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Lifestyle Polarization on a College Campus: Do Liberals and Conservatives Behave Differently in Everyday Life?

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Socializing, moving, working, and leisure form the foundation of human experience. We examined whether these foundational, ostensibly nonpolitical behaviors are nevertheless bifurcated along political fault lines, revealing "lifestyle polarization." Study 1 quantified the association between political identity and 61 social, movement, work, and leisure behaviors collected from smartphone sensors and logs (i.e., GPS, microphone, calls, texs, unlocks, activity recognition) and ecological momentary assessments (i.e., querying activity level, activity type, interaction partners, locations) at multiple temporal levels (i.e., daily, mornings, afternoon, evenings, nights, weekends, weekdays) in a sample of up to 1,229 students on a college campus. We found that liberals and conservatives behave differently in everyday life; the behavioral differences were small but robust, not accounted for by other plausible factors (e.g., demographics), and most pronounced in the leisure domain. Study 2 showed that the behavioral differences between liberals and conservatives were not accurately discerned by other students, who overestimated the extent of lifestyle polarization present on their campus. Together, these studies suggest that political identity has penetrated some of the most foundational aspects of everyday life, but not to the degree that people think. We discuss how communities may feel divided not only because of deep ideological disagreements across partisan lines but also because such disagreements are accompanied by distinct lifestyles—both real and (mis)perceived—that may prevent liberals and conservatives from engaging in cross-partisan contact and developing mutual understanding.

Keywords: political identity, behavior, lifestyle polarization, smartphones, misperceptions

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Political polarization is increasing in many countries around the world (Garzia et al., 2023; Reiljan et al., 2024). This is especially true in the United States, where it has increased over time in the form of ideological polarization (e.g., divergence in cross-partisans' policy preferences and attitudes) and affective polarization (e.g., feeling positively toward co-partisans and negatively toward cross-partisans; Boxell et al., 2024; Lelkes, 2016). Both forms of polarization are also increasingly salient on university campuses, where ideological disagreements triggered by current events and distrust of the other side have led to conflict inside and outside the classroom (Ben-Porath, 2024; Twenge et al., 2016). However, stereotypical media portrayals of liberals and conservatives imply that polarization goes beyond differences in political attitudes, beliefs, and values. Familiar tropes of latte-sipping, Prius-driving liberals and gun-toting, Hummer-driving conservatives suggest that polarization even extends to lifestyle (DellaPosta et al., 2015).

Here, we examine whether lifestyle polarization extends to everyday behaviors that hold no overt political or stereotypical content. Specifically, using smartphone-based measures of behavior, we examine whether liberal and conservative students on a U.S. university campus engage in different social, movement, work, and leisure behaviors in the stream of their everyday lives. We also examine whether these everyday behaviors are accurately perceived by members of the campus community. Our data reveal that some of the most mundane and ostensibly nonpolitical aspects of everyday life are nevertheless organized along political fault lines, but not always in the ways that people think.

Why Lifestyle Polarization Matters

One might wonder why lifestyle polarization matters. In universities or other communities, the things people do every day may appear inconsequential, but they can have important consequences for social cohesion. Communities may feel divided not only because of deep ideological disagreements between cross-partisans but also because such disagreements are increasingly accompanied by distinct lifestyles that prevent the development of shared ties and contact between cross-partisans (Bennett, 1998; Hetherington & Weiler, 2018; Hunter, 1991; Pettigrew, 1998). Such cross-partisan contact will simply be less likely if liberals and conservatives are, on a day-to-day basis, doing different things.

Independent of the presence of actual lifestyle polarization, *perceived* lifestyle polarization can also undermine cross-partisan contact. For instance, people tend to project the opposite of their own lifestyle preferences onto cross-partisans (Denning & Hodges, 2022) and also rely on stereotypes about everyday behaviors (e.g., eating organic foods) to infer others' political identities (e.g., liberal; Carlson & Settle, 2022; Hiaeshutter-Rice et al., 2023). Once a person uses lifestyle choices to infer that someone's political identity is different from their own, they are subsequently less willing to converse and socialize with that person (Carlson & Settle, 2022; Lee, 2021). Although this research suggests that perceptions of lifestyle polarization matter, no research to our knowledge has investigated whether these perceptions are accurate or inaccurate.

Evidence of Lifestyle Polarization

Given the consequences of lifestyle polarization for social cohesion and other outcomes, the extent and contours of lifestyle polarization have been examined by researchers in a variety of disciplines, including psychology, political science, sociology, marketing, and economics. This past research has interpreted "lifestyle" quite loosely, often without a clear and accepted definition across disciplines (Brivio et al., 2023). Sociological research on lifestyle politics, for example, has examined everything from social attitudes toward premarital sex and homosexuality to the frequency with which people visit art museums and recycle plastic (e.g., DellaPosta et al., 2015). In marketing and economics research, much focus has been on liberals' and conservatives' consumer preferences, such as their penchant for specific brands and luxury goods (e.g., Hoewe & Hatemi, 2017; J. C. Kim et al., 2018; Rogers, 2022). Across disciplines, a great deal of research has focused on liberals' and conservatives' popular culture preferences, such as their favorite TV shows, movies, music genres, and sports teams (e.g., Rawlings & Childress, 2024; Rentfrow & Gosling, 2003; Rogers, 2020). According to these definitions of lifestyle, liberals and conservatives do indeed differ in their social attitudes, consumer preferences, and cultural tastes (Dhont & Hodson, 2014; Fox & Williams, 1974; Kannan & Veazie, 2018; North & Hargreaves, 2007; Ordabayeva & Fernandes, 2018; Rogers & Jost, 2022; Shepherd et al., 2015; Witzling & Shaw, 2019). By some measures, this form of polarization has increased over time in the U.S. population (Bertrand & Kamenica, 2023; DellaPosta, 2020). At the same time, many of the documented partisan differences in the aforementioned domains are unsurprising because they align with stereotypes—perpetuated by the media and reinforced by targeted marketing campaigns—about what it means to be liberal or conservative in the United States (e.g., conservatives listen to country music and watch Fox news).

To complement the existing research, we define lifestyle as patterns of behavior in everyday life (Veal, 1993) and focus on the kinds of nonpolitical social, movement, work, and leisure behaviors that citizens across cultures and contexts engage in as a matter of routine. We know surprisingly little about partisan differences in such behaviors, even though they must play an important role in determining the level of cross-partisan contact within communities.

The limited emphasis on liberals' and conservatives' everyday patterns of behavior may be a consequence of much of the lifestyle polarization literature's methodological reliance on self-reports. Self-reports lend themselves more easily to the measurement of tastes, preferences, attitudes, and intentions than to the measurement of real-world behavior (Baumeister et al., 2007; Furr, 2009; Gosling et al., 1998). As a result, it is unclear the extent to which lifestyle polarization on university campuses or elsewhere extends beyond the minds of liberals and conservatives to encompass the things they actually do in their everyday lives (see Carney et al., 2008, for an important exception). This is important because preferences and intentions do not always translate into behavior. For instance, research shows that liberals say they prefer urban and racially diverse neighborhoods, but this is not reflected in where they actually move (Mummolo & Nall, 2017). Discrepancies between preferences and behaviors may be caused by a variety of factors, including the limitations of self-report such as social desirability and memory biases (Paulhus & Vazire, 2007).

To examine actual behavior and address the limitations of self-report, researchers have begun studying the behavioral traces liberals and conservatives leave in their digital environments. They find that liberals and conservatives differ in their social media messages (Sterling et al., 2020), follows (Boutyline & Willer, 2017; Shi et al., 2017), and likes (Kosinski et al., 2013; Praet et al., 2022);

their profile bios (Essig & DellaPosta, 2024) and photos (Kosinski, 2021); as well as their web searches (Bi et al., 2013) and streamed videos (Wojcieszak et al., 2023). However, even this innovative research largely captures behaviors that people engage in online, shedding light on only a fraction of everyday life. Moreover, because research on digital traces relies on data from large samples of internet users living in communities across the country, it is unclear how differences in users' online activity matter for cross-partisan contact in their immediate community.

Opportunities Presented by Smartphone-Based Methods

Against this backdrop, smartphones offer new and untapped opportunities for capturing a broad array of everyday behaviors as they occur both online and offline. Smartphones can collect information longitudinally about individuals' social behavior, mobility behavior, and other ostensibly nonpolitical activities from onboard sensors (e.g., GPS, microphone) and logs (e.g., calling, texting, unlocks, Harari et al., 2016). Such information can be complemented with ecological momentary assessments (EMAs) delivered to individuals' devices to obtain a more comprehensive assessment of liberals' and conservatives' everyday behaviors *in situ* (Harari & Gosling, 2023). Smartphone sensing and EMA each offer unique advantages. Sensor and log data can provide behavioral information that respondents themselves would have difficulty reporting (e.g., average length of incoming text messages in characters), while EMA data can provide behavioral information that is difficult to collect from sensors or logs (e.g., whether one has spent time with a significant other, which requires a subjective assessment of whether the other is "significant"). Smartphone-based methods can be cumbersome to deploy in nationally representative samples but have been deployed effectively in large student samples (e.g., Harari, Müller, et al., 2020; Müller et al., 2020), making them well-suited for studying lifestyle polarization in a university setting.

Another benefit of the intensive longitudinal methods enabled by smartphones is their ability to capture temporal differences in people's everyday lives (Schoedel et al., 2020; Schoedel & Mehl, 2023). This is relevant because prior research shows reliable associations between political identity and self-reported chronotype (i.e., morningness and eveningness; Ksiazkiewicz, 2020). Conservatives tend to report that they are "morning larks," whereas liberals tend to report being "night owls," so lifestyle polarization could emerge not only as a result of differences in *what* liberals and conservatives do but also *when*. Even if liberals and conservatives in a community engage in similar behaviors, doing so at different times of the day or week could yield fewer opportunities for cross-partisan contact and thus fewer opportunities for developing shared ties and mutual understanding. Despite this possibility, no work to our knowledge has investigated whether the behavioral tendencies of liberals and conservatives differ depending on the time of day or week.

Evidence of (Mis)Perceived Polarization

As previously mentioned, research has examined perceptions of lifestyle polarization but not whether these perceptions are accurate or inaccurate. This is somewhat surprising because there is a great deal of research examining whether other forms of polarization are accurately perceived. This research finds that people overestimate the extent to which liberals and conservatives differ across a wide array of attributes and beliefs (Ahler & Sood, 2018; Levendusky & Malhotra, 2016; Mernyk et al., 2022; Novoa et al., 2023), and such misperceptions predict reduced trust and poor cross-partisan evaluations (Westfall et al., 2015). In fact, misperceived polarization can be more strongly related to negative cross-partisan outcomes than actual polarization (Enders & Armaly, 2019). Overestimating ideological differences between liberals and conservatives can even cause people to adopt more extreme political positions, producing a self-fulfilling prophecy wherein misperceptions of polarization become realized (Ahler, 2014). In contrast to the proliferation of studies on misperceptions of ideological and other forms of polarization, whether lifestyle polarization is misperceived—with similarly detrimental effects for cross-partisan relations—remains unknown.

One fundamental challenge when studying the accuracy with which people perceive lifestyle polarization is the need for a "truth criterion." To understand whether lifestyle is misperceived, one first needs to know how liberals and conservatives truly act in everyday life. In the related domain of ideological polarization, researchers have examined misperceptions by comparing people's perceptions of cross-partisans' positions on various issues to cross-partisans' true positions on those issues, as measured in comprehensive panel studies (Westfall et al., 2015). Thus, one benefit of using smartphone-based or other ecologically valid methods to measure the lifestyles of liberals and conservatives is that these measures can then serve as the truth criterion against which perceptions of lifestyle can be compared. Measuring perceptions of lifestyle, in turn, can help researchers empirically evaluate the extent to which any lifestyle polarization revealed by smartphones is (or is not) surprising, mitigating hindsight bias (DellaVigna et al., 2019; DellaVigna & Pope, 2018).

The Present Research

In summary, to understand whether lifestyle polarization occurs in ways that undermine cross-partisan contact and social cohesion, a comprehensive portrait of liberals' and conservatives' everyday behaviors is needed. Ideally, this portrait would include a wide range of everyday behaviors in which liberals and conservatives frequently engage at different times of the day and week. Evidence of actual lifestyle polarization could then be compared to perceptions of lifestyle polarization, recognizing that perceptions do not always track reality and can have important consequences of their own. We address these issues with descriptive data from two preregistered, exploratory studies.

Study 1 investigated *actual* lifestyle polarization by quantifying associations between political identity and 61 behavioral tendencies in everyday life (a) across a broad range of ostensibly nonpolitical domains (i.e., social, movement, work, and leisure behavior), (b) at multiple temporal levels (i.e., mornings, afternoons, evenings, nights, weekends, and weekdays), (c) accounting for potential individual-level confounds (e.g., gender, ethnicity, socioeconomic status [SES], relevant personality traits), (d) while holding potential geographic confounds constant (e.g., population density, campus residence), and (e) using ecologically valid methods that measure behavior in their natural context (i.e., smartphone sensing, EMA). Study 2 investigated *perceived* lifestyle polarization by asking participants to rate the extent to which other liberal or conservative students in their campus community engage more in each of the behavioral tendencies measured in Study 1. We probed consensus and accuracy of these perceptions, comparing participants' perceptions measured in Study 2 to the actual extent to which liberals or conservatives engaged more in each behavior as measured in Study 1. Together these studies provide a unique opportunity to study how lifestyle is behaviorally bifurcated (and perceived to be bifurcated) along political fault lines on a college campus.

Both studies draw on samples of undergraduates at the University of Texas at Austin, which limits the generalizability of our findings but offers some important advantages. Polarization and political conflict on university campuses—triggered by current events and resulting in the harassment of students and faculty—have permeated media headlines, discourse among political leaders, and the public imagination. As such, polarization in a university setting deserves careful empirical scrutiny. Here, we study lifestyle polarization on a public university campus with a substantial proportion of conservatives and with a diverse student body that is demographically similar in many ways to nationally representative samples of students (see Supplemental Table S1). Finally, the university setting provides a surprisingly stringent test of actual lifestyle polarization (Study 1) and (mis)perceived lifestyle polarization (Study 2), as we will explain.

