Time Zones, Tiredness, and Turnout: A Natural Experiment on How Time Constraints Influence Elections

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Theories of voting suggest that many people don’t vote because they don’t have enough time. However, we possess little causal evidence about the effects of time constraints on electoral behavior. In this article, we leverage a novel geographic natural experiment to show that exogenous disruptions in time allocations have significant political consequences. Namely, we show that voter turnout is lower on the marginally eastern side of U.S. time zone boundaries. Time zones also appear to exacerbate participatory inequality and push election results towards Republicans. While we explore several plausible mechanisms, our results suggest that time zones trigger a bundle of changes resulting from increased tiredness. This implies that turnout is affected not only by how much time individuals possess, but also by how motivated they are to use their time productively. Our work speaks to the precursors of participation and lends insights to interventions seeking to increase voter turnout.

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Why don’t more people vote? Over time in many democracies, the administrative barriers to voting have tended to decline (Blais 2010). Nevertheless, many people still fail to cast a ballot. In the U.S., for example, about 40% of registered voters do not participate in Presidential Elections, with abstention rates soaring as high as 60% in Midterms and 70% in local elections (Hajnal and Trounstine 2016). Moreover, rates of political participation have remained stubbornly low among certain disadvantaged subgroups—particularly among young, minority, uneducated, and low income citizens. Though, in many respects, voting has gotten easier, many eligible individuals still fail to vote.

When asked why they do not vote, many citizens report “not having enough time”—or a close derivative (e.g. “I’m too busy.” or “[Voting] takes too long.”). Foundational models of voting suggest that available time is a strong input for civic participation (Verba, Schlozman and Brady 1995). Previous work indicates that levels of turnout are shaped by time costs such as how long it takes to register to vote (Leighley and Nagler 2013), to find and travel to a polling location (Brady and McNulty 2011; Dyck and Gimpel 2005), and to wait in line to vote (Pettigrew 2016).

While time plays a key role in models of turnout, teasing apart the role of time constraints from other barriers to voting has been difficult. For example, a drop in turnout after a shift in polling location may reflect selection effects such as increased costs of transportation, rather than how long it takes to vote. Moreover, the extant literature may underestimate the role

of time constraints by placing too little emphasis on opportunity costs. That is, for busy citizens with jobs, families, and friends, the time costs of voting are shaped not only by the convenience of casting a ballot, but also by the broader social and biological constraints that regulate their everyday lives.

In this article, we explore the political consequences of an exogenous disruption to how citizens allocate their time. Specifically, we use a novel Geographic Regression Discontinuity Design (GRDD) that leverages the precise location of U.S. time zone boundaries to show that long-term changes to individuals’ day to day schedules influence whether they vote. This approach takes voting rules as given and instead investigates the effects of exogenous shifts in daily schedules. By leveraging a natural quasi-experiment conducted at the national level, our design overcomes several challenges that have plagued previous work in this area while also preserving a high degree of external and ecological validity.

With our GRDD models, we show that time zone boundaries substantively shape levels of electoral participation, the composition of the electorate, and, ultimately, election results. Voter turnout is 1.5-3 percentage points lower among individuals on the marginally eastern side of U.S. time zone cutoffs than among all-else-equal individuals on the western side. This effect is present across all three time zones (i.e. Eastern, Central, and Mountain) and persists across different electoral contexts (i.e. Presidential and Midterm). It is also particularly pronounced among low propensity citizens, and appears to push election results towards Republicans.
Our preferred specification augments the GRDD models with state and year fixed effects that absorb many potential confounds. Furthermore, our results hold across numerous robustness checks. First, we show that a host of covariates are balanced across the cutoffs, which indicates that our findings do not follow from differences in observable county characteristics. Importantly, we find no evidence of sorting around the time zone borders in population sizes, migrations, housing prices, and commute distances, to name a few. Second, we conduct permutation tests to demonstrate that our effects are not the result of the idiosyncratic distribution of counties around time zone borders. Finally, we use archival county-level data to estimate county fixed effects models that leverage historical changes in time zone boundary locations. This shows that our findings are robust to unobserved local factors that remain constant over time.

To further explore the potential mechanisms behind these effects, we develop a theoretical framework and provide empirical evidence indicating that time zone discontinuities trigger a bundle of behaviors following from increased levels of individual tiredness. We build on recent work in behavioral economics (e.g. Doleac and Sanders 2015; Giuntella and Mazzonna 2015; Gibson and Shrader 2016)—which uses natural experiments similar to ours to show that tiredness influences productivity, health, and risky behaviors—and argue that tiredness also shapes individual decisions in the civic domain. Replicating and extending work by Giuntella and Mazzonna (2015), we demonstrate that while individuals on the marginally eastern side of time zone cutoffs are similar to their counterparts on the western side on many dimensions,
they are different in that they sleep less. Individuals exogenously nudged an hour forward by
time zones sleep about 20 minutes less a night (on average) and, as a result, are less likely to
get sufficient levels of sleep. Additional analyses also suggest that while individuals on the
eastern side have more waking time, they use this time less productively.

Our findings have significant theoretical implications for the study of low and unequal
participation. Although previous research has acknowledged time as an important resource
for voting, it has not yet fully recognized that not only may the quantity of time one possesses
influence voting, but so too may how efficiently citizens spend their time from day to day.
Our most likely mechanism provides a new, straightforward explanation for why many people
fail to vote: because they are too tired do so. While many factors influence voting, the forces
that shape individuals’ everyday experiences appear to play an under-appreciated role in
political behavior (Egan and Mullin 2012).

Further, while a large body of work contends that many citizens don’t vote because
they “don’t want to” (Verba, Schlozman and Brady 1995)—or, in other words, because they
lack political motivation—our results are consistent with recent work suggesting that many
individuals abstain because they have insufficient general motivation to provide the effort
to cast a ballot (e.g., Rogers, Fox and Gerber 2013; Hobbs, Christakis and Fowler 2014;
Hillygus, Holbein and Snell 2015; Hill 2016; Schafer 2016). Our results speak directly to this
literature, as tiredness is, at its core, a lack of general motivation.

Our work also has several important practical implications. In showing that turnout is
disrupted by time zone boundaries and outlining the most likely mechanism, our results provide evidence of both where and how reformers might best target their interventions to better increase voter turnout. In providing evidence that our results may be driven by sleep deprivation, our results speak to a salient public health debate. Over the past six decades, Americans have consistently tended to sleep less and less (Gallup 2013; Huffington 2016). Our work provides the first causal evidence of the potential link between sleep deprivation and another individual well-being outcome: civic engagement. It suggests that trends towards less sleep may work to restrict overall levels of citizen participation, magnify existing patterns of participatory inequality, and ultimately lead to distortions in representation.

Conceptual Framework

In previous work, time has been viewed as an important component of the costs of voting. Studies suggest that individuals’ voting decisions may depend not only on how long it takes to vote (Brady and McNulty 2011; Corvalan and Cox 2017; Leighley and Nagler 2013; Pettigrew

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3 We note that the relationship between time constraints and turnout is consistent with various theories of voting. These comprise rational choice (Downs 1957), resource (Verba, Schlozman and Brady 1995), psychological (Campbell et al. 1960), and sociological models (Rosenstone and Hansen 1993). While these frameworks differ in many of their assumptions, they agree that the act of casting a ballot is costly and that time is a key input of voting.
2016), but also on how much free time they have overall. For example, individuals who have less time because of family (Stoker and Jennings 1995), work (Dyck and Seabrook 2010), or social constraints (Potoski and Urbatsch 2017) are less likely to vote. However, few, if any, studies have explored how exogenous time shocks shape voting decisions. As a result, it has proven difficult to tease apart the role of time from other influences such as income, employment, and other components that shape the cost of voting.

**Time Efficiency, Sleep, and Voting**

In their seminal work on civic participation, Verba, Schlozman and Brady (1995) argue that time increases the chances that individuals participate and lowers the participatory gap between advantaged and disadvantaged citizens. To Verba, Schlozman and Brady (1995), more time means more participation. However, we argue that not only may the *quantity* of time one possesses influence voting, but so too may the *efficiency* with which that time is used.

Studying the effect of sleep on voting allows us to illustrate this broader point. While, strictly speaking, sleep takes away from one’s daily time allotment, it may increase the efficiency with which time is used. Those who get sufficient levels of sleep avoid tiredness; allowing them to do more than they would have had energy to do otherwise.4

Despite its important role, to our knowledge no work has systematically examined the

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4In this article, we use the terms “tiredness,” “sleep deprivation,” “constantly tired,” and “a lack of sleep” interchangeably.
link between sleep and voting. Verba, Schlozman and Brady (1995, 284) perhaps come the closest, by arguing—but not testing—that “time devoted to an informal community effort is time away from work, family, recreation, or sleep.” This viewpoint is consistent with their broader theory, which treats time as uniformly increasing participation. It suggests that if one slept less, one would have more free time to engage in civic and political activities. Yet, this premise may be misleading. For instance, recent studies in economics show that individuals who achieve an adequate level of sleep tend to be more productive, even though—strictly speaking—time spent sleeping is time away from work (Gibson and Shrader 2016; Giuntella and Mazzonna 2015). The unacknowledged possibility is that the same may hold true in the political realm—obtaining sufficient levels of sleep, though technically leaving less room for civic participation, may actually enhance one’s capacity to engage.

