Time Use and Productivity: The Wage Returns to Sleep

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Abstract

We investigate the productivity effects of the single largest use of time–sleep. Using time use diaries from the United States, we demonstrate that later sunset time reduces worker sleep and wages. Sunset time one hour later decreases short-run wages by 0.5% and long-run wages by 4.5%. After investigating this relationship and ruling out alternative hypotheses, we implement an instrumental variables specification that provides the first causal estimates of the impact of sleep on wages. A one-hour increase in average weekly sleep increases wages by 1.5% in the short run and by 4.9% in the long run. (JEL No. J22,J24,J31)

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1 Introduction

Economists have long been interested in determinants of productivity. The question of what makes workers more effective is fundamental to economics, important for both individual decisions and public policy. While there are traditions of research in human capital (Becker, 1962, 1964) and health (Leibenstein, 1957; Mushkin, 1962), less attention has been paid to the influence of time use on worker productivity. Many types of time use, from reading to vacationing, plausibly impact productivity on the job. In this study we examine one of the most important influences on human performance—the time a worker spends sleeping.

Evidence from medical research indicates sleep may play an important part in determining worker productivity. Tired doctors make more mistakes (Ulmer et al., 2009). Tired students perform worse on tests (Taras and Potts-Datema, 2005). Poor sleep impairs health (Cappuccio et al., 2010). These results suggest inadequate sleep lowers productivity, impedes the development of human capital, and imposes large direct costs on society. Moreover for the average individual, sleep takes up more time than any other activity. Despite the manifest importance of sleep, economists have largely treated it as a biological phenomenon outside their purview. We investigate an important question that has been overlooked almost entirely in economics: what are the effects of sleep on wages?

Answering this question poses substantial challenges. First, a pioneering study by Biddle and Hamermesh (1990) shows that higher wages raise the opportunity cost of sleep time, leading individuals to decrease their sleep. This result demonstrates that causal relationships between sleep and wages may run in both directions. Additionally, sleep may be correlated with unobservable worker characteristics that also influence wages. Finally, because sleep is a large portion of the time budget and potentially complementary to almost all human activity, it is difficult to isolate exogenous variation in sleep.

Motivated by medical research on circadian rhythm, we resolve this endogeneity by using sunset time as a source of exogenous variation. In general the first-stage relationship is straightforward: earlier sunset causes workers to begin sleeping earlier, and because work and school start times do not respond as strongly to solar cues (Hamermesh et al., 2008), this earlier bed time translates into more sleep. In fact, sunset timing provides two types of variation, short-run and long-run. In the short run, within a location, earlier sunset in winter induces longer sleep duration. In the long run, comparing two locations in the same time zone, the location farther east will experience earlier average sunset than the location farther west. As a consequence, residents of the eastern location will sleep longer. These two sources of sunset variation provide two instruments for sleep.

Before proceeding to instrumental variables estimates, we investigate the reduced-form effect of sunset time on wages. Consistent with our sleep hypothesis, we find that later sunset times decrease wages. Intra-annually, a one-hour increase in sunset time decreases worker wages by 0.5%, while a one-hour difference in long-run average sunset time decreases worker wages by 4.5%. Using alternative econometric specifications, we rule out a number of other, non-sleep hypotheses and obtain similar results in two different data sets. These results suggest that the exclusion restriction required for instrumental variables estimates—that the effect of sunset time on wages operates only through sleep—is reasonable. Conditional on this assumption, we exploit sunset-induced sleep changes to identify both short and long-run wage effects.

To implement our empirical strategy, we geocode observations from the American Time Use Survey (ATUS). ATUS provides rich labor market information about individuals, a wealth of control variables, and detailed time use data from daily diaries. Using the diary date and location, we assign each observation a diary-date sunset time and an annual-average sunset time. We then use these sunset time instruments to estimate the short and long-run causal effects of sleep on wages, controlling—in the case of the short-run estimates—for fixed location characteristics, year effects, and individual characteristics, and—in the case of the long-run estimates—for geographic characteristics (distance to the coast, latitude) and location-level demographic characteristics.

Our results show that a short-run, one-hour increase in average weekly sleep increases worker wages by 1.5%. A permanent one-hour increase in weekly average sleep increases average wage by 4.9%. These are, to our knowledge, the first causal estimates of how sleep affects wages.[1] Because our identification relies on location-level variation, these estimates should not be interpreted as individual effects. Both short and long-run estimates potentially include productivity spillovers across workers. In addition, our long-run estimate may include general-equilibrium effects induced by exogenously higher worker productivity. We also investigate whether this marginal effect exhibits discernible nonlinearity. Although we are limited in the range of sleep variation identi-

[1] Biddle and Hamermesh (1990) includes a regression with wages on the left-hand side and sleep on the right and finds a negative relationship. This is consistent with reverse causality and highlights the difficulty of isolating quasi-experimental variation in sleep.

fiable with our instrument, we find no evidence of non-monotonicity. Because sleep is time-intensive, however, the relationship between sleep and income remains inherently nonlinear.

We buttress our empirical work with a theoretical model of optimal time use based on classical assumptions. This illustrates the simultaneous determination of wages and sleep that biases naïve OLS estimates and clarifies how sunset time changes time allocation for an optimizing worker. Under an assumption on parameter magnitudes, the average worker will respond to later sunset time by increasing leisure. The model predicts, however, that two particular types of worker will respond to later sunset time by decreasing leisure and increasing work, and we test this prediction in our data.

Our study demonstrates that sleep is not just an economic curiosity but rather a vital determinant of productivity. A one-hour increase in a location's weekly mean sleep raises wages by roughly half as much as a one-year increase in education for all workers (Psacharopoulos and Patrinos, 2004).[2] These results point to the large impact that non-labor market activities can have on labor market performance. They suggest governments and schools must account for the productivity impacts of sleep to design optimal scheduling and time-use policies. By examining the largest use of human time, our study contributes to the time-use literature following Becker (1965). It complements the important work on the evolution of leisure time by Aguiar and Hurst (2007). We also contribute to the growing literature on how environmental forces influence worker productivity (Zivin and Neidell, 2012) and to the broader productivity literature on factors like information technology (Bloom et al., 2012) and workplace practices (Black and Lynch, 2001). Future work should extend these results to compare them to non-time intensive changes in leisure or lifestyle attributes.

The rest of the paper proceeds as follows: Section 2 presents a time use model with sleep as a choice variable, illustrating identification challenges, and discusses related literature. Section 3 presents the estimating equations and discusses our identification strategy. Section E describes the data used in the study. Section F reports the main results. We first show the effect of sunset on sleep and wages, then perform robustness checks and investigate the validity of using sunset as an instrument for sleep. Next we test model predictions. Finally we show instrumental variable estimates of the effect of sleep on wages and conduct tests of whether the relationship between sleep and wages is nonlinear. Section 4 concludes.

[2] While the effect of increasing wages for all workers in a location might differ from the partial-equilibrium estimate of (Psacharopoulos and Patrinos, 2004), the latter nonetheless provides an instructive benchmark.

2 Identifying the effect of sleep on productivity and wages

2.1 Previous research

Existing studies of the relationship between sleep and wages in economics are few and are largely concerned with addressing the question of whether sleep should be treated as a choice variable rather than simply a biological necessity. Biddle and Hamermesh (1990) is the first paper to provide empirical evidence on this issue and remains one of the only empirical investigations of labor market impacts of sleep. The authors lay out a model with agents optimizing over sleep, work, and leisure time in an otherwise standard setting. While their theoretical model allows sleep to affect productivity, Biddle and Hamermesh do not focus on this relationship in their empirical work. Instead they emphasize the causal mechanism operating in the opposite direction, modeling sleep as a function of instrumented wage (see, for instance, Biddle and Hamermesh (1990) Table 6). Brochu et al. (2012) and Szalontai (2006) also estimate the impact of changes in wage on sleep using more recent data from Canada and South Africa. Finally, Bonke (2012) has examined the impact of two chronotypes—whether the individual is a "morning" or "evening" person—on income. That study provides evidence on the related question of whether sleep quality impacts labor market outcomes.

Daylight savings time (DST) has been used in a variety of settings in economics as a proxy for sleep changes. For example, Smith (2014) finds the spring DST transition results in more automobile accidents and attributes the change to sleepiness behind the wheel. However, the short-term nature of any sleep change induced by DST limits its use in studying slower-moving outcomes like wages. Moreover, examination of ATUS data shows that the relationship between DST and sleep is complex. Transition into DST reduces sleep by 40 minutes on the day of the change, but transition out of DST is not associated with a noticeable change in sleep time (Barnes and Wagner, 2009).

Medical studies concerned with the effect of long term differences in sleep on health or mortality[3] are closest to our study in terms of time horizon. In recent years, a series of papers starting with Mckenna et al. (2007) have assessed the impact of short-term sleep loss on laboratory tasks. These studies provide suggestive insight into how sleep might impact work performance.

[Table 1 about here.]

[3] For instance Cappuccio et al. (2010) and Krueger and Friedman (2009).

Van Dongen et al. (2003) conducted the longest laboratory-controlled study on the relationship between sleep levels and cognitive performance. The researchers kept subjects in the lab for two weeks, placing them into groups receiving 4, 6, and 8 hours of sleep. The subjects were given daily tests of attention, memory, and cognition. The research found that the groups subjected to 4 and 6 hours of sleep performed progressively worse on all three tests, relative to the 8-hour group. Intriguingly, the subjects' subjective assessments followed a different pattern, declining for a few days and then leveling off. Observed cumulative effects quickly achieved large magnitudes: after one week, subjects in the 6-hour group performed as badly as subjects who were deprived of sleep entirely for one night. This indicates that small sleep reductions over long periods of time can have very large effects.