A Stringent Test of Actual Lifestyle Polarization (Study 1)

Although prior research has found empirical support for lifestyle differences between liberals and conservatives, it is not clear that lifestyle polarization will emerge in the university setting examined here. This is because several elements of Study 1's research design produce a particularly stringent test of lifestyle polarization.

First, our participants are students who are of roughly the same age, have a similar educational background, live in the same city, and occupy the same social role. These similarities ensure that the educational, geographic, and occupational differences, which would be present in more representative samples, cannot account for any behavioral differences we may observe between liberals and conservatives. Statistically controlling for gender, ethnicity, and SES further ensures that observed behavioral differences between liberals and conservatives are not caused by demographic factors that are typically associated with political identity. It is also worth noting that liberals and conservatives who go to college may be more similar to each other on a variety of nondemographic dimensions (e.g., values, worldview) than would be the case in a more representative sample. Again, similarities on nondemographic dimensions may produce similarities in the lifestyles of college conservatives and liberals.

Second, relying on a student sample restricts the extent to which liberals and conservatives could lead different lifestyles, which should make identifying behavioral differences between liberals and conservatives particularly difficult. Unlike representative samples where the range of participants' everyday activity is not restricted by the rhythms of classes and campus life, our participants must engage in similar kinds of activities as members of the same student body.

Third, the behaviors we measured were selected because they were available from smartphones, not because we expected them to be associated with political identity. These measures capture the *structure* more so than the *content* of everyday behavioral patterns (e.g., whether and when people watch TV, rather than which shows they watch; whether and when they go to bars, rather than which bars they go to). As such, these measures will not capture content-based differences in lifestyle. Moreover, the behaviors we measured hold no overt political relevance, unlike studies of lifestyle polarization that include measures with clear political overtones (e.g., attitudes toward abortion and gay marriage; DellaPosta, 2020) or stereotypical content (e.g., conservatives listen to country music; Rawlings & Childress, 2024).

Last, our method of using real-world behavioral data collected in context together with political identity measured in a one-time survey should yield smaller effects due to the lack of common method bias (Podsakoff et al., 2024). That is, shared variance between our independent and dependent variables should be lower than in prior lifestyle polarization studies that rely exclusively on surveys (DellaPosta et al., 2015; Lee, 2021; Witzling & Shaw, 2019) or exclusively on digital traces (Praet et al., 2022; Sterling et al., 2020) to measure both political identity and lifestyle.

Together, these features of our research design strengthen the study's internal validity by isolating the impact of political identity on lifestyle, while producing smaller but perhaps more realistic effect size estimates.

Despite the strength of the study's internal validity, interpretative caution is warranted given the study's limited external validity. Therefore, when interpreting our results, we focus more on whether there is a general pattern of behavioral difference between liberals and conservatives and less on specific indicators of behavioral difference. Our claim is not necessarily that the specific behavioral differences we observe will generalize to other contexts but rather that if a general pattern of lifestyle difference emerges on a college campus where the liberals and conservatives are similar, then lifestyle differences are also likely to emerge in some form in other contexts where liberals and conservatives are considerably more different from each other (see the General Discussion section for more information about when and why we would or would not expect our results to generalize). Our interpretative focus on a general pattern of behavioral difference rather than on specific indicators of behavioral difference is consistent with accepted definitions of lifestyle as *a set of behavioral patterns* (Brivio et al., 2023; Jensen, 2009; Stebbins, 1997). More importantly, this focus is consistent with the idea that what matters for the likelihood of cross-partisan contact is that partisan lifestyle differences exist at all, not which specific lifestyle behaviors liberals and conservatives engage in.

A Stringent Test of (Mis)Perceived Lifestyle Polarization (Study 2)

Although prior work has found that people misperceive ideological and other forms of polarization, it is not clear that lifestyle polarization will be similarly misperceived in the present university setting. We simply do not know whether observers' misperceptions generalize beyond attitudes and beliefs to include misperceptions about lifestyle. However, it is also unclear whether lifestyle polarization will be misperceived because Study 2's research design presents a particularly stringent test of inaccuracy, such that it may be harder for observers to be inaccurate.

Specifically, Study 2 asks observers about their perceptions of the lifestyles of liberals and conservatives within their own campus community, whereas prior research asks observers about their perceptions of partisans in the general population. Observers may perceive partisans in their community more accurately than partisans in the general population because they may have better information about the former than the latter (Funder, 1995; Talaifar et al., 2021; West & Kenny, 2011). When judging partisans in the general public, observers must rely on exaggerated media portrayals and stereotypes. When judging partisans in their community, observers can rely on first-hand information and experiences with community members. In addition to having better information about liberals and conservatives in one's community than in the general public, observers should also be more motivated to accurately understand members of their own community (Swann, 1984). People have the opportunity to come into contact with liberals and conservatives in their community, and holding accurate perceptions of community members should help them achieve their goals during these social interactions. Thus, Study 2's research design allows us to examine whether inaccurate perceptions of polarization persist even under circumstances where accuracy is likely—in observers' perceptions of their own community.

Again, our interpretative focus lies more in identifying a general pattern of (in)accurate perceptions across behaviors than in specific instances of (in)accurate perceptions of specific behaviors. This focus is motivated by the fact that general patterns of (in)accuracy may be more likely to generalize to nonuniversity settings than specific instance of (in)accuracy, which may be a unique to our setting.

Open Science Practices and Ethics Approval

We followed open science guidelines in this research. Data, code, and other materials for this article are available at https://osf.io/rf9k8/. We preregistered all research questions, data cleaning and aggregation steps, and statistical analyses for Study 1 and Study 2 here and here, respectively. The methods and results reported in this article followed all aspects of these preregistrations except for the deviations that we report in the Supplemental Materials (Supplemental Supporting Texts A and B). Still, we describe our studies as "exploratory" because we did not have a priori hypotheses about whether or to what extent actual and perceived lifestyle polarization would be observed, and the Study 1 preregistration was submitted after the Fall (but not Spring or pooled) sample was analyzed.

Study 1 was approved by the University of Texas at Austin IRB No. 2012070064 and Stanford University IRB No. 54300. Study 2 was approved by the University of Texas at Austin IRB No. STUDY00004065. Participation in both studies was voluntary. Parts of the data set reported in Study 1 have been published in prior research (Harari, Müller, & Gosling, 2020; Harari, Vaid, et al., 2020; Matz & Harari, 2021; Roehrick et al., 2023; W. Wang et al., 2018). The current article differs substantively from this previously published research in its focus on political orientation. Neither the political orientation data nor the associations between political orientation and any other variables have been published previously.

Study 1: Actual Lifestyle Polarization

To measure actual lifestyle polarization, Study 1 correlated participants' political orientation (i.e., their self-placement from 1 = "extremely liberal" to 7 = "extremely conservative") and their everyday behaviors. We measured 61 behaviors in the social, movement, work, and leisure domains across 2 weeks at the daily, time-of-day, and time-of-week levels using a research app that participants downloaded onto their smartphones. The app collected behavioral data from the smartphones' sensors and logs (i.e., passive sensing of GPS, microphone, calling, texting, unlocks, activity recognition) and EMA surveys or "EMAs" completed by the participant (i.e., active logging of activity level, activity type, interaction partners, locations). Figure 1 provides an overview of Study 1 data collection methods, and Table 1 provides a list of each behavioral tendency measured, including its data source, description, sample size, and descriptive statistics. (Note that correlations between political orientation and behavioral tendencies from Study 1 will be used as the truth criterion in Study 2, where we will examine whether perceptions of lifestyle polarization are accurate.)

Method

Participants

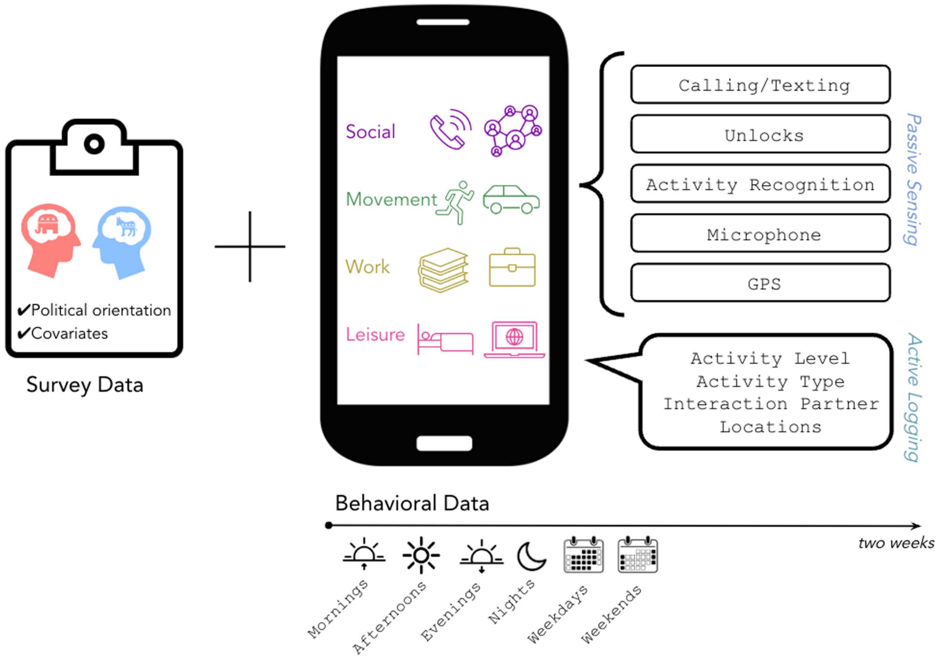
Participants were undergraduates enrolled in a large introductory psychology class at the University of Texas at Austin during two academic semesters: Fall 2016 ($N = 905$ participants) and Spring 2017 ($N = 514$ participants). This class fulfills a degree requirement for undergraduates and therefore includes students from a wide variety of majors. Data from the two semesters were pooled for a total possible $N = 1{,}419$ (i.e., participants who had political orientation and behavioral measures after all exclusions). The final sample was 61% women, 38% men, and 0.6% another gender; 38% White, 5% Black, 21% Hispanic, 23% Asian, 0.1% Native American, 0.1% Pacific Islander, 2% another ethnicity, and 12% of mixed ethnic background; 5% lower class, 17% working class, 39% middle class, 32% upper middle class, and 7% upper class. Participants with smartphones in the study were mostly iPhone users (80%), with Android users (19.5%) and other smartphone users (0.50%) comprising a minority of the sample. The smartphone app used in this study could collect data only from iOS and Android smartphone sensors or logs, so participants who did not have one of these operating systems could not download the app and provided only EMA data via email. See Supplemental Table S1 for further information about the demographic makeup of the sample and for comparisons to nationally representative student and nonstudent samples.

Prior to exclusions, of the 1,565 students who completed the demographic survey and indicated their political orientation, 1,540 students (i.e., 98%) provided behavioral data via smartphone sensing and/or EMA before exclusions. Thus, the rate at which students were willing to participate was very high, especially as compared to prior studies which found rates of actual willingness to participate in smartphone tracking among the general public to be as low as 10.8%–16.4% (Elevelt et al., 2019; Keusch et al., 2022). The political orientation of these students did not differ significantly from the political orientation of the 25 students (i.e., 2%) who chose not to provide their behavioral data, $t(25) = .51$, $p = .62$. The sample was about 53% liberal, 27% moderate, and 20% conservative, which is similar to the political identification of nationally representative U.S. university students.

Procedure

As part of their course activities during the academic semester, students completed a broad array of surveys measuring demographic characteristics (e.g., age, gender) and psychological constructs (e.g., political orientation, personality; see Supplemental Supporting Text G). They also provided informed consent for their data to be

Figure 1
Study 1 Methods: Measuring Lifestyle Polarization With Smartphones



Note. In Study 1, participants (N = up to 1,229 students) completed a survey in which they indicated their political identity (independent variable) and demographic characteristics (covariates). They then downloaded an app onto their smartphones that collected behavioral data from their phones' sensors and logs (i.e., passive sensing of GPS, microphone, calling, texting, unlocks, activity recognition) and ecological momentary assessments (i.e., active logging of activity level, activity type, interaction partners, locations up to four times a day). Behavioral data collection occurred over 2 weeks and was used to characterize participants' social, movement, work, and leisure tendencies (61 behaviors total) at the time-of-day level (i.e., mornings, afternoons, evenings, nights), time-of-week level (i.e., weekdays, weekends), and the daily level (i.e., all times of day and week). See the online article for the color version of this figure.

used for research. In the research reported here, we focus primarily on the participants' demographic characteristics and political orientation. In addition, students completed a course assignment in which they used self-tracking to monitor their lifestyle behaviors. In this assignment, data about lifestyle behaviors were collected over a 2-week period using EMAs and smartphone sensing methods. The 2-week study period did not include any student holidays.

To complete the assignment, participants could use one of two self-tracking modalities: (a) the CampusLife smartphone app (based on the StudentLife app; R. Wang et al., 2014), which delivered notifications to respond to EMA surveys and collected smartphone sensing data, or (b) emailed Qualtrics surveys, which only delivered notifications to respond to EMA surveys. Participation in the surveys and self-tracking component of the course assignment was voluntary. The main incentive for participants was personalized feedback about their survey responses and self-tracking data. Students who did not wish to participate in the data collection via smartphone app or email completed the assignment via a handwritten journal and were not included in our sample.

Measures

Independent Variable and Covariates: One-Time Survey Data. The independent variable is political orientation ($M = 3.43$, $SD = 1.26$), which was measured on a unidimensional, bipolar, 7-point scale. Participants were asked, "How would you characterize your political orientation?" Response options were reverse-coded such that higher numbers indicate greater conservatism, with 1 = "extremely liberal" and 7 = "extremely conservative." Single-item measures such as this one are among the most common and well-established ways of measuring political orientation (see Carney et al., 2008; Jost, 2006; Jost et al., 2009). Participants reported their gender, ethnicity, and SES in the same survey in which they reported their political identity (see Supplemental Supporting Text F).

