8, mean break frequency was 4.89 versus 3.60 breaks/day (difference 1.29, 95% CI 0.82–1.75; $p < 0.001$). The adjusted condition coefficient was 0.89 (95% CI 0.75–1.03; $p < 0.001$). Sedentary time was 0.30 h/day lower ($p = 0.005$), whereas the MVPA difference did not survive FDR correction (adjusted $p = 0.049$). The condition $\times$ log(k) interaction was -0.28 (95% CI -0.52 to -0.04 ; $p = 0.022$). The condition $\times$ period interaction (differential carryover) was not statistically significant ($\beta = 0.56$, 95% CI -0.09 to 1.21 ; $p = 0.092$), and the period-1-only contrast remained significant ($\beta = 0.61$, 95% CI 0.25 – 0.97 ; $p = 0.001$), arguing against material carryover.

CONCLUSION: Immediate randomized micro-rewards were associated with more sedentary interruptions in the bundled comparison. Because the two conditions differed in several design features, the findings should be interpreted as comparative evidence for the two reward packages and as preliminary evidence regarding short-term behavior change.

Keywords: Sedentary behavior, delay discounting, behavioral economics, digital nudge, behavioral design, creative economy, micro-incentives, mHealth, randomized crossover trial

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1. Introduction

Prolonged occupational sitting is a prominent feature of desk-based work and an important public-health concern. The

World Health Organization recommends that adults accumulate 150–300 minutes of moderate-intensity aerobic activity weekly and limit prolonged sedentary bouts [1]. In 2022, 31.3% of adults globally were insufficiently active,

compared with 23.4% in 2000 [2]. Office workers commonly spend a large proportion of their working hours seated [3], and high sedentary time is associated with all-cause mortality, cardiovascular disease, type 2 diabetes, and metabolic risk independently of leisure-time physical activity [4][5][6][7][8]. Remote and hybrid work may further reduce incidental movement [9].

The resulting intention–behavior gap is not adequately explained by lack of knowledge. Many adults intend to be more active but fail to enact this intention when a break disrupts work, requires effort, or conflicts with a perceived workplace norm [10][11]. Delay discounting provides one relevant explanation: immediate effort and interruption can be weighted more strongly than delayed and probabilistic health benefits [12][13]. Higher delay discounting has been associated with adverse health behaviors and lower physical activity in observational and intervention research [14–18].

We also position the study within behavioral design and a bounded choice-architecture perspective. A nudge is commonly framed as a change to the choice environment that predictably alters behavior while preserving choice [19]. In workplace physical-activity research, prompts and immediate feedback can alter the informational microenvironment, whereas financial rewards are often treated as a distinct incentive component rather than a pure nudge [20]. Our intervention therefore combines a fixed contextual prompt with reward features; it does not implement default options, loss framing, or a personalized choice architecture.

Exercise snacks—brief, repeatable periods of standing, walking, or light activity—provide a feasible target for interrupting sitting. Financial-incentive and gamification studies, including research on digital reward engagement and its ethical risks, indicate that reward structures can support short-term physical activity engagement [21–27]. Research on exercise snacks and interrupted sitting supports the plausibility of frequent short activity bouts for acute metabolic outcomes [28–31]. Prospect theory and its cumulative representation provide complementary accounts of valuation under uncertainty [32][33], and mHealth research has tested related reward and deposit-contract structures [34]. Workplace evidence also links sedentary interruptions with reduced fatigue and musculoskeletal discomfort [35]. Sensor-enabled just-in-time adaptive intervention frameworks can tailor support to changing context [36][37], and office-based mHealth intervention research identifies this application area as rapidly developing [38]. The design also needs to preserve autonomy because external rewards may not sustain behavior after reward withdrawal [39–41]. Related social-design research suggests that behavioral data can be used to tailor prompts and feedback, while retaining a balance between individual and social interests [42].

We conducted a randomized AB/BA crossover comparison of a delayed deterministic point system and an immediate randomized micro-reward package for hourly exercise snacks in sedentary office workers. We hypothesized that the immediate randomized package would be associated with a higher daily sedentary-break frequency. We interpret this contrast as a comparison between two bundled

intervention packages, as the conditions differed in reward timing, certainty, interface presentation, and practical attainability. This study focuses on office-based physical-activity behavior among sedentary workers. The study focuses on behavioral effects of the reward packages rather than on AI-driven personalization or economic evaluation.

2. Literature Review

2.1. Sedentary behavior and fragmented activity

Sedentary behavior is conceptually distinct from insufficient moderate-to-vigorous physical activity (MVPA), and the pattern of sitting matters in addition to total duration [1][4][5]. Brief interruptions can reduce acute postprandial glucose and insulin responses, with evidence from systematic reviews and controlled studies supporting light walking and simple resistance activities as feasible interruption strategies [30][31]. Recent work has extended this rationale to exercise snacks distributed across the working day [28][29]. These physiological findings provide a rationale for behavior-activation research, although further intervention studies are needed to determine their long-term health implications.

2.2. Delay discounting, incentives, and ethical behavioral design

Delay discounting describes the devaluation of a delayed outcome relative to an immediate outcome. Economic models of present bias explain why an individual can value activity prospectively yet defer it repeatedly when immediate work demands arise [12][13]. Immediate contingent consequences may reduce this temporal mismatch; however, evidence on incentive systems also indicates that effect persistence, the appropriate reward structure, and autonomy-supportive implementation remain uncertain [18][21][25][26].

Evidence from consumer and gaming contexts suggests that variable rewards may enhance engagement, but their role in health interventions requires direct empirical examination. Uncertain rewards have been associated with arousal and urges to continue engagement in loot-box studies [23], and loot-box expenditure has been associated with problem-gambling indicators [24]. Accordingly, any health implementation using randomized rewards requires transparent probabilities and expected value, bounded exposure, a penalty-free opt-out, and prospective monitoring for excessive checking, reward preoccupation, distress, or adverse events. Evidence from health-food consumption research also illustrates how attention cues, expectations, and perceived value shape behavioral choices, a useful parallel for interpreting reward framing [43].

