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Impact of daylight saving time on physical activity patterns

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Hayoung Jeong [1,6], Srikar Katta [2,6], Will Ke Wang[1], Alexander Volfovsky [3] & Jessilyn Dunn [1,4,5] ✉

Daylight saving time (DST) remains contentious: some policymakers highlight behavioural benefits, while others emphasize health risks. Here we estimated the behavioural and physiological impacts of DST using longitudinal Fitbit measures from the National Institutes of Health *All of Us* Research Program. Avoiding strict modelling assumptions, we used a natural difference-in-differences design with Arizona (no DST) as a control against neighbouring Mountain Time states (observing DST). Contrary to common belief, DST transitions produced no net change in total daily steps. Instead, activity was reallocated to other times of day: fall transitions increased morning steps by 202 (confidence interval = [78, 326], $P = 0.001$) while reducing evening steps by 180 (confidence interval = [−263, −97], $P < 0.001$); spring transitions showed the opposite. Importantly, these treatment effects varied by demographics and across data-driven activity phenotypes ('morning walker', 'neutral walker' and 'evening walker'). These disparities suggest that structural factors (for example, rigid work schedules, perceived safety) may constrain the capacity to flexibly adapt to time shifts for some populations. Physiologically, resting heart rate showed subtle intraday shifts mirroring behavioural changes, although differences were clinically insignificant. Our study provides a large-scale causal analysis of DST's influence using continuous wearables data, illustrating how observational data can generate real-world evidence to inform health-relevant policies.

Daylight saving time (DST) was first implemented in 1918 as a means to conserve energy by aligning waking hours with daylight. Over a century later, DST remains practised in more than 70 countries, yet it has become increasingly controversial. Recently, multiple countries have abandoned seasonal clock changes altogether, while repeated legislative efforts in the USA have sought to make DST permanent[1,2]. This sustained political and public debate underscores the need for population-scale empirical evidence to inform policy decisions[3].

A growing consensus, including position statements by the American Academy of Sleep Medicine, advocates for abolishing seasonal time changes in favour of permanent standard time[4]. The misalignment between biological rhythms and external time cues from DST has been linked to sleep disruption, circadian misalignment and increased risks of myocardial infarction, ischaemic stroke and mood disorders[5–8], with some studies reporting differential effects observed in spring versus fall transitions[2,5,6,8,9]. However, proponents argue that DST generates behavioural benefits, particularly in creating additional opportunities for outdoor physical activity by shifting evening light later. Based on common daily activity routines, the shift in time has been estimated to substantially increase opportunities for outdoor leisure among adults and children[10]. Because even modest increases in regular physical activity are known to confer measurable health benefits, DST-related

[1]Department of Biomedical Engineering, Duke University, Durham, NC, USA. [2]Department of Computer Science, Duke University, Durham, NC, USA. [3]Department of Statistical Science, Duke University, Durham, NC, USA. [4]Department of Biostatistics and Bioinformatics, Duke University, Durham, NC, USA. [5]Duke Clinical Research Institute, Durham, NC, USA. [6]These authors contributed equally: Hayoung Jeong, Srikar Katta. ✉e-mail: jessilyn.dunn@duke.edu

behavioural changes could translate to meaningful implications for population health[11,12]. Resolving the debate around DST for public health benefits requires reliable, causal evidence that can evaluate both the behavioural and physiological responses to DST, yet such evidence remains limited.

Early studies on DST and health-related behaviours have relied on large-scale administrative data sources, such as hospital admission registries, health insurance claims, traffic crash databases or self-reported surveys like the American Time Use Survey[7,13–16]. Using American Time Use Survey data from 2003 to 2009, no detectable change in total time spent in moderate-to-vigorous physical activity was found among adults residing in Arizona, Colorado, New Mexico and Utah following DST transitions[16]. Drawing on a telephone survey in Australia, research found that nearly half the study participants reported perceived changes in time spent in physical activity during a typical week under daylight saving conditions[17]. Although these studies provided early insights into the impact of DST on physical activity patterns, their reliance on self-reported surveys and infrequent measurement limits their ability to precisely capture the day-to-day impact of DST on physical activity behaviour and physiology. More recent work has begun to leverage wearable devices to move beyond self-reported measures and continuously quantify health-related behaviours and physiology in daily living settings. Data from accelerometers ($n = 439$) has been used to demonstrate that longer daylight is associated with increased moderate-to-vigorous physical activity in children living in Europe and Australia, although no consistent changes were observed in the US paediatric population[18]. Associations have also been found between DST transitions and shifts in sleep timing, consistency and duration among users of wrist-worn fitness trackers ($n = 24{,}000$)[19]. Additional smaller-scale studies have used wearables to examine cardiovascular responses and light exposure changes during DST[20,21]. Despite this progress, existing wearable-based studies have largely remained descriptive and depend on strict modelling assumptions (for example, no unobserved confounders); because they rely on simple before-and-after comparisons without robust control groups, rigorous causal claims cannot be made.

In this study, we address these gaps by taking advantage of the natural variation over time and geography in one of the largest continuous wearable datasets available, the *All of Us* Research Program[22,23], spanning diverse demographic groups across the USA. Arizona, the only state in the contiguous USA that does not observe DST, serves as a natural control group, enabling comparison with neighbouring DST-observing Mountain Time states (Colorado, New Mexico and Utah). Compared with previous studies that relied on simple before-and-after comparisons[16,18,19], we use a difference-in-differences (DiD) analysis that leverages this natural variation. Because DiD methods do not require adjustment for any individual-level confounders, our analysis relies on weaker assumptions that enable more rigorous causal inference. We then stratify our analysis by demographics and physical activity phenotypes to reveal heterogeneous effects across the population, providing a large-scale causal analysis of DST's influence on behaviour and physiology from continuous wearable data.

Results

Cohort description

The *All of Us* Research Program collected Fitbit data via two pathways: the Bring-Your-Own-Device program, in which participants shared retrospective Fitbit data; and the *All of Us* WEAR program, which prospectively collected data beginning in March 2021 using study-provided devices. Our analytic cohort comprised 1,157 participants located in Arizona, Colorado, New Mexico and Utah with valid intraday heart rate and step data for the 2 weeks surrounding each spring and fall DST transition during 2021–2023 (Supplementary Fig. 1)[24]. Of these participants, 66% ($n = 763$) were enrolled through the Bring-Your-Own-Device program and 34% ($n = 394$) through the *All of Us* WEAR program.

Table 1 | Demographic characteristics of participants in the final analytic cohort

	Subgroup	Count (%)
Overall	Total *n*	1,157
Age (years)	18–44	268 (23.2)
	45–64	406 (35.1)
	≥ 65	483 (41.7)
Biological sex	Female	790 (68.3)
	Male[a]	350–375 (30.3–32.4)
	Unknown[a]	≤20 (≤1.7)
Race	Asian[a]	≤20 (≤1.7%)
	Black or African American	26 (2.2)
	White	986 (85.2)
	Other	41 (3.5)
	Unknown[a]	80–90 (6.9–7.8)
Ethnicity	Hispanic or Latino	123 (10.6)
	Not Hispanic or Latino	1013 (87.6)
	Other/unknown	21 (1.8)
Annual household income ($)	<50,000	348 (30.1)
	50,000–100,000	372 (32.2)
	>100,000	377 (32.6)
	Unknown	60 (5.2)

[a]Counts below 20 are suppressed, and additional cells are obscured as needed to prevent back-calculation, complying with the *All of Us* Data and Statistics Dissemination Policy.

Participants represented diverse demographic groups across the four states (Table 1).

Behavioural response to DST: changes in physical activity

Visual inspection of hourly steps in Arizona (DST non-observing) and the surrounding Mountain Time states (DST-observing; Colorado, New Mexico and Utah) revealed shifts in activity around the spring and fall transitions, although Arizona showed no consistent change (Extended Data Fig. 1). After the 'spring forward' transition, people in the DST-observing states were slower to start their activity in the mornings, particularly on Sunday (the morning of DST), and showed more activity in the late afternoon and evenings during the following days (Fig. 1a). By contrast, the 'fall back' transition produced an opposite pattern: activity shifted earlier, with increased morning movement and reduced late-day steps in the DST-observing states (Fig. 1b).