Demographic variables are included as covariates due to their potential confounding effects on the relationship between political orientation and behavioral tendencies. Gender was coded as a binary variable (0 = "male," 1 = "female"), and participants selecting "other" ($N = 8$) were not included in the analyses. Participants were asked to select one or more ethnic group(s) from the following options: "African American/Black," "Asian/Asian American," "Hispanic/Latino," "Anglo/White," "Native American," "Pacific Islander," and "Other." These responses were coded 1 = "White" and 0 = "any other ethnicity," including if they selected multiple ethnic groups. Participants reported their subjective socioeconomic class ($M = 3.19$, $SD = 0.97$) on a 1 = "lower" to 5 = "upper" scale. Conservatives were more likely to be male, $t(1142.7) = 2.95$, $p = .003$; White, $t(984.6) = -5.14$, $p < .001$; and upper class, $r(1417) = .12$ [.08, .16], $p < .001$.

Table 1
Descriptive Statistics for Daily Behavioral Tendencies Measured via Smartphone Sensing and EMA

Domain	Variable	Description and data source	*N* (participants)	*N* (days)	Minimum	Maximum	*M*	*SD*
Social behavior								
Calling and texting	Receiving text messages (frequency)[a]	Number of incoming text messages (text logs)	142	1,988	0	113.91	18.71	21.72
	Receiving text messages (length)[a]	Length in characters of incoming text messages (text logs)	142	1,988	0	1319.20	217.09	173.11
	Sending text messages (frequency)[a]	Number of outgoing text messages (text logs)	142	1,988	0	94.11	13.63	18.04
	Sending text messages (length)[a]	Length in characters of outgoing text messages (text logs)	142	1,988	0	696.79	137.71	138.06
	Receiving phone calls (frequency)[a]	Number of incoming phone calls (call logs)	142	1,988	0	4.73	1.05	1.09
	Receiving phone calls (duration)[a]	Duration of incoming phone calls in hours (call logs)	142	1,988	0	2.33	0.08	0.23
	Making phone calls (frequency)[a]	Number of outgoing phone calls (call logs)	142	1,988	0	11.42	1.53	1.71
	Making phone calls (duration)[a]	Duration of outgoing phone calls in hours (call logs)	142	1,988	0	1.62	0.11	0.21
Socializing	Being alone	"I spent MOST of my time with the following people": "No one, alone" (interaction partner EMA)	1,229	10,442	0	1.00	0.32	0.18
	Being with family	"I spent MOST of my time with the following people": "Family" (interaction partner EMA)	1,229	10,442	0	0.70	0.06	0.10
	Being with friends	"I spent MOST of my time with the following people": "Friends" (interaction partner EMA)	1,229	10,442	0	1.00	0.20	0.17
	Being at a friend's house	"I spent MOST of my time in the following place": "Friend's house" (location EMA)	1,229	10,442	0	0.54	0.03	0.07
	Being with roommates	"I spent MOST of my time with the following people": "Roommates" (interaction partner EMA)	1,229	10,442	0	0.60	0.10	0.12
	Being with a significant other	"I spent MOST of my time with the following people": "Significant other" (interaction partner EMA)	1,229	10,442	0	0.86	0.06	0.13
	Being with strangers	"I spent MOST of my time with the following people": "Strangers" (interaction partner EMA)	1,229	10,442	0	0.33	0.03	0.06
	Socializing	"I spent MOST of my time": "Talking, texting, socializing" (activity type EMA)	1,229	10,442	0	0.73	0.11	0.10
	Being in or near conversations (frequency)[a]	Number of conversations detected from the ambient sound of voices in the environment (microphone)	740	10,374	0	209	17.68	13.05
	Being in or near conversations (duration)[a]	Duration of conversations detected from the ambient sound of voices in the environment in hours (microphone)	740	10,374	0	15.43	2.26	1.82
Movement behavior								
Physical activity	Exercising	"I spent MOST of my time": "Exercising, physical activity, sports" (activity type EMA)	1,229	10,442	0	0.40	0.04	0.06
	Biking[a]	Detected biking duration in hours (activity recognition API)	741	10,388	0	3.32	0.15	0.32
	Running[a]	Detected running duration in hours (activity recognition API)	602	8,442	0	1.57	0.02	0.07
	Walking[a]	Detected walking duration in hours (activity recognition API)	741	10,388	0	5.94	1.40	0.85

(*table continues*)

Table 1 (*continued*)

Domain	Variable	Description and data source	*N* (participants)	*N* (days)	Minimum	Maximum	*M*	*SD*
	Being stationary[a]	Detected not moving duration in hours (activity recognition API)	741	10,388	0	23.21	16.22	4.69
	Being active	"Have you been sedentary (sitting, reclining) or active (on your feet, walking)?" (activity level EMA) "1—Almost always sedentary to 5—Almost always active"	1,229	10,442	1	5.00	2.23	0.56
	Being at the gym	"I spent MOST of my time in the following place": "Gym" (location EMA)	1,229	10,442	0	0.26	0.02	0.05
Geographic mobility	Driving[a]	Detected in vehicle duration in hours (activity recognition API)	741	10,388	0	21.91	0.90	1.38
	Being in the car	"I spent MOST of my time in the following place": "Vehicle" (location EMA)	1,229	10,442	0	0.32	0.03	0.06
	Changing locations[a]	Number of location changes (GPS)	771	6,149	1	31.00	6.23	3.33
	Visiting locations[a]	Number of locations visited (GPS)	771	6,149	1	9.12	3.46	1.41
	Routinely visiting locations[a]	Routine index, quantifying how different the locations visited by the person today are to the locations visited during the same time interval yesterday, with higher numbers indicating a lower degree of similarity (GPS)	771	6,149	0	100.17	29.48	21.48
	Spending time evenly between locations[a]	Location entropy, measuring how evenly a participant's time was distributed over location clusters, with higher numbers indicating a more even distribution of time spent over different location clusters (GPS)	771	6,149	0	1.89	0.65	0.31
	Spending time evenly between locations[a] (normalized)	Location entropy, normalized by the log of the total number of location clusters (GPS)	771	6,149	0	0.93	0.49	0.18
	Traveling between farthest locations[a]	Maximum distance in kilometers between two locations measured as the largest distance between any pair of latitude and longitude coordinates visited (GPS)	772	6,149	0	1810.97	16.60	92.70
	Traveling distance from home[a]	Maximum distance in kilometers from the location cluster labeled home (i.e., the location where an individual is most often found at 2:00 a.m., 6:00 a.m., and 8:30 p.m. during weekdays) and the most distant location visited (GPS)	725	5,879	0.16	1871.66	39.60	132.76
	Traveling distance[a]	Total distance covered, measuring the sum of distance in kilometers between each longitude and latitude pair and subsequent pair; the overall length of the path that connects the GPS coordinates a person visited (GPS)	772	6,149	0	35.02	0.40	1.69
	Spending time at each location[a]	Average duration spent at each location in hours (GPS)	771	6,149	0.01	0.38	0.10	0.05
	Being in transit[a]	Average duration spent moving between locations in hours (GPS)	772	6,149	0	4.87	1.10	0.70
Work behavior								
Working	Commuting	"I spent MOST of my time": "Commuting, traveling" (activity type EMA)	1,229	10,442	0	0.38	0.05	0.07
	Working	"I spent MOST of my time": "Working at a job" (activity type EMA)	1,229	10,442	0	0.74	0.03	0.07

(*table continues*)

Table 1 (*continued*)

Domain	Variable	Description and data source	*N* (participants)	*N* (days)	Minimum	Maximum	*M*	*SD*
	Being at a workplace	"I spent MOST of my time in the following place": "Work" (location EMA)	1,229	10,442	0	0.74	0.02	0.06
	Being with coworkers	"I spent MOST of my time with the following people": "Coworkers" (interaction partner EMA)	1,229	10,442	0	0.74	0.02	0.06
Studying	Attending classes/meetings	"I spent MOST of my time": "Attending classes, meetings" (activity type EMA)	1,229	10,442	0	0.86	0.20	0.13
	Studying/reading	"I spent MOST of my time": "Studying, reading, preparing for an exam" (activity type EMA)	1,229	10,442	0	0.90	0.22	0.15
	Being on campus	"I spent MOST of my time in the following place": "Campus" (location EMA)	1,229	10,442	0	0.96	0.27	0.16
	Being at the library	"I spent MOST of my time in the following place": "Library" (location EMA)	1,229	10,442	0	0.71	0.03	0.08
	Being with classmates	"I spent MOST of my time with the following people": "Classmates, students" (interaction partner EMA)	1,229	10,442	0	0.90	0.20	0.14
Leisure behavior								
Domestic pursuits	Doing chores/errands	"I spent MOST of my time": "Doing household chores, running errands" (activity type EMA)	1,229	10,442	0	0.68	0.05	0.07
	Resting/napping	"I spent MOST of my time": "Resting, napping, doing nothing" (activity type EMA)	1,229	10,442	0	0.63	0.11	0.10
	Being at home	"I spent MOST of my time in the following place": "Home (dorm, apartment)" (location EMA)	1,229	10,442	0	1.00	0.45	0.19
	Being at home[a]	Proportion of time spent at home (i.e., the location an individual is most often found at 2:00 a.m., 6:00 a.m., and 8:30 p.m. during weekdays; GPS)	771	6,149	0	1.00	0.62	0.25
Recreational pursuits	Being in noisy places[a]	Ambient noise level, 0–200,000+ (microphone)	737	10,332	457.3	931406.5	190878.2	134297.1
	Being around loud voices[a]	Ambient voice level (microphone)	712	9,982	0	13.18	2.69	1.67
	Being at bars/parties	"I spent MOST of my time in the following place": "Bar, party" (location EMA)	1,229	10,442	0	0.21	0.01	0.02
	Being at restaurants	"I spent MOST of my time in the following place": "Cafe, restaurant" (location EMA)	1,229	10,442	0	0.67	0.05	0.07
	Being at fraternities/sororities	"I spent MOST of my time in the following place": "Fraternity, sorority house" (location EMA)	1,229	10,442	0	0.83	0.02	0.06
	Being at religious facilities	"I spent MOST of my time in the following place": "Religious facility" (location EMA)	1,229	10,442	0	0.25	0.01	0.03
	Being at stores	"I spent MOST of my time in the following place": "Store, mall" (location EMA)	1,229	10,442	0	0.34	0.02	0.04
Media use	Browsing the internet/social media	"I spent MOST of my time": "Browsing the internet, using social media" (activity type EMA)	1,229	10,442	0	0.78	0.07	0.09
	Watching TV/movies	"I spent MOST of my time": "Watching TV, movies" (activity type EMA)	1,229	10,442	0	0.53	0.05	0.08
	Using smartphone (frequency)[a]	Number of times the device is unlocked (unlock logs)	821	11,508	0	344.00	68.90	53.05
	Using smartphone (duration)[a]	Duration device is unlocked in hours (unlock logs)	821	11,508	0	21.17	2.57	2.07

Note. Variables represent participants' average daily tendency to engage in each behavior during the 2-week study period. The specific data source for each variable is listed in parentheses. Data from the Fall sample and Spring sample are pooled. *N*s reflect the sample size for each variable after data exclusions. EMA = ecological momentary assessment; API = Applicaton Programming Interface.
[a] Variables measured via smartphone sensors and logs. All other variables were measured via EMA questions that asked participants to reflect on whether they engaged in each behavior in the past hour (Fall sample) or in the past 15 min (Spring sample).

Additionally, we conducted targeted exploratory (not preregistered) analyses where it was theoretically relevant to control for religiosity ($M = 2.50$, $SD = 1.15$), campus residence (51% lived on campus), and two personality traits—openness to experience ($M = 3.36$, $SD = 0.57$) and conscientiousness ($M = 3.22$, $SD = 0.57$)—known to correlation with political orientation. Participants reported their religiosity on a 1 = "I am not at all religious" to 5 = "I am extremely religious scale." To indicate their current residence, participants responded to the question "Where are you currently living in Austin?" and selected from the following options: "parent's home," "dorm," "fraternity/sorority," "apartment," "co-op," "house," and "other." If participants indicated that they lived in a "dorm" or "fraternity/sorority," their campus residence was coded 1; all other responses were coded 0. Personality traits were measured using Big Five Inventory (BFI-44; John & Srivastava, 1999). Conservatives were more religious, $r(1417) = .35$ [.30, .40], $p < .001$; more conscientious, $r(1091) = .06$ [.03, .09], $p < .001$; and less open to experience, $r(1090) = -.08$ [−.11, −.05], $p < .001$ than liberals. Conservatives and liberals were equally likely to live on campus, $t(1417) = -0.15$, $p = .881$.

Dependent Variables: Smartphone-Based Behavioral Data. The dependent variables are 61 behavioral tendencies measured via EMA or smartphone sensors/logs. To provide an organizing framework, we categorized each of the 61 behavioral tendencies into one of four domains: social, movement, work, or leisure. This organizing framework is used to guide the interpretation of results and is not an empirically driven taxonomy. Indeed, some variables could arguably belong to multiple domains or subdomains.

We focus on average behavioral tendencies across the study period, adopting a dispositional view of behavior (Buss & Craik, 1983) wherein repeated assessments capture the tendency for individuals to engage in social, movement, work, and leisure behaviors over time. This dispositional view is warranted given research showing that people behave highly consistently from week to week (Fleeson, 2004), that political orientation can be considered the kind of stable individual difference (Peterson et al., 2020) that could predict behavior over time, and definitions of lifestyle that emphasize habitual behavior (Brivio et al., 2023). To represent participants' behavioral tendencies, data collected via EMA and smartphone sensing at the momentary level were aggregated to the daily level, time-of-day level, and time-of-week level.

Smartphone Sensing Data. Of the 61 behavioral tendencies, 30 were measured via smartphone sensor and log data: activity recognition, microphone, GPS, and phone logs. The CampusLife app used to collect the data had the following system requirements: iOS Version 9 or higher for iPhone users and Version 4.4 ("KitKat") or higher for Android users.