2.3. mHealth delivery, nudge boundaries, and study contribution

mHealth systems can detect opportunities for a break and provide prompts in near real time. JITAI theory describes personalization and adaptation as optional design features that depend on decision rules and changing individual state [36][37]. The present system used a fixed 60-minute sitting threshold and is therefore better characterized as a fixed-rule intervention rather than a personalized JITAI. Accordingly, the intervention can be characterized as a sensor-triggered, fixed-rule mHealth system with behavioral-design features.

The literature supports examining whether a reward package can activate exercise-snack behavior during use, but it also identifies two safeguards for interpretation. First, choice-architecture effects often attenuate after an intervention is removed [20]. Second, financial incentives, delivery timing, interface experience, and perceived attainability can each affect engagement. The present findings provide short-term comparative evidence for two intervention packages and suggest that reward design may influence the frequency of exercise-snack behavior.

3. Methods

3.1. Design, ethics, registration, and participants

We used a randomized two-period AB/BA crossover design, reported with reference to the CONSORT extension for

randomized crossover trials [44]. Each participant received both intervention conditions in randomized order. The crossover design reduces confounding by stable individual characteristics but remains vulnerable to period and carryover effects [44][45].

The study protocol was reviewed by an institutional ethics committee before recruitment (The ethics review approval number is WTBU-LL-2026-096). The review explicitly considered the sensor-based monitoring, randomized cash and digital-asset-fragment rewards, reward-probability disclosure, monetary-exposure cap, voluntary participation, penalty-free withdrawal, and data privacy. All participants provided written informed consent.

We recruited 120 full-time information-technology professionals from three mid-sized technology enterprises in a metropolitan area. Participants were aged 22–55 years, worked at least 35 hours per week in desk-based employment, and accumulated more than 6.0 hours/day of occupational sedentary time during a one-week objective screening assessment. We excluded people with conditions that made light activity unsafe, current pregnancy, participation in another workplace health program, or use of a commercial gamified activity tracker. We randomized eligible participants 1:1 with computer-generated blocks of four to sequence AB (LongTerm then MicroReward; $n=60$) or BA (MicroReward then LongTerm; $n=60$). The visible interfaces precluded participant blinding; sensor-derived outcomes reduce recording bias but do not remove novelty, performance, demand-characteristic, or Hawthorne effects. (see Figure 1)

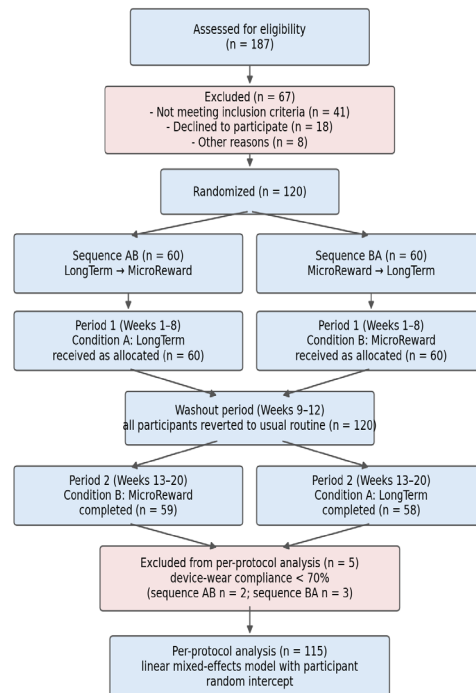


Figure 1. CONSORT flow diagram of participant enrolment, allocation, crossover, and per-protocol analysis

3.2. Schedule and washout

Figure 2 shows the study schedule. Baseline assessment occurred at Week 0. Intervention Period 1 ran from Weeks 1–8, followed by a four-week washout (Weeks 9–12), Intervention Period 2 (Weeks 13–20), and a final assessment

at Week 21. During washout, we deactivated intervention prompts and rewards and instructed participants to follow their usual occupational routine. This design permits comparison during intervention periods but includes no post-withdrawal follow-up to evaluate durability of behavior after rewards ended.

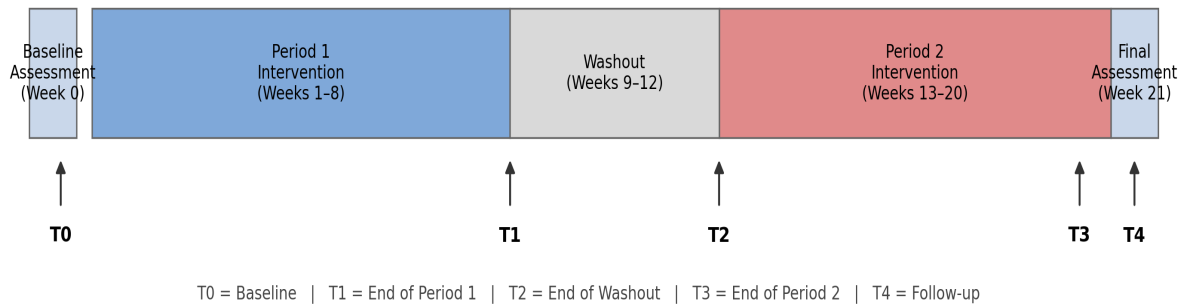


Figure 2. Randomized crossover trial schedule: baseline at Week 0; Period 1 (Weeks 1–8); washout (Weeks 9–12); Period 2 (Weeks 13–20); and final assessment at Week 21

Baseline characteristics were balanced between sequence groups. In the per-protocol cohort, mean age was 31.9 years (SD 5.1) in sequence AB and 31.6 years (SD 4.8) in sequence

BA; no baseline sequence comparison was statistically significant (all $p > 0.05$; Figure 3).