We review the evidence from Van Dongen et al. (2003) and similar medium-term causal studies in Table D4. Each study manipulated sleep duration by one to four hours per night, over periods of one to three weeks. In almost every case they find very large effects. The typical elasticity of task performance with respect to sleep duration is approximately four. Although these studies provide evidence that a positive relationship between sleep and work productivity is plausible, the question of external validity remains. Laboratory elasticities sometimes come from error rates or reaction times, and the relationship between such measures and real-world task performance is unknown. Moreover medical studies typically limit subjects' scope for adaptation, often prohibiting caffeine use and requiring subjects to undergo assessments at particular times.

2.2 A productive sleep model

The following analytical model, adapted from Biddle and Hamermesh (1990), illustrates the trade-offs between consumption, leisure, and sleep when sleep affects wages. It also demonstrates the reverse causality from wages to sleep that makes identification difficult and clarifies how we think about our instrument. Consider a consumer optimizing over sleep time T_S and a composite leisure good Z, which requires inputs of both time T_z and goods X such that $T_z = bZ$ and $X = aZ$.[4] The good X trades at the exogenous price P. The consumer has non-labor income I and time endowment T^*. Denote work time T_w. Let an individual's market wage w_m depend on sleep as

[4]Allowing Z to be a flexible function of T_z and X does not change the qualitative predictions of our model. We impose a functional form assumption here, while allowing a more flexible relationship between sleep and wages, for clarity of exposition.

follows

$$w_m = w_1 + f\left(T_S\right), \tag{1}$$

with $w_1 > 0$, $f'\left(T_S\right) > 0$, and $f''\left(T_S\right) < 0$.

Note that this theoretical model could easily be adapted to study other non-work time uses, but the function linking wage to time use would likely be different. We assume that a function of sleep, αT_S, enters the utility function, where α is the relative utility enjoyed by the individual per hour of sleep.[5] The parameter α provides a convenient link between our analytical model and our instrumental variables estimation strategy, as discussed below. The worker optimizes over sleep and composite leisure, subject to time and income constraints, as follows.

$$\max_{Z,T_S,\lambda} U\left(Z, \alpha T_S\right) + \lambda\left(I + \left(w_1 + f\left(T_S\right)\right)\left(T^* - T_S - bZ\right) - aPZ\right)$$

Combining first-order conditions yields a two by two system of equations that implicitly describe the worker's optimal choice.

$$U_1 w_m - U_1 f'\left(T_S\right) T_w - \alpha U_2\left(aP + bw_m\right) = 0$$

and

$$I + \left(w_1 + f\left(T_S\right)\right)\left(T^* - T_S - bZ\right) - aPZ = 0$$

Applying the implicit function theorem, we can evaluate several interesting derivatives. First, consider the effect of an exogenous wage increase on sleep time.

$$\frac{\partial T_S}{\partial w_1} = \left(aP + bw_m\right)\left(U_1 - \alpha U_2 b\right) D^{-1} + T_w \frac{\partial T_S}{\partial I}$$

In the previous expression, $D^{-1} < 0$ equals the negative of the Jacobian. This is a variant of the usual Slutsky equation. The first term captures the substitution effect, which differs from the typical form in that it includes $-\alpha U_2 b$. When $\alpha = 1$ the value $\left(U_1 - \alpha U_2 b\right) > 0$ and the first term is negative. Increased wages raise the opportunity cost of sleep, decreasing optimal sleep. This means that a naïve regression of wages on sleep will not recover causal effects.

To motivate our later use of sunset time as an instrument for sleep, consider the

[5] Our predictions are qualitatively unchanged if we assume that sleep does not enter the utility function directly, but rather as an input to the production of the composite leisure good Z.

effects of an exogenous increase in α. Since α controls the relative attractiveness of sleep, an increase in the parameter will induce agents to want to consume more sleep.

$$\frac{\partial T_S}{\partial \alpha} = U_2 \left(aP + bw_m\right)^2 \left(-D\right)^{-1} > 0$$

The effect on leisure can operate in either direction.

$$\frac{\partial T_z}{\partial \alpha} = bU_2 \left(aP + bw_m\right) \left(f' \left(T_S - w_m\right) T_w\right) \left(-D\right)^{-1} \lesseqgtr 0 \tag{2}$$

The ambiguous sign comes from the expression $\left(f' \left(T_S - w_m\right) T_w\right)$, which is the opportunity cost of an additional leisure hour. More specifically, this expression is the gross opportunity cost of an additional leisure hour, $-w_m$, adjusted for the additional income generated by increased sleep, $f' \left(T_S\right) T_w$ (recall that T_S increases in response to an increase in α). Individuals with low wages (low w_1), or a combination of high work hours and low sleep hours, will tend to decrease leisure time in response to decreased α. This is because the income effect dominates the substitution effect and income is a complement of leisure time. For low-wage workers, the substitution effect is small. For high-work, low-sleep workers, the income effect is large; any change in wage applies to many hours. We test these theoretical predictions in Section 3.4.

3 Empirical strategy

3.1 Estimating equations

We would like to recover the relationship between sleep and wages as in Equation (1), where $\partial f / \partial T_S > 0$ would provide evidence for productivity-enhancing sleep. Given the reverse causality between wages and sleep, however, we might erroneously find $\partial f / \partial T_S < 0$.[6] To avoid this problem and to account for the wide variety of other

[6] The general form is given in the model above, but we can also illustrate the issue with a simple two equation system that will prove useful below. Let the sleep-wage relationship be given by

$$\mathbf{w} = \mathbf{T}_S \beta + \varepsilon$$
$$\mathbf{T}_S = \mathbf{w}\theta + \nu$$

where ε and ν are error terms, $\mathbb{E}[\varepsilon \cdot \nu] = 0$, $\mathbb{E}[\mathbf{T}_S \cdot \varepsilon] = 0$, and $\mathbb{E}[\mathbf{w} \cdot \nu] = 0$. Then if $\theta < 0$ as is argued by the previous literature, the bias from OLS estimation can be signed as follows:

$$\text{plim}\, \hat{\beta} = \beta + \underbrace{\theta \frac{\mathbb{E}[\varepsilon w]}{\mathbb{E}[T_S^2]}}_{<0}$$

omitted variables that might co-vary with sleep and wages, we predict sleep using two instruments based on local sunset time, then use the instrumented values of sleep to estimate wage impacts.

The first instrument uses daily variation in sunset within a given location. Because this instrument varies on a daily basis, it will identify short-run variation in sleep (Frazis and Stewart, 2012). Thus, we can use it to estimate a short-run first stage

$$T_{S,ijt} = \alpha_1 \text{sunset}_{jt} + \gamma_{1,j} + \mathbf{x}'_{it}\delta_1 + \eta_{1,ijt} \tag{3}$$

and reduced form

$$\ln(w_{ijt}) = \alpha_2 \text{sunset}_{jt} + \gamma_{2,j} + \mathbf{x}'_{it}\delta_2 + \eta_{2,ijt} \tag{4}$$

where $T_{S,ijt}$ is nighttime sleep for individual i in location j on date t, sunset_{jt} is the sunset time on that date in that location, γ_j is a location fixed effect, $\mathbf{x}_{it}$ is a vector of individual level controls, w_{ijt} is a measure of wages or earnings observed at time t, and $\eta_{k,ijt}$ is the error term for the first stage ($k = 1$) and reduced form ($k = 2$). Controls are four race indicators; age and age squared; a full-time indicator; a gender indicator; indicators for holidays, day of week, and year; and detailed occupation code indicators. More details on these control variables can be found in the data discussion in Section E. Following the suggestions of Winship and Radbill (1994) and Solon et al. (2013), we do not weight observations, but we do control for weekends since they are over-sampled in our dataset.

If seasonal sunset time is a valid instrument for sleep, then this first stage and reduced form can be used to construct causal estimates of the effect of sleep on wages by taking the ratio of α_2 to α_1. In practice, we will calculate the instrumental variables estimator using two-stage least squares. We denote this estimate β_{SR}, or the effect of sleep on wages in the short run.

In addition to instrument validity concerns, there is an important caveat to the interpretation of this coefficient, which is that the rate of adjustment of the wage variable influences the magnitude of the reduced form and two-stage least squares estimates. In Section D.1 we discuss this issue in more depth and provide bounds on the potential bias.

So plim $\hat{\beta} < \beta$. Naive OLS will tend to understate the effect of sleep on wages if this is the dominant source of bias.

The second instrument is annual average sunset. This instrument exploits spatial differences in sunset time within and across time zones. Because this is a fixed feature of a location, it will identify long-run differences in sleep (Frazis and Stewart, 2012). For estimation, we collapse the ATUS data to the location level and estimate the following first stage

$$T_{S,j} = \varphi_1 \text{sunset}_j + \mathbf{x}_j' \zeta_1 + \varepsilon_{1,j} \tag{5}$$

and reduced form

$$\ln(w_j) = \varphi_2 \text{sunset}_j + \mathbf{x}_j' \zeta_2 + \varepsilon_{2,j} \tag{6}$$

where $T_{S,j}$ is average nighttime sleep in location j, sunset_j is the average sunset time in that location, $\mathbf{x}_j$ is a vector of controls, w_j is average wage in that location, and $\varepsilon_{k,j}$ is an error term for $k \in \{1, 2\}$. We control for both geographic characteristics (coastal distance and latitude) and demographics (gender, age, race, and occupation shares, plus population density).

Following the recommendation in Solon et al. (2013), we weight location-level observations using counts of the underlying individual ATUS observations to correct for heteroskedasticity. Appendix Table D13 provides evidence of heteroskedasticity from a modified Breusch-Pagan test and Appendix Table D14 presents unweighted results.

Again, if average sunset time is a valid instrument, the causal effect of long-run changes in sleep on wages can be found by taking the ratio of φ_2 to φ_1. We denote this coefficient β_{LR} and estimate it by two-stage least squares. Although the control variables primarily serve to reduce residual variance, as discussed in Section F, we do find evidence that average sunset is not unconditionally exogenous with respect to coastal distance and population density. Therefore the identifying assumption underlying our long-run IV estimates is one of conditional exogeneity, as discussed in Sections D.0.2 and 3.3.2.