The activity recognition data provided information about the duration of physical activity behaviors detected using the Android and iOS activity recognition Applicaton Programming Interfaces (Google Activity Recognition API, 2017; iOS Core Motion, 2017). These five variables included the duration of stationary behavior, as well as specific physical activities of walking, running, biking, and driving.

The microphone data provided information about conversational behaviors and the ambient sound in the surrounding environment detected using an on-device audio classifier developed in prior work (Lane et al., 2012; Rabbi et al., 2011). The microphone sensor on participants' smartphones was sampled every third minute (on for 1 min, off for 2 min), and an audio classifier was applied to infer users' duration of time spent around other voices (vs. silence or noise) and the frequency of separate instances of conversation (R. Wang et al., 2014). The application saved the audio inferences as a "0" for silence, "1" for noise, "2" for voices, and "3" for unknown. The four variables derived from the microphone data included the frequency and duration of conversations detected, as well as the presence of voices and degree of ambient noise in the environment (R. Wang et al., 2014).

The GPS data provided information about the geographic mobility of participants during the study period. The GPS data were captured as a series of time-stamped longitude and latitude coordinates and were scheduled to collect one sample every 10 min. As described in more detail in our previous work (Müller et al., 2020), we computed a standard set of mobility features that aggregated raw GPS data into more interpretable behavioral variables that reflect an individual's movement patterns. The 11 variables we focus on here included the distances traveled (in general, and to and from home), the number of locations visited, and the distribution of time spent in different locations and at home.

The phone log data provided information about calling and texting behaviors, and smartphone use. These data are collected from event-based system logs that indicate each time the phone was used in general (i.e., the screen was unlocked) and for calls and texts. The 10 variables included the frequency and duration of incoming and outgoing phone calls, the frequency and character length of incoming and outgoing text (i.e., SMS) messages, as well as the frequency and duration of the phone being used.

EMA Data. Of the 61 behavioral tendencies, 31 were measured via EMA. Participants completed EMA surveys (see Supplemental Supporting Text G) in response to notifications sent four times a day (12:00 p.m., 3:00 p.m., 6:00 p.m., and 9:00 p.m.) to their phones (or email accounts, for participants who did not wish to download the smartphone app). The EMA questions asked participants to select one response option per question that best reflected what they were doing *in the prior hour* (in the Fall sample) or *in the prior 15 min* (in the Spring sample). There were four EMA questions about activity level, activity type, interaction partner, and locations that we used to derive the behavioral tendencies.

The activity level EMA item asked participants the following question "Have you been sedentary (sitting, reclining) or active (on your feet, walking)?" The response options for this question were 1 = "Almost always sedentary," 2 = "Mostly sedentary, a little active," 3 = "Equal amounts of time sedentary and active," 4 = "Mostly active, a little sedentary," and 5 = "Almost always active." The activity type EMA item asked participants to complete the following phrase: "I spent MOST of my time…" Participants could select one response option from the following 10 activity categories: "studying, reading, preparing for an exam," "talking, texting, socializing," "attending classes, meetings," "browsing the internet, using social media," "doing household chores, running errands," "working at a job," "resting, napping, doing nothing," "watching TV, movies," "exercising, physical activity, sports," and "commuting, traveling." The Spring sample included an additional response option, "eating, drinking," which is not included in the pooled analyses. The interaction partner EMA item asked participants to complete the following phrase: "I spent MOST of my time with the following people." Participants could select one response option from the following eight interaction partner categories:

"alone," "classmates, students," "coworkers," "family," "friends," "significant other," "roommates," and "strangers." The location EMA item asked participants to complete the following phrase: "I spent MOST of my time in the following place." Participants could select one response option from the following 12 place categories: "home," "bar, party," "café, restaurant," "campus," "fraternity or sorority house," "friend's house," "gym," "home, dorm, apartment," "library," "religious facility," "store, mall," "work," and "vehicle." Participants had the opportunity to "skip" any EMA item that they did not wish to answer. Skip responses were coded NA. Other EMA questions that did not ask about participants' behaviors are not included here.

The activity level EMA was the only item with a numeric response scale. The activity, interaction partner, and location EMA items all had categorical response scales such that each categorical response option was turned into a separate behavioral variable. For instance, if a participant selected "coworkers" in response to the interaction partner EMA item, then the "Being with coworkers" variable was coded 1 and all other interaction partner variables (e.g., "Being alone," "Being with a significant other") were coded 0, since participants could select only one response option per item.

Analytic Strategy

Data Cleaning and Aggregation. Behaviors measured via EMA and smartphone sensing were originally collected at the momentary level. The raw sensor or log data (e.g., GPS latitudes and longitudes) and EMA data (e.g., reported location) were then processed into variables that reflect each participant's behavioral tendencies (e.g., being at home) over the study period. To estimate participants' daily behavioral tendencies across the study period, we aggregated these momentary estimates to the day level and then averaged across days. To estimate participants' time-of-day tendencies, we aggregated momentary estimates to each time of day (i.e., morning: 6:00 a.m.–11:59 a.m., afternoon: 12:00 p.m.–5:59 p.m., evening: 6:00 p.m.–11:59 p.m., night: 12:00 a.m.–5:59 a.m.) and then averaged across days. Notifications did not expire, so we used the timestamp for when participants started the EMA survey to determine time-of-day estimates, not when the notification was initially sent. To estimate participants' time of week tendencies, we aggregated momentary estimates to the day level and then averaged across weekdays (i.e., Monday–Friday) and weekends (i.e., Saturday–Sunday).

Days were excluded from aggregation if 50% or more of data for a given day was missing (i.e., missing 12 or more hours of sensing data or missing two or more EMA surveys). Similarly, times of day were excluded from aggregation if 50% or more of data for a given time of day was missing (i.e., missing 3 or more hours of sensing data or no EMA surveys completed at that time of day). Behavioral tendencies were calculated for a participant only if they had at least 2 days worth of data for a given variable that could be aggregated across days. Note that behavioral tendencies measured via EMA have a minimum of 0 and a maximum of 1 because they reflect the average daily proportion of EMAs completed on a given day in which participants reported engaging in the given behavior. (For instance, if participants completed four EMA surveys in a given day and indicated they were at home in two of these surveys, this would be represented as 0.5.) Proportions were chosen over counts to account for the fact that participants varied in the number of EMAs they completed each day.

To check whether there were systematic differences in missingness between liberals and conservatives in the EMA data, we examined whether political orientation was associated with a greater likelihood to "skip" EMA questions or miss EMA surveys. Political orientation was unassociated with participants' average number of skips per day (ps > .62) or the average number of EMA surveys they completed per day (p = .44) after exclusions. We did however find partisan differences in rates of missingness for eight sensing variables. We could not think of any clear reason for these differences and could not find any record of similar differences in the literature (Currey & Torous, 2023; Kiang et al., 2021), but we report them in Supplemental Table S17 in case these differences are of interest to future researchers.

We conducted a few data transformations to improve the interpretability of the sensing variables. Specifically, distance measures were converted from meters to kilometers, and sensing variables measuring durations were converted from minutes to hours. Further information regarding data cleaning and aggregation can be found in Supplemental Supporting Text F.

Correlational Analyses. To investigate actual lifestyle polarization, we conducted Spearman correlations between political orientation and each behavioral tendency. Spearman correlation coefficients were chosen because they are more suitable than Pearson correlations for heavy-tailed distributions and distributions with outliers (de Winter et al., 2016), both of which are likely to occur with smartphone sensing and EMA data. We interpret correlations as statistically significant when p < .05 level and 95% confidence intervals do not include 0. Associations with liberalism are significantly negative, whereas correlations with conservatism are significantly positive.

To contextualize the overall size of our effects, we calculated the average of the absolute value of all correlations between political orientation and the 61 behaviors at the daily level as well as the range and absolute value of the *significant* correlations between political orientation and the behaviors at the daily level. To benchmark the size of these effects, we compared the range and average correlations between political orientation and behaviors to the range and average correlations between demographics (i.e., gender, ethnicity, and SES) and behaviors. Average correlations were computed using the *rbar* function from the "psychometric" (Fletcher, 2023), and were weighted by each variable's sample size such that behavioral tendencies with larger samples were given more weight. Although we report the average size of the correlation coefficients, the values should be interpreted with caution because they do not account for nonindependence between the behavioral tendencies. Readers concerned by nonindependence should refer instead to the range.

Robustness Checks. We probed the robustness of our simple correlational analyses using partial correlations and randomization tests. First, we examined whether each Spearman correlation between political orientation and each individual behavioral tendency at the daily level holds when controlling for ethnicity, gender, and SES simultaneously and separately. We also examined how, across all statistically significant correlations between political orientation and daily behavioral tendencies, the average size of the correlation coefficient changed when controlling for ethnicity, gender, and SES simultaneously and separately. (In addition to controlling for the demographic variables simultaneously, controling for each variable separately provides a more fine-grained view of how each variable influences the association between political orientation and behavioral tendencies.) We focused on controlling for demographic characteristics at the daily level (not the time-of-day or time-of-week levels) because the daily tendencies are based on all available data, whereas time-of-day and time-of-week tendencies are calculated using smaller subsets of data and are thus less reliable.

Second, we conducted randomization tests to determine whether the pattern of correlations between political orientation and behavioral tendencies is merely capitalizing on chance as a result of conducting a large number of significance tests (Sherman & Funder, 2009). We conducted these randomization tests using Spearman correlations between political orientation and behavioral tendencies for each domain at the daily level and across all behavioral domains at the time-of-day and time-of-week levels. Unlike Bonferroni adjustments, which focus on determining which individual correlations should be considered statistically significant (often of interest in confirmatory hypothesis testing), randomization tests focus on determining whether a *set* of correlations provides evidence beyond what would be expected by chance (often of interest in exploratory research, as is the case here).[1] Because randomization tests apply to *sets* of correlations rather than to specific correlations. Our interpretations focus mainly on the pattern of results overall and by domain rather than for specific behaviors.

To run randomization tests, we wrote a custom R function that estimated (a) the number of statistically significant correlations that were *observed*, (b) the number of statistically significant correlations that would be *expected* by chance (i.e., in a distribution of 10,000 simulations conducted under the null hypothesis), and (c) the probability (i.e., p value) of finding the observed number of statistically significant correlations by chance. The randomization tests worked by randomly redistributing the original political identity scores provided by the participants to the 61 behavioral tendencies without replacement such that each behavioral tendency had an equal probability of being assigned any one of the political identity scores and each original score is represented in the original data set. The randomly assigned political identity scores were then correlated with each of the 61 behavioral tendencies, and the number of statistically significant correlations at the .05 level was recorded. These randomization tests were sensitive to different sample sizes and missing values for each variable. The procedure of random reassignment, correlation computation, and recording of results was repeated 10,000 times to form an approximate chance sampling distribution against which the observed number of significant correlations can be compared.

Statistical Power. We conducted an a priori power analysis to determine the sample size needed to detect the average correlation between political orientation and lifestyle variables that was reported in a seminal study ($|r| = .12$; DellaPosta et al., 2015). However, as previously mentioned, methodological considerations such as the lack of common method bias make it likely that our effect sizes may be smaller than the effects found in this seminal study. Therefore, we ran another a priori power analysis to determine the sample size needed to detect a conventionally small correlation ($|r| = .10$; Funder & Ozer, 2019). These two power analyses revealed that we would need 547 and 787 participants to detect Spearman correlations of $|r| = .12$ and $|r| = .10$, respectively, with 80% power in two-tailed tests and $\alpha = .05$.

The final *N*s for each behavioral tendency vary depending on the data source, so the precision and statistical power with which we are able to estimate and detect effects also varies. The results of the first power analysis showed that we had more participants than needed (i.e., >547 participants) in the pooled sample to detect an effect of $|r| = .12$ with 80% power for all variables except those in the "calling and texting" subdomain, where we had only 142 participants (because calling and texting logs were only available from Android users, not iPhone users). The results of the second power analysis showed that we had more participants than needed (i.e., >787 participants) to detect an effect of $|r| = .10$ with 80% power for the 31 variables measured via EMA and the two sensing variables computed from Unlock Logs. The rest of the variables measured via smartphone sensors or logs (with the exception of calling and texting variables, and the running variable) had sample sizes greater than what was needed to achieve 70% power (i.e., >620 participants) but less than what was needed to achieve 80% power. This means that the probability of a Type II error, or a false negative, is higher for variables measured via smartphone sensors than for variables measured via EMA in the pooled sample.

Data Pooling. As outlined in our preregistration, we analyzed and interpreted the pooled data from both Fall and Spring samples while controlling for semester (Curran & Hussong, 2009) rather than analyzing and interpreting the data from each sample separately. This decision was based on the following considerations. First, the procedure in the Fall and Spring was virtually identical and thus easily lent themselves to pooling. Second, the pooled data set is larger and thus has greater statistical power to detect small effects. Based on the power analyses reported above, none of the variables in the Spring sample had enough participants to detect a small effect ($|r| = .12$ or $|r| = .10$) with 80% power. Maximizing our ability to detect small effects is important in a study whose purpose is discovery and exploration (Götz et al., 2022). Third, a larger sample should also result in more precise point estimates. Pooling can improve the precision of estimates on the tail end of the political orientation distribution (where there were few "extremely conservative" participants, especially in the Spring) by increasing the number of conservative participants on which our inferences are based. Last, we would not be able to meaningfully interpret differences in the Fall and Spring samples' results. Such differences could be caused by any number of factors including (a) the time of year, (b) cohort characteristics, (c) the time interval in the EMA question stem (i.e., 1 hr in the Fall vs. 15 min in the Spring), or (d) statistical power. Controlling for semester in all pooled analyses helps account for differences between the Fall and Spring due to these factors.