Our DiD analysis corroborated these qualitative patterns and revealed contrasting effects between fall and spring DST transitions on intraday activity. Throughout, we report the results of a *t*-test on the estimated treatment coefficient (β) from the DiD regression (Methods), with standard errors clustered at the subject level. DST transitions did not change total daily activity levels, but rather restructured when people were active. Total daily steps did not alter significantly after either of the transitions (spring: $\beta = 31.9$, confidence interval (CI) = [−214, 277], $P = 0.799$, *t*-test; fall: $\beta = -9.5$, CI = [−308, 289], $P = 0.950$, *t*-test). Yet, both transitions produced significant changes in activity distribution across the day, with opposing directional effects that mirror the shift in daylight availability. In spring, when sunset is delayed by 1 hour, evening activity slightly increases by 90 steps (CI = [14, 167], $P = 0.021$) (Fig. 1c) whereas morning activity showed a non-significant reduction ($\beta = -66$, CI = [−156, 24], $P = 0.148$). In fall, when sunrise is earlier, morning activity increased by 202 steps (CI = [78, 326], $P = 0.001$), roughly equating to one and a half city blocks, whereas evening activity decreased by 180 steps (CI = [−263, −97], $P < 0.001$) (Fig. 1d). This significant reciprocal change observed in the fall suggests that people respond to DST

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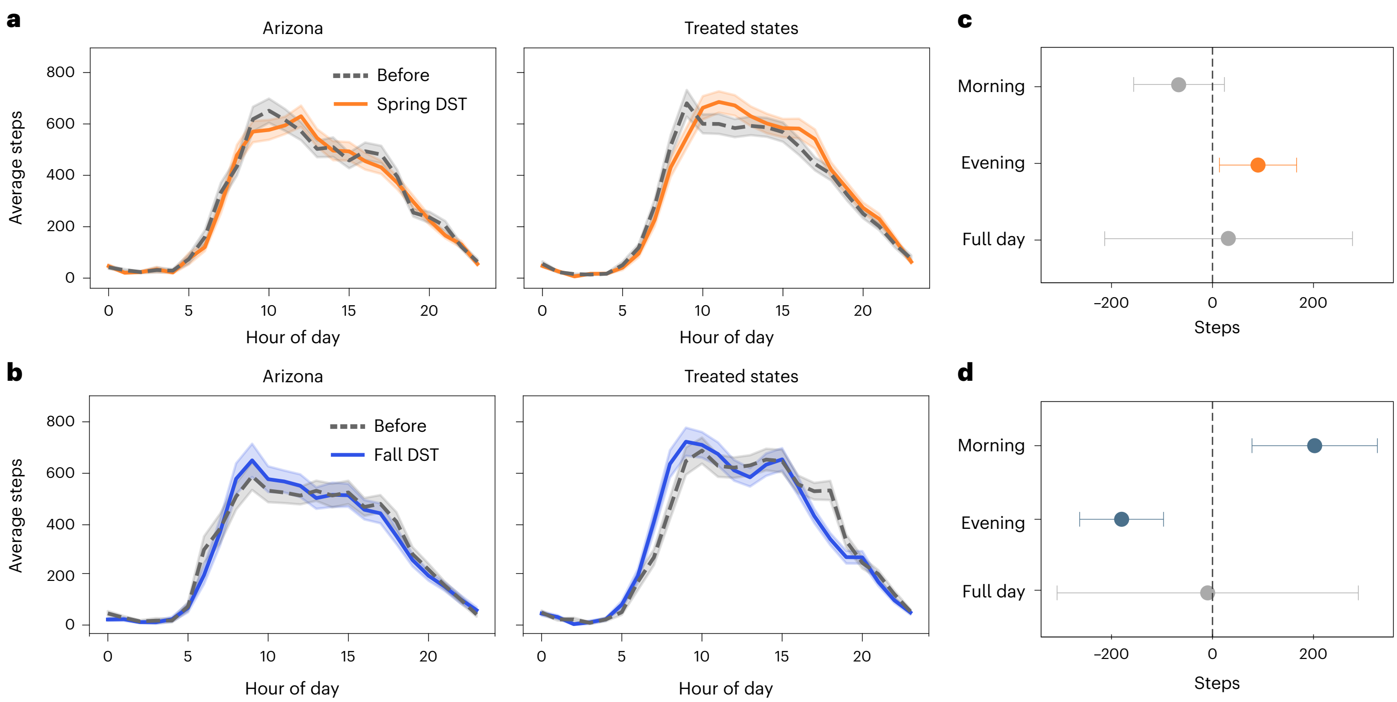

Fig. 1 | Behavioural effects of DST transitions. a,**b**, Average hourly step counts on the Sunday before and after the spring transitions in 2022 (n = 812) and 2023 (n = 1,072) (**a**) and the fall transitions in 2021 (n = 820) and 2022 (n = 837) (**b**), combined. Shaded areas represent 95% CI. The near-parallel pre-transition trajectories across groups support the parallel-trends assumption underlying the DiD analysis. **c**,**d**, Population-level DiD estimates for the effect of DST transitions on step counts across time-of-day periods. Points represent mean DiD coefficients, and error bars denote 95% CI from two-sided t-tests. Effects that are not marginally statistically significant ($P \geq 0.05$) are displayed in grey. **c**, Spring transitions in 2022 and 2023 across the morning (n = 383), evening (n = 454) and full day (n = 480). **d**, Fall transitions in 2021 and 2022 across the morning (n = 284), evening (n = 338) and full day (n = 342).

transitions by reallocating their activity timings (earlier movement) rather than by changing their overall activity level.

These intraday activity shifts were robust to the choice of post-transition observation window (Methods). Re-estimating treatment effects using post-transition periods ranging from 1 to 6 weeks produced qualitatively similar patterns (Supplementary Fig. 2), indicating that activity redistribution was not driven by a specific analysis window. The only attenuation observed was for fall morning activity, where the initial increase of 202 steps (CI = [78, 326], P = 0.001) was no longer statistically significant beyond the first post-transition week. Changes in evening activity following both spring and fall transitions persisted for up to 6 weeks.

We explored whether these temporal shifts varied by an individual's typical activity timing. Based on steps data from 6 weeks preceding each transition, participants who consistently walk more in the morning than in the evening were labelled as 'morning walkers'; participants who consistently walk more in the evening than in the morning were labelled as 'evening walkers'; and participants who did not demonstrate differential levels of activity during any specific time period were labelled as 'neutral walkers' (Methods). We hypothesized that daylight shifts that align with a person's preferred activity time would amplify activity levels. In particular, we expected this amplification for evening walkers in the spring because existing literature suggests evening hours represent a flexible, opportunistic window for leisure[18].

The hypothesis held true for morning walkers in the fall (Fig. 2). When daylight moved earlier, they actively capitalized on the change, significantly increasing their morning activity (β = 636, CI = [146, 1126], P = 0.011) while reallocating some movement away from the evening (β = −144, CI = [−267, −21], P = 0.022). Interestingly, the reciprocal during the spring did not hold true for the evening walkers. We expected that the 'spring forward' change, by providing more evening daylight, would create an opportunity for more physical activity in evening walkers. Despite daylight now being better aligned with their typical active times, evening walkers did not show a significant increase in evening steps (β = 39.3, CI = [−166, 245], P = 0.708).

Neutral walkers, who lack a strong temporal preference, primarily reallocated their activity across the day rather than amplifying activity in a specific time window. In fall, neutral walkers had a slight decrease in evening activity (β = −169, CI = [−272, −65], P = 0.001) with a non-significant increase in morning steps (β = 120, CI = [−40, 280], P = 0.142), behaving more like morning walkers. In the spring, neutral walkers took fewer steps in the darker morning (β = −120, CI = [−239, −0.2], P = 0.05) and compensated with an almost identical increase in the lighter evening (β = 118, CI = [22, 213], P = 0.016), behaving more like evening walkers. Stratification by perceived neighbourhood walkability revealed that this reciprocal morning–evening reallocation was evident among neutral walkers perceiving their neighbourhood as more walkable (Extended Data Fig. 2). Across the four walkability features examined (perceived availability of transit, sidewalks, bicycle facilities and recreational facilities), high transit availability was associated with the largest shift in activity following the spring transition, with a decrease (β = −239, CI = [−402, −77], P = 0.003) and increase (β = 234, CI = [98, 370], P < 0.001) in the morning and evening steps, respectively (Extended Data Fig. 2a). Neutral walkers reporting low perceived walkability showed no evidence of reciprocal morning–evening substitution following either DST transition (Extended Data Fig. 2b).