3.2 Local sunset time instruments

3.2.1 Relevance of sunset to sleep

The relevance of sunset time as an instrument for sleep stems from the biological relationship between sleep patterns and daylight. Human circadian rhythm is synchronized with the rising and setting of the sun through a process known as entrainment. This force is powerful, with Roenneberg et al. (2007) showing that "the human circadian clock is predominantly entrained by sun time rather than by social time." Using data from Germany, the authors demonstrate that living in a location with a later sunset induces individuals to begin sleep later. The detailed ATUS files enable us reproduce this result: workers experiencing a later sunset go to bed later and this causal connection between sunset and bedtime persists even if the worker goes to bed well after dark. In a vacuum, a later sunset time might cause workers to go to bed later and also rise later, leaving sleep duration unchanged. But workers face morning coordination constraints due to work and school scheduling (Hamermesh et al., 2008), so later sunset and later bedtime decrease sleep duration. This relationship between annual average sunset time in a location and mean weekly sleep duration is the basis for the relevance of our long-run instrument.

Intra-annual changes in sunlight also influence human sleep patterns through a similar process of entrainment (Hubert et al., 1998), combined with fixed constraints on wake-up time. Therefore, we expect that both long-run average sunset and daily changes in sunset time will affect the sleep of workers. We verify these hypotheses in Section F.1. The evidence bearing on validity of sunset time as an instrument is different for the two variables and we discuss them separately below.

3.2.2 Short-run validity

For our short-run estimates of the effect of sleep on wage, validity requires that other wage determinants not consistently co-vary with daily sunset time within a location. The primary threat to this assumption is seasonally varying wage determinants, since sunset time exhibits a regular seasonal pattern, as shown in Figure 1 for all ATUS observations. One can see that sunset time is generally described by cosine wave with a period of one year. This wave is phase shifted by roughly 10 days relative to the calendar year. The amplitude of the wave is determined by the latitude of the location, and vertical translations are due to within-time zone variation, which we use for our long-run estimate. The final important features of sunset time are the prominent jumps in the spring and fall caused by daylight savings time. The timing of daylight savings time observance changed in 2005, causing these jumps to not be sharp in the figure.[7]

[Figure 1 about here.]

To arrive at the figure, we first residualize log wage and average sunset time using a control set similar to our preferred ATUS cross-sectional specification. Controls are time zone indicators, interactions of those indicators with coastal distance for the Pacific and Eastern time zones, a linear spline in latitude, median age, percent female,

[7] For more discussion of solar mechanics, see Section A.

percent black, and percent white. We plot a separate kernel regression of the relationship between this residualized wage and residualized average sunset time for each time zone. For all four time zones, locations with the earliest average sunset time have higher wages than locations with the latest sunset time. The relationship is largely linear, and the average relationship is similar across time zones—particularly for for the three widest US time zones where identifying power is highest. The linear regression version of these results is reported in Appendix Table D19, and the relationship between within-time zone longitude and wage is shown in Appendix Figure 12.

In summary, sunset time affects sleep duration for American workers both intra-annually and in the long-run. There is also a detectable, practically large sunset-wage gradient in both ATUS and QCEW data.

3.3 Robustness

3.3.1 Short-run robustness

We now test the sensitivity of our short-run results. Table D5 presents six of the most important short-run robustness checks. Each entry in the table, demarcated by a descriptive title, is a version of the reduced form estimate from Table D2, with a change to sample, control set, or standard error calculation. For brevity, we do not report any first-stage robustness checks because the estimates (available upon request) are exceptionally stable.

The first robustness check re-estimates the reduced-form relationship without any control variables or fixed effects. The estimate is similar in magnitude to the baseline estimate, but with slightly lower precision. This specification indicates that the short-run variation in sunset time may be unconditionally exogenous.

[Table 2 about here.]

The next specification controls for a quadratic in usual hours worked and returns estimates very similar to our primary short-run results. We deliberately do not include usual hours worked as a control variable in the main specification to allow workers to take additional sleep time out of either work time or other (non-work, non-sleep) time. By controlling for work time, we would be forcing all changes in sleep to come out of other time, which might bias our estimates if, as theory predicts, sunset time responses co-vary with unobserved earnings determinants. Nonetheless the robustness of our result to this control is encouraging and helps address concerns like those raised in Borjas (1980) about the use of constructed wage measures. Finally, in the first column we cluster standard errors at the state for all observations and find that inference does not change.

We next conduct several checks aimed at possible seasonal confounding—a crucial issue for the validity of any instrumental variables interpretation of our results. The first check adds quarter fixed effects, showing that the estimated effect is, if anything, stronger in this case. The next removes the holiday season (Thanksgiving through January 15th), again finding little change. The last adds a daily temperature control. The estimate does not change, but the precision diminishes. In further results found in the appendix, we also show that dropping the entire first and fourth quarters does not substantially change inference. Indeed, in unreported results, the estimate is robust to dropping any two quarters from the sample.[8] Further robustness checks of this form are reported in Appendix Section C.1.

Although these checks show our results are robust to some forms of seaonality, it remains true that any variable with a seasonal pattern that perfectly matches the frequency and phase of sunset time cannot readily be identified separately from daily sunset time.[9] On the other hand, a seasonal variable with a very different phase or frequency poses no omitted variable problem, because the correlation with sunset time will be low. At the extreme, a variable with the same frequency but phase shifted by one-quarter wavelength will have zero expected correlation with daily sunset time.

In between these two extremes, a non-zero amount of confounding can occur. We can characterize this potential bias by including seasonal control variables that mirror the sinusoidal pattern of daily sunset time: $x_{\theta,\varphi} = -\cos[(360/365)(d+10+\theta)\varphi]$, where $\theta \in \mathbb{R}$ is the phase shift relative to sunset time and $\varphi > 0$ is the frequency relative to sunset time. We focus first on the set of variables where the relative frequency is the same ($\varphi = 1$) and the phase shift, θ, ranges over the set of natural numbers less than 183.[10] We then, one at a time, include these variables as additional covariates when estimating Equation (4). The black curve in Figure 4a shows the resulting coefficients on sunset time from these regressions along with the 95% confidence interval in dashed lines. The red horizontal line shows the reduced form coefficient reported in the baseline

[8]Dropping three quarters removes too much data for precise inference, but the point estimates are similar.

[9]The one exception, discussed in Section 3.2.2 is that daylight savings time breaks the smooth sinusoidal pattern of sunset time. As discussed above, however, daylight savings time does not, by itself, induce enough variation in sleep to credibly estimate wage effects in a sample of our size.

[10]Since sunset time has a period of one year, this set of phase shifted variables will cover the full range of possible correlations with sunset time. In theory, we can let θ range over $\mathbb{R}^+$, but our highest data frequency is daily.

results in Table D2.

[Figure 2 about here.]

One can see that over a wide range of seasonal controls, the reduced form estimate is essentially indistinguishable from the baseline. Indeed, at no point in the space can the baseline value be rejected. For controls that are close to sunset time (phase shifted less than 18 days or more than 174 days), the sign of the point estimate changes and the confidence interval widens considerably. Overall, our estimate is robust to sign error for a seasonal pattern that peaks or troughs between January 28th and December 23rd.

Figure 4b shows the same result, but now for a set of regressions that include covariates with different relative frequencies ($\theta = 0$ and $\varphi \in (0, 3)$). Here, one can see that the reduced form estimate is highly robust to seasonal patterns with alternative frequencies. Outside of a narrow band of relative frequencies between 0.96 and 1.09, the estimate has the expected sign.

In principle we cannot rule out the possibility that the short-run estimates are the result of a spurious correlation between an unrelated seasonal pattern in wages that closely matches the frequency and phase of sunset time. Practically, however, Figure 4 suggests that such a variable would need to exhibit a pattern that is strongly similar to sunset time to introduce large bias. Moreover the corroborating results we recover using the orthogonal identification provided by long-run sunset further mitigate confounding concerns.

As a final note on seasonality, even if the seasonal pattern in wages is due to sleep, one might worry about confounding trends in sample composition, for example the rate and composition of employment. Appendix Figure 9a shows that occupation shares in our sample are constant over the months of the year, and Appendix Figure 9b shows that the share of ATUS respondents reporting a positive wage is likewise constant over the year. Together, these figures reassure us that such selection bias is not driving our results.

3.3.2 Long-run robustness

We now test the sensitivity of our long-run reduced form estimate. Again, the first stage estimates are very stable across specifications and are omitted. Table D6 shows estimates of the reduced form equation (6) with variations in controls, sample, and clustering. First, we report estimates using only geographic controls, without any other covariates. Although the exclusion restriction for the validity of our instrumental variable estimate is based on the error term for the full model, it is reassuring to see that the coefficient from this minimal specification is just over one standard error away from our preferred estimate. The additional demographic controls do make the coefficient estimate more precise, however, as can be seen by comparing standard errors between the main result and robustness check. This result implies that sunset time is not highly correlated with the demographic covariates in the main specification, which also provides initial evidence against sorting on sunset time. Next, a linear control for longitude does not change the estimate substantially. Clustering standard errors at the state level for all observations does not change the results of our hypothesis tests.

[Table 3 about here.]

The next robustness checks mimic the specification estimated using QCEW data and reported in Figure 3. We drop counties at border longitudes where time zones overlap, as such counties might have selected into a time zone based on economic considerations. Again the results are not statistically distinguishable from our preferred estimates. Adding time zone indicators produces a similar result.

Next, we directly test for spatial confounding by excluding the wealthier, denser Eastern time zone, then excluding selected high-wage cities. Both of these models return similar estimates to our baseline model, reiterating the result from the QCEW sample that this effect is not restricted to a single time zone or portion of each time zone. Finally, to test whether long-run average sunset time co-varies with local amenities, we include a control for the state-level quality-of-life index from Albouy (2008). Again the change in the coefficient is less than one standard error and it remains significant at the ten percent level.

