Time Zones, Tiredness, and Turnout: A Natural Experiment on the Effect of Sleep Deprivation on Voter Turnout and Election Results

John B. Holbein
Postdoctoral Research Fellow
CSDP
Princeton University
jholbein@princeton.edu
307 Robertson Hall
Princeton, NJ 08544

Jerome P. Schafer*
PhD Candidate
Department of Political Science
Yale University
jerome.schafer@yale.edu
115 Prospect st.
New Haven, CT, 06511

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Why don’t more people vote? In this article, we show that many citizens fail to cast a ballot because they are too tired. To do so, we leverage a natural quasi-experiment that exploits discontinuous decreases in sleep times on the eastern side of U.S. time zone boundaries. Our preferred model specification indicates that this exogenous source of sleep deprivation depresses county-level turnout in general elections by about 2 percentage points, and pushes election results towards Republicans. This effect appears to exacerbate participatory inequality—depressing turnout in low propensity communities most—and is magnified in areas where obstacles to voting are greatest. Our findings have important theoretical implications for the study of political participation. They highlight that life constraints may stand in the way of civic engagement. This suggests that many citizens hold the precursors to participation but lack the general, rather than expressly political, motivation to follow-through and actually vote.

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Why don’t more people vote? Over time in many democracies, the administrative barriers to voting have declined (Blais 2010). Nevertheless, many people still fail to cast their vote. In the U.S., for example, about 40% of registered voters do not participate in Presidential Elections, with abstention rates above 60% in Midterms and 70% in local elections (Hajnal and Trounstine 2016).

In this article, we suggest that the act of casting a ballot is not easy. Specifically, we explore whether many citizens fail to vote because they are too tired to do so. This question has significant theoretical implications. 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 (e.g., Carpini and Keeter 1993; Finkel 1985; Niemi, Craig and Mattei 1991; Prior 2010). In contrast, we argue that many non–voters have insufficient general, rather than explicitly political, motivation to cast a ballot. That is, large portions of the electorate have insufficient general drive and energy to overcome the obstacles standing in the way of the act of voting. This approach builds on recent descriptive studies, which have suggested that most citizens hold the attitudinal precursors to participating—whether those be measured through vote intentions, political interest, campaign attention, or sense of civic duty—yet, many of them fail to follow through and actually vote (Hill 2016).

To demonstrate that sleep deprivation exerts a negative effect on turnout, our identification strategy exploits discontinuities in sleep times at U.S. time zone boundaries. Building on prior work by (Giuntella, Han and Mazzonna 2016), we validate that all-else-equal individu-
als on the *eastern* side of time zone cutoffs sleep approximately 20 minutes less, on average, than those on the western side of time zone cutoffs. With a host of placebo tests, we validate that this exogenous shock separates voters into two similar groups—one that receives a bundled disruption to their sleep patterns and the other that doesn’t. Using a geographic regression discontinuity design (GRDD) that compares county-level turnout on either side of time zone cutoffs, we estimate that the downstream effect of this sleep-reduction is to depress voter turnout at the county-level by 1.5-3 percentage points. This result holds across a number of stringent robustness checks, including our preferred models which augment the GRDD with state-year fixed effects, and with separate nationwide individual-level voter file data.

Going one step further, we find that the effect of this exogenous source of deprivation is stronger among Democratic voters than among Republican voters. While our results do not allow us to exactly pinpoint which mechanisms lead to this shift in election returns, further analyses from quantile regression models show that the demobilizing effect of sleep deprivation is strongest among those least likely to vote at baseline—thus exacerbating participatory inequalities that may drive differences in election results. We also show that these negative effects are stronger in areas with inclement Election Day weather—adding credence to our theory (outlined in more detail in the next section) that sleep deprivation makes it more difficult for voters to overcome the barriers that get in the way of voting.

Our findings have important implications for the study of the roots of low and unequal
participation. Conventional explanations for these patterns have tended to place a great deal of emphasis on the role of political motivation (e.g. political interest, political efficacy, political knowledge). However, an emerging literature suggests that the willingness to vote is also a function of general, not explicitly political, motivations. These include the general motivation to persevere in the face of obstacles (Hillygus, Holbein and Snell 2015), to delay gratification (Fowler and Kam 2006; Hill 2016; Schafer 2016), and to believe that one can accomplish what they set out to do (Condon and Holleque 2013; Hobbs, Christakis and Fowler 2014; Ojeda 2015). This alternate framework implies that many citizens are biologically, psychologically, and socially constrained from voting by aspects of modern life that, on their face, seem to have little to do with politics.

Our results speak directly to this new literature, as tiredness is, at its core, a lack of general motivation. Although recent studies have begun to lay the groundwork for a broader framework that takes non-political motivational characteristics into account, the empirical evidence currently available is largely limited to findings that condition only on observable features, which raises concerns about confounding. By leveraging a natural quasi-experiment conducted at the national level, our design overcomes several challenges that have plagued previous work while also preserving a high degree of external and ecological validity. In short, our work provides causal evidence that general motivational attributes should be considered foundational in understanding what drives people to participate in politics. In so doing, our article helps break down the dichotomy between the public and private sphere, as has long
been advocated by political psychologists (e.g., Brody and Sniderman 1977) and political theorists (e.g., Habermas 1991).

This article also adds to the study of institutional rules and their effects on political behavior. Over the last two decades, many states have enacted provisions such as no-excuse absentee voting, mandatory vote-by-mail, same-day registration, and in-person early voting. Although recent studies show that some of these laws have increased turnout (Burden et al. 2014; Gerber, Huber and Hill 2013; Holbein and Hillygus 2016; Leighley and Nagler 2013), others appear to have engaged individuals who would have already voted (Meredith and Endter 2015; Keele and Minozzi 2013). Our study takes existing voting rules as given and instead focuses on non-political institutional rules (i.e., where time zone borders are located). Our results document that despite being mostly set long ago, these rules can substantively shape levels of electoral participation, the composition of the electorate, and, ultimately, the outcome of elections. As such, our work highlights the necessity of taking non-political institutional rules into account when designing measures that seek to increase voter participation.