Indeed, there are multiple direct and indirect reasons to expect that sleep influences voting. If being sleep deprived and tired means that individuals are less motivated generally, the link between sleep deprivation and turnout may be straightforward. Sleep-deprivation may augment the costs of voting by draining general motivation, and decreasing the likelihood that individuals prepare for (i.e. gain information, register, etc.), and ultimately engage in voting. Hence, less motivated tired individuals may struggle to get out and vote. This view is consistent with descriptive studies, which show that individuals who lack grit or perseverance, self-efficacy, or patience are less likely to overcome voting obstacles (Hill 2016; Hillygus, Holbein and Snell 2015; Schafer 2016). By the same token, individuals who lack general mo-
ivation because they are tired may not have the wherewithal to overcome the barriers to voting.

Tiredness may also influence turnout indirectly through its bundle of downstream consequences. A broad literature indicates that sleep deprivation affects a host human behaviors that work in political science has shown to be associated with political participation. For example, chronic sleep deprivation causes a noticeable deterioration of individual health (Giuntella and Mazzonna 2015), which may influence voting (Burden et al. Forthcoming). Tiredness may also decrease productivity (Giuntella and Mazzonna 2015; Gibson and Shrader 2016), leaving prospective voters with fewer voting resources. Similarly, a lack of sleep may lead to noticeable declines in cognitive ability (Lim and Dinges 2010)—a known predictor of voter turnout (Verba, Schlozman and Brady 1995). Finally, experimental work suggests that sleep-deprivation lowers self-control and social skills (Ferrara et al. 2015), which have recently been experimentally linked to voting (Holbein Forthcoming).

Furthermore, the political consequences of tiredness may depend on context and individual characteristics. For instance, the demobilizing effect of tiredness may be especially pronounced in areas where the cost of voting is higher. In these areas, voting may require even more energy for citizens to actually get out and vote. Similarly, the effects of tiredness may vary as a function of individuals’ underlying propensity to participate. Citizens who are predisposed to vote (i.e. those who are politically motivated) may be more willing to overcome the potentially demobilizing effects of tiredness, whereas those with less political
motivation may be less willing to do so. As a result of these heterogeneities, the negative effect of tiredness on turnout may exacerbate participatory inequality. Given that Democrats tend to be more sensitive to the cost of voting than Republicans (Brady and McNulty 2011; Henderson and Brooks 2016), this may shift election results to the right.

To summarize, there are various theoretical reasons to expect that how one spends their time from day to day, such as the amount of time one sleeps, affects voter participation. After all, sleep deprivation affects multiple aspects of life, which may also include civic participation. Furthermore, tiredness may have differential effects based on context and individual background.

Descriptive evidence supports the theorized relationship. Data from The National Longitudinal Study of Adolescent to Adult Health (Add Health) indicate that people who report getting less sleep and being more tired vote at a rate 7-14 percentage points lower than observationally similar less tired individuals (for more details see the Supplemental Information). Yet, in order to test our theoretical predictions, we use a quasi-experimental design. To do so, we leverage variation in daily schedules that is plausibly exogenous to traditional voting inputs.
**Time Zone Discontinuities**

To explore the effect of time constraints on voter turnout, our identification strategy utilizes a naturally occurring quasi-experiment or shock to daily schedules.\(^5\) Our specific approach leverages the U.S. time zone boundaries. For reasons discussed below, individuals living on the eastern side of time zone cutoffs are nudged to a noticeably different lifestyle than all-else-equal individuals on the marginally western side of the time zone boundaries. In particular, individuals on the eastern side sleep less, thus inducing a bundle of downstream consequences related to tiredness. Here we provide theoretical and historical background substantiating the use of time zone borders for our purposes.

**Quasi-Experimental Design**

The four main U.S. time zones (Eastern, Central, Mountain, and Pacific) were officially established with the Standard Time Act in 1918. Borders were originally drawn by the Interstate Commerce Commission, and based on an agreement the major railroads had reached in 1883 to coordinate their clocks in order to regulate traffic. The Uniform Time Act of 1966 placed the authority to make changes to time zone boundaries with the U.S. Department of Transportation (USDOT), which since then occasionally altered the location of these boundaries.

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\(^5\)Ethical considerations set bounds on the types of randomized experiments that can be conducted to disrupt day to day activities.
boundaries.\textsuperscript{6}

The brief intuition behind our natural experiment is that individuals possess a biological sleep cycle, driven by the presence of “circadian rhythms” or “circadian clocks.” These circadian rhythms are biologically influenced by levels of ambient light (Giuntella and Mazzonna 2015). In this paper, we leverage the fact that the arbitrary time zone cutoffs introduce a discontinuity in ambient light patterns that affect sleep patterns (Giuntella and Mazzonna 2015). Our approach takes advantage of time being a continuous function of geographic location, with the exception of discrete changes at time zone cutoffs. Put differently, while what we define as the “actual” or “continuous time” is equivalent close to both sides of the time zone cutoff, the “labeled time” on the clock is one hour ahead on the eastern side of the cutoff. Although circadian rhythms do adjust somewhat to these differences in labeled times, fundamental biological processes make this adjustment difficult as the human body releases sleep hormones once ambient light decreases and it becomes darker. Simply put, because the sun sets later on the eastern side of timezone cutoffs, people are nudged to go to bed later. As morning social schedules do not respond to this time shift, individuals tend to sleep less on the eastern side of the cutoff (Giuntella and Mazzonna 2015). The result is more waking hours, but also more tiredness. To reinforce this idea, we provide an illustrative example of why time zones affect sleep patterns and discuss alternate mechanisms in the Supplemental

\textsuperscript{6}As we show below, our results are robust to utilizing these changes in our identification strategy.
This theoretical process is borne out clearly in previous causal studies. In the work most similar to ours, Giuntella and Mazzonna (2015) show that individuals marginally on the Eastern side of U.S. time zone boundaries sleep 20 minutes less than individuals as-good-as randomly assigned to be on the Western side of time zone cutoffs—suggesting that while individuals do adapt to their environment, their sleep patterns are fundamentally influenced by these arbitrary cutoffs (see also Giuntella and Mazzonna 2015; Gibson and Shrader 2016).\footnote{Beyond focusing on a different outcome than Giuntella and Mazzonna (2015) (i.e. health and productivity), our paper differs from theirs in important ways. As recommended by Keele and Titiunik (2015), we model proximity to the time zone cutoff in two dimensions (i.e. latitude and longitude) rather than one. Further, we augment our models with a set of potentially important state, year, and county fixed effects.}

Giuntella and Mazzonna (2015) also provide evidence that this decline in sleep acts as a first mover that affects a bundle of attributes—such as health and productivity—which we have already suggested mediate a possible relationship between sleep deprivation and voting.

As time zones are arbitrarily defined administrative boundaries that split individuals into areas with different daily schedules, these boundaries are well-suited for a geographic regression discontinuity design (GRDD). In their work discussing the conceptual and practical issues associated with the GRDD, Keele and Titiunik (2015) show that under certain assumptions this model behaves as any other standard RDD with two running variables
(in this case, latitudinal and longitudinal distances to the cutoff). As long as these barriers divide subjects in an as-good-as random manner (something that we explore in great detail below), the GRDD will estimate causal effects.

We note that our identification strategy—just as most designs in the social sciences (Sen and Wasow 2016)—leverages a bundled treatment. That is, in shifting individual sleep patterns, the time zone boundaries also move other related outcomes downstream. For example, Giuntella and Mazzonna (2015) show that people who live east of a time zone boundary eat out more, exercise less, are generally less healthy, and are less productive. In a similar vein, scholars have shown that changes in sleep patterns due to daylight savings time can have large negative downstream consequences on health (e.g., Jin and Ziebarth 2015), school performance (e.g., Carrell, Maghakian and West 2011), road safety (e.g., Smith 2016), and criminal activity (Doleac and Sanders 2015). While theoretical and empirical work suggests that tiredness is the first-mover, our design cannot precisely pinpoint which downstream mechanism is driving out results.

Data

Our dataset combines information from election results, geographic maps, the Census, and the American Time Use Survey (ATUS). We outline these data sources here.

*Elections Data:* For our primary outcomes of interest—voter turnout and election results—we use Dave Leip’s electoral atlas of county-level electoral returns for general elections between 1992-2014. This repository provides the most comprehensive collection of election
results over time at the county-level. As such, it has been used in a number of political science applications (e.g., Nall 2015). The unit of observation in this data-set is the county-year. With about 3,000 counties in the contiguous United States over a 12 year period spanning Midterm and Presidential Elections, our total sample size is just over 36,000. We calculate voter turnout as the number of votes divided by the total population. We do this because estimates of the county-level voting age population vary in their availability over the time period studied, while the total population is available in all years. However, the results do not change when adjusting for available measures of the voting-age population (see Figure A8).8

**Geographic Data:** To execute the analysis for this natural quasi-experiment, we use the geographic software ArcGis 10.3. We retrieved county shapefiles from the census and matched these with historically accurate time zone shapefiles.9 In order to estimate chordal distances between the county centroid and the time zone cutoff, both shapefiles were projected into a two-dimensional plane.10 We then computed distances to the nearest time zone cutoff using the near function in ArcGis.

8To supplement our county-level analysis, we also conduct our GRDD at the individual-level. To do so, we match nationwide voter file data from Catalist with running variable scores computed at the ZIP code level. Results are similar to those provided at the county level; we report them in the Supplemental Information.

9We accessed these maps using http://efele.net/maps/tz/us/.