These robustness checks emphasize possible spatial confounding because our long-run estimates rely on spatial variation. Individual sorting is one of the primary potential threats to this identification. The robustness checks above offer initial evidence against this hypothesis. Before we consider further evidence, however, it will be helpful to consider a few theoretical points. First, for sorting to threaten identification, workers would have to sort based on the *timing* of daylight. Sorting on daylight duration would not bias our estimates, as average sunset time is independent of daylight duration. Second, a worker who decides to sleep more need not move to another location; she can simply sleep more. Third, an optimizing worker responds to real, not nominal, income. If home prices in more productive (higher sleep) locations adjust to offset wage gains, workers will not have a financial incentive to move. This is exactly the prediction of a sorting model like Roback (1982). With perfect worker and firm mobility, the gains from a productive location-specific amenity accrue to owners of land, the fixed factor. Such a model predicts that locations with earlier average sunset times will have higher rents and house prices, even without worker sorting on ability. Using county-level Census data from 2010, Table D7 provides evidence that this is indeed so. We regress log median county home value on average sunset time and a set of controls similar to our long-run ATUS specification.

$$\ln(\text{median home value})_j = \beta\overline{\text{sunset}}_j + \mathbf{x}_i'\gamma + \varepsilon_j$$

A county experiencing sunset one hour earlier will have, on average, a median home value approximately 6% higher. This result is statistically significant at the 1% level. In levels, the estimated effect on median home value is approximately \$7,900 to \$8,800. Based on the discussion following Table D10, a worker's annual income gain from moving to a location where sunset is an hour earlier is approximately \$1,570.

Thus the capitalization of sunset time into home prices reflects the present discounted value of wage gains from approximately 8 to 9 years of work (assuming a five percent discount rate). This result is roughly consistent with the prediction of the Roback model: a substantial fraction of the wage gains from earlier sunset and additional sleep in a location accrue to landowners, not workers. This blunts the incentive of workers to sort on sunset time. In Appendix Table D18 we show this estimate is robust to additional controls.

[Table 4 about here.]

To buttress the hedonic results, we also conduct direct sorting tests: first, we examine historical population growth patterns in response to time zone creation in 1883 and alteration in 1918. We do not see any population change consistent with sorting on sunset time. Second, we examine the relationship between current county-level characteristics and sunset time. Aside from population density, we do not find significant confounding relationships between average sunset time and any variable. To account for population density we employ a spline in this variable as a control.

3.4 Time shifting and model-implied heterogeneity

[Table 5 about here.]

Motivated by the model presented in Section 2.2, we now investigate heterogeneity in the relationship between sunset time and time uses other than sleep. Recall that in the model, we found that the sign of the derivative $\partial T_z/\partial\alpha$, the change in leisure time with respect to sunset time, depends on wage and hours worked. We can test this model prediction using variations on our first stage estimating equation.

For comparison, first consider the average effect of sunset time on non-sleep time in the full sample, reported in Table D11. These results show that when faced with an earlier sunset, workers allocate about a tenth of an hour per week less time to work in the short run and 0.7 hours more in the long run. Investigation reveals that the difference in these estimates is partly due to non-work days in the short-run sample. Excluding days with zero reported work hours makes the short-run estimate zero to the third significant digit, while estimating with usual work hours as the dependent variable returns a small, positive coefficient. These modified short-run estimates are not statistically distinguishable from the long-run estimate.

[Table 6 about here.]

By adding the two coefficients from panel B and then comparing them to the first stage coefficient in Table D3, one can see that in the long run, work and non-work waking time account for essentially all of the change in sleep time induced by sunset. In the short run, the excluded time category—daytime sleep—accounts for a substantial amount of the time use change, offsetting about 0.18 hours per week of the sleep change observed in the first stage of Table D2. Thus, naps appear to be an important adaptation strategy in the short run but not in the long run. Medical evidence (Van Dongen and Dinges, 2005) indicates the effect of naps on task performance is ambiguous: performance typically declines immediately after the nap, but then recovers to well above pre-nap levels. The effect of a nap on full-day productivity thus may depend on the timing and duration of the nap, among other factors.

In both the short and long run, a sunset one hour later causes workers to increase non-work, non-sleep time by about 20 minutes per week. Together these results suggest that in the short-run, workers mainly trade off sleep with leisure time and daytime sleep, while in the long run they trade off sleep with work and leisure.

In contrast, the model predicts that for low-wage workers and high-work, low-sleep workers, the effect of sunset time on leisure time should be smaller than for the full sample. To test this prediction, we estimate separate regressions for these groups and report them in Table D9. Consistent with our theoretical predictions, both short- and long-run estimates for these groups are smaller than the corresponding results for all workers. Three of four are negative, in contrast to our overall positive results. The estimates are not statistically distinguishable from zero, although in the case of the high work hour sample, the short-run coefficient is distinguishable from the coefficient for the full sample.

3.5 IV estimates of the effect of sleep on wages

Having clarified the assumed exclusion restrictions, we present our instrumental variables estimates of the effect of sleep on wages in Table D10. A one-hour increase in short-run mean weekly sleep in a location increases wages by 1.5%. A one-hour increase in long-run mean weekly sleep in a location increases wages by 4.9%.[11] The larger long-run effect may reflect several factors, including greater long-run wage flexibility and long-run effects from labor-capital complementarity. Together these results demonstrate that exogenous location-level sleep changes have important wage effects.

[Table 7 about here.]

In the short-run model, the first-stage F statistic of 91.4 well exceeds the relevant Stock-Yogo critical value of 16.38, so we reject the null hypothesis of weak instruments, where "weak" is defined as true size greater than 10% for a nominal 5% test (Stock and Yogo, 2002). This reassures us that the result of our t-test is reasonable. In the long-run model, the first-stage F statistic of 10.66 falls short of the Stock-Yogo critical value for true size of 10%, but exceeds the critical value of 8.96 for true size of 15%. The result of our long-run t-test should be thus be viewed with more skepticism, but we can rule out gross failures of size control.

Our estimates reflect location-level local average treatment effects, so it is important not to generalize too much from them, and several nuances of interpretation warrant discussion. First, all workers in a location experience the same sunset time. While we do not know if sunset-induced sleep differences generate productivity spillovers across workers, Moretti (2004) finds evidence that human capital does. In such a case our estimated β will capture not the effect of increasing individual sleep, but rather the effect of increasing mean sleep in a location. Second, managers might set wages based on average productivity in a location rather than individual worker productivity. Under this assumption, an increase in sleep by an individual would have no effect on

[11] The long-run instrumental variables estimate is quite close to the reduced-form estimate because the first-stage change in mean weekly sleep is close to 1.

her wage, as it would not appreciably change average productivity. In a case like this, our estimate will capture the effect of increased sleep by all workers on average wages, rather than an individual-level effect. Finally, it is possible our instrument influences both sleep duration and sleep quality. This is true, however, of any exogenous variation in sleep, even in a laboratory setting. In such a case our estimates are still consistent for the effect of an exogenous sleep change, but the interpretation changes slightly.

Our analysis demonstrates that workers experiencing an earlier sunset get more sleep. As discussed in Section 3.4, in the short run the additional sleep largely comes at the expense of leisure, while in the long run it comes at the expense of both work and leisure. Insofar as these changes in other time uses impact worker productivity, our instrumental variables estimate of the effect of sleep on wages will also contain those effects. While this might seem undesirable at first glance, it is unavoidable. An agent's time constraint always binds with perfect equality. Even in a laboratory setting, it is not possible to change the time use of interest without also changing at least one other time use.

Expressed as an elasticity, our short-run estimate is 0.84 and our long-run estimate is 2.6. These magnitudes are consistent with the experimental evidence summarized in Table D4. Medical researchers have typically found elasticities of task performance with respect to sleep duration of approximately four. If wages are equal to a worker's marginal physical product multiplied by output price, we expect such performance effects to produce equally large wage effects. The smaller magnitudes of our estimates, relative to the medical literature, may reflect differences between laboratory tasks and actual work tasks or the broader scope for adaptation (for instance the use of stimulants like coffee) outside the lab. The larger magnitude elasticity in the long-run is also consistent with the attenuation bounds calculated in Section D.1.

Unaided intuition might suggest smaller effects of sleep on performance, but intuition provides a poor sense of this relationship: Van Dongen et al. (2003) showed that subjects' self-reported fatigue quickly stabilized after a few days of sleep reduction, even as their performance continued to decline. Van Dongen et al. (2003) also found that several days of two–hour sleep reductions reduced performance by as much as a night of complete sleep deprivation. This study implies that working after a completely sleepless night likely provides a better visceral sense of the performance effects from moderate sleep deprivation than does reflection on one's own long-term experience.

Taking average values for earnings and assuming 50 work weeks per year, one can calculate the annual income effects implied by our long-run estimates. If mean weekly sleep in a location increased by one hour and work time remained unchanged, mean annual income would rise by about $2,350. In reality, extra sleep comes out of both work and non-work time. If workers took roughly 70% of the extra sleep hour out of work time, as we find in Table D11, then a one-hour increase in weekly mean sleep in a location would increase mean annual income by about $1,570. If extra sleep came solely at the expense of work time, the income increase would be $1,250.

Figures of this magnitude naturally lead one to ask why workers don't work less and sleep more. Perhaps the most direct answer is that increased income need not imply increased utility. (For example, the increased utility from greater income might be offset by decreased utility from foregone leisure.) Another possible explanation lies in the spillovers and general-equilibrium effects our estimate incorporates. Because our estimates are based on location-level variation, they likely overstate the effects an individual worker would experience from changing her sleep in isolation. It is also conceivable that observed sleep reflects optimization failure by workers, as hypothesized by Mullainathan (2014). Such failure could occur even under classical assumptions. For example, the inaccurate self-perceptions of fatigue found by Van Dongen et al. (2003) could lead to sleep below the utility-optimizing level even if workers are behaving optimally, conditional on their information set. Such sub-optimal sleep could, in turn, contribute to the type of time-use poverty trap analyzed by Banerjee and Mullainathan (2008).[12] On the other hand, sub-optimal sleep could arise from behavioral considerations like time inconsistency or constraints on cognitive resources (see for example Mani et al. (2013)). This type of mechanism could also generate a poverty trap. While optimization failure and its possible mechanisms are beyond this scope of the current paper, we are exploring them in ongoing experimental work.