Finally, in establishing that sleep deprivation has a causal effect on voting, our work contributes to a salient public health debate. By many accounts, sleep deprivation is a growing concern in the United States: producing what some have called a “crisis” (Pilcher and Huffcutt 1996; Cappuccio et al. 2010). Our work adds to the literature on sleep deprivation’s effects on multiple aspects of individual well-being (e.g., Ferrara et al. 2015; Giuntella, Han
and Mazzonna 2016; Spaeth, Dinges and Goel 2013), providing the first causal evidence of the link between sleep deprivation and civic engagement. We discuss recommendations for campaigns and the administration of elections in the conclusion.

**Conceptual Framework**

Why would sleep deprivation influence voting?\(^1\) To our knowledge, no prior work has examined this possibility. Verba, Schlozman and Brady (1995, 284)—in their seminal work on civic participation—perhaps come the closest, by mentioning that “time devoted to an informal community effort is time away from work, family, recreation, or sleep.”\(^2\) This viewpoint implies that time spent sleeping is time away from civic participation. Yet, the premise that more available free time uniformly increases participation may be misleading. For instance, recent studies 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). The unacknowledged possibility is that the same may hold true in the political realm—sufficient sleep, though technically leaving less room for civic participation, may actually enhance one’s capacity to engage.

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\(^1\)In this article, use the terms “tiredness,” “sleep deprivation,” “constantly tired,” and “a lack of sleep” interchangeably. Conceptually, we define our treatment as a set of personal attributes associated with a lack of general motivation.

\(^2\) *emphasis added*; A similar point is also made in Brady, Verba and Schlozman (1995).
Indeed, there are multiple direct and indirect reasons to expect that sleep deprivation influences voting. In a complex social environment, being more tired may lead to a bundle of behavioral changes. Before outlining the multiple channels potentially linking sleep deprivation and turnout, we briefly note that it is not our intent to fully unpack potential mechanisms in this article. While we do include some exploration in auxiliary analyses, here we focus on the first-order question of whether a causal relationship exists between sleep deprivation and turnout.

We start by noting that the potential relationship between sleep deprivation and turnout is consistent with various theories of voting. Tiredness may play a role if we examine voting as a rational trade-off between costs and benefits, or, alternatively, emphasize behavioral, psychological, or social influences.

From a rational-choice perspective, tiredness may shape the underlying “voter calculus” by affecting the opportunity costs that citizens face. The act of voting requires registration, locating and traveling to polling locations, waiting in line at the ballot box, navigating inclement weather on Election Day, and learning about the candidates and issues, to name a few (Brady and McNulty 2011; Gomez, Hansford and Krause 2007; Wolfinger and Rosenstone 1980; Pettigrew 2016). Together, these obstacles exert a non-trivial strain on individuals’ time, energy, and cognitive resources. Sleep-deprivation may augment these costs by draining general motivation, and decreasing the likelihood that individuals prepare for (e.g. gain information, register), and ultimately engage in voting. The notion that tiredness increases
the cost of voting is consistent with the empirical literature on choice fatigue in elections, showing that voters tend to resort to cheap decisions (such as abstaining) when ballots get longer and more complex (Augenblick and Nicholson 2016). In advanced democracies, the costs of voting are relatively low, but so are its expected benefits. Thus, a small negative shock may have significant effects on levels of turnout (Palfrey and Rosenthal 1985).

Tiredness may also be a useful concept if we take a psychological or behavioral perspective. Under this view, tiredness may get in the way of citizens’ following through on their predisposition to vote (Brody and Sniderman 1977; Hill 2016; Nickerson and Rogers 2010). This may occur if citizens want to vote, but fail to do so because of attitudinal biases, miscalculations, or other limited capacities that are exacerbated when individuals are tired. These effects may be magnified given behavioral persistence in voting patterns (Fowler 2006; Meredith 2009).

Tiredness could also explain lower turnout under a sociological framework. From this perspective, citizens participate in politics because it is a norm in their social group (Bond et al. 2012; Pietryka and DeBats Forthcoming; Verba, Schlozman and Brady 1995). Sleep deprivation may weaken individuals’ sensitivity to social norms and disrupt social connections (Ferrara et al. 2015), thus making them less likely to vote.

Regardless of the theoretical approach chosen, a direct relationship may exist between sleep deprivation and turnout. If being tired means that individuals are less motivated generally, the link between sleep deprivation and turnout may be straightforward. Less motivated
individuals may struggle to get out and vote. Put differently, general drive may play a role in whether or not citizens are able to overcome barriers to voting. This view builds on descriptive studies showing that individuals’ general levels of grit or perseverance, self-efficacy, and patience appear to be consistently correlated with participation (Condon and Holleque 2013; Fowler and Kam 2006; Hill 2016; Hillygus, Holbein and Snell 2015; Schafer 2016). By the same token, individuals who lack general motivation simply because they are tired may not have the wherewithal to overcome the barriers to voting.

Tiredness may also influence turnout indirectly through its 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; Cappuccio et al. 2010; Giuntella, Han and Mazzonna 2016) that may influence voting (Burden et al. Forthcoming; Ojeda 2015). Similarly, a lack of sleep may lead to noticeable declines in cognitive ability (Giuntella, Han and Mazzonna 2016; Lim and Dinges 2010)—a known predictor of voter turnout (Denny and Doyle 2008; Nie, Junn and Stehlik-Barry 1996; Verba, Schlozman and Brady 1995). Finally, experimental work suggests that sleep-deprivation lowers so called non-cognitive attributes associated with self-control, risk-aversion, and sociability (Ferrara et al. 2015), which have recently been linked to voting (Holbein 2016).