10We restrict the analysis to the 48 contiguous U.S. states.
Archival Data of Time Zone Boundary Changes: In a robustness check, we leverage historical changes in the location of U.S. time zones. Most of these changes occurred prior to the period of analysis used in the rest of the paper (1992-2014). Thus, we extend the timeframe of our study to also include data from 1948-1992. In order to code an original set of historically accurate geo-data, we use the archive of U.S. time zone changes documented in Shanks (1987). We also use county-level turnout data from Fujiwara, Meng and Vogl (2016). Because of incomplete midterm election data during this time period, our within-county estimates use only presidential election years.

Census Data: To explore potential mechanisms, we use data from the U.S. Census Bureau and the American Time Use Survey (ATUS) conducted by the U.S. Bureau of Labor Statistics (BLS) since 2003.\textsuperscript{11} The ATUS sample is drawn from the existing sample of Current Population Survey (CPS) participants. Respondents are asked to fill out a detailed time use diary of their previous day that includes information on time spent sleeping. These data allow us to show that our natural experiment provides exogenous variation in sleep patterns and to rule out other factors potentially driving the results we present below.

\textsuperscript{11}We specifically employ data from the ATUS over the years 2003-2015. The sample size for these several waves varies between 8,000 and 13,000.
Figure 1: Visualizing the Time Zone GRDD

Figure 1 shows counties (with their geographic centroids marked) on either side of the time zones in the continental United States as of Election Day on 2010. The map shows counties within 1 degree (latitude and longitude) of the time zone boundaries.
Figure 1 visualizes the data that result from pairing voter data to geographic data on time zone boundaries. For illustration purposes, dots are shaded by whether they are on the marginal eastern or western side of the time zone cutoffs and sized by their voter turnout in 2010. In our GRDD application, the two running variables are the latitudinal and longitudinal distances (in degrees) from a county centroid to the nearest continental U.S. time zone cutoff.\textsuperscript{12} Consistent with our identification strategy, we code the running variables as positive on the eastern side of the border and negative on the western side.\textsuperscript{13} As can be seen in Figure 1, our application has three cutoffs—from left to right, the Mountain, Central, and Eastern. The time zone boundaries always follow county borders, and sometimes cut through state borders — a fact that we leverage in our GRDD & fixed effects models outlined below.

**Methods**

To estimate our GRDD models, we fit the following equation:

\[
V_{ct} = \alpha + \beta_1 T_{ct} + \beta_2 Y_{ct} + \beta_3 X_{ct} + \beta_4 T \times Y_{ct} + \beta_5 T \times X_{ct} + u_{ct} \tag{1}
\]

In equation (1), \( V_{ct} \) is the proportion of individuals that turn out to vote in a given county (c) and a given election year (t).\textsuperscript{14} The variable \( T \) is an indicator variable taking the value

\textsuperscript{12}One degree of latitude (North-South) is approximately 69 miles. One degree of longitude (East-West) is approximately 53 miles at the latitude of New York City, and 62 miles at the latitude of Miami.

\textsuperscript{13}Note that this set-up excludes time zone boundaries at sea and with Canada and Mexico.

\textsuperscript{14}We conceptualize treatment at the county-year level (\( ct \)), given that all decisions to be
of one if a county is situated to the east of the closest neighboring time zone boundary, and zero if it is on the western side.

Following Keele and Titiunik (2015), we model the running variable in two dimensions. The variables $Y$ (distance latitude) and $X$ (distance longitude) are positive on the eastern side of a time zone boundary, and negative on the western side. As recommended in regression discontinuity applications (e.g., Lee and Lemieux 2010), we allow these to vary flexibly on either side of the cutoff. We also report results using the local specification of the running variable and optimal bandwidth criterion suggested by Calonico, Cattaneo and Titiunik (2014).

As in other regression discontinuity applications, the key identifying assumption of a GRDD is the continuity of the conditional expectation function of the running variable (Keele and Titiunik 2015). As we show below, observable time-varying factors unrelated to our treatment of interest overwhelmingly show balance at the cutoff.

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on one side of the time zone or the other near the cutoff are determined for each county in a specific period. Hence, our election data are collapsed to the level of the treatment, addressing potential correlations in our standard errors (Angrist and Pischke 2008). However, our preferred estimates with state-year fixed-effects remain discernible from 0 over a large bandwidth even if we cluster standard errors at the county-year level. Moreover, we conduct a robustness check in which we leverage variation within-county over time.
Fixed Effect Augmentation

As can be seen in Figure 1, time zone borders often follow state lines. This raises the concern that other state-level factors may confound the estimates from equation (1). However, we address this issue by leveraging the states that are split between two time zones. At present, 13 states in the continental United States fit this description.\textsuperscript{15} Moreover, some counties in states such as Arizona and Indiana do not observe daylight savings time (DST), whereas others do. Finally, several states are located in one time zone, but span large distances between borders, and thus contain both treated and control observations.\textsuperscript{16}

These sources of variation offer an opportunity to utilize an even stronger model specification that addresses potential state-level confounds. This specification estimates the effect of treatment with the inclusion of the full set of state by year fixed effects. It absorbs the confounding effect of all state (battleground status, electoral rules, the time the polls close in a given state, etc.), as well as time (electoral contexts, presidential vs. midterm, etc.), and state-time (differences in candidates or campaigns, competitiveness, etc.) factors that may be imbalanced at the time zone cutoff.

To formally estimate this model, we add a full set of state by year fixed effects to eq-

\textsuperscript{15}States with counties in two time zones include Idaho, Oregon, Arizona (during DST), North Dakota, South Dakota, Nebraska, Kansas, Texas, Michigan, Indiana, Kentucky, Tennessee, and Florida.

\textsuperscript{16}These include Montana and New Mexico (see Figure 1).
tion (1). This more conservative specification represents our preferred model. However, the addition of fixed effects does not significantly alter our results (see below), lending credence to the validity of the discontinuity we use.

In further robustness checks we leverage archival data on historical changes to time zone boundaries. This approach uses data from Indiana and Kentucky—states where the time zone boundary were most significantly changed. Before the 1960's, Indiana and Kentucky (for the most part) were located in the Central Time Zone. However, between 1960 and 1961 both states were divided into roughly equal parts between the Central and Eastern zones. In 1967, after conducting numerous hearings, the U.S. Department of Transportation placed most of Indiana in the Eastern zone—leaving counties adjacent to Chicago in the Central Zone. The distribution of counties was changed again in 2005-2007, with 7 counties switching across the timezone boundary during that period. The most recent change in Kentucky occurred in 2000, when Wayne County switched from the Central to the Eastern Time Zone. With these historical data we estimate a similar model with county and year fixed effects, which allows for even stronger comparisons.

**Heterogeneities**

We also investigate several theoretically compelling heterogeneities. First, we explore whether participation on the eastern side of time zone cutoffs is not only lower but also more unequal. To do so, we examine whether our effects vary by baseline vote propensity, by using quantile regression. This approach is an empirically-driven way of exploring treatment heterogene-
ity (Gamper-Rabindran, Khan and Timmins 2010). By comparing various quantiles across treatment and control rather than the mean, it permits us to show how treatment affects the entire distribution of the outcome.

Second, to examine whether our effect is moderated by voting costs — i.e. that tired citizens have a harder time overcoming other obstacles or hurdles that stand in their way — we explore whether our GRDD estimates vary along exogenous increases in voting obstacles. To do so, we use rainfall as a proxy. Several studies have shown that rainfall negatively affects voting by placing an additional hurdle in voters’ way (Fujiwara, Meng and Vogl 2016; Gomez, Hansford and Krause 2007; Henderson and Brooks 2016). This literature has also shown that rainfall is exogenous, hence making this subgroup analysis plausibly causal. (Our results do not change if we instead split our sample by how hard it is to register and vote in a given state, which may be endogenous.)

Finally, to examine the potential partisan implications of time zone borders, we investigate potential effects on party vote share. To do so, we substitute Democratic Party vote share in races for the House of Representatives ($D_{ct}$) as the dependent variable in our GRDD and GRDD & fixed effects models. If time zones move this outcome, we can conclude that shocks to time constraints not only affect electoral participation, but also election results.
Results

Do exogenous shifts in time allocation affect turnout? Figure 2 provides evidence from a non-parametric GRDD model. We observe a clear decline in voter turnout of about 2.6 percentage points (p < 0.001), at the boundary between observations on the East (the right side of the graph) and those on the West side (the left of the graph) of the time zone cutoff.\footnote{This result is equivalent if we use the Euclidean distance to the cutoff.} This effect is noticeable in size—representing approximately 24% of a standard deviation in voter turnout.

Figure 2 also shows that the relationship between geographic location and turnout overall is relatively smooth and continuous across geographic location. The function modeling the relationship is especially constant on either side of the cut-off within five degrees of the time zone border (the range over which we restrict our widest window in the models below). This suggests that little evidence of competing treatments relevant to voting varying near the cutoff (Lee and Lemieux 2010).

Figure 3 shows the estimated effect at different bandwidths. This comes from a GRDD specification with state by year fixed effects—our preferred modeling approach. In the narrowest specification—the least exposed to confounding influences away from the cutoff—the estimate suggests that treatment assignment decreases turnout by 1.81 percentage points (p < 0.001). The lowest point estimate (-0.38 percentage points) includes observations within
Figure 2 plots the local polynomial (order 4) fit of county-level turnout over the period 1992-2014 (as share of the total population), implemented with the \texttt{rdplot} command in \texttt{STATA}. Results come from a specification of the GRDD with the CCT optimal bandwidth (1.4 degrees) and local polynomial regression (Calonico, Cattaneo and Titunik 2014). Points represent bin averages, with corresponding 95\% confidence intervals shown with the corresponding bars. The figure shows that when the distance crosses the threshold from being located barely to the West to being located barely to East, the level of turnout drops noticeably: being lower by approximately 2.6 percentage points (p <0.001). N=35,520.