3.6 Nonlinear effect of sleep on wages

Although our setting is not ideally suited to study nonlinear effects of sleep on earnings, it is a question of natural interest due to the routine reporting of such relationships in medical research.[13] Nonlinearity in the sleep-wage relationship is intuitively appealing—at the extremes, a worker cannot work if she sleeps all day, and this logic would likely extend to shorter sleep durations that still impinge on work hours. Over more moderate sleep durations, however, the question of whether the marginal effect

[12]While the authors interpret their work as a model of inattention, they note, "In fact, our model is formally identical to a rational time allocation model, if we think of comfort goods as time saving devices."

[13]See, for instance Cappuccio et al. (2010) and Leng et al. (2015) on mortality or Taheri et al. (2004) on BMI.

of sleep on wages is non-monotonic has important implications, and the answer is not obvious. For instance, in contrast to the above cited studies, Van Dongen et al. (2003) shows that the marginal effect of sleep on attention is linear over a wide range of sleep durations. Moreover, the reverse causality discussion that prefaced the analysis of this paper suggests that workers have an incentive to sleep moderate amounts even if sleep is productivity enhancing Thus, selection into working as well as other forces might lead one to erroneously conclude that long, in addition to short sleep, is bad for productivity, health, and other outcomes.

We can empirically examine this question to some degree in our setting. We are limited by the small changes in sleep induced by sunset time relative to many laboratory studies, but we benefit from much larger sample sizes and the fact that the nonlinearity—if the medical research is correct—should be most apparent near mean sleep levels where our identifying variation exists.

We use a variety of methods to investigate nonlinearity. Appendix Figure 7 reports the results of two of these methods: a kernel regression of residual log earnings on residual sleep instrumented by daily sunset time and a control function-based estimate using techniques from Kim and Petrin (2013). For details about the construction of these estimates, see the appendix. Both approaches estimate a semi-parametric relationship between sleep and earnings. Visually, for the short-run sample, this relationship is linear over the identified range. The Kim and Petrin (2013) estimates allow us to conduct a hypothesis test on higher order polynomial terms, and if anything, these terms suggest that the relationship is convex—more sleep has a slightly increasing marginal effect on earnings. In the long-run case, the kernel regression suggests slight concavity of the sleep-earnings relationship, but hypothesis testing of the Kim and Petrin (2013) estimates fails to reject linearity.

A formal test of monotonicity based on Gutknecht (2013) also fails to reject monotonicity of effect in both the short and long-run estimates, but this test is conservative. Even though we do not find evidence for a nonlinear sleep-wage relationship, the time-intensiveness of sleep means that there is an inherent nonlinearity in the relationship between sleep and income.

4 Conclusion

Although time use is entangled in a causal web with labor market outcomes, economists have paid little attention to these relationships. In particular, the profession has scarcely examined sleep. Our results demonstrate that sleep has a powerful impact on labor market outcomes and should be considered an integral part of a worker's utility maximization problem. Using individual time-use diaries matched with labor market variables from ATUS, we show that increasing short-run weekly average sleep in a location by one hour increases worker wages by 1%. Increasing long-run weekly average sleep in a location by one hour increases wages by 4.5%. Our use of instrumental variables techniques addresses the reverse-causality and omitted variable problems that would bias naïve estimates. We buttress this finding with a battery of short and long-run robustness checks, and a hedonic model of home prices showing that long-run wage increases are partially capitalized into housing.

Our results suggest that sleep is a crucial determinant of productivity, rivaling ability and human capital in importance. These findings have significant implications for individuals, firms, schools, and governments. A worker who desires higher wages might be able to obtain them by increasing sleep. Firms might be able to increase profit by altering start times, providing workers with incentives to sleep more, or with information interventions (e.g. information on how to improve sleep quality or consistency). Governments conducting cost-benefit analyses of policies that change sleep time, for example daylight savings time, should consider the productivity effects to design efficient policies. Countries spanning a wide range of longitudes might benefit from abolishing time zones and adopting a single standard time, preserving ease of coordination while allowing firms and schools to set schedules optimally with respect to local solar cues.

Further attention should be paid to industries characterized by chronic sleep shortages. In addition to wages, optimal sleep plausibly depends on other factors like leisure complementarities, direct sleep utility, and health optimization. Each of these tradeoffs suggests an interesting research question. More broadly, our results demonstrate that non-labor time uses can have first-order effects on labor outcomes—effects that warrant further investigation in future work.

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A Solar mechanics

Here, we provide a brief summary of how sunset time is calculated and a glossary of terms. For a detailed glossary, see NOAA's ESRL website. We calculate sunset, sunrise, solar declination, and sunlight duration each day using the algorithm of Meeus (1991) as implemented by NOAA's Earth Systems Research Laboratory (ESRL). The calculator takes as inputs the date, time zone offset, latitude, and longitude. It outputs the solar declination, right ascension, sunrise time, sunset time, and sunlight duration. The Stata code that we used for calculation is available on Github.

Solar declination is the angle of a line segment from the sun to the earth relative to a plane projected from the equator of the Earth. The solar declination is a function only of the day of year and time zone offset (to compute fractional days for high-resolution local time sunset), and changes in solar declination correspond to the average seasonal movement of the sun. The highest solar declination, 23.44° occurs on the summer solstice, and the lowest solar declination, -23.44°, occurs on the winter solstice. On the equinox, solar declination is 0°. A rough calculation of solar declination, which we use as the basis of our seasonal control variable in Section 3.3.1, can be calculated as

$$-23.44 \cos\left(\frac{360}{365}(d+10)\right)$$

where d is the day of the year.

Sunset and sunrise time are both calculated assuming 0.833° of atmospheric refraction, or the bending of the path of light as it passes through the Earth's atmosphere. A more precise refraction correction would need to incorporate information on air pressure and humidity. Also, we calculate sunset assuming an observer with a zero-elevation change view of the horizon (not to be confused with the assumption of being at 0 elevation). These two variables take latitude as an additional input, reflecting the location-specific amplitude of sunlight changes.

Sunlight duration is simply calculated as the difference between sunrise and sunset time for a location on a given day. We do not utilize right ascension in this study.

B Measurement error in short-run estimates

Here we derive expressions for the expected bias in our short-run estimates. The final expression and degree of bias is reported in Section D.1. Estimation results subject to these calculations are reported in Sections F.1 and 3.5. Although we state the results in terms of wages and sleep, the model applies equally well to estimates of the reduced form equation (4).

Assume that for a given individual i surveyed on day t, wages are equal to the average of D past sleep observations plus random noise. Therefore the true model relating sleep to wages is

$$w_{it,\tau} = \beta \left(D_i^{-1} \sum_{k=\tau-T}^{\tau-1} T_{S,ik} \right) + \varepsilon_{i\tau} \tag{7}$$

$$= \beta T^{*}_{S,i\tau} + \varepsilon_{i\tau} \tag{8}$$

Thus, we are assuming that earnings change for this individual every D days, and sleep only matters during the earnings determination period. The subscript τ indexes the day that these earnings start to be *observed* in the data. Because of the fixed earnings change frequency for a given individual, these earnings will be observed for days τ through $D + \tau - 1$. To be concrete, consider the case of $D = 2$. Then we, the researchers, can only sample the individual on either the day after they received an earnings change or 2 days after, so τ will be equal to t or $t - 1$.

We further assume that τ is uniformly distributed across the year (a person has an equal probability of receiving an earnings change on any given day). This is a strong assumption, but the best available evidence from Barattieri et al. (2010) suggests that it is not broadly incorrect. Of course, for a given year, there will be weekend or holiday effects, but asyptotically, these become less relevant. Moreover, we do not have any information on when a given individual in our sample last experienced an earnings change, so this uniform assumption is a non-dogmatic baseline.

Finally, we assume that the researcher has isolated exogenous variation in sleep so that $\mathbb{E}T_{S,t}\varepsilon_{\tau} = 0$ for all t and τ.

If we observed past sleep and knew the earnings change frequency, we could estimate Equation (7) and return the correct estimate. Instead, We observe wages and sleep on date $t \geq \tau$, with which we estimate

$$w_{it,\tau} = \beta_1 T_{S,it} + \varepsilon_{i\tau}$$

We wish to know the relationship between β_1 and β.

We will exploit the wage setting structure given above and the functional form for the time series of sunset time from Section A to calculate this relationship. First, given the results from Frazis and Stewart (2012) we can use sunset time to both isolate daily, exogenous variation in sleep and to predict daily sleep for any day of the year, even though we only observe sleep on one day. This individual time series of sleep will have a similar functional form to the instrument, namely

$$T_{S,it} = A\cos(\theta t)$$

where A is the population coefficient on the unconditional version of the first stage Equation (3) and where we drop an ignorable, uncorrelated error term. The value $\theta = 360/365$ scales the wavelength to one year, so we make an additional simplification by assuming that a year is 360 days long so that this term can be ignored. Alternatively, one could, as we do when we analytically calculate the bias, rescale t to incorporate the term. Thus

$$T_{S,it} = A\cos(t) = A\cos(\tau + j) \tag{9}$$

where $j = t - \tau$ is the number of days since the latest earnings change for this observation.