We report all results of the Fall and Spring samples analyzed separately in the Supplemental Materials for the sake of transparency,

[1] A few other considerations about Bonferroni adjustments and similar techniques are worth noting. First, Bonferroni adjustments have been criticized for being overly conservative for large sets of variables, where the adjusted critical p values would present "an almost insurmountable threshold" (Sherman & Funder, 2009). In addition, the $p < .05$ critical threshold and Bonferroni adjustments to this threshold were selected to minimize the probability of Type I errors (i.e., false positives) rather than Type II errors (i.e., false negatives; Maier & Lakens, 2022). When the cost of Type I and Type II errors are relatively equal, or when the Type II errors are more costly than Type I errors, researchers may even be justified in increasing their critical p value (Maier & Lakens, 2022). Ultimately, given the exploratory nature of our research, we were less concerned with controlling the Type I rate at 5% via adjusting the critical p value than we were with Type II errors that would prevent novel discoveries (Fiedler et al., 2012).

though we caution against interpreting correlations in the Spring since all analyses in that sample are underpowered. Correlations for the Fall sample analyzed separately can be found in Supplemental Table S7 (descriptive statistics), Supplemental Table S8a–S8d (daily level + covariates), and Supplemental Table S9a–S9d (time-of-day + time-of-week). Correlations for the Spring sample analyzed separately can be found in Supplemental Table S10 (descriptive statistics), Supplemental Table S11a–S11d (daily + covariates), and Supplemental Table S12a–S12d (time-of-day + time-of-week).

Software. To analyze data in Study 1, we used the following software and software packages: the R language (R Core Team, 2024), "psych" (Revelle, 2024), "DescTool"s (Signorell, 2024), "tidyverse" (Wickham et al., 2019), "stringr" (Wickham et al., 2019), "lubridate" (Wickham et al., 2019), "readr" (Wickham et al., 2019), "tidyr" (Wickham et al., 2019), "ppcor" (S. Kim, 2015), "stats" (R Core Team, 2024), "psychometric" (Fletcher, 2023), "ggplot2" (Wickham et al., 2019), "RColorBrewer" (Neuwirth, 2014), "wesanderson" (Ram et al., 2018), "gghighlight" (Yutani, 2018), "gridExtra" (Auguie & Antonov, 2017), and "parallel" (R Core Team, 2024).

Results

At the daily level, 18 of 61 behavioral tendencies (i.e., 30%) were associated with political orientation (see Figure 2 and Supplemental Table S2). Of the 61 daily behavioral tendencies, eight were associated with conservatism (i.e., a positive association) and 10 were associated with liberalism (i.e., a negative association). Political orientation was associated with seven of 30 daily behavioral tendencies measured by smartphone sensors or logs and 11 of 31 daily behavioral tendencies measured by EMAs. Together, these results suggest that both liberalism and conservatism are associated with behavior measure via both EMA and smartphone sensing.

Moving beyond the daily level, the number of significant associations was greater when considering other times of the day and week. Twenty-nine of 61 behavioral tendencies (i.e., 48%) were associated with political orientation at least one time of day or week (see Figure 2 and Supplemental Table S3). This finding suggests that relying on daily tendencies alone, which aggregate across times of day and week, would underestimate lifestyle polarization. Our results do not suggest greater activity in the mornings among conservatives and greater activity in the evenings among liberals, as has been suggested by prior research on political orientation and chronotype. In the mornings, only three behavioral tendencies were associated with conservatism and seven were associated with liberalism. In the evenings, four behavioral tendencies were associated with conservatism and three were associated with liberalism. In addition, associations between political orientation and behavior occurred on both weekdays and weekends. Thirteen behaviors were associated with political orientation on the weekdays, as compared to seven behaviors on the weekends.

Behavioral differences between liberals and conservatives were robust. First, randomization tests showed that the number of significant associations observed exceeded the number of significant associations that would be expected by chance (i.e., the number of expected false positives; see Supplemental Table S4). This was true at the daily level ($p < .001$), on weekdays ($p = .002$), mornings ($p = .009$), and afternoons ($p = .004$)—but not on evenings ($p = .079$) or nights ($p = .354$), when participants may have been sleeping, or on weekends ($p = .079$), where behavioral tendencies relied on a maximum of only 4 days. Second, only four out of 18 statistically significant correlations at the daily level became nonsignificant when controlling for demographic characteristics (see Supplemental Tables S2 and S5). Similarly, only two out of 21 tested significant correlations became nonsignificant when accounting for exploratory (not preregistered) covariates such as personality traits, religiosity, and campus residence where theoretically relevant (see Supplemental Table S6 and Supporting Text C). Third, the range and average size of statistically significant associations between political orientation and daily behavior ($r_{\text{range}} = [-.11, .17]$ and $|r_{\text{mean}}| = .09$, 95% CI [.07, .10]) changed only minimally when controlling for participants' demographic characteristics simultaneously ($r_{\text{range}} = [-.11, .14]$ and $|r_{\text{mean}}| = .09$, 95% CI [.07, .10]) or separately (controlling for gender: $r_{\text{range}} = [-.12, .17]$ and $|r_{\text{mean}}| = .09$, 95% CI [.08, .10]; controlling for ethnicity: $r_{\text{range}} = [-.11, .15]$ and $|r_{\text{mean}}| = .09$, 95% CI [.08, .10]; controlling for SES: $r_{\text{range}} = [-.11, .14]$ and $|r_{\text{mean}}| = .09$, 95% CI [.07, .10]). These effect sizes are small by conventional standards but similar (e.g., $|r's| = .09$–.16) to those found in studies of lifestyle polarization that sample liberals and conservatives in the general population and use traditional survey methods (e.g., DellaPosta et al., 2015; Rawlings & Childress, 2024).

We examined whether the number of behavioral differences between liberals and conservatives was comparable to the number of behavioral differences found between participants with other identity characteristics. The number of statistically significant associations between daily behavior and political identity was 18 (as reported above), as compared to 18, 14, and 13 statistically significant associations between daily behavior and gender, ethnicity, and SES, respectively (see Supplemental Table S5 for details). Thus, there were as many lifestyle differences between liberals and conservatives as there were between men and women in our sample, and there were more lifestyle differences between liberals and conservatives than between individuals with different ethnicities and socioeconomic backgrounds.

Next, we sought to understand how liberals' and conservatives' lifestyle behaviors differed in each of the following domains: (a) *social behavior*, including "calling and texting" and "socializing" subdomains; (b) *movement behavior*, including "physical activity" and "geographic mobility" subdomains; (c) *work behavior*, including "working" and "studying" subdomains; and (d) *leisure behavior*, including "domestic pursuits," "recreational pursuits," and "media use" subdomains. Randomization tests in all domains except social behavior revealed that the number of significant associations observed at the daily level was statistically significantly greater than the number that would be expected by chance ($p_{\text{Social}} = .507$, $p_{\text{Movement}} = .015$, $p_{\text{Work}} = .028$, $p_{\text{Leisure}} < .001$; see Supplemental Table S4).

As previously mentioned, our focus was on overall behavioral patterns rather than specific behavioral indicators. That said, in what follows, we describe specific behavioral differences between liberals and conservatives for illustrative purposes, moving from the domain with the fewest to the most pronounced differences. Within each domain, we present results by subdomain; within each subdomain, we first present behavioral tendencies significantly associated with conservatism followed by those associated with liberalism. Associations between political orientation and behavioral tendencies at the daily level remained significant when controlling for gender, ethnicity, and SES simultaneously unless otherwise indicated.

Figure 2

Study 1 Results: Actual Lifestyle Polarization

(*Figure continues*)

We further describe in Supplemental Supporting Text C and Table S6 how plausible confounds—personality traits (i.e., conscientiousness and openness) and geographic factors (i.e., population density and campus residence)—did not account for most of our results unless otherwise noted below. The results described below can be found in Figure 2 (daily + time-of-day + time-of-week), Supplemental Table S2 (daily + covariates), and Supplemental Table S3 (time-of-day + time-of-week) for the social (a), movement (b), work (c), and leisure (d) behavior domains, respectively.

Social Behavior

Few reliable differences were observed in the domain of social behavior. Two out of 18 social behaviors (11%) were associated with political orientation at the daily level but, as mentioned above, these results were not robust according to randomization tests. In addition, observed associations tended to become nonsignificant or included 0 in their confidence intervals when controlling for demographic factors. Thus, we do not describe any of the statistically significant associations between political orientation and social behaviors here. The lack of robust findings in this domain may be partially due to small samples in the "calling and texting" subdomain (where we only had data from Android users, not iPhone users), leading to inadequate power to detect small effects.

Movement Behavior

Six out of 19 movement behaviors (32%) were associated with political orientation at the daily level. In this domain, conservatives tended to engage in more "physical activity," whereas liberals tended to engage in more "geographic mobility."

In the "physical activity" subdomain, conservatives tended to be more active on weekdays and at the daily level than liberals were. They also tended to walk more on weekdays and at the daily level than liberals did. Liberals, on the other hand, tended to be more stationary at the daily level and on weekends than conservatives were.

In contrast, in the "geographic mobility" subdomain, liberals tended to change locations more frequently in the mornings, visit more locations in the mornings, spend time more evenly between locations in the mornings, and be in transit more in the mornings, on weekdays, and at the daily level than conservatives did. Liberals also tended to spend more time at each location at nights, on weekends, and at the daily level, and tended to visit locations more routinely on weekdays and at the daily level than conservatives did.

Associations between political orientation and behavior in this domain at the daily level remained significant when controlling for covariates with the following two exceptions: walking became nonsignificant when controlling for demographics or consienciential; routinely visiting locations became nonsignificant ($p = .051$) when controlling for demographics.

Work Behavior

Three out of nine work behaviors (33%) were associated with political orientation at the daily level. In this domain, conservatives tended to be "studying," whereas liberals tended to be "working."

Specifically, in the "working" subdomain, conservatives tended to commute more on weekends than liberals did. Liberals, on the other hand, tended to work and be with coworkers more at the daily level, in the afternoons, and on weekdays than conservatives were.

In the "studying" subdomain, conservatives tended to attend classes or meetings in the evenings more than liberals did, and they tended to study or read more on afternoons, on weekdays, and at the daily level than liberals did. Liberals tended to be on campus in the mornings more than conservatives were.

Associations between political orientation and behavior in this domain at the daily level remained significant when controlling for covariates with one exception: studying became nonsignificant when controlling for conscientiousness.

Leisure Behavior

The most pronounced differences between liberals and conservatives were observed in the domain of leisure behavior, where seven out of 15 leisure behaviors (47%) were associated with political orientation at the daily level. In this domain, conservatives teneded to engage in more "recreational pursuits," whereas liberals teneded to engage in more "domestic pursuits" and "media use."

In the "recreational pursuits" subdomain, conservatives tended to be in noisier places at all times of the day and week except at nights than liberals were. Conservatives also tended to be at fraternities or sororities more at all times of the day and week except mornings than liberals were. Conservatives tended to be at bars or parties more in the afternoons, on weekdays, and at the daily level than liberals were. And conservatives tended to be in religious places more in the afternoons, in the evenings, on weekends, on weekends, and at the daily level than liberals were.

In contrast, in the "domestic pursuits" subdomain, liberals tended to do chores or errands more in the mornings and evenings than conservatives did. Liberals also tended to be at home more (as measured by GPS) at nights and at the daily level than conservatives were. Both liberals and conservatives rested or took naps, but conservatives tended to do so more in the mornings, whereas liberals tended to do so more in the afternoons, on weekdays, and at the daily level.

Figure 2 Note. Spearman correlation coefficients and their 95% confidence intervals represent associations between political orientation and (a) social, (b) movement, (c) work, and (d) leisure behavior. Line and symbol colors correspond to the behavioral domain. Lines and symbols in gray denote coefficients with 95% confidence intervals that include 0 or p values >.05. Symbol type corresponds to time of day; line type corresponds to time of week. Daily behavioral tendencies represent the aggregated tendencies at all times of day and week. Data from the Fall and Spring samples were pooled; all reported correlations include the sample as a dummy variable. The plots should be interpreted with some caution. Specifically, the correlation coefficients should not be compared to each other because they were estimated on different subsamples. (In other words, each point represents a coefficient from a separate model, not coefficients from the same regression model.) Freq. = frequency; dur. = duration; loc. = location; norm. = normalized. See the online article for the color version of this figure.

[a] Behavioral tendencies measured via smartphone sensors. All other behavioral tendencies were measured via ecological momentary assessment.

In the "media use" subdomain, liberals tended to browse the internet or social media more in the afternoons, in the evenings, on weekdays, on weekends, and at the daily level than conservatives did.

Associations between political orientation and behavior in this domain at the daily remained significant when controlling for covariates with two exceptions: being at bars/parties became nonsignificant when controlling for demographics, and being in religious places became nonsignificant when controlling for religiosity.

Study 2: (Mis)Perceived Lifestyle Polarization

Study 2 measured participants' perceptions of lifestyle polarization. To probe the accuracy of these perceptions, we analyzed how perceived behavioral differences between liberals and conservatives correspond to actual behavioral differences between liberals and conservatives. Specifically, participants from the same university as Study 1 completed a survey in which they rated their perceptions of the extent to which liberal or conservative students at their university engaged in each of the 61 lifestyle behaviors. Perceptions were measured on a -1 = "liberals always do this more" to 1 = "conservatives always do this more" scale, with the midpoint representing 0 = "liberals and conservatives do this equally." This rating scale was meant to match as closely as possible the correlation coefficient that measured actual polarization in Study 1. Accuracy was examined correlationally (i.e., Is there a positive and significant association between actual and perceived lifestyle polarization across behaviors and domains?) and categorically (i.e., Do participants correctly categorize whether liberals or conservatives engage in a given behavior more?).

Method

Participants

Participants were 156 undergraduate students enrolled in the psychology department subject pool at the University of Texas at Austin during the Spring or Summer of 2023. Thus, participants from Studies 1 and 2 were drawn from the same population, but the data for Studies 1 and 2 were collected in different years. All participants were 18 years or older and consented to participate in the study. The sample was 61% women, 37% men, and 2.6% another gender; 28% White, 3% Black, 25% Hispanic, 30% Asian, .10% Native American, 2% another ethnicity, and 13% of mixed ethnic background; 4% lower class, 22% working class, 35% middle, 32% upper middle, and 8% upper class; 69% liberal, 19% moderate, and 16% conservative. Sample characteristics and comparisons to nationally representative samples are described in Supplemental Table S1.