5 degrees of the cutoff. However, the plot shows evidence of a bias-variance trade-off in the choice of the bandwidth: point estimates are stable and become more precise in the first 3 degrees, but decrease in size afterwards. Consistent with our theory, this pattern may reflect that differences in sleep patterns level off the greater the distance away from the time zone cutoffs (Gibson and Shrader 2016). Given that narrower bandwidths are generally preferred to wider (Lee and Lemieux 2010), this result suggests that estimates including observations far from the cutoff may be susceptible to attenuation bias. Even in these models, however,
Figure 3 shows the coefficient estimates for the effect of being marginally on the Eastern side of the time zone cutoff on county-level turnout for the period 1992-2014, comparing only counties in the same state and in the same election year. Coefficient estimates are shown as points, with corresponding 90% (wider) and 95% (narrow) confidence intervals also shown. Results come from a GRDD specification outlined in Equation (1), augmented with state-year fixed effects. Model N from left to right: 3089, 6293, 9206, 12002, 14597, 17290, 19906, 22262, 24671, 27032.

there is evidence of an effect in the expected direction. In all, these estimates suggest that
the time disruption that occurs at the time zone boundaries has a negative effect on voter turnout.

As shown in Figure A6 in the Supplemental Information, the decline at the time zone border remains negative and significant in both Presidential and Midterm election years. The effect is also robust when examining the time zone cutoffs individually (Eastern, Central, and Mountain) and to including a large number of political and demographic controls.
Robustness Checks

Covariate Balance

To formally explore whether our results are confounded by other factors that affect voting, we employ data of county-level characteristics from the U.S. Census Bureau, and report the results in Figure A3 in the Supplemental Information. Across the 39 covariates observed in the Census files, only 1—the proportion American Indian—shows signs of imbalance, and we do not find that the covariates are jointly different from 0. We find balance on demographics including age, race, gender, education, income. Importantly, we find no evidence of imbalances in population, migrations, commute distances, and housing prices—suggesting that individuals are not precisely sorting around the time zone cutoffs. As we discuss further below in the potential mechanisms section, we also use ATUS data to show that our results do not appear to reflect other changes to time usage—with the 60 ATUS variables balanced at the cutoff. In short, our 99 tests of covariate balance do not indicate that our results are confounded by observables.

Permutation Tests

To investigate whether our results might be driven by a chance distribution of counties around the time zone cutoff, we execute a series of permutation tests often used in geographic regression discontinuity designs (e.g. Clinton and Sances 2016). This entails randomly shuffling counties, and iteratively estimating the same specification as in Equation (1). This series of
placebo tests offers a strong check of natural experiments’ validity. With these, we show that only 1.6% of our placebo estimates are as large as those we observe in our data with our models with state and year fixed effects (see Figure A4 in the Supplemental Information). Only very rarely do we obtain a placebo estimate that is greater than or equal to the observed estimates. This suggests that our results are not driven by the idiosyncratic distribution of counties unrelated to our treatment.

**Within-County Estimates: Leveraging Historical Changes to Time-Zone Boundaries**

Next, to demonstrate that our effects are not biased by unobserved county-level differences—such as the local administration of elections or other local political factors—we use historical data from changes in time zone boundaries in the states of Indiana and Kentucky. We estimate the specification outlined in Equation (1) augmented instead with county and year fixed-effects. This approach is identified based on Indiana and Kentucky counties that change time zones. This even stronger specification allows us to rule out any lingering time-constant local differences.

Figure 4 shows that being in the Eastern rather than the Central Time Zone causes about 2 p.p. lower turnout over the period of interest. These within-county effects are consistent with our within-state estimates, which further corroborates our quasi-experimental design.
Figure 3 shows the coefficient estimates for the effect of being marginally on the eastern side of the time zone cutoff on county-level turnout in Indiana and Kentucky (presidential election years only) for the period 1948-2014, comparing only same counties and while controlling for election year. Coefficient estimates are shown as points, with corresponding 90% (wider) and 95% (narrow) confidence intervals also shown. Results come from a GRDD specification outlined in Equation (1), augmented with county and year fixed effects. Model N from left to right: 2196, 4027, 5598, 6415, 6746, 6811, 6841, 6866, 6874.

Potential Mechanisms

What explains the noticeable decline in voter turnout on the eastern side of U.S. time zone cutoffs? Our identification strategy allows us to rule out a host of potential imbalances that may be driving this effect; such as idiosyncratic state, year, state by year, or county differences. As we laid out earlier in the paper, these effects could potentially be explained by changes to individuals time allotment, particularly their levels of sleep. While teasing out individual mechanisms is inherently difficult (Green, Ha and Bullock 2010), we provide suggestive evidence that lower turnout is, indeed, a reflection of voters being too tired to follow through and vote.

To explore this possibility, Figure 5 shows the effect of being on the eastern side of the
time zone border on levels of sleep reported in the ATUS. These come from the GRDD & state/year fixed effects specification to provide the most internally valid estimates possible. Consistent with our theoretical predictions and prior empirical work (Giuntella and Mazzonna 2015), we find that, on average, individuals living on the eastern side of a time zone border report sleeping about 21 minutes less than individuals living on the western side of the border (p <0.02).

This effect is meaningful in size and statistical significance, representing 15.3% of a standard deviation in sleep. The results are equivalent if we specify the dependent variable as a traditional indicator of getting sufficient sleep. Living on the eastern side of a time zone cutoffs leads to a 5% increase in the probability of having less than seven hours of sleep a night (p <0.03). This suggests that voters may not make it to the polls because they are too tired. This lower level of sleep may induce a bundle of downstream consequences which could, in turn, contribute to lower levels of turnout.

We show in the Supplemental Information that our results do not appear to reflect other changes in how individuals spend their time beyond sleeping less. To do so, we use other individual-level time use measures from the ATUS. These include measures of how much time individuals spend working, doing various types of housework, and engaging in leisure. Across

\[18\]

Data from fitness/sleep trackers show a similarly sized discontinuity in sleep patterns. See Figure A1 in the Supplemental Information. Further, Giuntella and Mazzonna (2015) show a similar effect with data from the Behavioral Risk Factor and Surveillance Survey (BRFSS).
Figure 5 shows the coefficient estimates for the effect of being marginally on the Eastern Side of the time zone cutoff on time spent sleeping. Data come from the American Time Use Survey (ATUS). Coefficient estimates are shown as points, with corresponding 90% (wider) and 95% (narrow) confidence intervals also shown. Standard errors clustered at the county-year level. Results come from a GRDD & fixed effects specification. N=20,042.

the 60 covariates observed in the ATUS relating to how one spends their day—be it working, doing house work, or engaging in leisure—59 (98.3%) are balanced.\(^{19}\) The one exception (time spent homeschooling children) is likely the result of chance. When bundled together in like categories, only time spent engaging in leisure shows some sign of increasing (\(\beta = 27.9\) mins., \(p = 0.069\)), which suggests that individuals who sleep less do not necessarily work more. This result is consistent with our broader theory; even with more free time, individuals may still not engage if they are tired. Not only does quantity of time matter, so too does the

\(^{19}\)For the full list of time measures used, see the Supplemental Information (Covariate Balance section).
motivation to use this time productively.

In the Supplemental Information, we also investigate two additional alternate mechanisms: patterns of ambient light during the day (Doleac and Sanders 2015) and TV shows aired simultaneously across time zones (Giuntella and Mazzonna 2015). To summarize, we find no compelling evidence that the inconvenience of voting when it is dark is driving our results. It appears, rather, that our effects are driven by individuals staying up later (and getting less sleep) because it stays lighter later. We also discuss how late night TV schedules may reinforce the effects of sunset time at the Central/Eastern cutoff.

**Heterogeneities**

To further unpack our effects, we look for theoretically compelling heterogeneities. We begin by estimating quantile regression models. If tiredness were to demobilize low propensity citizens more—as theory would predict—we would expect to see stronger effects at the bottom of the voter turnout distribution than at the top.

Our empirical findings comport with this prediction. Figure 6 shows this visually by plotting the estimated coefficients across turnout deciles. While the demobilizing effect is robust in all specifications aside from the top decile in the narrower bandwidth, the effect of tiredness on turnout is noticeably larger (in absolute value) in lower propensity areas than in higher propensity areas. These results indicate that time zones have their largest negative impact where turnout is already low—thus exacerbating participatory inequality.
Figure 6 shows the coefficient estimates for the effect of being marginally on the eastern side of the time zone cutoff on county-level turnout decile levels for the period 1992-2014. Coefficient estimates are shown as points, with corresponding 90% (wider) and 95% (narrow) confidence intervals also shown. Each coefficient shows the results from a separate RDD, quantile regression discontinuity model with a 1 degree bandwidth (latitude and longitude). Fixed effects are not included given the inherent difficulties of estimating quantile regression models with fixed effects (Gamper-Rabindran, Khan and Timmins 2010). N=6,293 for all quantile models.