We also observed that the ability to reallocate intraday activity following the DST transition varied by age, household income and sex (Extended Data Fig. 3). During the fall transition, adults aged 65 and older increased their morning steps (β = 281, CI = [80, 483], P = 0.006) while reducing their evening steps (β = −227, CI = [−341, −113], P < 0.001) (Extended Data Fig. 3a). By contrast, younger adults (18–45) exhibited no significant changes in either morning (β = 209, CI = [−58, 476], P = 0.124) or evening steps (β = −90, CI = [−286, 105], P = 0.365). This

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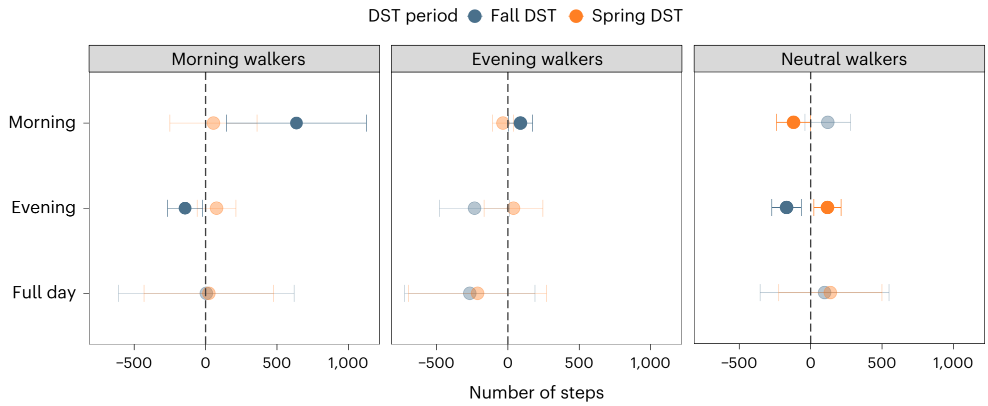

Fig. 2 | Heterogeneity in behavioural responses to DST by physical activity phenotype. Effects stratified by activity phenotype (morning, evening and neutral walkers) based on pre-DST activity timing classifications. Points represent mean DiD coefficients, and error bars denote 95% CI from two-sided *t*-tests. Effects that were not marginally statistically significant ($P \geq 0.05$) are displayed with reduced opacity.

divergence may suggest that older adults are more flexible in adapting their daily schedules and more apt to follow daylight availability. Younger adults, by contrast, may be more constrained by fixed social schedules that limit temporal adjustments in their activity. This pattern is also directly reflective of the underlying distribution of activity timing preferences. The 65+ age group contains a substantially higher proportion of habitual morning walkers (36% versus 8–18% in younger adults), which is also consistent with documented age-related shifts towards morning chronotypes[25]. As established in our primary analysis, individuals with pre-existing morning activity patterns capitalize on additional morning daylight, while those without such patterns show no to little increase in morning steps (Fig. 2).

Evening steps declined universally across income groups during fall (low income: $\beta = -207$, CI = [−353, −61], $P = 0.005$; middle income: $\beta = -202$, CI = [−369, −34], $P = 0.018$; high income: $\beta = -153$, CI = [−287, −19], $P = 0.025$) (Extended Data Fig. 3b). However, only the lowest income bracket ($50,000 or below) compensated for this decline in evening activity with increased morning steps ($\beta = 335$, CI = [100, 570], $P = 0.005$), whereas middle- and high-income groups showed no changes in morning activity (middle income: $\beta = 237$, CI = [−2, 476], $P = 0.052$; high income: $\beta = 60$, CI = [−128, 249], $P = 0.532$). The distribution of morning versus evening walkers was similar across income groups, suggesting that income level does not play a role in activity phenotype. Instead, the morning increase among lower-income adults may reflect greater schedule flexibility—encompassing retirees or those with flexible work schedules (for example, part-time, self-employed, freelance and/or mainly outdoor workers)—who continue their pre-DST behaviour or reallocate activity towards daylight. By contrast, higher-income individuals may experience stronger occupational timing constraints, limiting their capacity to adjust morning behaviour despite additional daylight or personal preference.

The spring transition, which shifts daylight towards evening hours, revealed an interesting difference by sex (Extended Data Fig. 3c). Women increased evening steps ($\beta = 158$, CI = [62, 253], $P = 0.001$), whereas men showed no significant change ($\beta = -61$, CI = [−185, 64], $P = 0.339$). Women may benefit from extended evening light induced by spring DST, reducing pre-existing barriers to evening activity (for example, perceived outdoor safety)[26].

Physiological response to DST: changes in average resting heart rate

Across both DST transitions, changes in average resting heart rate (RHR) were small (ranging from −2 to +2 beats per minute), differences that are unlikely to be clinically meaningful (Extended Data Fig. 4). In fall, RHR during the evening declined by 0.8 beats per minute ($\beta = -0.8$, CI = [−1.4, −0.2], $P = 0.014$), while morning RHR rose slightly ($\beta = 1.3$, CI = [0.7, 2.0], $P < 0.001$). In spring, RHR decreased in the morning ($\beta = -1.0$, CI = [−1.6, −0.5], $P < 0.001$) without any changes in the evening RHR ($\beta = 0.4$, CI = [−0.1, 1.0], $P = 0.127$). We found no significant differences in daily average RHR during either seasonal transition (spring: $\beta = 0.0$, CI = [−0.3, 0.4], $P = 0.763$; fall: $\beta = -0.1$, CI = [−0.5, 0.3], $P = 0.621$).

Discussion

We used intraday Fitbit data from more than 1,000 participants in the *All of Us* Research Program to investigate the behavioural and physiological effects of DST policy. Our DiD analysis reveals that although DST transitions influence when people are active, they do not significantly alter how much they are active. In line with previous work, we found no significant net gain or loss in total daily steps following either the spring or fall transitions[10,12]. The 1-hour shift in daylight instead triggered a significant temporal reallocation of physical activity (Fig. 1). This was most evident in the ‘fall back’ transition, which prompted a clear reciprocal shift, with a 202-step increase in the morning and a 180-step decrease in the evening (Fig. 1d).

A common intuition in DST-related physical activity research is that extended evening daylight creates additional opportunities for discretionary movement[10]. Our findings suggest that although these opportunities do emerge, they do not translate into a net increase in physical activity. Although evening steps increased following the spring transition (Fig. 1), total daily step counts remained unchanged, even among evening walkers whose typical activity patterns were most closely aligned with the newly illuminated hours (Fig. 2). These results challenge the assumption that evening light functions as an effective intervention for increasing total daily activity levels. Rather, while light availability may influence the timing of movement, it does not inherently generate the motivation or capacity to expand the overall volume of activity.

Previous investigations into DST have documented increases in pedestrian and cycling activity following transitions that extend daylight, suggesting that ambient light promotes active travel[27,28]. Separately, a large body of work has shown that walkable built environments support higher levels of routine walking, particularly for transport and commuting[29–31]. These findings motivate the expectation that DST-related increases in pedestrian activity might be most evident in highly walkable settings, where opportunities for active travel are greatest. Our results, however, do not support this hypothesis. Neutral walkers—those who do not exhibit preferred activity timing—did not increase their overall numbers of steps taken, even in areas perceived to be highly walkable (Extended Data Fig. 2a). Instead, they consistently reallocated existing activity to periods of greater ambient light. The reallocating pattern, however, was not observed in neutral walkers residing in lower perceived walkability areas. This suggests that

DST-related increases in pedestrian activity reported in prior studies may reflect temporal and spatial redistribution of routine movement—enabled by pedestrian infrastructure—rather than increases in overall physical activity.

Demographic stratification further reinforced that no subgroup achieves a net gain in total steps (Extended Data Fig. 3), but the pattern of reallocation varied by age, annual household income and sex, suggesting distinct behavioural (for example, rigid work hours) and structural considerations (for example, perceived safety). Older adults, women and lower-income individuals exhibited significant increases in morning steps and a decrease in evening steps following the fall transition. On the other hand, following the spring transition, women and participants aged 45–65 showed greater evening activity, whereas men primarily reduced morning movement. Yet, in all cases, these gains were offset by compensatory reductions at other times of day, resulting in a net-zero change in total activity volume.