Our target sample size was 150 participants. This decision was based on several considerations, including pragmatic considerations like the number of available participants in the subject pool and a desire to complete data collection quickly. Simulation studies recommend a sample size of 50–100 or greater for multilevel models to achieve unbiased regression coefficients, variance components, and standard errors (Hox & Maas, 2006). However, we aimed for a larger sample size of 150 to increase our statistical power to detect interaction effects, which tend to be small. Assuming that we would have to drop some participants due to preregistered exclusion criteria, we preregistered stopping data collection at 160 participants. Ultimately, we stopped data collection once we had data from 161 participants. To ensure we retained only those participants who had thoughtfully completed the survey, we dropped three participants for completing the survey too quickly (in less than 2 min), one participant because they started the survey but did not rate any behaviors, and one participant who started the survey twice but never completed it, leaving a final sample of $N = 156$ participants.

Procedure

Participants completed a survey "about how people view liberals and conservatives as they go about their daily lives" in exchange for 0.5 research participation credits. Participants were told that the survey will ask them about their perceptions of the daily behaviors of liberal and conservative undergraduate students at their university. After they completed informed consent, participants rated their perceptions of the extent to which liberal or conservative undergraduates at their university are more (or equally) likely to engage in each of the 61 daily behavioral tendencies measured in Study 1. The behavioral tendencies participants rated were organized into four domains (i.e., social behavior, movement behavior, work behavior, and leisure behavior), which were presented in a randomized order. Finally, participants completed the demographics block of the survey, including measures of political orientation (1 = "extremely liberal" to 7 = "extremely conservative") and how certain they felt about their ratings (0 = "not at all certain" to 4 = "very certain"). The survey included two attention checks. Once data collection was complete, we merged the survey data on perceived lifestyle polarization data with the actual lifestyle polarization data from Study 1 in preparation for the accuracy analysis.

It is worth pointing out that participants were not asked specifically about their perceptions of cross-partisans (e.g., liberals rating conservatives, and vice versa); instead, *all* participants rated the extent to which they believed liberals or conservatives were more (or equally) likely to engage in a given behavior. This approach is slightly different from some prior research on misperceived or "false" polarization. However, it still allowed us to examine whether "people's beliefs about polarization are substantially more extreme than the actual partisan gap," which is how false polarization has been defined in prior research (Fernbach & Van Boven, 2022).

Measures

Independent Variable: Actual Lifestyle Polarization. The independent variable is actual lifestyle polarization as measured in Study 1 (i.e., the "truth criterion"). Actual lifestyle polarization reflects the actual behavioral tendencies of liberals and conservatives, represented in terms of correlation coefficients between political orientation and each of the 61 behavioral tendencies at the daily level. These coefficients could range from -1 to 1, with negative and positive values indicating behavioral associations with liberalism and conservatism, respectively.

Dependent Variable: Perceived Lifestyle Polarization. The dependent variable is perceived lifestyle polarization as measured in the present study (i.e., the "observer judgments"), reflecting participants' perceptions of the association between political identity and daily behavioral tendencies. These perceptions are represented in terms of 61 continuous ratings per participant (i.e., $N = 9{,}445$ observer judgments) ranging from −1 to 1. The observer rating scale and labels were meant to match the meaning of the truth criterion as closely as possible without asking lay participants to understand or estimate statistical correlations (see Supplemental Supporting Text H for the full survey.)

Specifically, participants were told,

> Below is a list of (communication/movement/work/leisure) behaviors. Please drag the sliding scale between −1 and 1 to indicate the degree to which you think liberal or conservative undergraduate students at UT Austin are more (or equally) likely to engage in each behavior in their daily lives. Ratings between −1 and 0 indicate that you think liberals engage in the behavior more than conservatives. Ratings between 0 and 1 indicate that you think conservatives engage in the behavior more than liberals. Ratings of 0 indicate that you think liberals and conservatives engage in the behavior equally.

On the sliding scale, −1 was labeled *liberals always do this more*, 0 was labeled *conservatives and liberals do this equally*, and 1 was labeled *conservatives always do this more* to reflect the meaning of perfect or zero correlations, respectively. Participants were asked only to respond with respect to the "daily" lives of liberals and conservatives and not their perceptions about different times-of-day or times-of-week. We focused on perceptions at the daily level because the daily tendencies measured in Study 1 are based on all available data (not subsets of data) and thus provide the most complete representation of liberals' and conservatives' everyday lives. In addition, it was not feasible to ask participants about all 61 behaviors at all seven times of the day and week without potentially compromising data quality due to participant fatigue.

Moderating Variable. To examine whether accuracy varied depending on the type of behavior participants were judging, behavioral domain was treated as a moderating variable using the same categorization as in Study 1. This moderator had four levels: social behavior, movement behavior, work behavior, and leisure behavior. We chose movement behavior as the reference category because it was the only domain showing no relationship (either positive or negative) between actual and perceived lifestyle polarization and thus served as a neutral comparison against which the other domains could be compared.

Attention Checks. One attention check question was included in the social behavior block of the survey, which asked participants to "Slide the scale to .5 to show you're paying attention." A second attention check question in the demographics block asked participants whether they were meant to rate behavioral tendencies during a COVID-19 lockdown period. (They had been told in the instructions to provide their perceptions of liberals' and conservatives' behavioral tendencies *not* during a COVID-19 lockdown period, to correspond to the context of Study 1.) As described in our preregistration, we retained all participants in the main article's focal analyses. However, we report in Supplemental Supporting Text D and Table S16 that our results are robust to participant inattention.

Analytic Strategy

Correlational Accuracy (Across and Within Behavioral Domains). We analyzed the accuracy of perceived lifestyle polarization across and within behavioral domains using a correlational approach. This approach draws on continuous observer ratings and defines accuracy as a positive and statistically significant relationship between actual and perceived lifestyle polarization. Multilevel regression models were conducted to predict perceived lifestyle polarization from actual lifestyle polarization. We expected the association between actual and perceived lifestyle polarization (i.e., accuracy) to vary for individual observers and individual behaviors. In other words, following research in person perception, which models variance in accuracy arising both from observers and targets, we assumed that some observers may perceive more accurately than others and some behaviors may be more accurately perceived than others. So, when examining the relationship between actual perceived lifestyle polarization, we included random intercepts (which computes a different intercept for each observer and each behavior) and random slopes (which computes a different slope for each observer and each behavior) instead of assuming uniform intercepts and slopes across behaviors and observers. We did not person mean-center observers' ratings because we wanted to preserve the meaning of the ratings' raw values (such that 0 corresponds to perceiving that liberals and conservatives engage in the behavior equally).

We built from less complex to more complex models:

Model 1: random intercepts for observers;

Model 2: random intercepts for observers + random intercepts for behaviors;

Model 3: random intercepts for observers + random intercepts for behaviors, random slopes for observers; and

Model 4: random intercepts for observers + random intercepts for behaviors, random slopes for observers + random slopes for behaviors.

We report results from all models for the sake of transparency but focus our interpretations on Model 4 according to our preregistration and because this model is the most theoretically and empirically justified. Model 4 (Akaike information criterion [AIC] = 7567.4), Model 3 (AIC = 7573.2), Model 2 (AIC = 7631.2), and Model 1 (AIC = 8953.5) each had incrementally and statistically significantly ($ps \leq .01$) poorer model fit compared with the model preceding it.

We first ran these models without considering behavioral domains. Then, to examine whether accuracy depended on the behavioral domain under consideration, we conducted additional multilevel regression models predicting perceived lifestyle polarization from actual lifestyle polarization, the domain (i.e., social behavior, movement behavior, work behavior, leisure behavior), and the Actual Lifestyle Polarization × Behavioral Domain interaction. As before, we attempted to model random intercepts and random slopes for both observers and categories, building up from less complex to more complex models. However, the more complex models—Models 2, 3, and 4—failed to converge when including the interactive term. Thus, in line with the preregistration, we report and interpret results from the simplest model (Model 1).

The effect size of the regression coefficient from our multilevel models represents the magnitude of the relationship between actual and perceived lifestyle polarization (i.e., the "truth force," West &

Kenny, 2011). The random intercept from the multilevel models indicates whether participants are overestimating (i.e., a positive intercept) or underestimating (i.e., a negative intercept) how much conservatives engage in the behavior relative to liberals (i.e., the "directional bias," West & Kenny, 2011). In other words, positive values of the intercept mean that participants think conservatives tend to engage in the behaviors more than liberals, and negative values of the intercept mean that participants think liberals tend to engage in the behaviors more than conservatives. However, the directional bias of observer judgments can be examined more directly using the categorical approach to accuracy.

Categorical Accuracy (for Individual Behaviors). To examine accuracy for individual behaviors, we took a categorical approach. This approach draws on aggregated classifications of observer ratings[2] and defines accuracy as a match between the group category (liberals, conservatives, or equal) that actually engaged in each individual behavior more and was perceived to engage in each individual behavior more. The categorical approach to accuracy provides a more fine-grained characterization of the nature of participants' (mis)perceptions. Specifically, lack of accuracy for any individual behavior could be a result of three different kinds of errors:

1. Overestimates: Perceiving differences between liberals and conservatives that do not exist. In other words, observers think conservatives (or liberals) engage in a behavior more when actually liberals and conservatives engage in that behavior equally.
2. Underestimates: Failing to perceive differences between liberals and conservatives that do exist. In other words, perceiving that liberals and conservatives engage in a behavior equally, when in reality, liberals (or conservatives) engage in that behavior more.
3. Perceptions in the wrong direction: Perceiving that liberals engage in a behavior more when actually conservatives engage in that behavior more, or vice versa.

We coded which group actually engaged more in each behavior using results from Study 1 as follows. If the correlation between political orientation and a behavioral tendency at the daily level was negative (positive), significant at the $p < .05$ level, and had confidence intervals that did not include zero, then we coded that behavioral tendency as something that liberals (conservatives) actually do more. If the correlation between political orientation and a behavioral tendency at the daily level was nonsignificant at the $p \geq .05$ level or had confidence intervals that included zero, then we coded that behavioral tendency as something that liberals and conservatives do equally.

We coded which group was *perceived* to engage more in each behavior in much the same way using the survey from Study 2. If observers' average rating was negative (positive) and less than (greater than) or equal to −.08, then we coded that behavioral tendency as something that liberals (conservatives) are perceived to do more. If a rating was between −.08 and .08,[3] then the behavior was coded as something that liberals and conservatives are perceived to do equally. This threshold allowed ratings that were not exactly equal to 0 but may effectively be interpreted as 0 by participants to be coded as such.

This analytic approach focuses on whether observers were accurate about the *direction*, rather than the magnitude, of the association between political identity and behavioral tendencies. We chose not to examine magnitude (i.e., compare mean differences between the perceived and actual association for each behavior) because we assumed that participants would not know that the average size of effects in the social sciences is small. Not knowing effect sizes tend to be small would naturally lead observers to inaccurately overestimate the association between political orientation and each behavioral tendency.

Software. To analyze data in Study 2, we used the following software and software packages: the R language (R Core Team, 2024), "psych" (Revelle, 2024), "tidyverse" (Wickham et al., 2019), "lubridate" (Wickham et al., 2019), "lme4" (Bates et al., 2015), lmerTest (Kuznetsova et al., 2017), "broom.mixed" (Bolker et al., 2022), "lattice" (Sarkar, 2008), sjPlot (Lüdecke, 2023), "performance" (Lüdecke et al., 2021), "interactions" (Long, 2019), and "gridExtra" (Auguie & Antonov, 2017).

Results

Overall, there was little consensus in observers' perceptions of liberals' and conservatives' behaviors. Observers felt only "a little certain" about their perceptions on average ($M = 2.2$, $SD = 0.82$), and there were low levels of agreement between observers, intraclass correlation coefficient(2,1) = .15 [.11, .21], $p < .001$.

Observers were also not accurate on average across behaviors, as indicated by a nonsignificant relationship between actual and perceived lifestyle polarization ($b = 0.02$ [−0.70, 0.75], $p = .950$; see Supplemental Table S13, Model 4; Figure 3a). However, there was heterogeneity in accuracy depending on the behavioral domain and the individual behavior.[4]

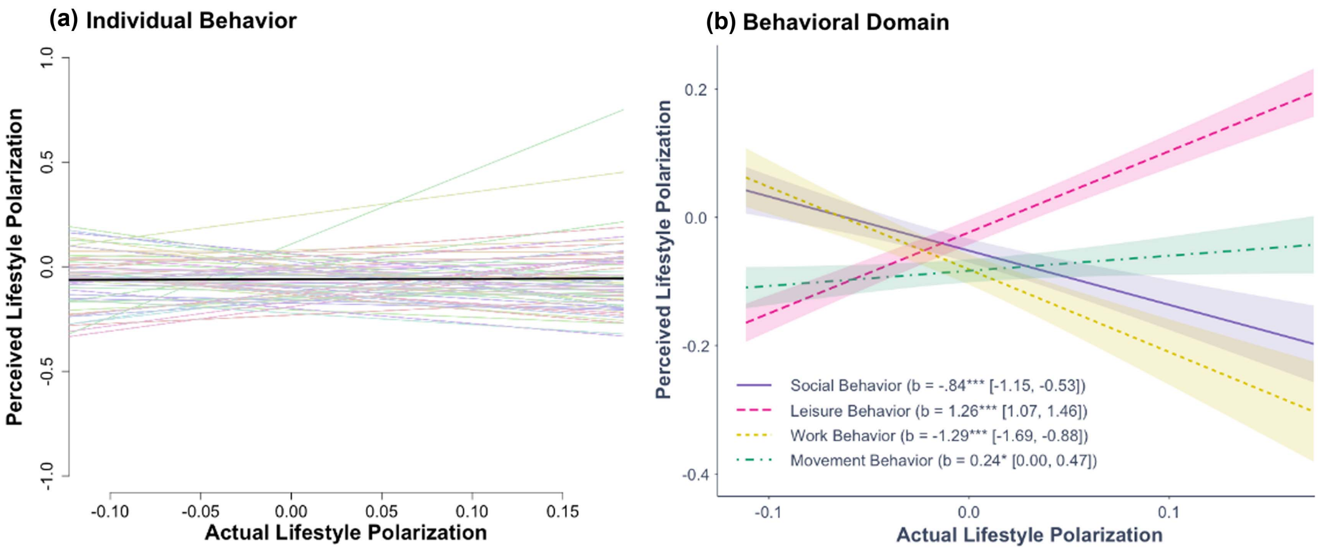
Probing heterogeneity by behavioral domain, we found that accuracy depended on the domain as indicated by statistically significant Behavioral Domain × Actual Lifestyle Polarization interactions on perceived lifestyle polarization (Social Behavior interaction: $b = -1.08$ [−1.47, −0.69], $p < .001$; Leisure Behavior interaction: $b = 1.03$ [0.72, 1.34], $p < .001$; Work Behavior interaction: $b = -1.52$ [−1.99, −1.05], $p \leq .001$; see Supplemental Table S14 for further details of these results). Specifically, as shown in Figure 3b, perceptions of lifestyle polarization were accurate in the domain of leisure behavior, as indicated by a positive and statistically significant simple slope between actual and perceived lifestyle polarization. However, observers were inaccurate in the domains of

[2] Aggregating the individual observer ratings produced a highly reliable composite measure, intraclass correlation coefficient (2,k) = .96 [.95, .98], $p < .001$, which served as the basis for computing the categorical measures of accuracy for individual behaviors.