In addition, if our estimates were indeed a reflection of a bundled tiredness effect, we might expect that these would be magnified where voting obstacles are higher—that is, where voting is more costly. To explore this possibility, we examine whether the effect of tiredness varies by levels of precipitation (a commonly-used exogenous proxy for voting costs). Figure 7 shows this heterogeneity visually, by plotting the GRDD coefficient estimates stratified by rainfall levels.
Figure 7 shows the coefficient estimates for the effect of being marginally on the eastern side of the time zone cutoff on county-level turnout by rainfall on Election Day (bandwidth=1 degree). Data come from House elections over the period 1992-2014. Comparison comes from only counties in the same state and in the same election year (i.e. state by year fixed effects). Coefficient estimates are shown as points, with corresponding 90% (wider) and 95% (narrow) confidence intervals also shown. Each coefficient plot shows the effect stratified by rainfall levels on Election Day at the median level (0.25 mm; results robust to changing this level). Rainfall data are drawn from Fujiwara, Meng and Vogl (2016). Model N from left to right: 3121, 2629.

As we would expect ex-ante if the effects were driven by tiredness, the coefficients are noticeably larger (in absolute terms) when rainfall is higher; that is to say, when voting obstacles are exogenously higher. Yet, when obstacles are lower, the impact of tiredness is muted. As we would predict, these results indicate that tiredness’ relationship with voting may be related to individuals’ ability to drum up the energy necessary to overcome voting obstacles.\footnote{Consistent with our effect being driven by tiredness, we also find that the negative turnout effect on turnout is larger in places where the sun sets later in November (i.e. the South over...}
we find a similar substantive pattern; tiredness’ effect is magnified where voting is harder (available on request).

**Partisan Effects**

We finally investigate whether the effect of our exogenous decrease in average sleep times produces any partisan advantages at the ballot box—shifting elections towards one party or the other. We do so by running the same regression discontinuity models as before, but this time with Democratic Party vote share as the outcome variable.

Figure 8: GRDD Effects on Democratic Vote Share

Figure 8 shows the coefficient estimates for the effect of being marginally on the eastern side of the time zone cutoff on county-level vote returns for Democrats for the period 1992-2014. Estimate on the right comes from a GRDD model, while that on the right comes from a GRDD & FE model comparing only counties in the same state and in the same election year. Coefficient estimates are shown as points, with corresponding 90% (wider) and 95% (narrow) confidence intervals also shown. Results come from a model specification of 1 degree on either side of the cutoff (both dimensions of the running variable). N=6,389 in both models.

Our results suggest that the exogenous shock in sleep times produces a distinct disad-
vantage to Democrats and, conversely, advantage to Republicans in elections for the House of Representatives. Figure 8 shows the effect of being on the eastern side of the time zone cutoff (i.e. the exogenous decrease in sleep) on Democratic vote share. We report results from both the simple GRDD and the GRDD supplemented with fixed effects. We find that the partisan heterogeneity in turnout appears to translate into decreases in vote shares by 1.6 percentage points for Democratic Party candidates. This effect is statistically significant, and substantively meaningful. It represents 7.5-10% of a standard deviation in Democratic vote shares, and is greater than or equal to the margin of victory in about 4-6% of Congressional races. In short, when races for Congress are close, time zone induced decreases in levels of sleep have the potential to swing election outcomes towards Republicans.

Our results are consistent with previous work showing that Democrats are particularly demobilized by exogenous increases in voting obstacles (e.g., Gomez, Hansford and Krause 2007; Brady and McNulty 2011; Henderson and Brooks 2016). Scholars have argued that this heterogeneity occurs because “Democratic voters, in particular, are sensitive to such costs, since they lack many of the participation-relevant resources of their wealthier Republican counterparts” (Henderson and Brooks 2016, 656). The effects of tiredness may be amplified by the fact that Democrats rely on more “peripheral” voters—who are demobilized by increased

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21 If we break the sample by electoral context, the results remain similar; Midterm ($\beta = -2.1$ p.p.; $p < 0.005$), Presidential ($\beta = -1.2$ p.p.; $p = 0.23$). These effects are robust and more precise at larger bandwidths.
tiredness—rather than “core” voters—who are not (Gomez, Hansford and Krause 2007, 652).

Conclusion

In this article, we have shown evidence of the importance of time allocation for voter turnout. Individuals exogenously nudged one hour forward by time zone boundaries vote at a rate 1.5–3 percentage points less than all-else-equal individuals on the other side of the cutoff. Consistent with our theory, this effect occurs unequally by vote propensity and obstacles to voting. Moreover, our results show that this treatment pushes election results towards Republicans. Our evidence indicates that these effects may follow from lower levels of sleep on the eastern side of time zone cutoffs.

These results have important implications for the study of the individual motives that drive people to participate in politics. They suggest that how one spends their time day to day—such as how much sleep they get—plays an important role in determining who votes and, ultimately, who wins elections. This implies that not only does the quantity of time one possesses matters—as others have prominently argued (Verba, Schlozman and Brady 1995)—but so too does the efficiency with which that time is used.

Our findings point towards tiredness as the likely primary mechanism. This indicates that casting a ballot requires non-trivial effort, and that many citizens may hold the predisposition to vote, but lack the energy to effectively act on those desires. While this has been suggested in previous descriptive research on general motivational attitudes such as grit or self efficacy,
ours constitutes, to our knowledge, the first study to explore the role of general motivation within a causal framework. In so doing, we have laid the groundwork for further research investigating the role of other personal experiences for voting.

To be clear about the limitations of our study, we only provide preliminary evidence that our findings are the result of tiredness. Our robustness checks are consistent with the view that lower levels of sleep are driving our results, but we acknowledge that they might be driven in part by other mechanisms, such as TV schedules or patterns of ambient light during polling hours. Moreover, we do not disentangle the multiple potential mechanisms through which this sleep-deprivation may affect civic engagement. While we have suggested several direct and indirect pathways, the mechanism may be unpacked in future work. Given the fundamental challenges facing causal mediation analysis (Green, Ha and Bullock 2010), doing so may be difficult. However, potential mechanisms may be tested by utilizing short-term sleep deprivation experiments, while remaining cognizant of their limited external and ecological validity. This approach will help address the inherently bundled nature of our natural experiment.

In conclusion, we note that our results have important implications for policy and practice. From a practical perspective, they suggest that interventions designed to increase voter participation may have to grapple with the fact that voters are constrained by their daily schedules. This may have implications for both how (e.g., getting prospective voters to get a good night’s rest before Election Day) and where (e.g., on the eastern side of time zone
boundaries) resources should be allocated to increase participation. Another recommendation pertains to polling hours. In Kentucky, for example, polls are open from 6 am to 6 pm, regardless of when sunrise and sunset occur. This schedule seems more conducive to the preferences of people on the western than eastern side of the time zone boundary. From a public health perspective, our results suggest that recent trends towards lower levels of sleep may have detrimental effects on levels and inequalities of citizen participation. Finally, our results suggest that electoral campaigns and advocates for higher levels of civic participation from a more diverse electorate would do well to not only focus their attention on the political institutions that shape participation decisions (i.e. electoral rules) but also consider the broader context that regulates citizens’ everyday lives.
References


**URL:** [https://escholarship.org/uc/item/8zp518hc](https://escholarship.org/uc/item/8zp518hc)


**URL:** [www.sethjhill.com/SJH_PresentBias.pdf](http://www.sethjhill.com/SJH_PresentBias.pdf)


Rosenstone, Steven and John M. Hansen. 1993. “Mobilization, participation and democracy in America.”.


Supplemental Information:
Time Zones, Tiredness, and Turnout: A Natural Experiment on How Time Constraints Influence Elections

Not intended for publication in printed versions
Time Zone GRDD

Here we use a qualitative example to reinforce the idea that time zone cutoffs lead to discontinuities in sleep patterns. Consider two people who are exactly the same, except for one person (call them person A) lives ever so slightly (a negligible distance, $\epsilon$) to the east of the other (call them person B). Both person A and person B make a decision about when to go to sleep, but are influenced by their circadian rhythms, which are driven in part by the amount of ambient light surrounding them. Without time zone boundaries, the time of day would be effectively the same for both these individuals as would the sunset each day (say at 7pm, $+/-.\epsilon$). As a result, their circadian rhythms would function the same and they would likely go to sleep at the same time (say at 10pm).

Now consider that a third actor—person C (i.e. the government)—comes in and draws an arbitrary line (i.e. a time zone boundary) in-between persons A and B, establishing that person A now lives one hour later that person B. This decision implies that the sunset time would now vary across the two individuals—being at 8pm for person A and 7pm for person B. As a result of this decision, person A would have to wait until later to get the cue from their body that it was approaching bedtime (what is now 11pm), whereas person B would get that signal at the same time (still at 10pm).\(^{A1}\) The result would be that person A (on the eastern side) would go to bed later than person B (on the western side). If both have to get up at the same labeled time the next day for work (say both at 6 am in their respective time zones), the resulting effect would be less overall sleep for person A (who would have had 7 hours of sleep) than person B (who would have had 8 hours of sleep).\(^{A2}\) In short, as individuals on the eastern side of the time zone cutoff go to bed later, they tend to be more sleep-deprived compared to all-else-equal individuals living on the western side of the time zone border.

\(^{A1}\)Nolan (2014) explains this phenomena simply, “our sleep [is] shaped by daylight. On the western extremes of time zones [i.e. on the eastern side of time zone barriers], people tend to go to bed later, and on the eastern edges [i.e. on the western side of time zone barriers] they go to bed earlier ... Unbeknownst to us, our body’s circadian rhythms tune to sunlight hours, silently influencing [us] to go to bed when the sun goes down irrespective of the exact time. (Nolan, Tyler. “Dance to the (Circadian) Rhythm” Jawbone Blog, October 7th, 2014, clarification about the geographic location added in [braces].)