Emerging evidence, particularly randomized crossover studies in which individuals switch the timing of physical activity while total volume is held constant, suggests that activity timing carries health relevance beyond total volume. These studies report physiological adaptations specific to the time of day, including differential engagement of carbohydrate and lipid metabolic pathways[32] and improvements in 24-h glucose levels[33]. With larger randomized trials in more diverse populations now underway to rigorously evaluate the causal effects of exercise timing on cardiometabolic, sleep and circadian outcomes, activity timing is increasingly recognized as a modifiable dimension of health behaviour rather than a secondary characteristic[34,35]. Findings from these future crossover studies would help contextualize our results and evaluate the public health trade-offs of DST policies.

As with physical activity, we found no net change in daily average RHR (Extended Data Fig. 4). However, we observed a similar pattern of intraday changes as physical activity. In fall, for example, RHR increased slightly in the morning while decreasing in the evening. Although these acute changes were too small to be clinically meaningful, they demonstrate that the body's rhythms are being subtly perturbed alongside behavioural shifts. In this analysis, we observed the immediate effects of DST transitions using minute-level heart rate data with a 2-week observation window. Future studies can collect higher-resolution data to detect the more subtle physiological changes (for example, heart rate variability) indicative of the circadian misalignments.

From a methodological standpoint, our approach utilizing large-scale wearable data establishes stronger causal evidence than prior observational research. Although previous studies have examined DST effects, they have largely relied on either cross-sectional comparisons (which fail to account for baseline population differences) or simple pre–post analyses (which left open the possibility that observed changes reflected seasonal trends rather than DST itself)[16,18]. We used a DiD approach that addresses these limitations and isolated the causal effect from concurrent seasonal confounders. In this framework, causal identification does not require explicit modelling of time-varying covariates as long as Arizona and its DST-observing neighbours would have experienced similar activity trajectories in the absence of DST transitions (that is, parallel-trends assumption). We reduced the likelihood of violating this assumption by focusing on a short pre-treatment time window and neighbouring Mountain Time states that share similar geography, latitude and time zone, and thus experience nearly identical daylight patterns (Fig. 1). Weather conditions in this region also evolve in close synchrony; spring warming, fall cooling and precipitation trends are broadly aligned across Arizona, Colorado, Utah and New Mexico. Because the DiD design is robust to individual-level confounding, our specification accounts for time-invariant climatic and behavioural differences. Moreover, given the shared seasonal conditions across these states, our model also absorbs region-wide weather shocks. Consequently, only idiosyncratic, state-specific anomalies coinciding precisely with the DST transition could bias our estimates, and—to our knowledge—no such shocks occurred during the study period.

Several limitations should be considered when interpreting these findings. Our sample skews towards women and higher socioeconomic demographics who probably have greater flexibility for physical activity (Table 1), suggesting our estimates may not represent the general US population[36–38]. In addition, sleep outcomes were not evaluated because of Fitbit's unreliable sleep tracking[39], especially during DST transitions, as reported by many Fitbit users. Given these data quality concerns and the absence of validated correction algorithms, we excluded sleep analyses to ensure methodological rigour, although future work could incorporate algorithmic calibration to enable more reliable sleep assessment. Our estimation of the behavioural effects through predefined time windows (for example, morning and evening) may not fully capture the temporal shifts in physical activity; alternative approaches using hourly bins or rolling activity windows could provide finer resolution.

Our study leveraged large-scale wearable data from the *All of Us* Research Program to evaluate the behavioural effects of DST transitions in the USA. Contrary to the common assumption that DST would promote physical activity through added evening light, we found no evidence that DST amplifies total physical activity. Instead, DST primarily reallocated when people were active, producing reciprocal shifts between morning and evening hours. We also found that the ability to adapt to this shift is not uniform across the population. As policymakers continue to debate the future of DST, such findings should be interpreted cautiously and weighed against the well-documented disruptions DST causes to sleep, circadian rhythms, cardiovascular health and public safety[2,4,40–46].

Methods

Data from the *All of Us* Research Program

We used the demographic, survey and Fitbit data from the National Institutes of Health *All of Us* Research Program[16]. Our analysis used the most up-to-date Controlled Tier dataset v.8 (C2024Q3R5), which includes non-date-shifted intraday Fitbit data (to 1 October 2023) and the first three digits of participant ZIP codes. All analyses were performed in the secure Researcher Workbench by authorized researchers who completed the *All of Us* Responsible Conduct of Research training. Use of the *All of Us* Controlled Tier dataset was determined by the *All of Us* Institutional Review Board not to constitute human subjects research; accordingly, this study was approved as exempt research from the Duke Institutional Review Board (Protocol no. 2021-0574).

From a total of 59,018 participants who shared any Fitbit data, we identified 53,812 individuals who contributed intraday heart rate data (Supplementary Fig. 1). Of these, 51,364 participants also had both state and ZIP codes. After excluding individuals with discrepancies in their geolocation information, we retained 4,375 participants across Arizona, Colorado, New Mexico or Utah. All demographic and geolocation information included in the study was self-reported by participants via the Basics survey. To focus on the immediate effects of DST, we included participants with valid data on any day during the first week of the spring or fall DST transition for the years 2021, 2022 and 2023. After applying our inclusion criteria, our cohort included 1,531 unique individuals with both intraday HR and steps data (Supplementary Fig. 1). Year-by-year transition breakdown is shown in Extended Data Table 1.

Data processing

To address missingness from inconsistent device wear, we defined 'valid days' using sensitivity thresholds ranging from 0% to 100% daily wear or wear during specific time windows (for example, morning and evening) (Supplementary Fig. 3a). Wear time was calculated as the sum of all minute-level heart rate data available for each person per day; wear threshold was defined as the proportion of observed wear time over the potentially observable data[24]. For example, the potentially observable data for a full day is 1,440 min (60 min h^{-1} × 24 h d^{-1}). We required participants' average wear time for each DST period to meet wear-time thresholds (75%) to enable within-person temporal comparisons of the specific time window. We additionally performed sensitivity analysis with different wear threshold choices (ranging from 75% to 95%), illustrating the robustness of our analysis to this hyperparameter (Supplementary Fig. 3b,c).

To reduce the influence of extreme activity values, we excluded person-days with step counts exceeding the 99.9th percentile in each time window (morning, evening, or full day). Fewer than 20 participants were removed following this criterion. From 1,531 unique participants, our final analytic cohort included 1,157 participants (Supplementary Fig. 1). We summarize the demographics of the full analytic cohort residing in the four states and who had valid data in any of the four DST transitions (Table 1). Step counts stratified by Arizona and Mountain Time states are shown in Supplementary Fig. 4. Of note, some individuals contributed data to multiple transitions, while others participated in only one.

Significant effects of DST in those with flexible walking patterns (neutral walkers) were observed (Fig. 2). We therefore examined whether built environment characteristics moderated the behavioural impact of DST in this group. We selected five questions from the *All of Us* Research Program's Social Determinants of Health survey to assess perceived neighbourhood walkability (Supplementary Table 1). The survey was distributed to all enrolled participants as a one-time voluntary questionnaire between November 2021 and September 2023. Questions assessed perceptions of amenities and infrastructure in participants' built environment, defined as a 10–15-min walk from their residence. Items covered environmental infrastructure (for example, presence of sidewalks, bicycle facilities, transit stops) and accessibility (for example, availability of recreational facilities). Response options that included 'Don't know' or 'Does not apply' were treated as neutral midpoint values (for example, neither agree nor disagree) to preserve ordinal structure while minimizing data loss, whereas questions that were skipped were considered missing. We then binarized responses as high (score >3) versus low accessibility (score ≤3) for each item.

Feature extraction

Behavioural features were derived from step data, calculated as total steps per day and across specific time windows. We chose RHR as the physiological feature of interest to minimize confounding influences from physical activity. RHR was calculated as the average heart rate during restful periods (steps = 0). We examined average RHR across the full day and in time-specific windows.