We apply Lagrange's identity to rewrite earnings-relevant sleep.

$$T^*_{S,i\tau} = D_i^{-1}\sum_{k=1}^{D_i} A\cos(\tau) = \frac{A}{D_i 2\sin(1/2)}(\cos(\tau - D_i + (\pi-1)/2) - \cos(\tau + (\pi - 1)/2))$$

The two cosine functions are simply phase shifts of each other, so we apply phasor addition to reduce this to

$$T^*_{S,i\tau} = AB_1\cos(\tau + \omega) \tag{10}$$

where

$$B_1^2 = \frac{(\cos((\pi-1)/2 - D_i) + \cos((\pi-1)/2))^2 + (\sin((\pi-1)/2 - D_i) + \sin((\pi-1)/2))^2}{2D_i\sin(1/2)}$$

$$\omega = \arctan(\cot((D_i + 1)/2))$$

This form is convenient because observed sleep can now be written as a phase shift of earnings-relevant sleep and thus suggests that a version of the error-in-variables formula will apply in this setting since we can linearly relate observed sleep to earnings-relevant sleep plus a correlated error term.

Now note that the only individual heterogeneity is in terms of the frequency of earnings changes, so without loss of generality, we can replace the D_i index with just D. Then, applying the usual variance-covariance formula for the OLS estimator of a single coefficient, we have that our estimator relative to the true coefficient is given by[14]

$$\operatorname{plim} \hat{\beta}_{1,D} = \frac{\operatorname{Cov}(w_D, T_{S,D})}{\operatorname{Var}(T_{S,D})} \tag{11}$$

$$= \beta \frac{\operatorname{Cov}(T^*_{S,D}, T_{S,D})}{\operatorname{Var}(T_{S,D})} \tag{12}$$

Where we have dropped the time subscripts based on the calculations below.

To derive closed-form expressions for Equation (12) observe that since seasonal sleep is mean zero, and, for all D, we are equally likely to observe sleep on any day of the year, then by the double angle formula and Lagrange's identity, the denominator is

$$\begin{aligned}
\operatorname{Var}(T_{S,D}) &= \lim_{T\to\infty} T^{-1} \sum_{t=0}^{T} A^2 \cos^2(t) \\
&= \frac{A^2}{2} + \lim_{T\to\infty} \frac{A^2 \csc(1)\sin(2T+1)+3}{T} = \frac{A^2}{2}
\end{aligned}$$

Where T (not sleep time, T_S) is the total number of time observations and the last equality follows from the boundedness of sine.

The numerator is, by application of the product-to-sum and Lagrange identities

$$\begin{aligned}
\operatorname{Cov}(T^*_{S,D}, T_{S,D}) &= D^{-2} \sum_{k=1}^{D} \sum_{j=1}^{D} \lim_{T\to\infty} T^{-1} \sum_{\tau=0}^{T} A^2 \cos(\tau - k)\cos(\tau + j) \\
&= D^{-2} \sum_{k=1}^{D} \sum_{j=0}^{D} \lim_{T\to\infty} T^{-1} \sum_{\tau=0}^{T} A^2 \cos(\tau - k)\cos(\tau + j) \\
&= \frac{A^2}{2D^2} \sum_{k=1}^{D} \sum_{j=0}^{D} \cos(k+j)
\end{aligned}$$

Taking the ratio of these two values gives the relative bias of the short-run estimate

[14] We loosely call this attenuation even though in practice the estimate can be negative even when the true coefficient is positive.

with respect to the true estimate for a given D.

$$\hat{\beta}_{1,D} \xrightarrow{p} \beta \left(D^{-2} \sum_{k=1}^{D} \sum_{j=0}^{D-1} \cos(k+j) \right) \tag{13}$$

Figure 5 shows this value for all frequencies of earnings changes less than a year.

[Figure 3 about here.]

This shape is the result of two factors. First, for any two of the same sinosoidal functions that are phase shifted from each other by less than a quarter or more than three-quarters of a wavelength, the product will be positive because the two functions are "in phase enough". For a phase shift greater than one-quarter but less than three-quarters of a wavelength, the product will be negative. The attenuation of the estimate is an average of these products, so for frequent earnings changes (small D), we are largely averaging sleep that is less than a quarter wavelength off from the truth. For intermediate values of D, we are averaging in sleep that is phase shifted enough to flip the sign on the estimate. For D near a year, however, we have "crossed the hump" again and are averaging in sleep values that are phase shifted so much that they are back to the beginning of the cosine wave. Beyond $D = 365$, the estimate remains nearly fully attenuated, with slight oscillations around 0.

The expected attenuation for the full population will depend, therefore, only on the distribution of the frequency of earnings changes. Assuming that the frequency of earnings changes, D, is a discrete random variable, this expectation can be calculated by a sum over D, weighted by the probability of observing that frequency.

$$\hat{\beta}_1 \xrightarrow{p} \beta \sum_{D} D^{-2} \sum_{k=1}^{D} \sum_{j=0}^{D-1} \cos(k+j) \Pr(D) \tag{14}$$

Barattieri et al. (2010) provide estimates of this density function (derived from Figures 12 and 13), with which we can calculate Equation (14). We discuss the measures provided by these authors in Section D.1, ultimately concluding that the degree of attenuation will be between 75 to 100%.

Finally, a note on alternative assumptions about the earnings determination process (Equation (7)): if earnings are based more on recent productivity rather than historical productivity (for instance if the manager is myopic when writing wage contracts), then our estimate will be closer to the true coefficient because we will be more likely to average together observed sleep that is less than a quarter-wavelength phase-shifted from the truth. If earnings are based on longer-term sleep or productivity patterns (for instance, the manager is very slow to update the wage contract and needs two earnings change cycles to fully incorporate current productivity changes), then our estimate will either be more biased or will be more likely to be attenuated all the way to zero.

C Additional ATUS results

[Figure 4 about here.]

[Table 8 about here.]

[Figure 5 about here.]

For the top two panels of Figure 7, in a first stage, we regress log earnings and sleep on all controls from Equation (4). We then predict the sleep residuals using daily sunset time. Finally, we fit a kernel regression through the resulting values. Under similar logic to the calculation of linear regression by fitting a line through conditional expectation functions, this figure provides a simple, semi-parametric method for showing the causal relationship between wages and sleep without imposing linearity. Note that a causal interpretation of this plot requires strong exogeneity (zero expectation of the error of log earnings conditional on sunset time) to rule out nonlinear dependence, as is typical of any nonlinear causal inference method.

The bottom two panels use the control function approach of Kim and Petrin (2013), which extends the nonparametric IV methods initially proposed by Newey et al. (1999). We allow sleep to enter as a quadratic, controlling for a quadratic in the first-stage residuals and the interaction of that quadratic with our instruments. The quadratic is chosen by an information criterion.

C.1 Additional short-run ATUS results

[Figure 6 about here.]

[Table 9 about here.]

[Figure 7 about here.]

C.2 Additional long-run ATUS results

[Figure 8 about here.]

Per the recommendation in Solon et al. (2013), we conduct a modified Breusch-Pagan test for heteroskedasticity of the residuals from the unweighted 2SLS model. The results in Table D13 show that location-level observations with smaller underlying counts of ATUS observations exhibit higher variance, as expected, and the relationship is statistically significant at the one percent level. While the constant term is statistically significant, it is an order of magnitude smaller. This suggests that the common error component within location is minimal, so weighting will likely result in an efficiency improvement, and indeed that is what we see in Table D14.

[Table 10 about here.]

Table D14 reproduces our long-run results from Table D3 above their unweighted counterparts. Weighting does indeed improve efficiency, reducing the standard errors in both the first stage and reduced form models. It does not appreciably alter the first-stage coefficient on sleep, but it does increase the magnitude of the reduced-form estimate. This suggests the presence of heterogeneity in the marginal effect of sleep on wages. High-skill urban workers have greater influence on the estimates in the weighted model, so the pattern of results below is consistent with larger marginal effects for such workers.

[Table 11 about here.]

We have also performed a variety of additional robustness checks with little or no change in estimates. We list them here without full tables, but all results are available upon request. 0.2% of the sample has topcoded wages. A tobit accounting for this does not change the results. Likewise, accounting for the truncation of sleep does not change inference. We have also estimated the models on only the sub-sample that is geocoded at the county or CBSA level. All of these robustness checks do not change inference.

In Table D15 we report estimates of county level characteristics as functions of average sunset time. We find a large and statistically significant relationship with population density, which motivates our use of a flexible control for this variable in estimating long-run effects. We also see a statistically significant relationship with unemployment, consistent with our estimated long-run wage effect.

[Table 12 about here.]

We report additional long-run ATUS robustness checks in Table D16. First, we remove all weekend diary observations. ATUS oversamples weekends so that roughly half of the total observations are from weekend dates (see Table D1). We test the sensitivity of our results to this by dropping the weekend diary entries entirely. The estimate is similar to baseline, albeit less precise. While the number of location-level observations is the same, this specification drops roughly half of the underlying ATUS sample. Second, we add education controls, and finally we add industry controls.

[Table 13 about here.]

C.2.1 Historical sorting

Figure 11 shows the county-level growth patterns around the dates of the 1883 and 1918 time zone implementations. For both figures, the 10% of counties that are closest to the eastern or western time zone boundary are considered to be on the eastern or western side, respectively. The dashed lines show median population growth rates (inter-census) for eastern side counties, and the solid lines show the same for western side counties. The composition of these groups differs between the two panels due to changes in the location of the 1883 versus 1918 time zones.

[Figure 9 about here.]

If gross sorting were occurring, one would expect eastern side counties to grow faster than western side counties after time zone implementation. Indeed, one might even expect the incentive to sort with respect to the 1883 time zones to be stronger than in the present day due to the lack of electrification. Instead, one can see that there is no evidence of gross sorting in response to the 1883 time zone. After implementation, the two regions of the time zones grow at almost identical rates. Growth rates around the 1918 law are more volatile but tell a similar story. Western side counties experience a slightly larger drop in growth rates after 1918 compared to eastern side counties, but the difference in changes between the two groups is not significant.

D Additional hedonic and QCEW results

[Table 14 about here.]

[Table 15 about here.]

[Table 16 about here.]