\(^{A2}\)On this point, Giuntella and Mazzonna (2015) note, “wake-up time is less affected by solar cues than bedtime. Instead, wake-up times are importantly affected by work schedules and other social constraints (such as children’s school start times) which, in turn, respond to social conventions, economic incentives, and regional coordination” (on this point, see also Gibson and Shrader 2016).
Figure A1 shows counties across the time zones in the continental United States. The map is shaded by the time at which people go to sleep based on sleep tracker data from 1 million Jawbone users. Map from Jawbone’s blog post entitled “Dance to the (Circadian) Rhythm” by Tyler Nolan dated October 7th, 2014.
Figure A1 provides additional evidence corroborating the ATUS results shown in Figure 5. This maps comes from the fitness tracker company Jawbone. While these data are non-representative of the U.S. population and come with some measurement issues discussed by Giuntella and Mazzonna (2015), they offer a chance to investigate whether our manipulation check is driven by reporting biases in the ATUS time-use diaries. Analyzing the underlying data produced by these trackers, Giuntella and Mazzonna (2015) show that time zones clearly push bedtimes later (by about 0.3 hours, or 18 minutes). Using the same data, Gibson and Shrader (2016, 27) argue that “later sunset time delays sleep onset by more than it delays awakening” and, as such, these result in about 0.3 hours ($\approx 18$ minutes) less sleep, on average. These point estimate are quite similar to that from the ATUS data, suggesting that our manipulation check is not driven by the self-reported nature of the time-use diaries.

Figure A2: Diagram of Shifts in Ambient Light during the Day

<table>
<thead>
<tr>
<th>Western side</th>
<th>Eastern side</th>
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</thead>
<tbody>
<tr>
<td><strong>Sunrise</strong></td>
<td>7am</td>
</tr>
<tr>
<td>8am</td>
<td>school/work starts</td>
</tr>
<tr>
<td><strong>Sunset</strong></td>
<td>6pm</td>
</tr>
<tr>
<td>go home</td>
<td></td>
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</tbody>
</table>

Figure A2 shows how patterns of ambient light during the day vary on either side of time zone boundaries. The physical sunrise and sunset times are identical. However, social schedules differ. As a result, individuals living on the eastern side tend to have less ambient light before going to work in the morning, but more after going home in the evening.

The sleep deprivation hypothesis runs counter to an alternate explanation for differences across time zone boundaries: that light itself—that is, the absence of it—would suppress voter turnout. Figure A2 illustrates how patterns of ambient light during the day shift at the
time zone border. Given that November weather is often cold after dark, one might expect that the evening darkness could suppress turnout much like rain. However, our data indicate the opposite. On the eastern side of the border, where the sun rises and sets relatively later, we find less voting, not more.

We cannot definitely rule out that individuals living on the western side take advantage of ambient light to vote in the morning, before going to work. Nevertheless, we provide additional evidence below indicating that time discontinuities do not significantly affect how individuals organize their daily schedules, except for how much they sleep. Thus, the available data clearly point at tiredness as the primary mechanism driving our results.

As discussed in Giuntella and Mazzonna (2015), part of the tiredness effect may be driven by late night television schedules. These may also lead to discontinuities in sleep patterns at time zone boundaries. In the continental U.S., television networks usually broadcast two separate feeds, namely the “eastern feed” that is aired at the same time in the Eastern and Central time zones, and the “western feed” for the Pacific time zone. In the Mountain time zone, networks may broadcast a third feed on a one-hour delay from the Eastern time zone. Television schedules are typically posted in Eastern/Pacific time, and, thus, programs are conventionally advertised as “tonight at 9:00/8:00 Central and Mountain”. As a result, in the two middle time zones television programs start nominally an hour earlier than in the Eastern and Pacific time zones. While the effect of TV schedules may reinforce the effect of solar cues on bedtimes at the Eastern/Central border, it may attenuate the effect at the Mountain/Pacific borders. Figure A6 shows that this prediction finds support in the data. The negative effect of living on the eastern side of a time zone boundary is significantly stronger at the Eastern/Central border than at the Mountain/Pacific border (but present across all time zones, still).

Placebo Tests/Robustness Checks

To explore whether we are, indeed, picking up on the effect of this exogenous change in time spent sleeping and not some other factor important in the turnout decision we run a multitude of covariate balance tests. In testing the validity of a GRDD, scholars recommend a set of covariate balance tests similar to those run in a randomized control trial (Keele and Titiunik 2015). The recommended route is to subject these balance tests to the same requirements used for the models of interest. Hence, in our models below we use our preferred, most-stringent specification that includes state, year, and state by year fixed effects. These provide a strong placebo check for potential imbalances that may threaten continuity of the conditional expectation function at the cutoff.
Figure A3: Covariate Balance at the County-Level

Figure A3 shows the covariate balance for the effect of being marginally on the eastern side of the time zone cutoff within a 1 degree of bandwidth. Coefficient estimates are shown as points, with corresponding 90% (wider) and 95% (narrow) confidence intervals also shown. Results come from a RDD with state-year fixed effects specification outlined in Equation (1). Data drawn from the U.S. Census Bureau decennial census for the period 1992-2008 and from the annual American Community Survey (ACS) for 2010-2014. N=1,601 for median home value N=501 for migrations, and N= 6,310 otherwise.
Figure A3 provides evidence of the validity of the time zone cutoff plotting covariates at the unit of our analysis—the county-year. With multiple hypotheses being tested across the 39 covariates observed, we would expect a few to be significant simply by chance. Indeed, scholars have long recognized the importance of adjusting significance levels when multiple hypotheses are being run. In our application, only 1 (proportion American Indian) out of the 39 covariates (2.5%) show signs of imbalance. This is less than what we may expect simply by random chance. Furthermore, the imbalance does not meet the multiple-comparison adjusted threshold for statistical significance.\(^3\) Moreover, we fail to reject the null that the covariates are jointly equal to 0 in a model predicting treatment assignment (p=0.56). These placebo tests provide evidence that the treatment and control counties are separated as-good-as randomly.

Importantly, Figure A3 shows no evidence of sorting along the cutoffs. Our data indicate that population, housing prices and rents, as well as commute distances are balanced between counties located on the eastern and the western side of time zone borders over the main period of the study (1992-2014). Furthermore, county-level migration data from the ACS for 2010-2014 show balance on net migrations (difference between arrivals and departures) and when looking just at the number of individuals moving to a county. These estimates are noisy because they draw on ACS data only. However, they are consistently closer to zero when expanding the bandwidth. This suggests that people are not leaving marginally eastern counties, nor are they sorting when they move to the area.

In addition to these, we note that our results square with the balance/placebo tests run by Giuntella and Mazzonna (2015). In their working paper, Giuntella and Mazzonna (2015) look for imbalances at the time zone cutoff before time zones were established in 1914. Using data from the 1900 Census the show, in particular, that literacy rates at balanced at the time zone cutoffs. They also find, using more recent data, balance on other biological variables such as individual height. Finally, they acknowledge the power of controlling for latitudinal location in the formal GRDD models, arguing that as a result any “sorting [at the cutoff] would bias ... estimates only if individuals sort based on the timing of daylight” (Giuntella and Mazzonna 2015, 25).

Finally, we note that other time-use variables from the ATUS show balance.\(^4\) These include

\(^3\)For Bonferroni adjustments, the critical \(p\)-value when looking at \(k\) dependent variables is \(p/k\), which equals 0.00128 in this case. For Sidak adjustments, the critical \(p\)-value is \(1 - (1 - p)^{(1/k)}\), which equals 0.00131.

\(^4\)For a full discussion on the strengths and limitations of the ATUS dataset, including its
the time one spends working and various measures of housework.\textsuperscript{A5,A6} The time one spends in leisure may be greater; suggesting that individuals shift from sleeping to engaging in non-work behavior. However, this imbalance is consistent with our broader framework: with more quantity free time not being the only determinant of voting. Time efficiency matters above and beyond time quantity. Those who are too tired to vote may simply not do so, even if it means—strictly speaking—that they have more free time to do so.

\textsuperscript{A5}Time spent working comes from a measure of total hours usually worked per week.

\textsuperscript{A6}We check all of the ATUS items on household activities and caring for and helping family members available in all of our years. These items include time spent engaging in: Interior cleaning; Laundry; Sewing, repairing and maintaining textiles; Storing interior household items, including food; Food and drink preparation; Food presentation; Kitchen and food cleanup; Interior arrangement, decoration and repairs ; Building and repairing furniture; Heating and cooling; Interior maintenance, repair and decoration, not elsewhere classified; Exterior cleaning; Exterior repair, improvements and decoration; Exterior maintenance, repair and decoration, not elsewhere classified; Lawn, garden and houseplant care; Ponds, pools and hot tubs ; Care for animals and pets (not veterinary care); Vehicle repair and maintenance (by self) ; Vehicles, not elsewhere classified; Appliance, tool and toy setup, repair and maintenance; Financial management; Household and personal organization and planning; Household and personal mail and messages (except email); Household and personal email and messages; Home security; Household management, not elsewhere classified; Household activities, not elsewhere classified ; Physical care for household children; Reading to/with household children; Playing with household children, not sports ; Arts and crafts with household children; Playing sports with household children; Talking with/listening to household children; Organization and planning for household children; Looking after household children (as a primary activity); Attending household children’s events ; Waiting for/with household children ; Picking up/dropping off household children; Caring for and helping household children, not elsewhere classified ; Homework (household children); Meetings and school conferences (household children); Home schooling of household children; Activities related to household child’s education, not elsewhere classified; Providing medical care to household children; Obtaining medical care for household children; Waiting associated with household children’s health; Activities related to household child’s health, not elsewhere classified; Physical care for household adults; Looking after household adult (as a primary activity); Providing medical care to household adult; Obtaining medical care and care services for household adult; Waiting associated with caring for household adults; Caring for household adults, not elsewhere classified; Helping household adults; Organization and planning for household adults; Picking up/dropping off household adult; Waiting associated with helping household adults; Helping household adults, not elsewhere classified
Overall, these checks (along with our fixed effects augmentation in the text) suggest that our GRDD models are robust to potential sources of bias, both observed and unobserved. Simply put, our identification strategy allows us to identify the causal effect of naturally-occurring exogenous variation in sleep patterns.