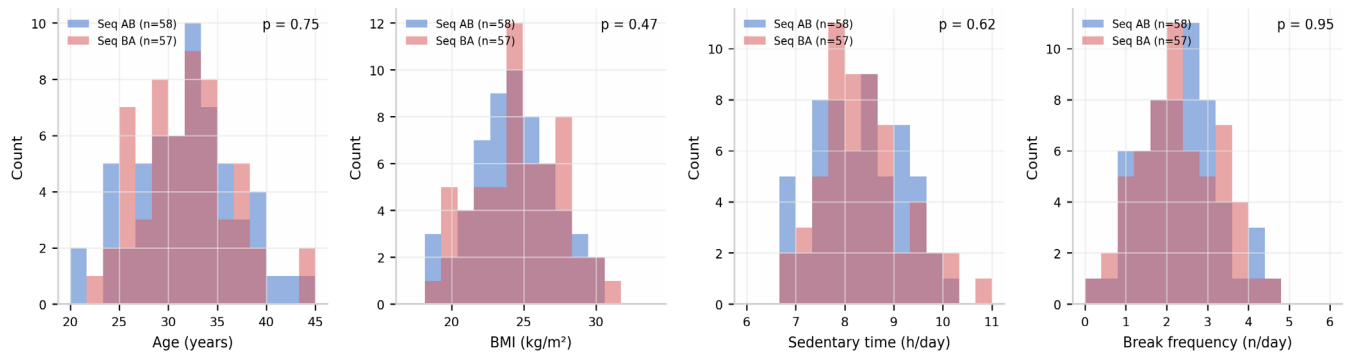


Figure 3. Baseline demographic and behavioral characteristics by randomized sequence. No baseline comparison was statistically significant (all $p > 0.05$)

3.3. Hardware and fixed-rule intervention delivery

The intervention combined a proprietary smart seat cushion with a custom smartphone application. The cushion contained piezoresistive pressure sensors, sampled weight-distribution and seating status at 10 Hz, and sent posture-state data to the app through Bluetooth Low Energy. The smart cushion used an embedded proprietary classifier to distinguish seated from

nonseated states; however, detailed technical specifications were not available in the archived materials. In this study, the cushion functioned primarily as a sensor-based measurement and prompting device within the intervention workflow.

For both conditions, an exercise snack was a minimum of three continuous minutes of standing or light ambulatory activity (e.g., walking or stretching) after at least 60 minutes of uninterrupted sitting. When the fixed 60-minute threshold was met, the app issued the same standardized notification. The intervention used a fixed, deterministic trigger based on

60 minutes of uninterrupted sitting. Thigh-worn activPAL4 micro accelerometers were used to quantify daily MVPA; validity and reliability evidence for the activity-monitoring approach has been reported previously [46].

3.4. Intervention Conditions

Condition A (LongTerm) was a delayed deterministic point system. Each completed exercise snack earned 10 health points toward a digital gift card valued at up to US\$50 at the end of the eight-week period. The theoretical US\$50 maximum required 10,000 points (approximately 25 breaks/day), whereas the realistically achievable maximum was approximately 3,200 points. The visible interface was a progress bar. Because the redemption threshold in Condition A was relatively high, participants may have perceived this condition as less attainable, which could have influenced engagement.

Condition B (MicroReward) delivered an immediate randomized consequence after a detected completed exercise snack through an animated interface. Outcomes were US\$0.10–US\$5.00 micro-cash transfers, digital asset fragments that could be assembled for redemption, or a null outcome paired with an encouraging message. The nominal maximum value was calibrated to the US\$50 maximum in Condition A. Although the nominal maximum value was matched across conditions, the two reward packages still differed in attainability, timing, certainty, and interface experience. Accordingly, the comparison reflects the combined effects of the two reward packages rather than a single isolated design element.

3.5. Outcome Measures

The primary outcome was daily sedentary-break frequency: the number of workday interruptions of continuous sitting lasting at least three minutes, captured by the smart cushion. Secondary objective outcomes were occupational sedentary time (hours/day; cushion) and MVPA (minutes/day; accelerometer).

The app also calculated a weekly 0–100 adherence score as the proportion of app-prompted exercise snacks that were completed. This score was used as a system-derived implementation metric to reflect participants' completion of prompted exercise snacks. At the end of each period, participants completed the System Usability Scale (SUS) [47]. Baseline delay discounting was measured with the Monetary Choice Questionnaire; we calculated k using $V=A/(1+kD)$ and analyzed $\log(k)$ to improve distributional behavior [48].

3.6. Statistical Analysis

We analyzed the primary outcome in a per-protocol linear mixed-effects model (LMM; $n=115$) with restricted maximum likelihood estimation and a participant random intercept (intraclass correlation coefficient=0.18). Fixed effects were condition (MicroReward versus LongTerm), period (2 versus 1), randomized sequence (BA versus AB), intervention week, baseline break frequency, and condition $\times$ baseline $\log(k)$. Models were fitted in Python 3.11 using statsmodels 0.14 (MixedLM, REML); residual normality and homoscedasticity were inspected graphically and were acceptable. In a two-period AB/BA crossover, the condition $\times$ period interaction (differential carryover) is aliased with the sequence effect, and the two terms cannot be estimated simultaneously [44][45]. We therefore retained sequence in the primary model and fitted an equivalent reparameterization in which sequence was replaced by the condition $\times$ period interaction; this reparameterization has identical fitted values and provides the numerical carryover diagnostic reported in Section 4.5.

We report two-sided raw p values and 95% confidence intervals. For 11 primary, secondary, intraday, moderation, sequence, and washout tests, we used Benjamini–Hochberg false-discovery-rate (FDR) adjustment; the condition $\times$ period interaction was evaluated as a carryover diagnostic and was not included in the FDR family. Because no prospective sample-size calculation was documented in the archived protocol, the sample size should be interpreted with appropriate caution [49]. In the AB/BA crossover framework, potential carryover should be considered when interpreting the period-related findings [44][45].

4. Results

4.1. Participant flow and analytical sample

Of 187 people assessed for eligibility, 120 met eligibility criteria, consented, and were randomized (AB $n=60$; BA $n=60$). Five participants did not meet device-wear compliance of at least 70% and were excluded from the per-protocol analysis. The primary analytical sample therefore comprised 115 participants.

4.2. Primary outcome: sedentary-break frequency

At the Week-8 endpoint, the descriptive mean daily break frequency was 4.89 (SD 2.36) in MicroReward and 3.60 (SD 1.36) in LongTerm. The unadjusted endpoint difference was 1.29 breaks/day (95% CI 0.82–1.75; $p<0.001$; Cohen's $d=0.49$). This endpoint comparison complements the longitudinal mixed-effects analysis. Figures 4 and 5 illustrate the temporal pattern of change across the intervention period.