Activity phenotypes

We estimated the effect of DST on individuals with different activity patterns by defining activity phenotypes based on step count data collected during the 6 weeks preceding the transition (Supplementary Fig. 5 and Supplementary Table 2). Participants were classified as morning, evening or neutral walkers using a rule-based approach. For each day in this period, we compared the number of steps taken in the morning versus the evening. Days with more morning steps were assigned a score of +1, days with more evening steps were assigned a score of −1, and days with no clear preference were assigned a score of 0. Each participant's mean daily score over the final 6 days before DST represented their time-of-day tendency. Participants with average scores above 0.5 were classified as morning walkers, those with scores below −0.5 as evening walkers, and those with scores between −0.5 and 0.5 as neutral walkers. This classification required participants to have at least one day of both morning and evening step counts with ≥75% wear level. The distribution of morning, evening and neutral Walkers is illustrated in Supplementary Fig. 5.

Difference-in-differences

Our analysis leverages state-level differences in DST observance. Specifically, because Arizona (with the exception of the Navajo Nation) does not participate in the time change, its residents are not exposed to DST changes[16]. We leveraged this natural variation over time and across states to conduct a DiD analysis using Arizona residents as our 'control' group.

The DiD analysis is robust to time-invariant omitted variables that are not accounted for in other DST studies, such as the accelerometer-based observational analyses in children that attribute higher levels of moderate-to-vigorous physical activity to longer evening daylight in Europe and Australia without a geographic counterfactual control[18], and a within-person pre–post wearable study in adults that detects only small shifts (for example, +8 min per day sedentary after DST ends) but lacks a no-DST control to separate seasonal effects[47]. Furthermore, although the study described in ref. 16 had a similar geographic comparison using Arizona, pre-DST baseline data were absent.

We assessed the pre-treatment trends by plotting the average hourly step counts during the week before and after the DST transition, separated by day of the week and control versus intervention (Fig. 1a,b). The pre-DST patterns for Arizona and the Mountain Time states follow highly similar shapes and magnitudes, indicating parallel pre-transition trends in activity.

Our DiD analysis controls for day-of-week and yearly behavioural differences[48]. The regression function is:

$$Y_{it} = \alpha Treat_i + \beta I(Time_t \geq EventTime) + \tau Treat_i \bullet I(Time_t \geq EventTime) + \phi_y + \phi_d + \epsilon_{it}$$

where Y_{it} measures the outcome for person i at time t, $Treat_t$ is a binary indicator for whether person i is in a region of DST exposure, $I(Time_t \geq EventTime)$ indicates whether the corresponding period is before or after the time change, ϕ_y is a matrix of year-level fixed effects, ϕ_d is a matrix of day-of-week fixed effects and ϵ_{it} is random and clustered by subject. Throughout the paper, we report the estimated treatment coefficient (β) from the DiD regression along with the P value from the t-test.

We estimate the effect of DST on the mean RHR or total step counts in three time windows: (1) morning (6–9 a.m.), (2) evening (5–7 p.m.) and (3) full day (24 h) (Fig. 1 and Extended Data Fig. 4). We fit separate DiD regressions for fall and spring DST changes and time window (morning and evening). Analyses are conducted using the statsmodels package (v.0.14.4) in Python (v.3.10.16). Visualizations were generated with ggplot2 (v.3.5.2) in R (v.4.5.0) and seaborn (v.0.12.2).

Sensitivity analyses

To test the robustness of our findings to key analytic choices, we conducted a series of sensitivity analyses. First, we repeated the primary analyses under alternative wear-time thresholds, ranging from 75% to 95%, demonstrating that the estimated effects are stable across this hyperparameter choice (Supplementary Fig. 3). Second, although our main results focus on the effects estimated over the first week following the time change to capture short-term impacts, we assessed the persistence of DST effects over longer horizons to evaluate the robustness to the choice of our analysis window. Specifically, we re-estimated the treatment effect using the same DiD model specification but with longer post-transition windows, ranging from 1 to 6 weeks, yielding qualitatively similar results (Supplementary Fig. 2).

Heterogeneous effects of DST

We examined heterogeneity in DST effects by activity phenotype (Fig. 2), perceived walkability measures (Extended Data Fig. 2) and by self-reported demographics, including income level, biological sex and age group (Extended Data Fig. 3). We estimated separate DiD regressions for each combination of subgroup and time period. Only individuals with non-missing information for that subgroup were included in these analyses.

Reporting summary
Further information on research design is available in the Nature Portfolio Reporting Summary linked to this article.

Data availability
To ensure participant privacy, data used for this study is available to approved researchers following registration, completion of ethics training and attestation of a data use agreement through the *All of Us* Research Workbench platform, via https://workbench.researchallofus.org/login.

Code availability
All registered Researcher Workbench users can access the code for this project at https://workbench.researchallofus.org/library, with workspace titled 'DST and Physical Activity'.

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Author contributions
H.J. and W.K.W. conceived the initial idea for the study. H.J., S.K. and W.K.W. developed the research framework. J.D. supervised the project. W.K.W. performed the literature review. H.J. curated the data for analysis. S.K. conducted the formal analysis under the supervision of A.V. The results were interpreted collaboratively by all authors. H.J., S.K. and W.K.W. drafted the initial manuscript. All authors contributed to data interpretation, critically revised the manuscript and approved the final version for submission.

Competing interests
J.D. sits on the Google Consumer Health Advisory Board and is a consultant to Samsung Research America and Jones Day. The other authors declare no competing interests.

Additional information
Extended data is available for this paper at https://doi.org/10.1038/s44360-026-00115-z.

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Correspondence and requests for materials should be addressed to Jessilyn Dunn.

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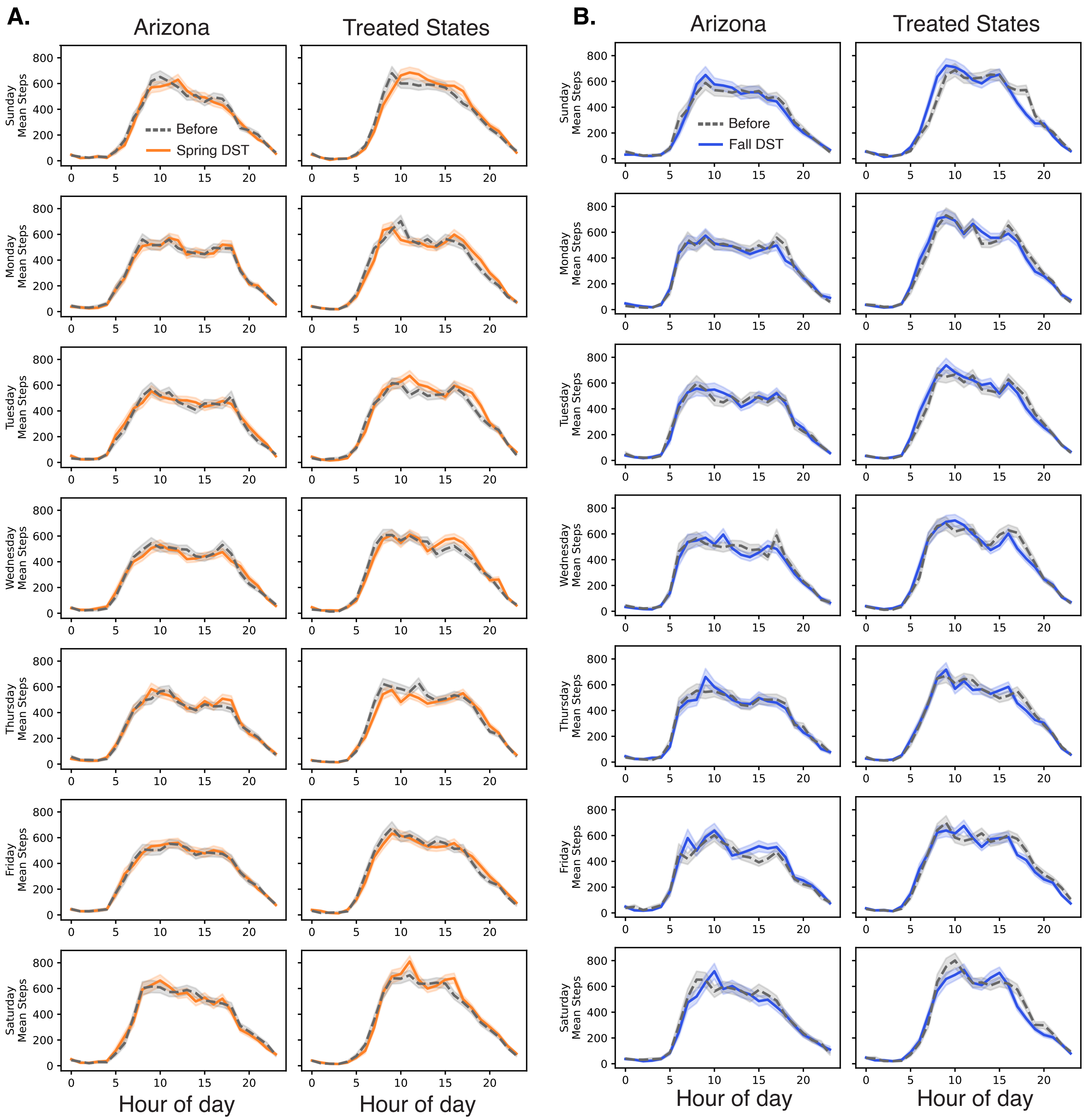