The effect of tiredness on turnout may also interact with contextual influences. In other
words, it may not only be mediated by indirect channels, but also moderated by factors magnifying (or depressing) its impact. For instance, the demobilizing effect of tiredness may be especially pronounced in areas where the obstacles that potential voters face are higher. In these areas, voting may require even more general motivation 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.

To summarize, there are various theoretical reasons to expect an effect of tiredness on voter participation. After all, sleep deprivation affects multiple aspects of life, which may also include civic participation. Yet, in order to convincingly demonstrate that it exerts a significant effect on voter turnout, our empirical analysis needs to rule out potential confounds. To do so, we leverage variation in tiredness that is plausibly exogenous to politics.

**Time Zone Discontinuities**

To explore whether tiredness influences turnout, our main identification strategy utilizes a naturally occurring quasi-experiment.\(^3\) Our specific approach leverages discontinuities at the

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\(^3\)Ethical considerations set bounds on the types of randomized experiments that can be conducted on the effects of tiredness on political participation. While we could deprive individuals of sleep for a short period of time (e.g., Ferrara et al. 2015), these short fluctuations
boundaries of U.S. time zones. For reasons discussed below, individuals living on the eastern side of time zone cutoffs sleep less than all-else-equal individuals on the western side of the cutoff. In this paper, we take advantage of this exogenous shift in sleep patterns to estimate their downstream effect on voter turnout. Here we provide theoretical and historical background substantiating the assumption that time zone borders induce an exogenous bundled shock to sleep patterns.

**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 made in 1883 to coordinate their clocks in order to regulate traffic. The precise locations of the original time zone borders were unrelated to politics. 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 to facilitate the “convenience of commerce.”

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For example, eight Indiana counties switched between Eastern and Central Time in 2006-2007 after petitioning their request to the USDOT. In general, though, these changes are few and far between, and our results are robust to holding out states where boundary changes were made.
The key intuition behind the natural experiment we employ is that individuals possess a biological sleep cycle, driven by the presence of “circadian rhythms” or “circadian clocks.” In this paper, we leverage the fact that these shared rhythms are influenced by arbitrary time zone cutoffs (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 to these differences in labeled times, fundamental biological processes make this adjustment difficult as the human body releases sleep hormones only once ambient light decreases and it becomes darker (Markwald et al. 2013). Moreover, social schedules such as late night TV shows aired simultaneously across time zones may reinforce the effect of sunset time. As a result of these processes, individuals on the eastern side of a time-zone boundary will tend to go to bed at a later time, but not necessarily to sleep longer in the morning (Giuntella and Mazzonna 2015). To reinforce this idea, we provide an illustrative example in the Online Appendix.

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 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, Han and Mazzonna 2016; Gibson and Shrader 2016).\footnote{Beyond focusing on a different outcome, the first stage of our paper differs from that by Giuntella and Mazzonna (2015) in important ways. Our paper models proximity to the time zone cutoff in two dimensions (i.e. latitude and longitude) rather than one (though they do control for raw latitude location in some of their models). Further, we supplement our models with a set of potentially important fixed effects to the model specification to ensure that the effects are not the product of potential imbalances at state cutoffs.}

As time zones are arbitrarily defined administrative boundaries that split individuals into areas that receive more sleep and areas that receive less, these boundaries are well-suited for a geographic regression discontinuity design (GRDD). In their work discussing the conceptual and practical issues associated with this design, Keele and Titiunik (2015) show that the GRDD 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 detail below), the GRDD will estimate causal effects similar to regression discontinuity approaches that leverage program eligibility (e.g., Cepaluni and Hidalgo 2016), close-elections (e.g., Caughey and Sekhon 2011; Eggers et al. 2015), or timing of treatment (e.g., Testa 2016). Put differently, if standard assumptions are met, the GRDD will approximate a standard randomized-control trial.

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 pat-
terns, 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 but exercise less, and are generally less healthy. In 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, it remains difficult to isolate which of any of these factors acts as the strongest mediator. Strictly speaking, what we estimate below is the bundled effect of being as-good-as randomly assigned to a lifestyle that includes lower levels of sleep. Below, we conduct robustness checks to address alternate explanations for our findings, but we also acknowledge the bundled nature of our treatment. While the mechanisms for any effect we observe may be many and complex, the effect of exogenously introducing lower levels of sleep is important in its own right.

**Time Zone Data**

To execute the analysis for this natural quasi-experiment, we compute counties’ distances to time zone boundaries using the geographic software **ArcGIS 10.3**. To do so, we retrieved county shapefiles from the census and matched these with historically accurate time zone shapefiles.\(^6\) In order to estimate chordal distances between the county centroid and the time

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\(^6\)We accessed these maps using [http://efele.net/maps/tz/us/](http://efele.net/maps/tz/us/).
zone cutoff, both shapefiles were projected into a two-dimensional plane.\textsuperscript{7} We then computed distances to the nearest time zone cutoff using the \texttt{near} function in \texttt{ArcGis}.\textsuperscript{8}

For our manipulation check, we use data from the American Time Use Survey (ATUS) conducted by the U.S. Bureau of Labor Statistics (BLS) since 2003.\textsuperscript{9} 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. This data allows us to show that our natural experiment provides exogenous variation in sleep patterns.

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.\textsuperscript{10} This repository provides the most comprehensive collection of election results over

\textsuperscript{7}We restrict the analysis to the 48 contiguous U.S. states to avoid bias due to the projection of Hawaii into a continental place space, and because Alaska’s electoral results are not reported at the county level.

\textsuperscript{8}With these shapefiles, we were able to successfully match over 99\% of our running variable scores to the corresponding county-year observations from the Dave Leip Congressional elections database.

\textsuperscript{9}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.

\textsuperscript{10}To supplement our county-level analysis, we also conduct our GRDD at the individual-level.
time at the county-level. As such, it has been used in a number of political science applications (e.g., Bickers 2013; Nall 2015). The unit of observation in this data-set is the county-year. With just over 3,000 counties in the continuous United States over a 12 year period spanning Midterm and Presidential Elections, our nationally-representative total sample size is just over 36,000.