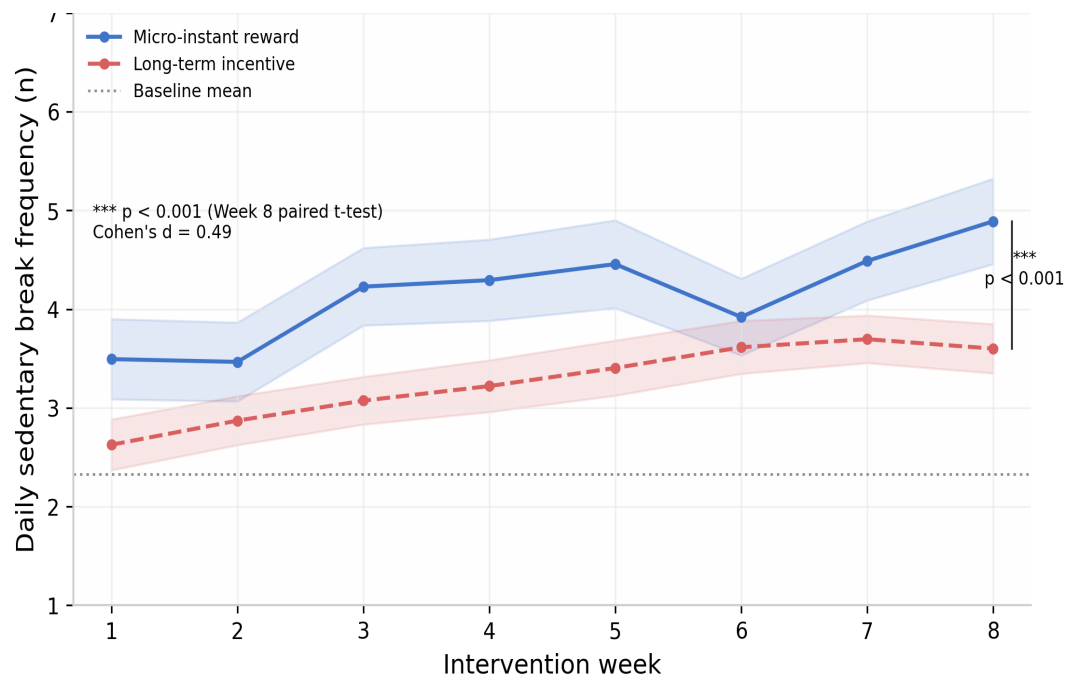


Figure 4. Longitudinal group trajectory of daily sedentary-break frequency during the eight-week intervention periods. Values are descriptive and displayed by intervention condition.

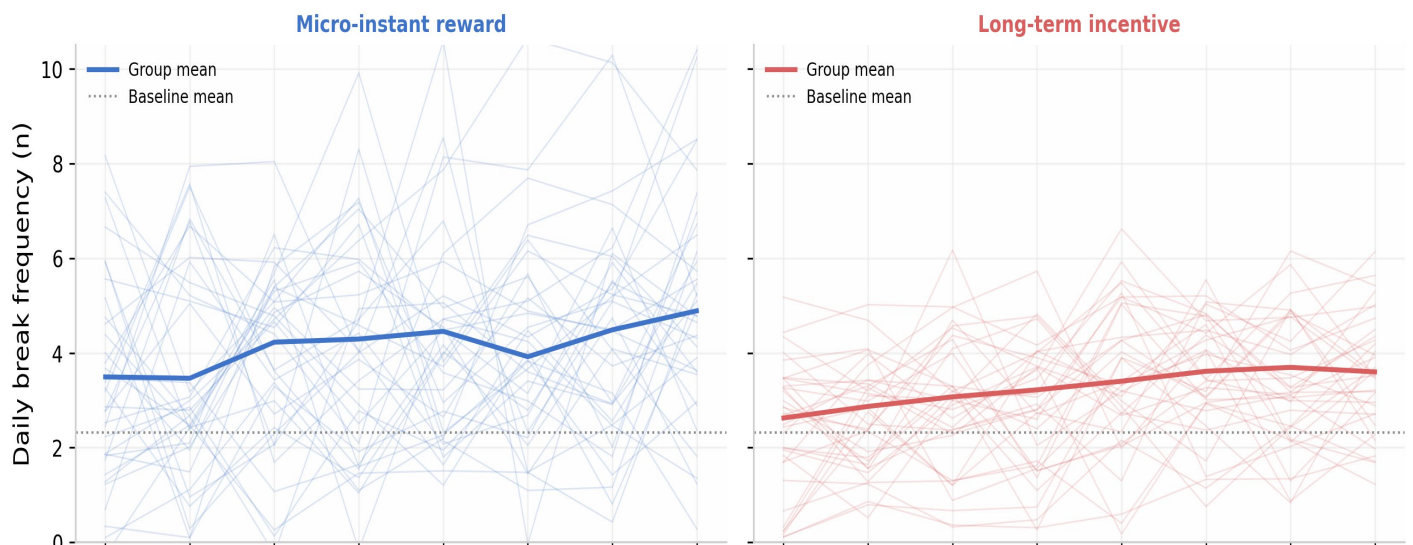
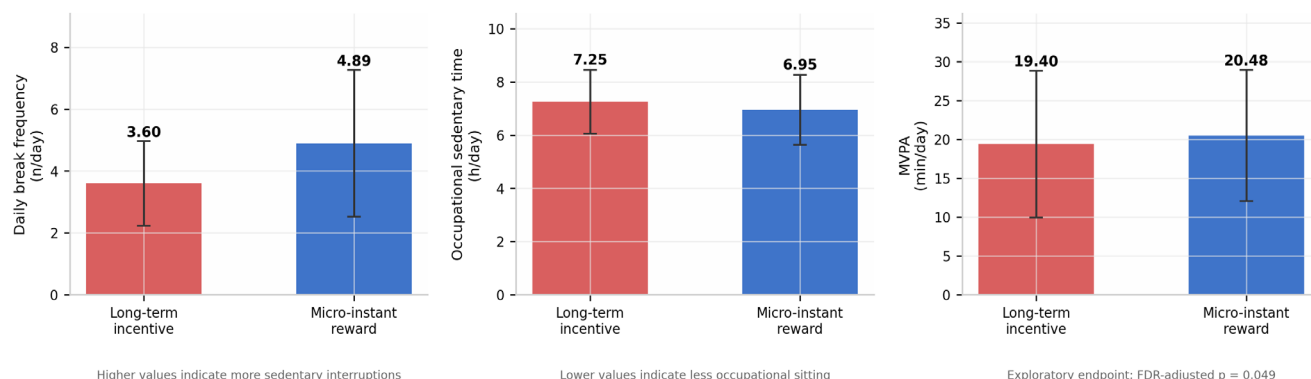