Extended Data Fig. 1 | Average hourly step counts during the week before and after Daylight Saving Time (DST). (a) Average steps taken during spring transitions in 2022 and 2023. **(b)** Average steps taken during fall transitions in 2021 and 2022. Treated States include Colorado, New Mexico, and Utah. Shaded areas represent 95% confidence intervals.

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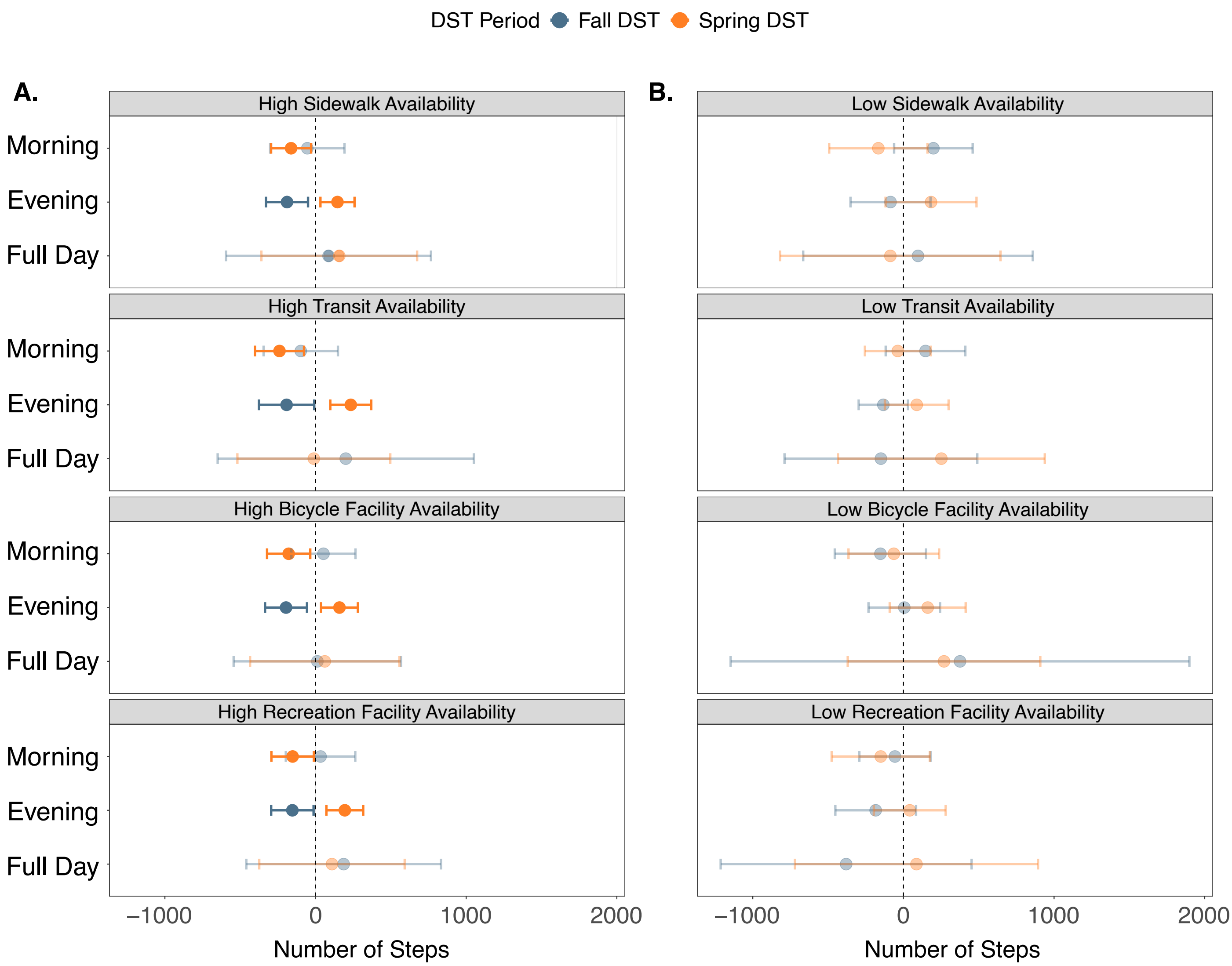

Extended Data Fig. 2 | Heterogeneous effects of DST transitions on step counts across walkability measures. Difference-in-differences (DiD) point estimates with 95% confidence intervals for the effects of DST transitions on step counts across different time-of-day periods for Neutral Walkers, stratified by **(a)** high and **(b)** low availability of walkability-related features, including sidewalks, public transit, bicycle facilities, and recreational facilities. Estimates are shown for full-day, morning, and evening periods. Points denote mean estimated effects from a DiD regression, horizontal bars indicate 95% confidence intervals from two-sided t-tests, and the vertical dashed line denotes no change. Estimates that are not marginally statistically significant (p ≥ 0.05) are displayed with reduced opacity. Sample size for each group is reported in Extended Data Table 2.

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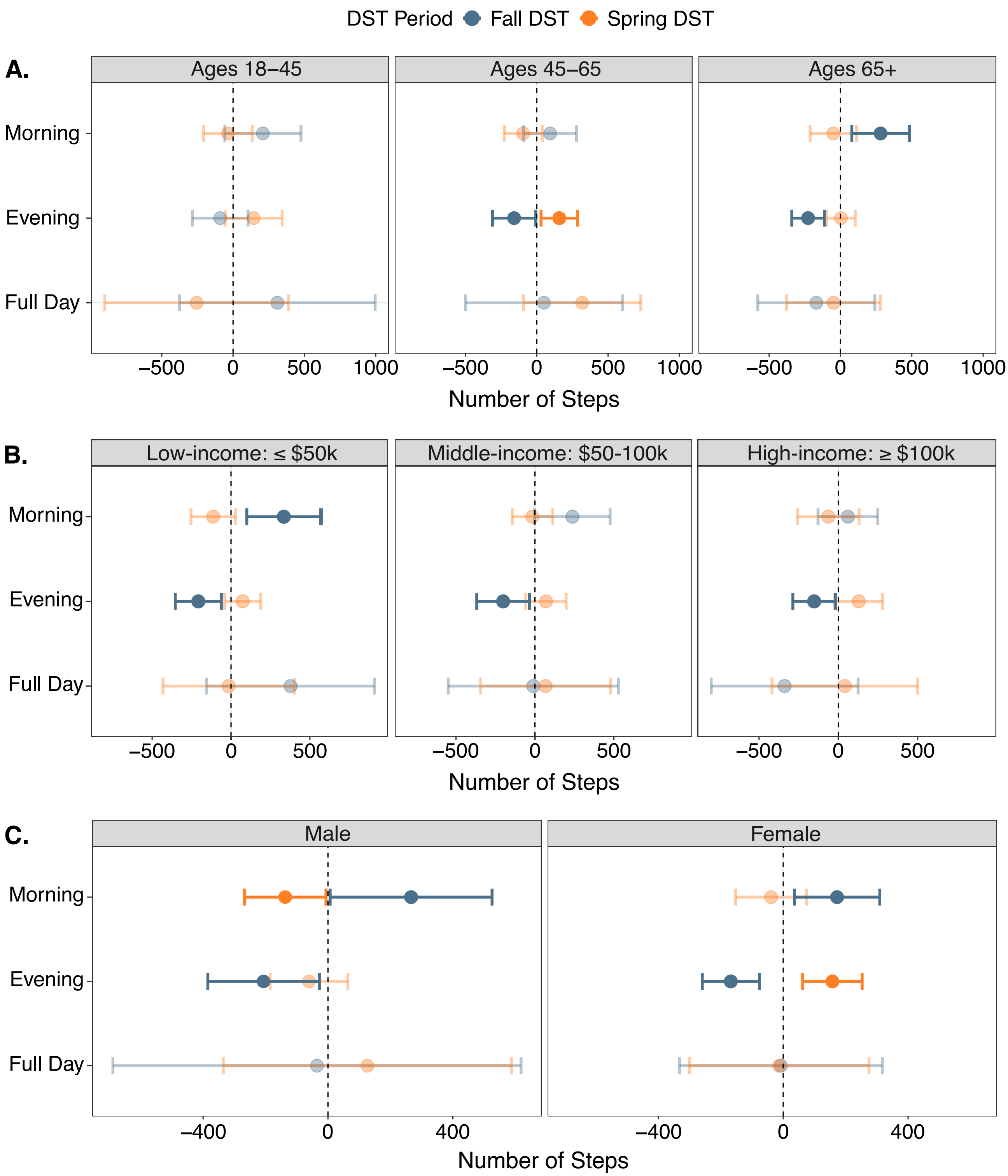
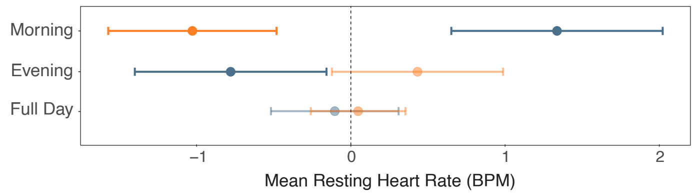