Figure 1 visualizes the data that result from this match. For illustration purposes, dots are shaded by whether they are on the 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. 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. 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, to do so, we match nationwide voter file data from Catalist with running variable scores computed at the ZIP code level. Results for these are similar to those provided at the county level; as a result we report these in the Online Appendix.

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11 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.

12 Note that this set-up excludes time zone boundaries at sea and with Canada and Mexico.
and sometimes cut through state borders (a fact that we leverage in our GRDD & fixed effects models outlined below).
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.
GRDD Estimation

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

\[
\text{Turnout}_{ct} = \alpha + \beta_1 \text{Treatment}_{ct} + \beta_2 \text{Latitude}_{ct} + \beta_3 \text{Longitude}_{ct} \\
+ \beta_4 \text{Treatment} \ast \text{Latitude}_{ct} + \beta_5 \text{Treatment} \ast \text{Longitude}_{ct} + u_{ct} \tag{1}
\]

In equation (1), \( \text{Turnout}_{ct} \) is the proportion of individuals that turn out to vote in a given county \((c)\) and a given election year \((t)\).\(^{13}\) The variable \( \text{Treatment} \) is an indicator variable taking the value 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.\(^{14}\) The variables \( \text{Latitude} \) and \( \text{Longitude} \) contain values of the two-dimensional running variables, which are positive on the eastern side of a time zone boundary, and negative on the western side. As generally recommended in regression discontinuity applications (e.g., Lee and Lemieux 2010), we allow these to vary flexibly on either side of the cutoff.\(^{15}\) In equation (1), the coefficient of interest is \( \beta_1 \): the effect

\(^{13}\)With the Leip data, 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 A7).

\(^{14}\)In our estimates below, we focus on counties close to the cutoff so as to avoid overlap with the next time zone barrier to the west.

\(^{15}\)While there are currently well-established methods for specifying a single running variable
of being on the eastern side of the time zone cutoff when the running scores are arbitrarily close to zero. This provides us with an estimate of the causal effect of exogenous decreases in sleep (i.e. increases in tiredness or sleep deprivation).

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 describe below in the Robustness Checks section, observable time varying factors overwhelmingly show balance at the cutoff. This is perhaps unsurprising as historical evidence behind the drawing of time zone boundaries suggests that these were set non-parametrically and for selecting optimal bandwidths with a single running variable, the methods do not yet exist for specifying multiple running variables non-parametrically with optimal bandwidths. Moreover, recent work has documented the potential pitfalls of specifying the running variable parametrically with high-order polynomials (Gelman and Imbens 2014; Gelman and Zelizer 2015). To work within these two constraints, we take two approaches. First, we model the running variable naively in one dimension and use the non-parametric methods and optimal bandwidth criterion suggested by Calonico, Cattaneo and Titiunik (2014). Second, in our primary models we use our two running variables as flexibly linear, while varying the bandwidth used from very narrow to very wide. This allows us to preserve the best-practice suggested by Keele and Titiunik (2015) of modeling the running variable in two dimensions. As Lee and Lemieux (2010) note, flexibly linear specifications of the running variable may be sufficient, as long as the bandwidth is sufficiently narrow.
independently of other factors influencing turnout today. To be brief, our checks indicate 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. While this approach does not allow us to fully disentangle potential mechanisms, it does explore the first-order question of whether any relationship exists between sleep deprivation and turnout.

As shown 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 can 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{16} These splits usually occur when the urban agglomeration closest to less populated counties is situated in a neighboring state (e.g. Chicago suburbs in Indiana), or when an urban agglomeration is situated on the eastern or western edge of a state (e.g. El Paso, TX). 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{17}

\textsuperscript{16}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{17}These include Montana and New Mexico (see Figure 1).
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 state by year fixed effects. This 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 (or shared across) the time zone cutoff. In robustness checks based on data from Indiana, we estimate a similar model with county and year fixed effects which allows for even stronger comparisons.

To formally estimate our GRDD model, we add a full set of state by year fixed effects to equation (1). In this specification $\gamma_c$ is an indicator variable for the state in which a county is situated, $\eta_t$ an indicator variable for the year of the election, and $\rho_{ct}$ represents the full set of interactions between the two.\footnote{One alternative specification may supplement the county-level analysis with congressional district fixed-effects. Unfortunately, this would lead to bias because counties that are split across district lines account for 58% of the U.S. population and are substantially more likely to have Democratic legislators (by 18 percentage points) (Dynes and Huber 2015). However, we show that our within-state estimates are robust in presidential elections. This mitigates concerns about confounding by district-level differences.}

\footnote{These state by year interactions are identified given that the variable of interest is measured
Turnout_{ct} = \alpha + \gamma_{t} + \eta_{t} + \rho_{ct} + \beta_{1}Treatment_{ct} + \beta_{2}Latitude_{ct} + \beta_{3}Longitude_{ct} \\
+ \beta_{4}Treatment \times Latitude_{ct} + \beta_{5}Treatment \times Longitude_{ct} + u_{ct} \tag{2}

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.

**Methods: Heterogeneities**

In addition to exploring the overall effect of an exogenous increase in tiredness, in this article we also consider whether there are potentially important heterogeneities in our effect estimates. Here we place our attention on three theoretically compelling moderators.

We first investigate potential differences across political party. To do so, we substitute Democratic Party vote share in races for the House of Representatives as the dependent variable in our GRDD and GRDD & fixed effects models. If our exogenous increase in tiredness moves this outcome, we can conclude that this factor influences not only electoral participation, but also election outcomes.