Figure 5. Individual-level longitudinal trajectories of daily sedentary-break frequency, with group-mean overlays. The display includes a random sample of 35 participants per condition and should be interpreted descriptively.

4.3. Secondary objective outcomes

At Week 8, occupational sedentary time was lower in MicroReward (mean 6.95 h/day, SD 1.30) than in LongTerm (mean 7.25 h/day, SD 1.19), for a descriptive difference of -0.30 h/day (95% CI -0.51 to -0.10 ; $p=0.005$). MVPA was

20.48 min/day (SD 8.39) in MicroReward and 19.40 min/day (SD 9.40) in LongTerm, a difference of 1.08 min/day (95% CI 0.05–2.10; raw $p=0.040$). The MVPA difference was modest and should be interpreted as exploratory after FDR adjustment (adjusted $p=0.049$). These endpoint differences suggest behavioral improvement, although their longer-term clinical significance remains to be established.



Descriptive means $\pm$ SD from the per-protocol analytical dataset ($n = 115$ participants); inferential estimates are reported in the text and Table 1.

Figure 6. Week-8 endpoint summaries by intervention condition. Values are descriptive means $\pm$ SD from the per-protocol sample ($n=115$); the MVPA comparison is exploratory (FDR-adjusted $p=0.058$).

4.4. Exploratory intraday pattern

At Week 8, the LongTerm condition declined from 1.78 breaks/half-day (SD 0.71) in the morning (09:00–12:00) to 1.54 (SD 0.71) in the afternoon (13:00–17:00; $p=0.011$). MicroReward increased from 2.39 (SD 0.82) to 2.67 (SD

0.90; $p=0.016$). The descriptive between-condition contrast was larger in the afternoon (1.13 breaks/half-day) than in the morning (0.61 breaks/half-day; Figure 7). Several factors, including workload, meeting schedules, prompt timing, and interface engagement, may have contributed to this intraday pattern.

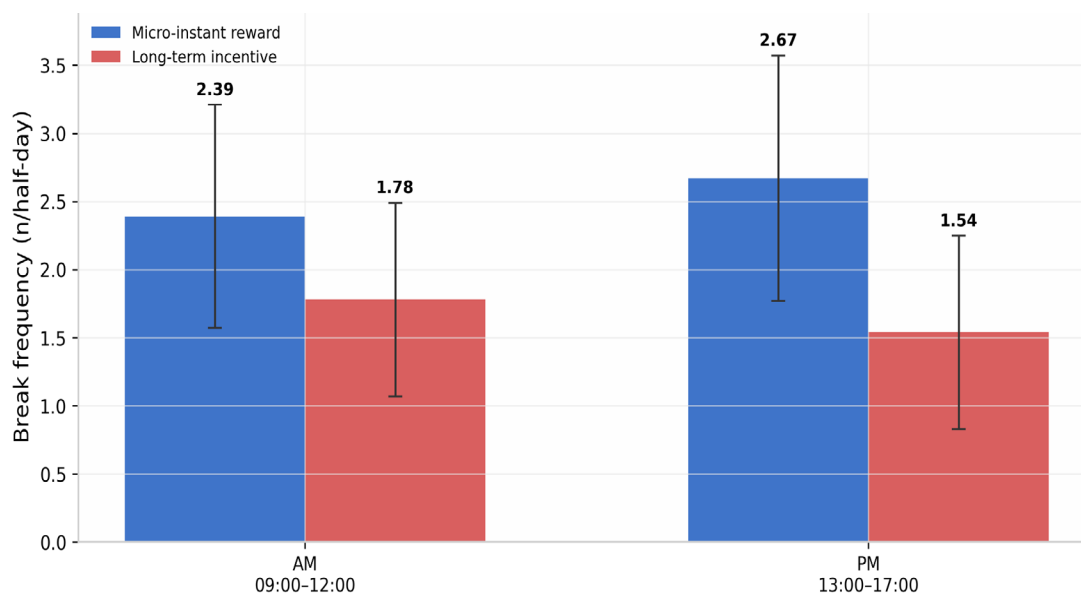


Figure 7. Exploratory intraday sedentary-break frequency at Week 8 by intervention condition. Values are descriptive means $\pm$ SD from the per-protocol sample ($n=115$).