Extended Data Fig. 3 | Heterogeneous effects of Daylight Saving Time (DST) on step counts across demographic subgroups. Mean difference-in-differences (DiD) point estimates with 95% confidence intervals from two-sided t-tests across different time-of-day periods, stratified by demographic characteristics: **(a)** age group, **(b)** annual household income **(c)** sex. Effects that are not marginally statistically significant ($p \geq 0.05$) are displayed with reduced opacity. Sample size for each group is reported in Extended Data Table 2.

https://doi.org/10.1038/s44360-026-00115-z



Extended Data Fig. 4 | Effects of Daylight Saving Time (DST) transitions on resting heart rate. For each time-window, population-level mean difference-in-difference point estimates are reported with 95% confidence intervals from two-sided t-tests. Fall transition effects are reported in blue across the morning (N = 375), evening (N = 459), and full day (N = 448). Spring transition effects are reported in orange across the morning (N = 519), evening (N = 628), and full day (N = 652). Effects that are not marginally statistically significant ($p \geq 0.05$) are displayed with reduced opacity.

https://doi.org/10.1038/s44360-026-00115-z

Extended Data Table 1 | Number of participants with heart rate and step count data across Daylight Saving Transit (DST) transitions in Arizona (no DST) vs. Mountain Time states (with DST)

Year	Region	Participants with Fitbit data, N	Participants after data preprocessing, N
2023 Spring	Total	1072	787
	Arizona	424	312
	MT States	648	475
2022 Fall	Total	837	549
	Arizona	376	218
	MT States	461	331
2022 Spring	Total	812	594
	Arizona	417	290
	MT States	395	304
2021 Fall	Total	820	453
	Arizona	418	194
	MT States	402	259

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Extended Data Table 2 | Number of participants included in analyses of heterogeneous effects of DST

		Fall			Spring		
Category		**Morning**	**Evening**	**Full Day**	**Morning**	**Evening**	**Full Day**
Activity phenotype	Morning Walker	55	94	90	85	106	116
	Neutral Walker	158	194	191	209	264	283
	Evening Walker	79	68	78	113	114	120
Sidewalk Availability (among Neutral Walkers)	High	144	120	146	219	156	207
	Low	47	38	48	64	53	57
Transit Availability (among Neutral Walkers)	High	112	101	109	181	134	166
	Low	79	57	85	102	75	98
Transit Availability (among Neutral Walkers)	High	156	132	159	224	168	209
	Low	35	26	35	59	41	55
Recreation Facilities Availability (among Neutral Walkers)	High	153	127	155	222	165	204
	Low	38	31	39	61	44	60
Age Group	18–45	64	64	58	95	99	108
	45–65	104	117	127	141	165	171
	65+	116	157	157	147	190	201
Household Income	Low: ≤50k	90	103	108	137	150	162
	Middle: 50–100k	93	113	106	117	145	152
	High: ≥100k	101	122	128	129	159	166
Sex at birth	Male	84	98	99	115	141	149
	Female	200	240	243	268	312	331

Stratified counts are shown by activity phenotype for the primary analysis (Fig. 2), walkability measures among Neutral Walkers (Extended Data Fig. 2), age group (Extended Data Fig. 3a), household income (Extended Data Fig. 3b), and sex at birth (Extended Data Fig. 3c). Counts are further disaggregated by season (fall, spring) and time-of-day windows (morning, evening, full day).

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Reporting Summary

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☐	☒	For null hypothesis testing, the test statistic (e.g. F, t, r) with confidence intervals, effect sizes, degrees of freedom and P value noted *Give P values as exact values whenever suitable.*
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☒	☐	For hierarchical and complex designs, identification of the appropriate level for tests and full reporting of outcomes
☒	☐	Estimates of effect sizes (e.g. Cohen's d, Pearson's r), indicating how they were calculated

Our web collection on statistics for biologists contains articles on many of the points above.

Software and code

Policy information about availability of computer code

Data collection	We used the demographic, selected items from the social determinants of health survey, and Fitbit data from the NIH's All of Us Research Program. Our analysis used the most up-to-date Controlled Tier dataset version 8 (C2024Q3R5). Demographic data were collected during the initial enrollment survey, social determinants of health survey was voluntarily completed by the participants, and Fitbit data were collected retrospectively through a Bring-Your-Own-Device study or the WEAR study, where All of Us program handed out devices to eligible participants.
Data analysis	All codes were written in Python (ver 3.10.16) and R (ver 4.5.0). In particular, difference-in-difference analysis was conducted using the statsmodels (ver 0.14.4) in Python. ggplot2 (ver 3.5.2) and seaborn (ver 0.12.2) were used for visualizations .

For manuscripts utilizing custom algorithms or software that are central to the research but not yet described in published literature, software must be made available to editors and reviewers. We strongly encourage code deposition in a community repository (e.g. GitHub). See the Nature Portfolio guidelines for submitting code & software for further information.

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All manuscripts must include a data availability statement. This statement should provide the following information, where applicable:

- Accession codes, unique identifiers, or web links for publicly available datasets
- A description of any restrictions on data availability
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We used the demographic, survey, and Fitbit data from the NIH's All of Us Research Program. Our analysis used the most up-to-date Controlled Tier dataset version 8 (C2024Q3R5), which includes non-date-shifted intraday Fitbit data (through October 1, 2023) and the first three digits of participant ZIP codes. All analyses were performed within the secure Researcher Workbench by authorized researchers who completed the All of Us Responsible Conduct of Research training. Use of the All of Us Controlled Tier dataset was determined by the All of Us IRB not to constitute human subjects research; accordingly, this study was approved as exempt research from the Duke IRB (Protocol #2021-0574).
To ensure participant privacy, data used for this study is available to approved researchers following registration, completion of ethics training, and attestation of a data use agreement through the All of Us Research Workbench platform, which can be accessed via https://workbench.researchallofus.org/login.

Research involving human participants, their data, or biological material

Policy information about studies with human participants or human data. See also policy information about sex, gender (identity/presentation), and sexual orientation and race, ethnicity and racism.