Second, we use quantile regression to examine differences across vote propensity. This approach is an empirically-driven way of exploring treatment heterogeneity. Rather than focus on effects on averages, quantile regression examines the effect of treatment on the conditional at the county-year level. However, the results do not change if we omit these interactions (see Figure A8 in the Online Appendix).
quantiles of the dependent variable (Koenker 2005; Yu, Lu and Stander 2003). The advantage of this approach over stratification-based (or similar interaction) approaches is that it avoids arbitrary decisions regarding how to define high and low propensity subgroups, and provides a more comprehensive mapping of treatment effects over the distribution of the dependent variable (Angrist and Pischke 2008, ch. 7).\textsuperscript{20} Combining GRDD and quantile regression allows us to explore whether tiredness shifts the top of the turnout distribution more than the bottom.\textsuperscript{21} This method preserves the internal validity of the geographic regression discontinuity models, while giving us a data-driven approach to examine what type of citizens are most affected by this exogenous decrease in levels of sleep. If tiredness shifts the bottom of the distribution downward more than the top, we can conclude this factor promotes participatory inequality.

Finally, to examine one of the potential channels by which tiredness may influence turnout, we explore whether our GRDD estimates vary along exogenous increases in voting obstacles. To do so, we use rainfall as a proxy. Numerous authors have shown that rainfall negatively affects voting by placing an additional hurdle in voters’ way (Fraga and Hersh 2011; Fujiwara, Meng and Vogl 2016; Gomez, Hansford and Krause 2007; Henderson and

\textsuperscript{20}Quantile regression requires the distribution of the dependent variable to be continuous.

\textsuperscript{21}In our GRDD, quantile regression models, we omit the state by year fixed effects, given the difficulties associated with combining these two methods (Gamper-Rabindran, Khan and Timmins 2010).
Brooks 2016). This literature has also shown that rainfall is exogenous, hence making this 
subgroup analysis plausibly causal.\textsuperscript{22} In our estimates below, we use data from Fujiwara, 
Meng and Vogl (2016) that measures rainfall totals across counties over time. With this 
information, we stratify our GRDD models by counties that had high levels of rainfall (i.e.
greater than the median level) in the current election.\textsuperscript{23} This allows us to explore whether 
one of the mechanisms driving our effect is the difficulty tired individuals face in overcoming 
obstacles to voting. That is, tired individuals may be less likely to drum up the energy 
necessary to vote when it is raining. If what we are picking up in our GRDD is the causal 
effect of tiredness, we would expect that when the obstacles to voting increase (i.e. rainfall 
is higher), any demobilizing effect of tiredness would be magnified.

\textbf{Manipulation Check}

Figure 2 shows the effect of treatment on sleep patterns reported in the ATUS—a manipulation 
check for our purposes. These come from the GRDD & fixed effects specification to provide 
the most internally valid estimates possible. Consistent with our theoretical predictions and

\textsuperscript{22}For this reason, we prefer this heterogeneity above those that would employ electoral laws, 
for example, as these may be endogenous. In our view, rainfall provides a cleaner test of our 
thetical prediction that the effect of tiredness is moderated by obstacles to voting.

\textsuperscript{23}Our results are robust to similar specifications in the level which we cut rainfall into high 
and low groups.
prior work, 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).24 This effect is meaningful in size and statistical significance, representing 15.3% of a standard deviation in sleep. If we specify the dependent variable differently—as an indicator of whether an individual reported getting less than seven hours of sleep a night (a common measure used by sleep scientists)—the results are equivalent. Living on the eastern side of a time zone cutoff leads to a 5% increase in the probability of having less than seven hours of sleep a night (p <0.03). This decline in sleep times is robust when we specify the model with one dimension of the running variable (i.e. degrees longitude—a common approach in GRDDs).

Giuntella and Mazzonna (2015) estimate an effect of 20 minutes: nearly identical to ours.
Figure 2: The Effect of Being on the Eastern Size of a Time Zone on Sleep Times (ATUS)

Figure 2 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. Results come from a GRDD & fixed effects specification. N=20,042.

Our estimates provide clear, causal evidence that the time zone cutoff introduces a systematic exogenous decrease in sleep. They suggest that individuals respond by sleeping less when their bedtime is arbitrarily nudged one hour forward. This result, combined with our placebo tests and fixed effect specification, corroborate the plausibility of GRDD assumptions when estimating the effects of time zone discontinuities on voter turnout and election outcomes.

25This effect can also be observed in other data sources. Data from fitness/sleep trackers show a similarly sized discontinuity in sleep patterns. See Figure A1 in the Online Appendix.
Results

As predicted, Figure 3 also shows a drop in turnout that corresponds with exposure to lower levels of sleep at the time zone cutoff. We observe a clear decline in proportion voting by about 2.2 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.\textsuperscript{26} This effect is noticeable in size—representing approximately 20.8\% of a standard deviation in voter turnout. For further comparison, this effect size is roughly equivalent to the demobilizing effect of an additional 1.73 inches of rain on Election Day.\textsuperscript{27}

Figure 3 also shows that the overall 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 is reassuring as it provides further evidence that the natural quasi-experiment that we are using is relatively well-controlled—with little evidence of competing treatment relevant to voting varying near the cutoff (Lee and Lemieux 2010).

\textsuperscript{26}This result remains the same if we use the Euclidean distance to the cutoff.

\textsuperscript{27}Based on Fujiwara, Meng and Vogl (2016) estimate of a 0.05 decline in turnout for each additional millimeter of rain (see their Table 2). 2.2 percentage points/0.05 percentage points per mm=44mm=4.4cm=1.73in.
As shown in Figure A5 in the Online Appendix, this decline at the time zone border is robust when examining the time zone cutoffs individually and including a large number of political and demographic controls. The effect also remains negative and significant in both Presidential ($\beta = -3.15 \text{ p.p.; } p < 0.001$) and Midterm ($\beta = -2.32 \text{ p.p.; } p < 0.001$) election years.\(^{28}\)

Figure 3: The Effect of Being on the Eastern Side of a Time Zone on Voter Turnout

Figure 3 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 `rdplot` command in STATA. Results come from a specification of the GRDD with the CCT optimal bandwidth and local non-parametric regression (Calonico, Cattaneo and Titiunik 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 2.2 percentage points ($p < 0.001$). N=35,520.