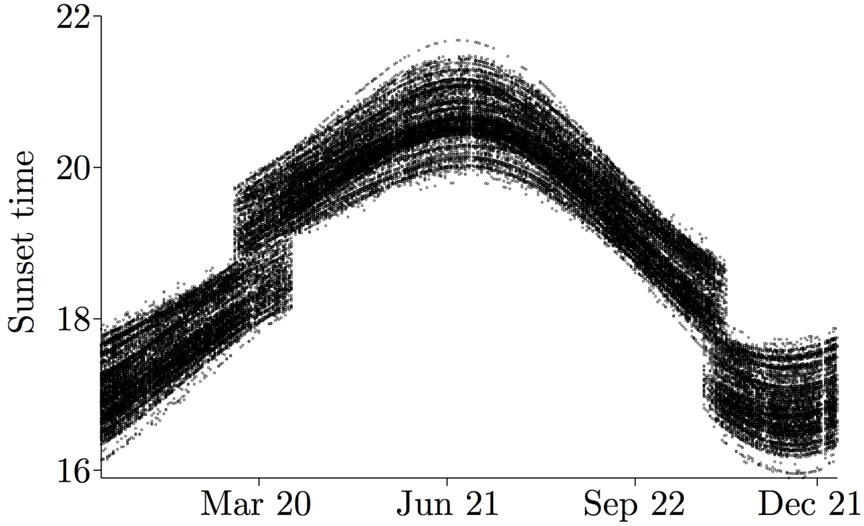
Within a time zone, average sunset time is a linear function of longitude.[15] To illustrate what the controls are doing in the QCEW estimates and to provide intuition for the sunset time instrument, we investigate the unconditional relationship between log wage and longitude within each time zone in Figure 12. Approaching the data in this way allows for an intuitive, map-like presentation, with the Pacific time zone at the left of the figure and the Eastern time zone at the right. For each time zone, we estimate

[15]Ignoring legal changes to the timing of daylight savings time, which did occur once during our sample period.

a separate linear kernel regression between longitude (and, implicitly, average sunset time) and log wage. Gray regions represent 95 percent confidence intervals. This figure strongly suggests that average sunset time is not unconditionally exogenous. Wages spike near the Pacific and Atlantic coasts. At inland time zone boundaries, where sorting is a concern (as discussed in Section 3.2) and labor markets may be integrated, the fitted relationships tend to intersect. To address these potential sources of endogeneity, we residualize log wages using time zone dummies, and interactions of the time zone dummy with coastal distance in the Pacific and Eastern time zones. This controls for the well-known gradients in population and home prices near the coast. We also exclude longitudes at which time zones overlap. Figure 12 plots a local linear relationship between residualized wage and longitude for each time zone. The pattern of results is striking, with similar linear relationships and upward slopes for all four time zones. Conditional on this parsimonious control set, western locations earn less on average than eastern locations within the same time zone.

[Figure 10 about here.]

Figure 1: Sunset time for the ATUS sample



Note: Each point shows the sunset time for an ATUS observation on the day of the year of that observation.

Knowing the particular pattern followed by sunset time allows us to characterize the degree of potential confounding that results from other seasonal variables in a precise way. These calculations are given in Section 3.3.1. Because of daylight savings time and the phase shift relative to the calendar, we can show that our estimates are robust to a wide range of seasonal confounders. Moreover, these features allow us to clearly disentangle short-run variation in sunset from calendar features like the December shopping season.

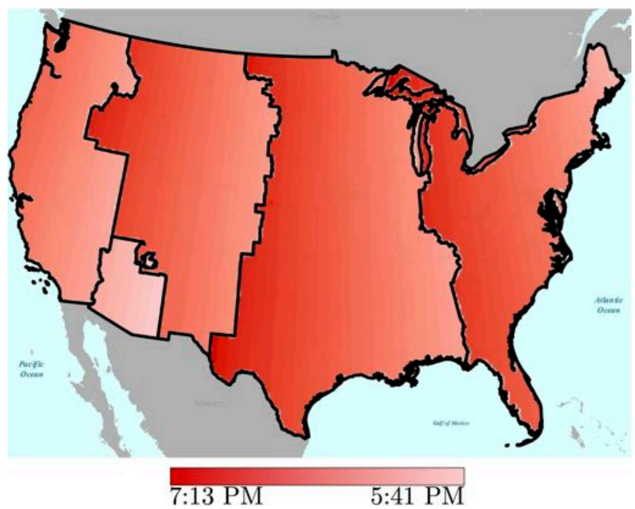
There is one identification issue we cannot address: seasonal variation in sunset time is almost perfectly correlated with seasonal variation in sunrise time and daylight duration.[16] Therefore in purely statistical terms, all short-run results could be recast in terms of either of these other variables. Our estimates are inconsistent with a causal story based solely on daylight duration, however. Note that our sleep model predicts higher wages in winter, when sleep is high and daylight duration is low. If low daylight duration leads to poor mood and reduced productivity, this will bias our specifications against finding positive wage effects from sleep. We choose to focus on sunset time rather than sunrise time because it is emphasized by existing medical literature and because it appears to be the driver of long-run differences in sleep, as discussed in the next section.

[16]Only changes in daylight savings time break this linkage.

D.0.2 Long-run validity

Figure 2 illustrates the source of our long-run variation in sunset time across locations. As the sun sets, eastern locations grow dark earlier than western locations, leading residents in more easterly locations go to bed earlier and sleep longer. By design, the maximum difference in sunset time within a U.S. time zone is approximately one hour.

Figure 2: Average sunset in the United States



Note: Map shows sunset time at the vernal equinox for the continental United States in 2012, which is a close approximation to average sunset time. Darker red indicates later sunset, lighter red indicates earlier. The time zone boundaries are given by bold black lines.

The difference in average sunset time between two locations over the year is plausibly unrelated to other factors influencing the labor market, making average sunset time a potentially valid instrument. In particular, time zone boundaries break the link between average sunset time and longitude. Average sunset time is also, by construction, orthogonal to latitude. All locations on earth experience the same average daylight duration over the year, so this is not an omitted variable in our long-run analysis.

The design of US time zones derived primarily from scientific, rather than commercial, considerations. Railroads implemented the first US time zones, called Standard Railroad Time (SRT), on November 18, 1883. They replaced a patchwork of railroad time standards and were quickly adopted by the US government and Western Union (Allen, 1883; Anonymous, 1883). While railroads were the first adopters, the primary impetus for standard time and the zone plan itself came from scientists concerned with problems like simultaneous observation of the aurora borealis at different points across the US (Bartky, 1989). The width of a zone, 15 degrees of longitude, was chosen to correspond with a one-hour difference in solar time (of Congress, 2010). Ultimately, US time zones derive from the speed at which the earth rotates and the historical accident that drew the Prime Meridian through Greenwich, England: King Charles II chose Greenwich as the site for the Royal Observatory in 1675.

Endogenous modifications to time zone borders could have undermined this initial randomization. Indeed, state and local governments may petition the Department of Transportation to switch time zones, which has resulted in a long-run westward movement of boundaries (USNO, 2014). This movement means that the precise location of the boundary is endogenous and research designs based on comparing nearby communities on opposite sides of the boundary might be biased. Note, however, that the westward movement of boundaries is the opposite of what we expect if counties are choosing their time zone based on sleep-driven productivity considerations. Switching from being on the eastern side of a time zone to the western side (which is what has happened to shift the time zone boundaries) moves the county from getting the "best" average sunset treatment to getting the "worst" in terms of sleep duration. Moreover, our design does not depend on the exact location of the boundary, but on the relative longitudes of cities within a time zone; the distance between the easternmost city in our data and the border is common to all observations in the time zone and does not contribute to our coefficients of interest. (In Table D6 we show our results are robust to the exclusion of all counties on time zone borders.) To avoid potential endogeneity, we drop locations that do not observe daylight saving time. Finally, while time zone borders often coincide with state borders, they frequently do not, and twelve of the lower 48 US states span multiple time zones (Hamermesh et al., 2008).

Current or past worker sorting on sunset time would also threaten the validity of our average sunset time instrument, but we find no evidence for this type of sorting in either historical or contemporary datasets. We do find a statistically significant relationship between average sunset time and population density, which motivates our use of a flexible control for this variable. Within a time zone, long-run average sunset time is a linear function of longitude and thus also correlated with coastal distance. Table D6 suggests coastal distance is indeed an important potential confounder, and we control flexibly for it in all our specifications. Thus the identifying assumption in our long-run models is exogeneity of average sunset time conditional on population density and coastal distance controls.

Firms might also sort on average sunset time, but simple firm optimization theory suggests they do not have strong incentives to do so. If a firm pays its workers their marginal product, managers may be indifferent to whether that marginal product is slightly higher or lower. Nonetheless this sorting is a theoretical possibility, and we test for it by regressing total wage bill in a county on sunset time and find no effect. In contrast, per-capita weekly wage bill is influenced by sunset time, as shown in Figure 3. Other possible channels for failure of our exclusion restriction are discussed below, and for issues that are amenable to empirical investigation, results are shown in Section 3.3.2.

D.1 Wage setting and measurement error

Our wage measure is the answer to a question about "usual weekly earnings" rather than wages on the day of the interview. Thus, even in the case where we correctly isolate exogenous changes in short-run sleep using daily sunset time, there is an additional identification issue inherent in studying wages rather than productivity: timing mismatch between observations of sleep and wages combined with a potentially low frequency relationship between wages and productivity. These issues mean that our short-run estimates of both the reduced form Equation 4 and the resulting two-stage least squares estimator will necessarily be biased.

For the survey day, we observe that day's sleep and the earnings reported by the individual. But if sleep is productive and earnings are a function of productivity, then the wage we observe is actually based on past sleep, not the contemporaneous sleep that we see. Luckily, using daily sunset to predict sleep provides us, under an assumption about the function that relates productivity to earnings, with an analytical expression for the relationship between observed sleep and earnings-relevant sleep.[17] We exploit this relationship to get bounds on the expected bias.[18]

This bias is present in all of our short-run estimates, but can be relatively benign. Consider, for instance, a piece-rate worker paid each day. Our observation of this worker's earnings could be based on yesterday's earnings and therefore sleep the previous night. We observe tonight's sleep, however, so the timing of our sleep observation is off by one night. Since daily sunset time is highly autocorrelated, the error in our estimate will be slight because we are using almost the correct variable.