4.5. Mixed-effects estimates and carryover considerations

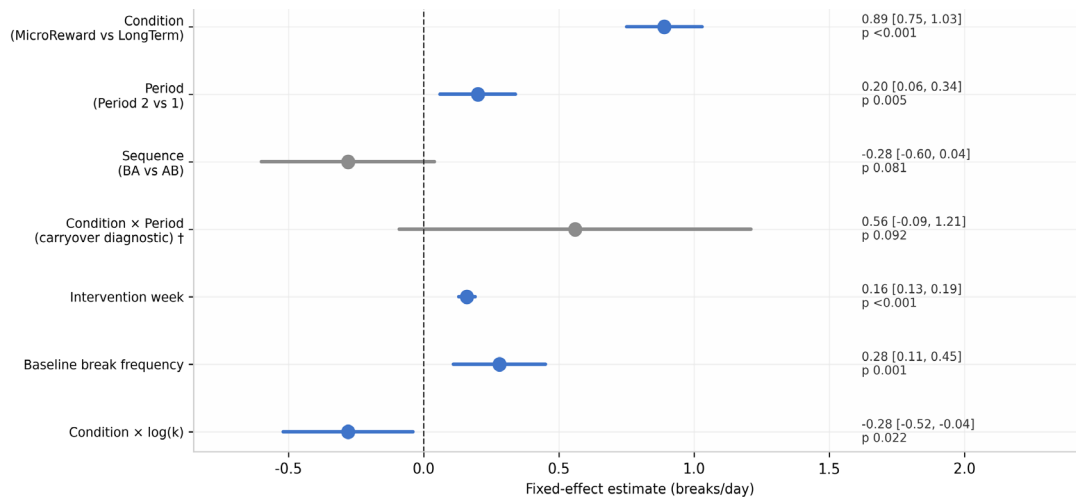
The primary LMM estimated an adjusted condition coefficient of $\beta=0.89$ breaks/day (95% CI 0.75–1.03; $p<0.001$) in favor of MicroReward (Table 1; Figure 8). The period effect was $\beta=0.20$ (95% CI 0.06–0.34; $p=0.005$), and the sequence effect was $\beta=-0.28$ (95% CI -0.60 to 0.04 ; $p=0.081$). The final-week washout mean was 2.45 breaks/day compared with a baseline mean of 2.33 ($p=0.088$), suggesting partial but incomplete return toward baseline. In the equivalent reparameterization, the condition $\times$ period interaction was $\beta=0.56$ breaks/day (95% CI -0.09 to 1.21 ; $p=0.092$), providing no statistical evidence of differential

carryover. Three features support the credibility of the primary condition effect. First, differential carryover of this magnitude would bias the condition contrast by at most half the interaction (approximately 0.28 breaks/day), and even under that bound the adjusted effect would remain positive and material. Second, the period-1-only contrast, which is structurally immune to carryover, remained significant ($\beta=0.61$, 95% CI 0.25–0.97; $p=0.001$). Third, the washout–baseline gap (0.12 breaks/day) is an order of magnitude smaller than the condition effect ($\beta=0.89$), so even complete attribution of that gap to carryover could explain only a small fraction of the primary result. Nevertheless, carryover tests in two-period crossover trials have limited power and the washout contrast is descriptive; residual carryover therefore cannot be fully excluded.

Table 1. Fixed-effect estimates from the primary restricted maximum likelihood linear mixed-effects model for daily sedentary-break frequency (per-protocol $n=115$)

Fixed effect	Estimate β (95% CI)	Two-sided p value
Intercept	2.09 (1.59 to 2.59)	<0.001
Condition: MicroReward vs LongTerm	0.89 (0.75 to 1.03)	<0.001
Period: 2 vs 1	0.20 (0.06 to 0.34)	0.005
Sequence: BA vs AB	-0.28 (-0.60 to 0.04)	0.081
Condition $\times$ period (carryover diagnostic) [†]	0.56 (-0.09 to 1.21)	0.092
Intervention week	0.16 (0.13 to 0.19)	<0.001
Baseline break frequency	0.28 (0.11 to 0.45)	0.001
Condition $\times$ log(k)	-0.28 (-0.52 to -0.04)	0.022

Note: The model included a participant random intercept (ICC=0.18), condition, period, sequence, intervention week, baseline break frequency, and condition $\times$ log(k). CI=confidence interval. No significance symbols are used; p values are unadjusted two-sided values. FDR-adjusted interpretation is reported in the text. [†]In the AB/BA design, the condition $\times$ period interaction is aliased with the sequence effect; this estimate was obtained from an equivalent reparameterization replacing sequence with the interaction term (identical model fit) and is interpreted as a carryover diagnostic outside the FDR family.



Exact point estimates and 95% CIs reproduced from Table 1; outcome = daily sedentary break frequency; per-protocol analytical sample ($n = 115$ participants).
† From the equivalent reparameterization replacing sequence (aliased with condition $\times$ period in the AB/BA design); identical model fit.

Figure 8. Fixed-effect estimates from the primary linear mixed-effects model
Point estimates and 95% confidence intervals reproduce Table 1 (per-protocol $n=115$)

After FDR correction across 11 tests, the primary outcome, sedentary time, adherence, SUS, intraday comparisons, and delay-discounting moderation term remained below 0.05. The MVPA comparison did not (raw $p=0.040$; adjusted $p=0.049$). FDR correction does not resolve the non-equivalent comparator, lack of participant blinding, or potential carryover. The condition $\times$ period interaction was assessed separately as a carryover diagnostic and was not part of the FDR family. The condition $\times$ period interaction was assessed separately as a carryover diagnostic and was not part of the FDR family.

4.6. Moderation by Baseline Delay Discounting

The condition $\times$ log(k) interaction was significant ($\beta=-0.28$, 95% CI -0.52 to -0.04 ; $p=0.022$). In a descriptive median-split display, the MicroReward–LongTerm difference was 1.58 breaks/day in the lower delay-discounting subgroup and 1.00 breaks/day in the higher delay-discounting subgroup (Figure 9). Figure 10 displays the continuous model-implied contrast. This pattern suggests that baseline delay discounting may be related to differential responsiveness across the two intervention packages.