\(^{28}\)This holds true in our models with fixed effects described further below: Presidential ($\beta = -1.8 \text{ p.p.; } p < 0.001$) and Midterm ($\beta = -1.0 \text{ p.p.; } p < 0.061$).
Figure 4 shows the estimated effect of an exogenous decrease in sleep at different bandwidths. This comes from a GRD 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 1.76 percentage points ($p < 0.02$). The lowest point estimate (-0.3 percentage points), which includes observations within 5 degrees of the cutoff, remains statistically significant at the $p < 0.10$ level. 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. 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), it suggests that estimates including observations far from the cutoff may be susceptible to attenuation bias. Even in these conservative models, however, there is robust evidence of a sizeable effect in the expected direction.

29Figure A9 in the Online Appendix shows that individual-level GRDD estimates using voter file data are consistent with our county-level results.
Figure 4 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 show as points, with corresponding 90% (wider) and 95% (narrow) confidence intervals also shown. Results come from a GRDD & fixed effects specification outlined in Equation (2). Model N from left to right: 3089, 6293, 9206, 12002, 14597, 17290, 19906, 22262, 24671, 27032.

In all, these estimates suggest that exogenous decreases in sleep duration lead to a significant reduction in voter turnout. This indicates that a causal relationship exists between expressly non-political individual characteristics, such as tiredness, and political participation. Further, these results show that arbitrary, seemingly benign non-political institutional rules, such as the specific location of time zone cutoffs, can substantively shape electoral participation.
Robustness Checks

Our main estimates show clear evidence of discontinuities in voter turnout at U.S. time zone boundaries, and our manipulation check suggests that these effects may be the result of the bundled tiredness treatment. Moreover, our main identification strategy based on split states allows us to rule out many potential confounds. That said, even this check does not allow us to rule out the possibility of local county-level factors—such as the local administration of elections—driving our result. In order to adjudicate this possibility, we conduct a number of placebo tests outlined here.

Balance on Observable County-Level Characteristics

We begin by first testing for covariate balance along a host of time-varying observable county-level characteristics from the U.S. Census Bureau (see Figure A2 in the Online Appendix). Across the 37 covariates observed, only 1 (2.7%) shows signs of imbalance—slightly less than what we may expect simply by random chance. For example, we find no evidence of imbalances in population, age, race, gender, education, income, commute distances, and housing prices, to name a few. This provides assurance that the location of time zones is not correlated with observed county-level variables affecting turnout. It further suggests that there is little to no evidence of sorting around the threshold: if there were, we would expect to see imbalances in observable characteristics (Lee and Lemieux 2010). In short, this is supportive that counties are separated as-good-as randomly by time zone boundaries.
Permutation Tests

Next, we investigate whether unobserved county-level differences that are uncorrelated with time zone borders may be responsible for the magnitude of the effects we identify. To do so, we randomly shuffle counties, and iteratively estimate the same specification as in Equation (2). Generally speaking, this series of placebo tests offers a strong check of natural experiments’ validity (Clinton and Sances 2016). With these, we show that only 1.8% of our placebo estimates are as large as those we observe in our data (see Figure A3 in the Online Appendix). This suggests that our results are not driven by the idiosyncratic distribution in counties unrelated to our sleep deprivation treatment.

Within-County Estimates: Evidence from Indiana

Finally, to demonstrate that our effects are not biased by rigid unobserved county-level factors—such as the local administration of elections—we use historical data from changes in time zone boundaries in the state of Indiana. Whereas time zones rarely change in most U.S. states, the precise location of time zone borders in Indiana has evolved over time. Before the 1960’s, the entire state was located in the Central Time Zone. However, in 1961 Indiana was divided into roughly equal parts between the Central and Eastern zones. In 1967, after conducting numerous hearings, the U.S. Department of Transportation placed

30For a history of time zones in Indiana, see Shanks (1987). We use his coding of counties in our analysis below.
most of the state 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.

Most of these changes occurred prior to the period of analysis used in the rest of the paper (1992-2014). Thus, with Indiana we extend the timeframe of our study to also include data from 1948-1992. To do so, we use turnout data from Fujiwara, Meng and Vogl (2016). With this approach, we estimate the specification outlined in Equation (1) augmented instead with county and year fixed-effects. This approach is identified based on Indiana counties that change time zones. This even stronger specification allows us to rule out any lingering time-constant local differences.

Figure 5 shows that being in the Eastern rather than the Central Time Zone causes 1.6-2.2 p.p. lower turnout in Indiana over the period of interest. While the estimates from a single state are inherently more noisy, this effect is in the same order of magnitude as our nationwide estimates, which corroborates our quasi-experimental design.
Figure 4 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 for the period 1948-2014, comparing only same counties and while controlling for election year. Coefficient estimates are show 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: 1223, 2058, 3124.

Given that we are using a natural experiment, we may never be able to fully rule out all differences. That said, our placebo tests allow us to reject a host of alternate explanations for the turnout effects in our geographic regression discontinuity design. The decline in turnout on the eastern side of the time zone border is not the result of: observed or unobserved time-constant differences in states or elections years, observable time-varying county characteristics, the idiosyncratic distribution of counties around time zone borders, or observed and unobserved factors that remain constant within counties. Our result is consistent with our turnout effects being driven by the bundled tiredness treatment we outlined earlier.
Heterogeneities

To unpack our effects, we first 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.

Our results suggest that the exogenous shock in sleep times produces a distinct disadvantage to Democrats and, conversely, advantage to Republicans in elections for the House of Representatives. Figure 6 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 2.2 percentage points for Democratic Party candidates.\(^{31,32}\) This effect is statistically significant (\(p < 0.003\) in models with fixed effects; \(p < 0.03\) in models without fixed effects), and substan-

\(^{31}\)Given the predominant two-party structure of elections in the U.S., the estimate for Republican vote share estimate is roughly the absolute value of the Democratic vote share estimate. There are some instances where Democratic candidates received either all or no votes. These occur when individuals run unopposed for the House. Our results are robust to leaving these observations out of our estimation (\(\beta = -1.4\) p.p.; \(p = 0.045\)) or to robust regression that better accounts for these potential outliers (\(\beta = -2.3\) p.p.; \(p = 0.024\)).