Reporting on sex and gender	Sex (biological) was included in this study. Sex was self-reported at enrollment in the All of Us Research Program. Individual-level information was available in the All of Us Research Workbench. Sex-stratified analyses were conducted to assess whether the effects of DST differed between males (N=268) and females (N=790).
Reporting on race, ethnicity, or other socially relevant groupings	We stratify our analyses by annual household income to evaluate whether DST effects varied across population subgroups. Both variables were self-reported in the All of Us enrollment survey. Aggregated counts of race and ethnicity are provided in Table 1; however, race and ethnicity were not used as a stratification variable in the analyses.
Population characteristics	The study population was predominantly older, female, and White. Age distribution was as follows: 18-44 years (268; 23.2%), 45–64 years (406; 35.1%), and ≥65 years (483; 41.7%). Biological sex at enrollment included females (790; 68.3%), males (approximately 350-375; 30.3-32.4%), and a small number of participants with unknown sex (≤20; ≤1.7%). Race was reported as Asian (≤20; ≤1.7%), Black or African American (26; 2.2%), White (986; 85.2%), Other (41; 3.5%), and Unknown (approximately 80–90; 6.9–7.8%). Of note, the All of Us Research Program suppresses reporting of cell counts ≤2 0 to protect participant privacy; therefore, values in this range are reported as obscured or indicated as ≤20
Recruitment	Participants were recruited through the All of Us Research Program. Fitbit data were obtained primarily through the Bring-Your-Own-Device (BYOD) pathway, which may introduce self-selection bias, as individuals who can afford a device and are willing to contribute data are more likely to participate. To mitigate this potential bias, the All of Us WEAR program distributed Fitbit devices directly to participants. Individuals enrolled through both pathways were included in this study. Overall, 66% (N=763) contributed data through the BYOD program and 34% (N=394) through the WEAR program.
Ethics oversight	This study was approved by the Duke Institutional Review Board (protocol #2021-0574). In addition, since the authors were not directly involved with the participants and Researcher Workbench employs a data passport model.

Note that full information on the approval of the study protocol must also be provided in the manuscript.

Field-specific reporting

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Behavioural & social sciences study design

All studies must disclose on these points even when the disclosure is negative.

Study description	This was a quantitative observational study using demographic and Fitbit data from the NIH All of Us Research Program. We analyzed longitudinal intraday heart rate and step data surrounding US spring and fall daylight saving time (DST) transitions from 2021-2023. The study examined the changes in daily and time-specific behavior and physiology, and leveraged geographic differences in DST observance to estimate causal effects using a difference-in-difference (DiD) framework.
Research sample	The All of Us Research Program is a large initiative that collects multiple streams of health-related data, including electronic health records, genomics, physical measurements, participant surveys, and wearable device data (e.g., Fitbit), from more than 600,000 participants in the United States, with a focus on populations historically underrepresented in biomedical research. For this study, we

included participants residing in Arizona, Colorado, New Mexico, or Utah who contributed valid intraday Fitbit data following quality control procedures, enabling comparison between DST-observing and non-observing regions using high-resolution heart rate and step data. Of 59,018 participants who shared any Fitbit data, 53,812 had intraday heart rate data; 51,364 also had ZIP code information. After excluding individuals with geolocation inconsistencies, 4,375 participants residing in Arizona, Utah, Colorado, and New Mexico remained. We then included participants who contributed valid data during at least one DST transition week in 2021–2023, yielding an analytic cohort of 1,531 individuals with both intraday heart rate and step data. After applying wear-time and extreme-value exclusions (steps), 1,157 participants were included in the final analysis (see "Data exclusions").
The study population skewed towards older, female, and White individuals. Age distribution was as follows: 18-44 years (268; 23.2%), 45–64 years (406; 35.1%), and ≥65 years (483; 41.7%). Biological sex females (790; 68.3%), males (approximately 350-375; 30.3-32.4%), and a small number of participants with unknown sex (≤20; ≤1.7%). Race was reported as Asian (≤20; ≤1.7%), Black or African American (26; 2.2%), White (986; 85.2%), Other (41; 3.5%), and Unknown (approximately 80–90; 6.9–7.8%). Of note, the All of Us Research Program suppresses reporting of cell counts ≤20 to protect participant privacy; therefore, values in this range are reported as obscured or indicated as ≤20. Relevant demographic information is summarized in Table 1, year-specific sample counts are provided in Extended Data Table 1, and CONSORT diagram is provided in Supplementary Figure 1.

Sampling strategy

We use a pre-existing observational dataset, the All of Us Research Program, consisting of participants who voluntarily share their data. Sample sizes were determined by data availability, wear-time thresholds, and geolocation criteria.

Data collection

All demographic, geographic, and Fitbit data were collected as part of the All of Us Research Program; no data were collected directly by the authors. Demographic information were self-reported at enrollment. Fitbit step counts and minute-level heart rate were obtained either through the Bring-Your-Own-Device (BYOD) pathway or via All of Us–issued Fitbit devices in the WEAR program. All Fitbit data were passively collected by participants' devices and were all retrospective; no researcher had direct interaction with participants or influence on data generation.

Timing

The All of Us Research Program began enrollment in 2017, with Fitbit data contributed through two mechanisms: Bring Your Own Device (BYOD), which began in 2018 and includes retrospective data (with some records extending back to 2010), and the WEAR initiative, which prospectively collected data using study-provided devices starting in March 2021. The All of Us Research Program data collection is still in progress. There were no gaps in data collection due to study design; however, missing data may occur due to non-wear of devices by participant. The Controlled Tier dataset version 8 (C2024Q3R5) used in this study included Fitbit data recorded up to October 1, 2023. We extracted data for three DST transitions (spring and fall) from 2021–2023. For each participant, we used data from the week before and week after each DST transition, and six weeks of pre-transition data for phenotype classification.

Data exclusions

From our sample size with 1,531 unique participants, we further excluded participants who did not meet predefined wear-time thresholds (≥75%) during each DST period [n = 285 (Spring 2023), 288 (Fall 2022), 218 (Spring 2022), and 367 (Fall 2021)] . Person-days with extreme step counts (>99.9th percentile within each time window) to remove outliers for each year-DST transition[(n= 105 (Spring 2023), 60 (Fall 2022), 89 (Spring 2022), and 61 (Fall 2021) person-day-time window]. Wear-time thresholds and step-count filtering thresholds were determined during the data quality control phase (Supplementary Fig. 3).

Non-participation

This study used an existing observational dataset, and researchers conducting the analyses had no direct interaction with participants. Participants in the All of Us Research Program were not required to contribute Fitbit data; only those who opted in through the Bring-Your-Own-Device or WEAR programs were included.

Randomization

No randomization was performed by the researchers. However, exposure to daylight saving time was effectively assigned by geography: Arizona does not observe DST, whereas neighboring Mountain Time states do. This natural variation created an "as-if randomized" comparison between exposed and unexposed groups, which we leveraged in a difference-in-difference framework. Covariate differences were controlled using year-level and day-of-week fixed effects.

Reporting for specific materials, systems and methods

We require information from authors about some types of materials, experimental systems and methods used in many studies. Here, indicate whether each material, system or method listed is relevant to your study. If you are not sure if a list item applies to your research, read the appropriate section before selecting a response.

Materials & experimental systems

n/a	Involved in the study	
☒	☐	Antibodies
☒	☐	Eukaryotic cell lines
☒	☐	Palaeontology and archaeology
☒	☐	Animals and other organisms
☒	☐	Clinical data
☒	☐	Dual use research of concern
☒	☐	Plants

Methods

n/a	Involved in the study	
☒	☐	ChIP-seq
☒	☐	Flow cytometry
☒	☐	MRI-based neuroimaging

Plants

Seed stocks	*Report on the source of all seed stocks or other plant material used. If applicable, state the seed stock centre and catalogue number. If plant specimens were collected from the field, describe the collection location, date and sampling procedures.*
Novel plant genotypes	*Describe the methods by which all novel plant genotypes were produced. This includes those generated by transgenic approaches, gene editing, chemical/radiation-based mutagenesis and hybridization. For transgenic lines, describe the transformation method, the number of independent lines analyzed and the generation upon which experiments were performed. For gene-edited lines, describe the editor used, the endogenous sequence targeted for editing, the targeting guide RNA sequence (if applicable) and how the editor was applied.*
Authentication	*Describe any authentication procedures for each seed stock used or novel genotype generated. Describe any experiments used to assess the effect of a mutation and, where applicable, how potential secondary effects (e.g. second site T-DNA insertions, mosiacism, off-target gene editing) were examined.*