**Permutation Tests**

To explore whether the discontinuities we observe could have arisen simply by chance distribution of counties around the time zone cutoffs, we conduct a set of permutation tests. These placebo tests benchmark the distribution of coefficients across random shuffles of counties. Over 1,000 iterations, we randomly assign counties to either the eastern or the western side of the cutoff with the corresponding running variable scores of the actual county located there. We then estimate the same regression discontinuity models as in Equation (1). We then benchmark our observed coefficients with the distribution of coefficients created by these placebo models. This offers a strong check of natural experiments’ validity (Clinton and Sances 2016).

Figure A4 shows the results from this series of placebo tests. It plots the distribution of coefficients we observe from each of the permutation tests (with a histogram) and the actual coefficients observed in our regression discontinuity models. As can be seen, very rarely do we observe a coefficient as large as our negative effect. When we run the regression discontinuity models without fixed effects, none of the 1,000 permutation tests produces an estimate less than or equal to our coefficient. When we run the model with fixed effects, only 1.8% of tests clear this threshold.

This placebo test suggests that our results are unlikely to be the product of chance distribution of counties around the time zone cutoffs.
Figure A4: Distribution of Permutation Test Coefficients
No Fixed Effects

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Figure A4 shows the distribution of permutation coefficient estimates (histogram) and the actual coefficient sizes (vertical lines). Results come from 1,000 iterations of random county shuffles with the regression discontinuity models with and without fixed effects. Results come from a specification with a bandwidth of 1 degree in both dimensions.

Are the Results Driven by Individual States or Counties?

Here we run a specification of our regression discontinuity models where we iteratively leave out one state at a time, to ensure our results are not driven by the idiosyncrasies of one state. Figure A5 shows the results from these. It shows that our results remain negative and statistically significant across all specifications. Georgia and to a lesser extent Texas and Wisconsin play appear to be influential for effect sizes, but the direction of these effects still remains the same without these states. The same holds true if we rerun the same check among split states with the full set of fixed effects. With this check, all checks remain negative, but we can see again that Texas (and perhaps Tennessee) play a role in explaining our effects. But again, our results do not seem to be driven by a single state.

\[\text{A7}\]

We find the same pattern when leaving out individual county-level observations. When we do so iteratively, the results do not change—suggesting that our results are also not driven by an individual county. Because there are over 3,000 counties in the United States, the figure for this test is quite unwieldy. However, even without the figure, the point clear: the results are not driven by one consistent outlier in our dataset.
Figure A5: Are ResultsDriven by One State?

Figure A8 shows the coefficient estimates for the effect of being marginally on the eastern side of the time zone cutoff on county-level turnout for the period 1992-2014, with a 5 degree bandwidth used and no fixed effects (so as to include portions of all states). Coefficient estimates are shown as points, with corresponding 90% (wider) and 95% (narrow) confidence intervals also shown. Results come from a RDD specification outlined in Equation (1).

Additional Robustness Checks

The finding reported in Figure 2 is robust to a number of alternate specifications. Figure A6 shows our RDD estimates across various specifications, this time modeling the running variable with both the proximity in degrees latitude and the degrees longitude as recommended by Keele and Titiunik (2015). In the base model (that on the far left of the graph), the point estimate for the effect of treatment is -2.68 percentage points (p <0.001).\textsuperscript{A8,A9} This negative and statistically significant effect is present across all three time zone cutoffs in our sample when we look at these separately, with the estimate for the Eastern Time Zone being the largest ($\beta = -3.61$ p.p.; p <0.001), that for the Central Time Zone the smallest ($\beta = -1.52$ p.p.; p <0.009), and that for the Mountain Time Zone being in-between the two ($\beta = -2.29$)

\textsuperscript{A8}Figure A7 in the Supplemental Information shows the based GRDD model effect estimates by bandwidth.

\textsuperscript{A9}A similar specification with a donut RDD—one that leaves out observations near the cutoff to deal with potential bias from heaping near the cutoff provides similar estimates. Leaving out observations within 0.5 degrees of the cutoff produces a result of $\beta = -3.10$ p.p. (p <0.001), while leaving our observations within 1.0 degrees of the cutoff produces a result of $\beta = -3.42$ p.p. (p <0.001).
p.p.; p < 0.003).\textsuperscript{A10} The effect is also negative and significant in both Presidential ($\beta = -3.15$ p.p.; $p < 0.001$) and Midterm ($\beta = -2.32$ p.p.; $p < 0.001$) election years.

Figure A6: Geographic Regression Discontinuity Results (Both Proximity Dimensions)

Figure A6 shows the coefficient estimates for the effect of being marginally on the eastern side of the time zone cutoff on county-level turnout for the period 1992-2014, with a 5 degree bandwidth used. Coefficient estimates are shown as points, with corresponding 90\% (wider) and 95\% (narrow) confidence intervals also shown. Results come from a RDD specification outlined in Equation (1). Demographic controls include population, gender, age, education, income, commute distances, and housing prices. Model N from left to right: 27032, 16474, 7568, 2990, 13695, 13337, 22466, 26820, 22254.

Our results are also robust to the inclusion of a vector of electoral rules, including the availability of registration at the DMV, early voting, no-excuse absentee voting, same-day registration, and preregistration, as well as a host of county-level covariates along with the electoral controls. Note that differences in point estimates partially reflect different samples sizes. However, the results show that our GRDD estimates are not driven by differences in electoral rules and county characteristics beyond tiredness.

\textsuperscript{A10}The reason for these heterogeneities may be the product of that the effect of treatment is stronger in urban than in rural areas, because sleep patterns are more constrained by social schedules.
Figure A7: Geographic Regression Discontinuity by Bandwidth

Figure A7 shows the coefficient estimates for the effect of being marginally on the eastern side of the time zone cutoff on county-level turnout for the period 1992-2014 across bandwidths. Coefficient estimates are shown as points, with corresponding 90% (wider) and 95% (narrow) confidence intervals also shown. Results come from a RDD specification without fixed effects.

Figure A7 shows the effect estimates for the base GRDD model without fixed effects by bandwidths. These correspond to the model on the far left of Figure A6. As can be seen, the effect estimates are robust across bandwidth.
Figure A8: Geographic Regression Discontinuity Results (VAP)

Figure A8 shows the coefficient estimates for the effect of being marginally on the Eastern side of the time zone cutoff on county-level turnout for the period 1992-2014, comparing only counties in the same state and in the same election year. Coefficient estimates are shown as points, with corresponding 90% (wider) and 95% (narrow) confidence intervals also shown. Results come from a GRDD specification outlined in Equation (1), augmented with state-year fixed effects. VAP estimates data retrieved from the Census for years 2000, 2005-2009, 2006-2010, 2008-2012, and 2010-2014 and matched to the closest year in the sample. Model N from left to right: 3082, 6277, 9180, 11976, 14551, 17237, 19850, 22200, 24582, 26857.

As mentioned in the text, we use population in the turnout denominator given the limited availability of voting age population numbers at the county-level over time.\textsuperscript{A11} If we are willing to make certain assumptions about population stability over time, we can use the Census' 5-year estimates of the number of individuals in a given county that are 18 and that are citizens. These estimates are available only for the 2000, 2005-2009, 2006-2010, 2008-2012, and 2010-2014 periods. Matching the years in our sample to the closest year available, we are able to reproduce the RDD estimates. When we do so, the effects are equivalent.

\textsuperscript{A11}Voting age population is available in some years in the Leip data, however this data suffers from a high degree of missingness—with many years having very few, if any, observations.
Figure A9: Geographic Regression Discontinuity & Fixed Effects (No State by Year Interactions)

Figure A9 shows the coefficient estimates for the effect of being marginally on the eastern side of the time zone cutoff on county-level turnout for the period 1992-2014 across bandwidths. Coefficient estimates are shown as points, with corresponding 90% (wider) and 95% (narrow) confidence intervals also shown. Results come from a RDD specification with state and year fixed effects, but no state by year interactions.

Figure A9 shows the GRDD & fixed effects specification without the state by year fixed effects. As in Figure 3 in the text, the results are shown across bandwidths. As can be seen, the results do not change with this slight change in specification.

GRDD Results from an Alternate Data Source

We view the Leip dataset as the best situated validated voting source to answer our question of interest given its availability over time. However, to check that our results were not an artifact of the Leip dataset or of our unit of analysis (county-year), we employed data from Catalist, LLC, a data vendor to political campaigns. Catalist supplements national voter
file data with consumer data, with their dataset containing about 250 million individuals. Specifically, we used a 1% sample (comprising approximately 3 million individuals) of their data file.\textsuperscript{A12} With this, we calculated our running variables (i.e. distance to the time zone cutoff) using zip-code-level geo-data provided by the U.S. Census Bureau and reran our GRD models. This dataset, while giving us more micro-level estimates, forces us to make assumptions about the randomness of purges from the voter file across the time zone boundaries given that we only have a single snapshot of the Catalist data.