\(^{32}\)If we break the sample by electoral context, the results remain similar; Midterm (\(\beta = -2.5\)
tively meaningful. It represents a 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.

Figure 6: GRDD Effects on Democratic Vote Share

Figure 6 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 show 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.

Teasing apart the exact mechanisms driving this partisan heterogeneity is inherently p.p.; p = 0.012), Presidential (β = -1.8 p.p.; p = 0.077). Presidential vote share in a county is highly correlated with House vote share in that same county (r = 0.6 – 0.8 depending on the years included). When we look at presidential vote share the results do not change (-1-3 p.p. depending on specification).
difficult. However, we conduct additional tests below showing that the effects of tiredness on turnout are strongest among those least likely to vote at baseline, and those facing greater obstacles to voting.

**Participatory Inequality**

We next turn to the effect of our exogenous decreases in sleep rates by baseline vote propensities. To do so, we estimate quantile regression models. If tiredness were to demobilize low propensity citizens, we would expect to see stronger effects at the bottom of the county turnout distribution than at the top.

Our empirical findings comport with this prediction. Figure 7 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. This pattern holds regardless of the bandwidth used. In wider bandwidths (5 degrees in both dimensions), the effect is almost twice as large for low turnout areas ($\beta = -3.62$ p.p.; $p < 0.001$) than it is for high turnout areas ($\beta = -1.90$ p.p.; $p < 0.001$). In narrower bandwidths, the difference is even more pronounced (bottom decile: -6.4 p.p.; top decile: -0.7 p.p.). These effect estimates are statistically distinct at the 1% level. This is the case despite the fact that there are inherently floor effects as we move lower in the turnout distribution.
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 decile levels for the period 1992-2014. Coefficient estimates are show 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. Fixed effects are not included given the inherent difficulties of estimating quantile regression models with fixed effects (Gamper-Rabindran, Khan and Timmins 2010). N=27,032 for all quantile models.

These results show that tiredness has its largest impact where turnout is already low—suggesting that tiredness appears to exacerbate underlying inequalities in voter participation.
These differential effects may play into the party differences shown in the last section. In-
somuch as Democratic candidates draw their support from low propensity subgroups, they
may find that tiredness is especially potent.

Voting Obstacles

In this section, we explore whether the effect of tiredness varies by voting obstacles. Figure 8
shows this visually, by plotting the GRDD coefficient estimates while breaking them by
rainfall levels. As we would expect ex-ante if our estimates were picking up on the effect
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.

These findings comport with the notion that our GRDD estimates capture the effect of
tiredness—or, a lack of general motivation to overcome voting obstacles. Simply put, they
indicate that tiredness’ relationship with voting may be related to individuals’ ability to
drum up the energy necessary to overcome voting obstacles. We also note that our results
are consistent with rainfall studies which show that Democrats are particularly demobi-
lized by exogenous increases in voting obstacles (e.g., Gomez, Hansford and Krause 2007;
Hansford and Gomez 2010; 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). A similar story may explain our tiredness
estimates. Regardless of the reasons behind partisan differences, however, these patterns are vitally important. In short, they suggest that arbitrary non-political rules (i.e. the location of time zone borders) driving exogenous negative shocks to individual general motivation (i.e. tiredness) have potentially large impacts not just on overall turnout, and on who votes, but also on how Congressional races play out.

Figure 8: GRDD Effects by Voting Obstacles (i.e. Rainfall)

Figure 8 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 comes 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 show 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 drawn from Fujiwara, Meng and Vogl (2016). Model N from left to right: 3121, 2629.
Conclusion

In this article, we have shown that sleep deprivation exerts a significant negative effect on voter turnout. Our geographic discontinuity design indicates that this effect pushes election results towards Republicans. While these results may be driven by multiple mechanisms, our estimates indicate that the effect of sleep deprivation occurs unequally by vote propensity and obstacles to voting, thus reinforcing participatory inequality. Overall, these findings suggest that behavioral characteristics such as sleep patterns play an important role in determining who votes and, ultimately, who wins elections.

These results have important implications for the study of the individual motives that drive people to participate in politics. They suggest that casting a ballot requires non-trivial effort, and that many citizens may hold the predisposition to vote, but lack the general motivation to effectively act on it. While this has been suggested in previous descriptive research ours constitutes, to our knowledge, the first study to demonstrate the causal effect of general motivational attributes on voting at the national level. In so doing, we have laid the groundwork for further research exploring the role of other general motivational characteristics. These include how much individuals generally believe they can do what they set out to do, how willing they are to deal with frustration in order to follow through on their goals, and how skillful they are in overcoming the obstacles that stand in their way. The role of these attributes may be even greater in other forms of participation such as civic activism that require more individual effort than voting.
Our findings also contribute to the study of the institutions that shape political behavior. They document that arbitrary, seemingly benign non-political institutional rules—such as the specific location of time zone borders—can substantively shape overall electoral participation, the composition of the electorate, and, ultimately, the outcome of elections. This suggests that scholars of civic engagement should not only focus their attention on expressly political institutions, such as electoral rules, but also on the broader set of institutions that govern citizens’ everyday lives. In showing that the effects of these non-political rules are moderated by political context, we help to bridge the divide between institutional and behavioral approaches in the study of elections.

To be clear about the limitations of our study, we have focused our attention on identifying the direct effect of sleep deprivation on turnout rather than on perfectly disentangling the multiple potential mechanisms through which this treatment may matter. While we have suggested several direct and indirect pathways, and explored some of these, more work is necessary to fully explore potential channels linking tiredness and turnout. Given the fundamental challenges facing causal mediation analysis (Green, Ha and Bullock 2010; Imai et al. 2011), doing so may be difficult. However, potential mechanisms may be tested by employing a comparative lens or by utilizing short-term sleep deprivation experiments, while remaining cognizant of their limited external and ecological validity. This approach in future research will also help address the inherently bundled nature of natural experiments.