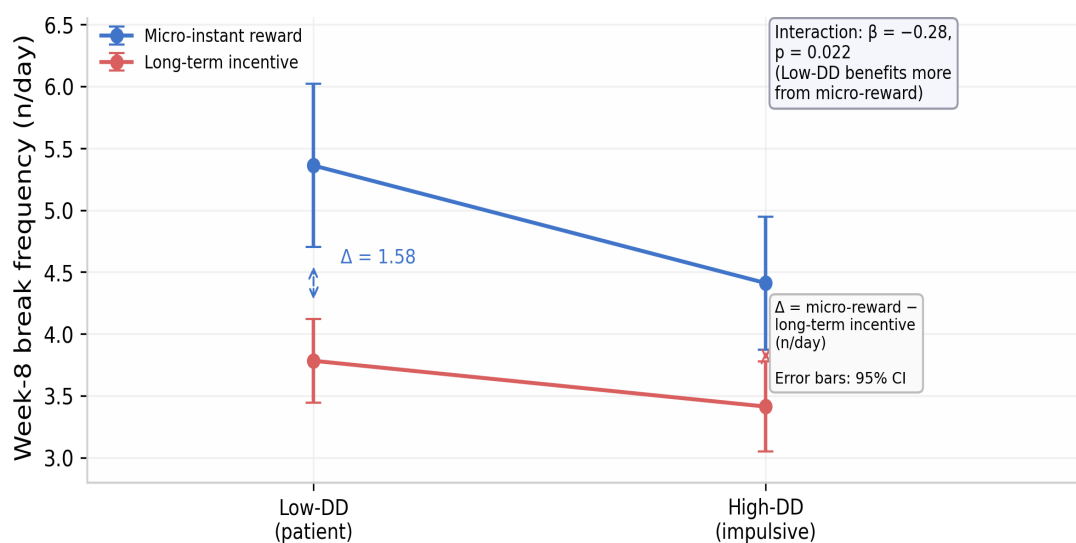


Figure 9. Descriptive delay-discounting subgroup $\times$ intervention-condition interaction at Week 8
Error bars denote 95% confidence intervals; subgroup results complement, but do not replace, the continuous interaction model

4.7. System-derived adherence and usability

Mean weekly adherence was higher in MicroReward (84.21, SD 13.00) than in LongTerm (67.91, SD 12.21; $p < 0.001$). As a system-derived metric, the adherence score primarily reflects implementation engagement. SUS scores were also

higher in MicroReward (80.38, SD 6.46) than in LongTerm (66.70, SD 6.11), with a difference of 13.68 points (95% CI 12.21–15.15; $p < 0.001$). Because participants could identify their assigned condition, interface novelty and demand characteristics may have influenced these subjective ratings. (see Figure 11)

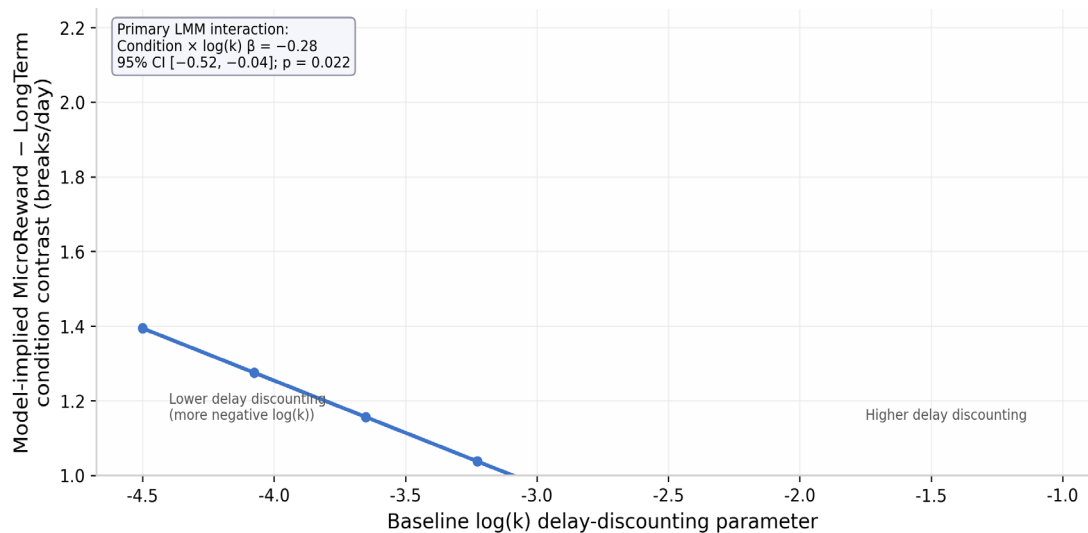
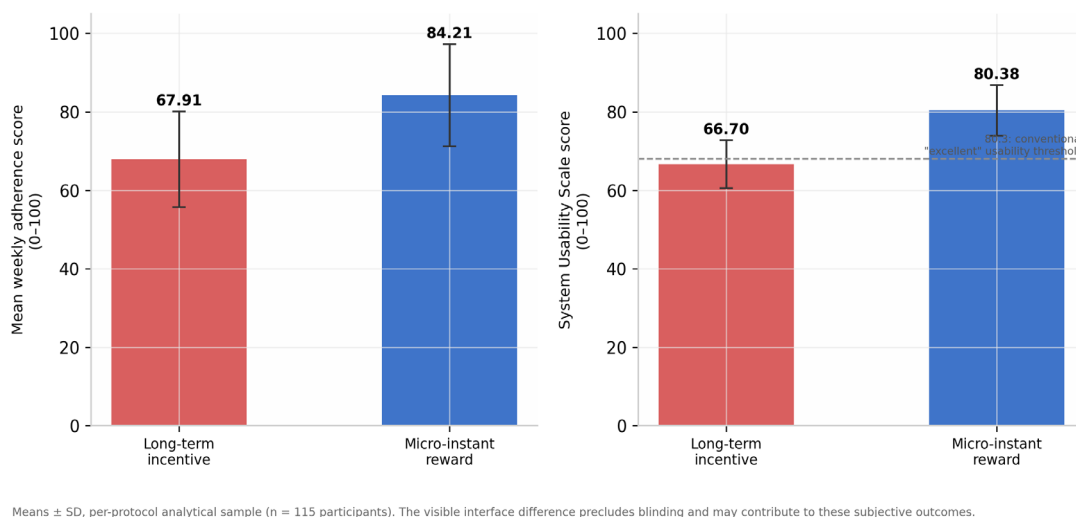


Figure 10. Continuous model-implied MicroReward–LongTerm contrast by baseline $\log(k)$
The line is calculated from the condition and condition $\times \log(k)$ coefficients in Table 1; it is not a display of individual observations



Means $\pm$ SD, per-protocol analytical sample ($n = 115$ participants). The visible interface difference precludes blinding and may contribute to these subjective outcomes.