Figure A10 shows the results of these models. In all specifications, we collapse the data to the zip-code-year level to deal with correlations within clusters and to improve computation time. On the left, we show the results for these collapsed data weighted at the individual level; on the right, we show the unweighted zip-code level results. The results show that our turnout effects shown in the paper are not the product of our dataset and its county-level unit of analysis. Across all model specifications, the effects are negative and statistically significant. These suggest that the demobilizing effect of time zone induced tiredness is about 2-4 p.p.\textsuperscript{A13} In short, tiredness’ effect on turnout is robust to this alternate individual-level data source.\textsuperscript{A14}

\textsuperscript{A12}The specific 1% sample was drawn in May 2014.

\textsuperscript{A13}The negative effect in the Catalist data is largely driven by a decline in turnout in primary elections, suggesting that tiredness may be especially important in lower salience elections.

\textsuperscript{A14}Unfortunately, the Catalist data is not well situated to look for party effects for two reasons. First, given that this source focuses on individual-level information and given the secret nature of the ballot, Catalist does not contain information on party vote shares. Second, as is well-known, the party variable in Catalist records suffers from a high degree of missingness given that many states do not disclose party registration in their voter files.
Figure A10 shows the coefficient estimates for the effect of being marginally on the eastern side of the time zone cutoff on individual (left) and zip-code–level turnout for the period 2008-2012 (primary and general elections). Data from Catalist 1% sample from May 2014. Coefficient estimates are shown as points, with corresponding 90% (wider) and 95% (narrow) confidence intervals also shown.

Observational Relationship Between Tiredness and Turnout

In this section we briefly present the results of an observational study showing that the relationship between tiredness and turnout holds with individual-level survey data. We note that these results are correlational in nature. However, this data source allows us to show that tiredness predicts turnout while controlling for a large number of potential confounds.

Our observational data come from the nationally representative National Longitudinal Study of Adolescent to Adult Health (Add Health). Add Health is a high-quality, non-political lon-
We employ this data source because it constitutes one of the few nationally-representative surveys with a comprehensive battery of measures of sleep patterns and also of voting. Moreover, it contains a large subsample of sibling pairs that allows us to control for a host of unobserved characteristics that may affect sleep patterns and voting.

From this data source, we created a composite scale—the Add Health Tiredness Scale (AHTS)—that measures individuals’ levels of sleep deprivation or tiredness. The AHTS is a composite measure of the items available in the Add Health survey related to sleep patterns, which have been used in other applications previously (Gangwisch et al. 2010). These include measures of whether individuals reported not getting enough sleep, sleeping less than seven hours a night, and having a lot of trouble falling and staying asleep, among others. The items in this scale have desirable levels of reliability (Cronbach’s $\alpha = 0.72$) and load on a common factor ($\text{Eigen}_1 \approx 2.45; \text{Eigen}_2 \approx 0.73$). As such these items were reduced to a single scale. Table A1 shows the correlates of the tiredness scale—showing that our tiredness scale

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$^A_{15}$Wave 1 of the Add Health survey was conducted during the 1994-95 school year and was a probability-based sample of adolescents ages 11-19 drawn from schools across the U.S. The Add Health sample was drawn at the school-level using a clustered sampling design to be representative of U.S. schools across region, urbanicity, size, type, and ethnicity. In addition to 80 randomly drawn high schools, 52 feeder schools (i.e. middle schools) were added for a total of 132 schools in the overall sample (Chantala 2006). The Wave 1 response rate was 79%. Among those initial respondents, 79% responded to the Wave 4 survey. Within sampled schools, all adolescents in grades 7 through 12 were invited to complete an in-school questionnaire and a random sample was additionally invited to participate in an in-home interview. In total, this process resulted in 20,745 student respondents in the first wave. The original cohort of students has been followed through three subsequent in-home interview waves, the most recent occurring in 2008-2009, when subjects were 25-33 years old.

$^A_{16}$This tiredness scale is a created as a composite measure of 18 items available in the Add Health survey. The specific items include: whether individuals reported not getting enough sleep (W1, W2), whether individuals got less than seven hours of sleep (W1, W2), Whether individuals reported having a lot of trouble falling asleep during the last 12 months (W1, W2), Whether individuals reported falling asleep at inappropriate times (W3), Whether individuals reported having a lot of trouble falling asleep during the last four weeks (W4), Whether individuals reported having a lot of trouble staying asleep during the last four weeks (W4), Whether individuals reported feeling tired for no reason (W1, W2), Whether individuals felt too tired to do things (W1, W2, W3, W4).
is not picking up cognitive ability, noncognitive ability, socioeconomic status, or personality.

Table A1: Correlates of the Add Health Tiredness Scale

<table>
<thead>
<tr>
<th>Variable</th>
<th>AHTS</th>
</tr>
</thead>
<tbody>
<tr>
<td>Noncognitive Ability (0-1)</td>
<td>-0.132</td>
</tr>
<tr>
<td>Cognitive Ability (0-1)</td>
<td>-0.003</td>
</tr>
<tr>
<td>Mother Education (1-9)</td>
<td>-0.007</td>
</tr>
<tr>
<td>Parents’ Income (1-11)</td>
<td>-0.008</td>
</tr>
<tr>
<td>Finish College (0/1)</td>
<td>-0.031</td>
</tr>
<tr>
<td>Political Motivation/Folded Ideology (0-2)</td>
<td>0.006</td>
</tr>
<tr>
<td>Non-White (0/1)</td>
<td>0.030</td>
</tr>
<tr>
<td>Age (10-19)</td>
<td>0.080</td>
</tr>
<tr>
<td>Female (0/1)</td>
<td>0.155</td>
</tr>
<tr>
<td>Church Attend (1-4)</td>
<td>-0.074</td>
</tr>
<tr>
<td>Big 5: Conscientious (0-1)</td>
<td>-0.101</td>
</tr>
<tr>
<td>Big 5: Extravert (0-1)</td>
<td>-0.013</td>
</tr>
<tr>
<td>Big 5: Agreeable (0-1)</td>
<td>0.048</td>
</tr>
<tr>
<td>Big 5: Open (0-1)</td>
<td>0.005</td>
</tr>
<tr>
<td>Big 5: Neurotic (0-1)</td>
<td>0.270</td>
</tr>
</tbody>
</table>

Table shows simple Pearson’s R correlation coefficients. Variables are coded as in the text. The scaling of individual items is included in parentheses next to the variable name in the first column.

We relate the AHTS with Add Health’s two measures of voting in the 2000 general elections (measured in Wave 3) and voting in local elections (measured in Wave 4). In our results below we show the results from bivariate, control, and sibling pair models that account for a host of observed and unobserved factors that may influence turnout that remain constant within families (e.g., shared genetics, political socialization, parents’ and siblings’ political motivation and vote propensity, etc.). This last specification provides us with a level of internal validity rarely achieved in observational studies of political participation (but for exceptions, see Burden et al. Forthcoming).

Figure A11 provides a coefficient plot displaying our estimates across three modeling approaches: bivariate, controls, and sibling fixed effects. The estimates provided in Figure A11 suggest that tiredness is associated with a 7-14 percentage point decrease in self-reported voting. These estimates are sizable and not statistically distinct across model specification. 4 out of 6 coefficients are statistically significant at the 5% level, one other is marginally significant.

\[A17\] These models, by construction, must omit individual (e.g. race) and contextual factors that remain constant within families.
insignificant at the 5% level (Wave 4 voting, with controls: p=0.055), and the last is not statistically precise (p \approx 0.30).

Figure A11: Observational Relationship Between Tiredness and Turnout (Add Health)

Figure A11 shows the coefficient estimates for the relationship between sleep deprivation and turnout (self reported). Data from the National Longitudinal Study of Adolescent to Adult Health (Add Health). Coefficient estimates are shown as points, with corresponding 90% (wider) and 95% (narrow) confidence intervals also shown. Individual-level controls include measures of individuals' cognitive ability, noncognitive ability (using the scale developed by Hillygus, Holbein and Snell (2015)), age, gender, religiosity, mother's education, family income, ideological strength (our only available proxy for political interest), race, educational attainment, the Big Five personality measures. We imputed missing cognitive ability (mean), family income (mean), mother's education (median), initial grade in school (median) and included indicators for imputed observations (results do not change if list-wise deletion is used instead). In the models, we also include data quality controls for straight lining to account for the possibility that tired individuals are less attentive survey takers. Geographic controls include measures of population, % urban, % white, % Hispanic, median age, std. dev. age, % males never married, fertility rate, abortions per 1000, residential stability, % foreign born, death rate per 1000, low birth weight, % in shelters, % in correctional institutions, % in college dormitories, median household income, std. dev. household income, % houses on public assistance, % below poverty, % with HS diploma, prop with college degree, unemployment rate, % employed in construction, median # of houses built, prop of houses moved into, % at risk school children, total physicians per 100000, total arrests per 100000, % of population is religious adherent, % conservative, % voting Republican in 1992, and the % of state spending towards education. Model N from left to right: 14316, 11280, 3798, 14796, 11277, 3797

The Add Health results report conditional correlations with self-reported turnout. Thus, the magnitude of the association and may not be comparable with our GRDD estimates. Nevertheless, our findings here are consistent with our quasi-experimental results. The correlation
between tiredness and turnout holds while controlling for a host of known predictors of voting, as well as unobserved confounds absorbed by sibling fixed-effects. In total, our results from both sources suggest that tiredness affects voting.