[3] Adjusting this |.08| threshold up (to |.1|) or down (to |.01| or |.05|) when coding each behavior did not substantially influence our categorical accuracy results. Regardless, people were much more inaccurate than they were accurate, tending to overestimate rather than underestimate lifestyle polarization.

[4] There was also heterogeneity among observers in the relationship between actual and perceived lifestyle polarization (see Supplemental Figure S2). This heterogeneity was partially explained by observers' political orientation. Conservative observers were more correlationally accurate than liberal observers (see Supplemental Supporting Text E), perhaps because conservatives were in the minority in our sample. See Talaifar et al. (2021) for a discussion of why minority group members tend to be more accurate about majority group members than vice versa.

Figure 3
Study 2 Results: Accuracy of Perceived Lifestyle Polarization



Note. In the left-hand panel (a), gray lines represents random intercept sand random slopes for the relationship between actual and perceived lifestyle polarization for each individual behavior ($N = 61$). The solid black line represented the average random intercept and random slope across behaviors. In the right-hand panel (b), actual lifestyle polarization interacts with the behavioral domain to predict perceived lifestyle polarization. The model includes random intercepts for observers. Results of simple slopes analyses are reported for each domain in the legend. Shaded regions and numbers in brackets denote 95% confidence intervals. Positive and statistically significant simple slopes reflect accurate perceptions of lifestyle polarization. See the online article for the color version of this figure.
$^{*}p = .05$. $^{***}p < .001$.

social, work, and movement[5] behavior, as indicated by simple slopes between actual and perceived lifestyle polarization that were negative or had confidence intervals that included 0.

Probing results for individual behaviors, we found that observers were more inaccurate than they were accurate. They classified 46 out of 61 individual behaviors (76%) inaccurately with respect to whether liberals or conservatives engaged in the behavior more (or equally). Of these inaccurately classified behaviors, 33 out of 44 (72%) were overestimates (i.e., observers perceived a difference in liberals' and conservatives' daily behavioral tendencies where there was none); 10 out of 44 (22%) were perceived in the wrong direction (i.e., observers perceived liberals to engage in the behavior more when in fact conservatives did, or vice versa); and only three out of 46 (7%) were underestimates (i.e., observers perceived no difference where there was one). Supplemental Table S15 depicts the results for each behavior.

General Discussion

> Ten, fifteen years ago, people could disagree without killing each other. They could disagree and still like the person with whom they disagree, on a different level—no matter how different their lives were. But now that human level has disappeared and there is only one level of agreement, and that's lifestyle. If your lifestyle is like my lifestyle, then we like each other; if your lifestyle is not like my lifestyle then we can see nothing in each other.
>
> —film director John Cassavetes, *The Village Voice*, 1971

Over 50 years ago, acclaimed film director John Cassavetes observed that lifestyle is a driver of social division. Our article suggests that this observation holds true today. We add, however, that lifestyle divisions in the digital era are not random but rather cluster around political identities, revealing "lifestyle polarization." We found support for this idea in two studies of lifestyle polarization on a U.S. university campus. Study 1 showed that polarization extends beyond differences in beliefs and values to permeate some of the most mundane, ostensibly nonpolitical activities in which students engaged every day. Study 2 demonstrated that, even though liberal and conservative students behaved differently, members of the campus community did not perceive these differences accurately and overestimated lifestyle polarization.

In Study 1, smartphone data collected via active logging and passive sensing revealed small but robust differences in liberals' and conservatives' movement, leisure, and work behavior at most times of day and week. These lifestyle differences between liberals and conservatives were generally not accounted for by demographic or other confounding factors and were similar in number and magnitude to lifestyle differences between men versus women, upper versus lower class, and White and ethnic minority individuals in our sample. We emphasized the general pattern of behavioral differences between cross-partisans more so than the specific nature of

[5] Exploratory (not preregistered) robustness checks suggested that accuracy in the movement domain depended on participant attention (see Supplemental Supporting Text D, Table S16, and Figure S1). In this domain, attentive participants who correctly rated behaviors during a nonpandemic period did achieve accuracy, whereas inattentive participants who did not follow directions and rated behaviors during the pandemic period (i.e., a period when movement behavior was unusually politicized and that did not match our truth criterion) did not achieve accuracy.

these differences. However, if forced to describe how exactly liberals and conservatives differed behaviorally, one might say that liberals and conservatives often made different exploration–exploitation trade-offs. Conservatives tended to prioritize the *exploitation* of their local environment—making the most of where they are (e.g., attending classes or meetings, spending time at sororities or fraternities). In contrast, liberals tended to prioritize the *exploration* of more distant and varied environments—going elsewhere for potentially better rewards (e.g., spending time in transit, working). An important exception to this exploration–exploitation pattern was that liberals tended to engage in more domestic pursuits (e.g., spending time at home) than did conservatives. Thus, the exploitation of local environments among conservatives and the exploration of more distant environments among liberals were observed only outside of the domestic sphere.

Study 2 revealed that the behavioral differences we observed between liberals and conservatives in Study 1 were not obvious to other students on campus. On average, students in our sample did not hold strong or consistent stereotypes about lifestyle polarization at their university and did not perceive lifestyle polarization accurately in most domains. More specifically, there was no association between actual and perceived lifestyle polarization overall, and students categorized 76% of individual behaviors incorrectly with respect to whether liberals or conservatives engaged in a particular behavior more. Despite being inaccurate overall, heterogeneity in participants' perceptions revealed two main drivers of inaccuracy. First, inaccuracy was driven by overestimates—and not underestimates—of behavioral differences between liberals and conservatives. Observers perceived differences between partisans where none existed. Second, overall inaccuracy was driven by inaccurate perceptions of work and social behavior, which masked accurate perceptions of leisure behavior. Behavioral differences between liberals and conservatives in Study 1 were most pronounced in the leisure domain, perhaps making them easier for observers to accurately discern in Study 2. The fact that participants were accurate in at least one domain suggests that their judgments were not totally random, even if they were generally inaccurate overall.

Contributions to the Literature on Lifestyle Politics

Our research contributes to the literature on lifestyle politics methodologically and substantively. Methodologically, this is the first article to use an array of smartphone sensors and logs to study political identity in everyday life. A systematic review of articles using digital trace data to infer identity characteristics (Hinds & Joinson, 2018) reported that 33 articles have used data from social media platforms, search engines, websites, and blogs to examine political identity. However, none used digital traces from smartphone sensors and logs. Studies published since this systematic review have examined partisan differences in movement using GPS data (Allcott et al., 2020; Barbieri & Bonini, 2021; Chen & Rohla, 2018; Gollwitzer et al., 2020; Zhang et al., 2023). However, these GPS studies do not use other data types available from smartphone sensors and logs (e.g., calling and texting, microphone, unlocks). EMA has been used in political psychology to study a variety of emotions and cognitions, especially those evoked in response to moral and political news and events (e.g., Baumert et al., 2017; Ford et al., 2023; Hofmann et al., 2014; Newman et al., 2019; Otto et al., 2020). However, researchers in political psychology have not capitalized on EMAs to study behavior in everyday life. In our study, political identity was associated with behaviors measured by both EMAs and sensors/logs, showing the utility of both methods.

Our smartphone-based methods produced at least three substantive contributions to various streams of research on lifestyle politics. First, our findings suggest that lifestyle polarization likely coexists on university campuses alongside other forms of polarization. This means that those interested in depolarizing campuses may be unsuccessful if they do not consider and account for lifestyle differences between students with different political identities. Although we did not measure actual cross-partisan contact, our results suggest that lifestyle polarization, especially with respect to behaviors that involve others, can reduce the opportunity for cross-partisan contact and the possible beneficial consequences of such contact. If our results generalize beyond the university, then lifestyle polarization may also be an underappreciated driver of lack of cohesion in communities and society more broadly. A fundamental premise of social and political theory is that political stability in diverse societies requires cross-cutting cleavages—domains of overlap among groups with otherwise distinct identities (Lipset, 1963). Less partisan overlap in nonpolitical domains does not bode well for liberals' and conservatives' ability to develop shared ties and mutual understanding, which are necessary building blocks of a tolerant and pluralistic society. Lifestyle polarization may even breed other forms of polarization. For instance, research suggests that lifestyle preferences, more so than the desire to affiliate with politically like-minded others, cause geographic polarization (Martin & Webster, 2020).

Our second substantive contribution is the possibility that the impact of lifestyle polarization may not be limited to the group- and societal-level outcomes (e.g., cohesion, cross-cutting ties) that have been emphasized in prior research but may also impact individual well-being. Many of the everyday behaviors that conservatives engaged in more than liberals (e.g., leisure pursuits, physical activity) have been linked to better individual well-being in prior research (Kroencke et al., 2019; Müller et al., 2020; Smeets et al., 2020). Conversely, many of the behaviors liberals engaged in more than conservatives (e.g., working, domestic pursuits, social media use) have been linked to worse well-being in prior research (Müller et al., 2020; Shao, 2022; Vaid et al., 2024). Therefore, future research should directly test whether there is an underappreciated behavioral basis for why conservatives tend to report being happier than liberals. Though the size, nature, and existence of this partisan well-being gap have been contested by some research (e.g., Choma et al., 2009; Onraet et al., 2013; Wojcik & Ditto, 2014; Wojcik et al., 2015), the gap appears quite robust in other research (Napier & Jost, 2008; Newman et al., 2019). For instance, liberals report being happier than conservatives in only five out of 92 countries, and the relationship between conservatism and greater reported happiness holds across countries even after accounting for a variety of factors like age, gender, and employment status (Stavrova & Luhmann, 2016). In the United States, conservatives have reported being happier than liberals in every iteration of the General Social Survey since 1972 (Al-Gharbi, 2023). Among American young adults in particular, mental health has declined over the past 20 years, but this decline has been sharper among liberals than among conservatives (Gimbrone et al., 2022). Prior research has attributed conservatives' higher well-being to their personality (Schlenker et al., 2012), higher SES (Jetten et al., 2013), religiosity (Butz et al., 2017), and system-justifying ideology (Napier & Jost, 2008). For those seeking to close the partisan well-being gap, these attributes may not be particularly amenable to change, making them difficult targets of intervention. Targeting modifiable lifestyle behaviors may be a more feasible intervention strategy.

The longitudinal nature of smartphone-based methods enabled a third substantive contribution: the introduction of temporal context into the study of lifestyle polarization. Considering temporal context allowed us to identify yet another potential reason, beyond those already proposed (e.g., Gift & Gift, 2015; Iyengar et al., 2012; Mummolo & Nall, 2017), for limited cross-partisan contact. Specifically, for serendipitous cross-partisan contact to occur, liberals and conservatives who live in the same community must not only engage in the same activities but do so at the same times. Instead, we found that liberals and conservatives behaved differently in the mornings, afternoons, and weekdays. These findings indicate that lifestyle polarization is not limited to the times of day or week typically reserved for personal interests (e.g., weekends). Although behavioral differences did depend on the time of day, we did not find greater activity among conservatives in the mornings and greater activity among liberals in the evenings as might be expected from prior research on partisans' chronotype (Ksiazkiewicz, 2020). Taken together, our findings point to the importance of considering temporal context in behavioral measuremnt, since assessing behavioral tendencies at the daily level alone would have caused many instances of lifestyle polarization to go undetected.

Contributions to the Literature on (Mis)Perceived Polarization

In some ways, our work suggests that misperceived polarization is even more entrenched than previously appreciated. Misperceptions extend to partisans' everyday behaviors and are not limited to misperceptions about their attitudes and beliefs. In other words, people overestimate differences between liberals and conservatives in yet another sphere of life—one that is totally nonpolitical. In addition, we found that misperceptions persist even when people rated members of their own community rather than typical party members, as has been the focus of prior work. This finding suggests that observers are inaccurate even when they do not have to rely on media portrayals and stereotypes about partisan strangers but can instead base their judgments on first-hand information about and experiences with the target group at hand.

In other ways, however, misperceived polarization may be less entrenched than it may first appear. Observers' perceptions of liberals' and conservatives' behaviors *were* accurate in the domain with the largest behavioral differences (i.e., leisure). This finding suggests that perceptions of polarization may be accurate on dimensions that are clearly observable. Put differently, inaccurate perceptions of liberals' and conservatives' behaviors in the other domains may be explained by the fact that these behavioral differences were too small or subtle to be easily noticed (Funder, 1995), and it may be unfair to expect observers to accurately judge such small differences. Lack of observability might also help explain prior findings demonstrating highly inaccurate perceptions of ideological and affective polarization (Fernbach & Van Boven, 2022). This prior research has focused on what partisans think and feel, but others' thoughts and feelings are not easily observable and thus may be particularly vulnerable to being inaccurately perceived (Funder, 1995).