In conclusion, we note that our results have important implications for policy and prac-
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 non-political biological and social factors. This may have implications for both how (e.g., encouraging prospective voters to get a good night’s rest before Election Day, distributing coffee in advance of the election) 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 policy 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, non-political institutional rules that govern citizens’ everyday lives.
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Online Appendix:
time zones, Tiredness, and Turnout: A Natural Experiment on
the Effect of Sleep Deprivation on Voter Turnout and Election
Outcomes

Not intended for publication in printed versions
We use an illustrative example to reinforce the idea that time zone discontinuities 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 than 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). 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). 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.

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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, Han and Mazzonna (2016) 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 Hamermesh, Myers and Pocock 2008; Gibson and Shrader 2016; Roenneberg and Merrow 2007).
As discussed in Giuntella and Mazzonna (2015), part of this 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.\textsuperscript{A3}

Figure A1 shows evidence that supports the ATUS results shown in Figure 2. This maps comes from the fitness tracker company Jawbone. While this data is imperfect—being non-representative of the U.S. population and coming with some tricky measurement issues discussed by Giuntella, Han and Mazzonna (2016)—it offers a chance to informally verify whether our manipulation check is driven by reporting biases in the ATUS time-use diaries. Analyzing the underlying data produced by these trackers, Giuntella, Han and Mazzonna (2016) 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 (≈ 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.

\textsuperscript{A3}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 A5 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).
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.
Placebo Tests/Robustness Checks

Covariate Balance at the Time Zone Border

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 any RDD, scholars recommend a set of covariate balance tests similar to those run in a randomized control trial (Eggers et al. 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 A2 shows the covariate balance for the effect of being marginally on the Eastern Side of the time zone cutoff within 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 (2). Data drawn from the U.S. Census Bureau decennial census and from the annual American Community Survey (ACS).
Figure A2 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 37 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 37 covariates (2.7%) show signs of imbalance. This is less than what we may expect simply by random chance. Furthermore, the imbalance fails to meet the multiple-comparison adjusted threshold for statistical significance. These placebo tests provide evidence that the treatment and control counties are separated as-good-as randomly.

In addition to these, we note that our results square with the balance/placebo tests run by Giuntella, Han and Mazzonna (2016). In their working paper, Giuntella, Han and Mazzonna (2016) look for imbalances at the time zone cutoff before time zones were established in 1914. Using data from the 1900 Census they 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, Han and Mazzonna 2016, 25).

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 Equations (1) and (2). 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

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\( A^4 \)For Bonferroni adjustments, the critical \( p \)-value when looking at \( k \) dependent variables is \( p/k \), which equals 0.00135 in this case. For Sidak adjustments, the critical \( p \)-value is \( 1 - (1 - p)^{(1/k)} \), which equals 0.00139.
Figure A3 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 A3: Distribution of Permutation Test Coefficients**

No Fixed Effects | Fixed Effects
---|---

Figure A3 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 A4 shows the results from these. It shows that our results remain negative and statistically significant at high levels across all specifications. Georgia and to a lesser
extent Texas and Wisconsin play an important role in the effect estimates, but the effects still remain in the same direction without these states.

The same can be said for 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 A4: Are Results Driven by One State?

![Figure A4: Are Results Driven by One State?](image)

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, with a 5 degree bandwidth used and no fixed effects (so as to include portions of all states). Coefficient estimates are show 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 3 is robust to a number of alternate specifications. Figure A5 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{A5} This negative

\textsuperscript{A5}\textsuperscript{A5} Figure A6 in the Online Appendix shows the based GRDD model effect estimates by bandwidth.

\textsuperscript{A6} 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 (Barreca, Lindo and Waddell 2016)—
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 (\( \beta = -2.29 \) p.p.; \( p < 0.003 \)).\(^7\) 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.

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.

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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. (\( p < 0.001 \)).

\(^7\)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 A5 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 show 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.

Figure A6 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 A5. As can be seen, the effect estimates are robust across bandwidth.
Figure A6: Geographic Regression Discontinuity by Bandwidth

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 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.

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{A8} 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 no different. As when population is used in the denominator, the effects range somewhere between 2.6 and 4.3 percentage points. The effect estimate for the Mountain Standard time zone (MST) is not significant at traditional levels ($p \approx 0.127$), however, this is likely due to power issues given that this cutoff is the most sparsely populated of the three individual cutoffs. Figure A7 shows these effects visually.

\textsuperscript{A8}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 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, with a 1 degree bandwidth used. Coefficient estimates are show as points, with corresponding 90% (wider) and 95% (narrow) confidence intervals also shown. Results come from a RDD specification outlined in Equation (1). 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.

Figure A8 shows the GRDD & fixed effects specification without the state by year fixed effects. As in Figure 4 in the text, the results are shown across bandwidths. As can be seen, the results do not change with this slight change in specification.
Figure A8: Geographic Regression Discontinuity & Fixed Effects (No State by Year Interactions)

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 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.

Zip-Code Data

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{A9} With this, we calculated our running variables (i.e. distance to the time zone cutoff) using zip-code-level geo-data provided by

\textsuperscript{A9}The specific 1% sample was drawn in May 2014.
the U.S. Census Bureau and reran our GRD models.

Figure A9 shows the results of these models. In all specifications, we collapse the data to the zip-code-year level—an approach suggested by Angrist and Pischke (2008) 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. In short, tiredness’ effect on turnout is robust to data source.

A10 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.

A11 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 A9: Geographic Regression Discontinuity at the Individual-Level

Figure A9 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 show as points, with corresponding 90% (wider) and 95% (narrow) confidence intervals also shown.