Figure 11. System-derived adherence and System Usability Scale summaries by intervention condition
Values are means $\pm$ SD from the per-protocol sample ($n=115$)

5. Discussion

5.1. Principal findings

In this randomized crossover comparison, the immediate randomized micro-reward package was associated with more daily sedentary interruptions, lower occupational sedentary time, higher system-derived adherence, and higher usability ratings than the delayed deterministic package. The primary model and Week-8 descriptive result point in the same direction. These findings indicate that the immediate micro-reward package was associated with stronger short-term engagement than the delayed deterministic package under the present study conditions. The absence of a statistically significant condition $\times$ period interaction, together with the persistent period-1-only contrast, indicates that these findings are unlikely to be explained by differential carryover.

5.2. Comparator attainability and plausible pathways

The central interpretive constraint is comparator attainability. LongTerm required 10,000 points to realize the nominal US\$50 maximum but offered a realistic maximum of only approximately 3,200 points during the intervention. MicroReward, by contrast, delivered an immediate consequence after each completed break. Timing, uncertainty, interface animation, tangible feedback, and perceived attainability were therefore entangled. The study focused on behavioral outcomes and did not include neurobiological measures. Future dismantling studies should independently randomize timing, certainty, interface presentation, and attainable expected value with an equivalently attainable delayed comparator.

5.3. Nudge framing and creative-economy relevance

The fixed 60-minute prompt and immediate feedback can be understood as behavioral-design elements that alter the informational choice environment without making a break mandatory. However, the cash and digital-asset components are incentives, not a pure nudge as conventionally defined [19][20]. This distinction matters because behavioral effectiveness, ethical proportionality, and participant autonomy should be evaluated separately for prompts, feedback, and monetary reinforcement.

The study's creative-economy relevance lies in the human-centered experience design of a low-friction animated interface, modular reward content, and a sensor-app interaction that could be localized across workplaces. From a creative-economy perspective, the intervention can be read as a designed behavioral product: its value derives not only from the monetary transfer but from the interaction craft—animation timing, reward-reveal mechanics, progress visualization, and message framing—that converts a health

prompt into an engaging micro-experience. This aligns the study with the creative industries' core competency of designing affective, culturally situated digital experiences, and it positions workplace wellness as an application domain for interaction designers, animators, and game-design practitioners.

The modular reward architecture also illustrates a reusable creative-asset pipeline. Reward skins, animation sets, sound design, and localized message libraries can be authored independently of the sensor backend, packaged as licensable digital content, and adapted across organizations, languages, and cultural contexts at comparatively low marginal cost. Such assets may be scalable through digital-content design studios, employee-wellness platforms, and partnerships between creative and health-service sectors, forming a value chain in which creative producers supply the engagement layer and health-service providers supply evidence, safeguarding, and evaluation. For creative-economy policy, the study therefore illustrates how behavioral-health demand can translate into commissions for design studios, illustrators, and localization teams—an employment and intellectual-property channel that is rarely quantified in health-technology research.

At the same time, the creative-economy contribution of this study is primarily at the level of interface and reward-design implications. We did not measure production costs, willingness to pay, licensing models, or the market structure of wellness-content supply, and economic evaluation remains a topic for future research. Public-space health guidance and healthy-eating choices were not examined because our intervention targeted sensor-detected desk-based sitting. Future work could pair the present behavioral evidence with production-cost accounting, experimentally compared creative variants, and cross-cultural localization studies to quantify the creative-economy value of behaviorally designed health interfaces.

5.4. Implementation and scalability

The observed 1.29-break/day Week-8 descriptive difference and adjusted $\beta=0.89$ suggest that frequent micro-activation warrants further implementation research. These findings should be interpreted cautiously in practice, especially because the two reward packages differed in practical attainability despite having similar nominal maxima. A deployment study should transparently report reward probabilities and expected value, compare cost per sustained behavior change, consider device procurement and accessibility, permit workers to control notification timing, and assess effects across occupations with different meeting patterns, privacy expectations, and physical-work constraints.

5.5. Autonomy, randomized-reward risk, and ethics

Our ethics review prospectively considered the gambling-adjacent features of randomized rewards. Although ethical

review addressed the randomized reward design, future studies should document participant consent language and safety monitoring measures in greater detail. The potential risks associated with randomized rewards warrant continued monitoring in future studies. Future trials and implementations should disclose reward probabilities and expected value, cap monetary exposure, use neutral rather than scarcity-based messaging, include a penalty-free pause or opt-out, avoid deployment in high-risk populations unless safeguards are validated, and prospectively record adverse events, excessive checking, reward preoccupation, distress, and autonomous activity after rewards end.

5.6. Limitations

Several limitations constrain the findings. First, the washout mean did not fully return to baseline (2.45 vs 2.33 breaks/day; $p=0.088$); although the condition $\times$ period interaction was not statistically significant ($\beta=0.56$, 95% CI -0.09 to 1.21 ; $p=0.092$) and the period-1-only contrast remained significant, carryover tests in two-period crossover designs have limited power, so residual carryover cannot be fully excluded. Second, the intervention packages were not matched for attainability, timing, certainty, or interface. Third, participants could not be blinded, and novelty and demand characteristics may have influenced behavior and subjective outcomes. Fourth, the protocol archive does not contain a prospective sample-size calculation. Fifth, the app-generated adherence score lacks retained validation evidence. Finally, the IT-worker sample, short intervention periods, hardware dependency, and absence of post-withdrawal follow-up limit external validity, scalability, and any inference about durable habit formation.

6. Conclusion

Within the non-equivalent reward packages tested, immediate randomized micro-rewards were associated with more sedentary interruptions than a delayed deterministic system with a high redemption threshold. The study contributes short-term comparative evidence on activating exercise snacks in a sensor-triggered workplace mHealth setting. The study provides comparative evidence for two reward packages in a workplace mHealth setting, while further research is needed to disentangle specific reward components, explore personalized delivery, and examine the durability of behavior change after reward withdrawal. Definitive evaluation requires equally attainable comparators, preserved analysis records and prospective sample-size planning, transparent reward disclosure, validated safety monitoring, and longer follow-up after incentive removal.

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