On balance, then, do our findings on misperceived lifestyle polarization suggest an optimistic or pessimistic outlook for cross-partisan relations? From the more pessimistic perspective, the fact that observers overestimated everyday behavioral differences between liberals and conservatives suggests that they may (mistakenly) struggle to find common ground with cross-partisans. People may be unmotivated and uninterested in engaging in contact with cross-partisans who they perceive to live vastly different lives from themselves (Lee, 2021). From a more optimistic perspective, it is possible that inaccuracy is positive because people would not be able to use their knowledge of the behaviors that are typical of cross-partisans to avoid these cross-partisans in everyday life.

However, any optimism we hold from our findings derives not from the potential benefits of false illusions but instead from the power of accurate perceptions to bridge political divides. We believe overestimates of lifestyle polarization are amenable to correction, and such corrections could reduce partisan animosity. Prior work has demonstrated that highlighting similarities between liberals and conservatives on attitudinal dimensions improves attitudes toward the political opposition and increases the belief that partisans can reach common ground on major social issues (Syropoulos & Leidner, 2025). Highlighting similarities between liberals and conservatives on everyday behavioral dimensions may be similarly fruitful, especially given the many real behavioral similarities we observed between liberals and conservatives. Indeed, prior research finds that when liberals and conservatives discuss mundane things they have in common, their attitudes toward cross-partisans improve (Santoro & Broockman, 2022). Moreover, many of the behaviors liberals and conservatives engage in to a similar degree are socially desirable and humanizing behaviors (e.g., being with loved ones), and research shows that highlighting the humanizing attributes of cross-partisans can reduce hostility and increase empathy toward those on the other side (Koetke et al., 2023). Interventions to correct misperceived lifestyle polarization may be successful since participants were on average only "a little certain" about their perceptions, suggesting that these perceptions may be malleable. And, as previously mentioned, people were accurate in the leisure domain, which suggests that accuracy is possible.

Origins of Lifestyle Polarization

Our work contributes to an ongoing theoretical debate on the origins of lifestyle polarization. On the one hand, scholars have theorized and found support for the idea that lifestyle polarization can be explained by the divergent characteristics of liberals and conservatives, such as their demographic background (Rawlings & Childress, 2024). From this perspective, gender, ethnic, and socioeconomic identities that increasingly coincide with political identity (Baldassarri & Gelman, 2008) cause partisan differences in lifestyle. Somewhat surprisingly, our data provide little support for this explanatory account. In contrast to prior studies (DellaPosta, 2020; Rawlings & Childress, 2024), controlling for gender, ethnicity, and SES hardly reduced the average size of the associations between political orientation and behavioral tendencies, and the vast majority of associations remained statistically significant with demographic controls. Similarly, research showing that geographic segregation drives lifestyle differences (Martin & Webster, 2020) cannot explain our findings because our student participants all lived in the same geographic area. What, then, might explain our results? It is possible that political identity itself exerts a direct influence on everyday behavior. In this view, our results would suggest that political identity is a latent trait-like factor that manifests as a stable tendency to engage in certain kinds of everyday behaviors consistently across contexts.

An alternative explanatory account attributes the cause of lifestyle polarization not to characteristics within individuals (i.e., their political or demographic identity) but to the relations between individuals (i.e., their social networks and influence; DellaPosta et al., 2015). This explanatory account would suggest that lack of cross-partisan contact is not only a *consequence* of lifestyle polarization, as we emphasized in the introduction, but may in fact be its cause. For instance, if liberals and conservatives at the university operate in segregated and homophilous networks (where liberals affiliate with liberals and conservatives with conservatives), the arbitrary behaviors of a few liberals and conservatives may spread via social influence through their respective networks (McPherson et al., 2001). Even in the absence of segregated and homophilous networks, students may observe the behaviors of copartisans and cross-partisans, learning to emulate what it means to behave like a copartisan and not a cross-partisan within their specific context (Goldberg & Stein, 2018). Social networks and influence are particularly plausible explanations of lifestyle polarization in our context because we sampled students from the same university, who have network ties and the opportunity to observe, associate with, and emulate one another (in contrast to nationally representative samples of Americans, who are unlikely to interact with each other).

Understanding whether lifestyle polarization is caused by trait-based or social network-/influence-based factors is important because each explanation has different implications for the extent to which specific behavioral differences between liberals and conservatives will generalize to other contexts and samples. Trait-based explanations suggest that the same behavioral differences should emerge among liberals and conservatives everywhere. In contrast, social network- and social influence-based explanations suggest that the specific behavioral differences that emerge among liberals and conservatives may vary from context to context, depending on which behaviors spread among liberals versus conservatives in a given context. Both explanatory accounts expect lifestyle polarization to emerge, but the trait-based account expects the nature of behavioral differences to be more universal, whereas the social network-based account expects the nature of behavioral differences to be more locally specific.

Ultimately, multiple reinforcing processes likely operate in tandem to produce lifestyle polarization. Both trait-based and social network-based factors are potentially valid and important explanations for the observed patterns of lifestyle polarization and thus warrant further study. Claims that political identity exerts a direct trait-like influence on everyday behavior would require methods and analyses that allow for causal inference (Bailey et al., 2024). Claims that behavioral practices diffuse through social network and influence processes would require longitudinal data on the development of partisans' behavioral tendencies as a result of peer interactions and ties, data which could be collected using smartphone-based methods (Rüegger et al., 2020).

Effect Sizes and What to Make of Them

One likely critique of our findings is that associations between political orientation and behavioral tendencies were small by conventional standards, and there were a large number of null results. Some would interpret these results as consistent with prior claims that lifestyle polarization is limited (Praet et al., 2022). However, we believe an interpretation of the evidence in favor of lifestyle polarization is warranted for a variety of reasons. First, our results were robust to a wide array of plausible confounds and to randomization tests, indicating that the evidence of lifestyle polarization was not spurious. (If results in the movement, work, and leisure domains were spurious, they would look like those in the social domain.) Second, small differences in the kinds of frequently enacted behaviors we studied can have meaningful consequences at scale and over time as effects aggregate and accumulate (Funder & Ozer, 2019; Götz et al., 2022, 2024). Third, lifestyle differences between liberals and conservatives were similar in size to the lifestyle differences we observed on the basis of gender, ethnicity, and SES. Thus, political identity is at least as important as these demographic factors in shaping students' lifestyle patterns.

Most importantly, our research design stacked the deck *against* finding associations between political orientation and behavioral tendencies. Consider that we (a) did not select behaviors that should be associated with political identity based on prior theory or stereotypes about liberals and conservatives; (b) did not select participants who lead vastly different lifestyles as a result of their location, age, or occupation; (c) did not select conservatives and liberals who hold particularly different values or life goals, given their shared commitment and ability to pursue higher education; (d) did not limit our behavioral estimates to waking hours when behavioral differences could reasonably emerge; (e) did not collect behavior in a controlled laboratory setting that minimizes measurement error; (f) did not measure our independent and dependent variables at the same time point or using the same method; and (g) did not include measures of behavior that better capture the content, rather than the structure, of activites (e.g., differences in the specific gyms or religious facilities liberals and conservatives freqeuent). In this light, it is remarkable that almost half of the 61 behaviors were associated with political orientation at least one time of day or week and that our average effect sizes were similar to those found in lifestyle polarization studies that use traditional survey methods (e.g., DellaPosta, 2020; Rawlings & Childress, 2024).

Limitations and Future Directions

We established internal validity by isolating the effect of political identity on behavioral tendencies above other factors and ecological validity by measuring behavior in the stream of participants' everyday lives. However, our research is limited in its external validity due to its reliance on a student sample from a single university. Future research should examine whether the current findings replicate and generalize to other samples (e.g., other university samples; nationally representative samples), geographic and cultural contexts (e.g., other regions of the United States, other countries), communities or organizations (e.g., companies), temporal periods (e.g., longer data collection intervals, aggregation at different temporal levels), measures of political ideology (e.g., multidimensional measures of social and economic orientations, right-wing authoritarianism, social dominance orientation), and data types (e.g., financial transaction records, app usage logs).

In future work testing the replicability and external validity of our findings, we expect lifestyle polarization to emerge in some form given that it emerged here under what could be considered a stringent test of lifestyle polarization. At the same time, it seems likely that variation in samples, contexts, and other factors will influence the nature and magnitude of lifestyle polarization. For example, as previously discussed, if network ties explain why lifestyle polarization emerges, then arbitrary behavioral differences between cross-partisans in other contexts could diffuse and manifest in different patterns of lifestyle polarization in those contexts, producing locally specific understandings of what it means to behave as a liberal or conservative.

That prior research could have predicted very few of the specific differences we observed (e.g., conservatives spend more time in religious places, fraternities and sororities; Butz et al., 2017; Jetten et al., 2013) underscores the need for better theorizing and evidence about how and to what extent lifestyle differences will emerge. One factor that could theoretically influence the extent of lifestyle polarization is the type of behavior under consideration. For instance, we examined behaviors that left private digital residue as people engaged in their normal activities, not behaviors that people actively and publicly chose to adopt to signal their political identity (e.g., wearing a MAGA hat). We expect that behaviors that serve as observable identity signals will show stronger associations with political orientation (Gosling et al., 2002). The observability of behaviors also has important implications for future research on misperceived polarization. Researchers could test whether the observability of liberals' and conservatives' behavior impacts the accuracy with which observers perceive lifestyle polarization.

Data collected from smartphones hold great untapped potential for further documenting the contours, causes, and consequences of lifestyle polarization, much beyond what was possible in the present article. For instance, we find that liberals tend to report using the internet and social media more than do conservatives. Future research could use smartphone app usage logs to understand which specific social media apps partisans use as well as the frequency, duration, and nature (e.g., active vs. passive) of this app usage (Sust et al., 2024; Verduyn et al., 2022). Information from app usage logs could be linked with momentary assessments of well-being to investigate whether social media use explains the negative mental health outcomes reported among liberals (Gimbrone et al., 2022).

Time-based considerations also deserve further attention in future work using smartphone-based methods. For example, researchers could examine whether self-reported differences in liberals' and conservatives' chronotypes map onto smartphone-based behavioral indicators of sleeping and waking times (Schoedel et al., 2020). To study temporal dynamics directly, researchers could use data collected from smartphones to examine how the behavioral tendencies of liberals and conservatives change over time (Schoedel & Mehl, 2023). For instance, it is possible that behavioral differences deepen and expand over the course of the academic year or even over the course of the lifespan, as liberals and conservatives select into increasingly homogeneous political niches that prevent cross-partisan contact. The effects of contact and network dynamics could be studied longitudinally using social interaction data collected from smartphones. For example, Bluetooth data could be used to determine whether participants are colocated in the same place at the same time, and EMA surveys could be triggered during colocation to collect information about whether and how those participants interacted (e.g., the nature and quality of the interaction, the perceived political identity of the interaction partner; Barnett et al., 2024; Roshanaei et al., 2024; Rüegger et al., 2020).

Finally, the interpretation of our findings that liberals and conservatives may engage in different exploration–exploitation trade-offs is also speculative and thus warrants direct testing and theorizing. A great deal of research in the animal, evolutionary, computer, organizational, psychological, and other sciences has examined exploration–exploitation trade-offs in a variety of goal-directed tasks such as foraging, mating, search, and learning (e.g., Doren et al., 2023; Gupta et al., 2006; Kembro et al., 2019; Kolze et al., 2021; Mehlhorn et al., 2015; Spreng & Turner, 2021; Tsang et al., 2024; von Helversen et al., 2018; Wilson et al., 2021). Future researchers could draw on this body of work to investigate whether political identity is a predictor of exploration and exploitation across domains and goal-directed tasks. For instance, researchers could identify exploration and exploitation behaviors a priori and then systematically test whether liberals engage more in the former and conservatives more in the latter. Identifying exploration and exploitation behaviors could help researchers translate our findings to other contexts, where it would make less sense to measure the exact behaviors we investigated (e.g., studying, spending time on campus) and more sense to measure the kinds of exploration and exploitation behaviors that would be prevalent or important in that context. In pursuing this research, researchers would need to decide how to conceptualize exploration and exploitation (e.g., as two ends of the same spectrum or as orthogonal constructs), consider optimal levels of exploration versus exploitation in different contexts and life stages (e.g., exploration may be more important in university settings or young adulthood when key developmental tasks include self-exploration and identity formation), and examine the potential benefits at the group level of parallel exploration and exploitation (e.g., if liberals and conservatives divide these between them; Gupta et al., 2006; Tsang et al., 2024). If liberals do indeed engage in more exploration and conservatives in more exploitation, this could be a reflection of latent differences in their basic values (e.g., universalism among liberals, security among conservatives, Schwartz, 2012) and worldviews (e.g., as an abundant place worthy of exploration among liberals or a dangerous place to be avoided among conservatives; Clifton & Kerry, 2023; Weber & Federico, 2007).

Conclusion

Collectively, the present research counters the notion that people are "ideologically naive" (Jost, 2006), showing instead that liberals and conservatives sort into "lifestyle enclaves" that can bifurcate a community (Bellah et al., 2007; Bennett, 1998). The present research also takes the perspective that the inauspicious things people do everyday, and the way these behaviors are perceived, matter (Hofmann & Grigoryan, 2023; Mehl et al., 2006). Methodologically, the present research emphasizes the importance of description and exploration (Gerring, 2012; Rozin, 2001), the measurement of actual behavior (Baumeister et al., 2007; Furr, 2009), and the potential for behavioral residue left in digital environments (Gosling et al., 2002, 2011) to reveal novel insights in political psychology.

We conclude by emphasizing that socializing, moving, working, and playing form the foundation of human experience. We mapped the ways in which these foundational activities are aligned (and are perceived to be aligned) along political fault lines. The reasons why ostensibly nonpolitical behaviors are associated with political identity and the consequences of this association have yet to be unraveled. What is clear is that partisanship is seeping into everyday life in ways that cannot be ignored by those concerned by the polarization of American society.

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