In general, however, earnings change more slowly. To calculate the bias in these cases, note that the equation for the seasonal component of sunset time is known. We can use this function to find the bias in the reduced form and—since daily sunset time will induce a similar pattern in sleep in the first stage—in the instrumental variables estimates. In the Appendix we show that if earnings are a linear function of average productivity, then the estimated seasonal coefficient has an asymptotic bias that depends only on the distribution of D, the frequency

[17] We also treat a worker's observable characteristics on date t as the correct observables at the time of wage setting. Since many such characteristics are fixed or vary extremely slowly (for example race, occupation, and gender), we believe this assumption is benign.

[18] Details on the derivation can be found in Appendix Section B.

of earnings changes in the population, and a known trigonometric function. In particular

$$\hat{\beta} \xrightarrow{p} \beta \left(\sum_{d=1}^{\infty} d^{-2} \sum_{k=1}^{d} \sum_{j=0}^{d-1} \cos(k+j) \Pr(D=d) \right) \tag{15}$$

where $\hat{\beta}$ is one of our short-run estimates (from either the reduced form Equation 4 or the instrumental variables estimate based on Equations 3 and 4).

Barattieri et al. (2010, Figures 12 and 13) provide estimates of the density function for D, allowing us to evaluate this expression. The authors provide two sets of estimates: one based on raw, reported earnings (what we use in this study) and another based on earnings that have been cleaned to remove measurement error. For a given individual, reported earnings can vary over time due to contractual wage changes, changes in real take-home pay unrelated to wage (like overtime or commission), and measurement error. Using this measure, Equation (15) evaluates to 0.25, indicating that our estimate will be one-quarter the size of the true coefficient.

Ideally, we would like to calculate the distribution of D using only contractual wage changes and other changes in take-home income caused by productivity changes, but due to the presence of measurement error, we view 0.25 as a lower bound on the attenuation of our estimate, with one important caveat. Since Barattieri et al. (2010) provide estimates of earnings changes only at 4 month intervals, this bound could overstate attenuation because it under-weights changes that occur in less than 4 months ($D \leq 120$). From Figure 5, one can see that underweighting these high-frequency changes could substantially increase the expected bias.

Using the cleaned series from Barattieri et al. (2010), Equation (14) evaluates to -0.006, indicating that our estimate would be fully attenuated. The cleaned series removes measurement error but also likely removes real take-home pay changes, which would raise the frequency of earnings changes. Thus, we view this as a upper bound on the degree of attenuation. In conclusion, we expect, *a priori* that our short-run estimates should be between 0 and one-quarter of the true parameter value. The long-run estimator captures permanent shifts in sunset time and sleep, and it is thus unaffected by this source of bias. If the same structural model relates long and short-run sunset to wages, then we also expect that the short-run estimates will be less than one-quarter the size of the long-run estimates.

E Data

The most recent and largest data set from the United States containing both sleep time and wage information is the American Time Use Survey, which asks a subset of Current Population Survey (CPS) participants to fill out a time use diary for one day. ATUS began in 2003 and the most recent data are for 2013. For this study, we use the sample of individuals between the ages of age 18 and 65 who report receiving positive weekly wages from a primary or secondary job and who work full time. We exclude individuals in locations that do not observe daylight savings time, as local sunset time is potentially a choice variable in such locations. Summary statistics for variables of interest, broken down by sunset time, are given in Table D1. The table shows values for all individuals who report earning a weekly wage.

Table D1: ATUS Summary Statistics

	Early sunset Mean/(SD)	Late sunset Mean/(SD)	Diff./(SE)	Total obs.
Weekly earnings ($/week)	964.9	949.6	15.3***	61368
	(629.3)	(618.2)	(5.04)	
Hourly wage ($/week)	16.6	16.3	0.21**	31053
	(9.40)	(9.10)	(0.10)	
Sleep (hour/week)	58.4	57.6	0.83***	61368
	(14.4)	(14.0)	(0.11)	
Sunset time (24 hr)	17.6	20.1	-2.47***	61368
	(0.76)	(0.52)	(0.0052)	
Work (hour/week)	31.1	30.8	0.28	61368
	(30.9)	(31.0)	(0.25)	
Female (0/1)	0.48	0.48	0.0049	61368
	(0.50)	(0.50)	(0.0040)	
Age (years)	42.2	42.1	0.14	61368
	(11.5)	(11.7)	(0.094)	
Race, white (0/1)	0.82	0.82	-0.0031	61368
	(0.39)	(0.38)	(0.0031)	
Race, black (0/1)	0.13	0.13	0.0013	61368
	(0.33)	(0.33)	(0.0027)	
Weekend (0/1)	0.51	0.51	0.0053	61368
	(0.50)	(0.50)	(0.0040)	
HS or less (0/1)	0.31	0.32	-0.0087**	61368
	(0.46)	(0.47)	(0.0038)	
Some college (0/1)	0.28	0.29	-0.0037	61368
	(0.45)	(0.45)	(0.0036)	
College (0/1)	0.25	0.25	0.0046	61368
	(0.43)	(0.43)	(0.0035)	
Number of children	0.96	0.95	0.0032	61368
	(1.11)	(1.12)	(0.0090)	
Ever married (0/1)	0.77	0.77	0.0014	61368
	(0.42)	(0.42)	(0.0034)	

Note: Summary statistics for two sub-samples from ATUS are shown. Early sunset is defined as having a sunset time earlier than the median, and late sunset time is later than the median. Significance is determined from a t-test on the difference between means. Total observations are given in the far right column. The early and late sunset time groups each have half of the stated observations.

Aside from giving basic information on the sample, Table D1 also provides initial evidence in support of our main results. One can see that early sunset observations have significantly higher wages and sleep duration than observations with later sunset times. In contrast, other individual characteristics are well balanced across the two groups. Out of 11 other tests, only one difference is significant—the fraction of the population with a high school degree or less. This difference works in the direction of explaining the difference in wages in the two groups, but other (insignificant) differences work in the opposite direction. Note that dividing sunset time in this way conflates short and long-run variation. Results controlling for individual characteristics and broken down by short- versus long-run sunset time are reported in Section F.

To assign locations to individuals in ATUS, we began by merging all ATUS data with the corresponding CPS data. For a given individual, the CPS data often contain location at the county level. This variable is censored for individuals living in counties with fewer than $100,000$ residents. When county is available, we assign the county centroid as an individual's location. We have county location for approximately 44% of ATUS observations. For an additional 28% of observations, we observe location at the level of Census CBSA, a small group of counties in the same metropolitan area. In total, we are able to geocode 72% of observations at the sub-state level. For the remaining 28% of observations, ATUS contains location at the state level. We assign the 2010 population-weighted state centroid (computed by the Census) as the location for these individuals. In all cases where we refer to Federal Information Processing Standards (FIPS) codes, we are referring to either the county (FIPS 6-4) or CBSA-level code, if available, or the state level code (FIPS 5-2) where more detailed location is unavailable.

Nighttime sleep is our primary sleep measure. We remove any sleep that starts and ends during daylight hours on the date of diary entry. This will exclude naps, which might be an adaptation strategy for some short sleepers, however it also removes night-shift workers, for whom the sunset instrument should not be relevant. Empirically, our point estimates are practically unchanged by the exclusion of daytime sleep, but precision increases substantially. ATUS gathers data on all sleep during the course of a single 24 hour period for each individual, so there are potentially other ways to calculate naps, and our results are robust to alternative definitions.

Our primary wage measure is "usual weekly earnings" as reported in ATUS. This variable is defined for all respondents who have positive labor income and are not self-employed. It is top-coded above \$2,884.61. We also estimate a version of our model including only workers who receive an hourly wage, "hourly earnings at main job" as reported in ATUS. This variable is likewise top-coded at the level such that hourly earnings multiplied by usual weekly hours equals \$2,884.61. Some control variables (e.g. occupation codes) appear in both ATUS and CPS files. Where possible we use ATUS variables, which are more recent.

Our preferred regression specifications include a set of 22 occupation dummies or shares based on the ATUS "trdtocc1" variable, which categorizes the respondent's main job. Examples include "education, training, and library occupations" and "food preparation and serving related occupations."

The main shortcoming of ATUS is that it asks a new cross section of individuals for time use diaries each year, so we cannot construct an individual-level panel. In addition, the sample is not balanced over the year within a location. To address this potential problem in our long-run analysis, we collapse ATUS to a cross section in locations. For a more detailed description of ATUS, see Hamermesh et al. (2005). Importantly, ATUS also releases the exact date that the survey was conducted. Using this date and respondent location, we are able to determine sunset time for each individual in the dataset using solar mechanics algorithms from Meeus (1991). We compute annual average sunset time by computing sunset for each day in an individual's location, then calculating the mean over days of the year.

The Quarterly Census of Employment and Wages (QCEW), collected by the US Bureau of Labor Statistics, includes information on wages and employment (workers, not hours) at the county level. We collapse a county-level panel (1990-2013) to a cross section in order to investigate the reduced-form effects of our long-run instrument. Appendix Table D17 presents summary statistics.

Various robustness checks employ additional datasets. We merge temperature data from the NCEP/NCAR reanalysis produced by Kalnay et al. (1996) at the day-location level to test for seasonal confounding. The data is available on a two-by-two degree latitude-longitude grid. We use the daily average temperature from the nearest grid point for estimation.

F Results

F.1 Effect of sunset on sleep and earnings

We begin by presenting the effect of sunset time on sleep and earnings. Table D2 shows results from estimating Equations (3) and (4) on daily ATUS data. The first column shows that the sun setting one hour later within a location reduces nighttime sleep by roughly 20 minutes per week, which is statistically significant at the 1% level. We observe about 5 hours of variation in daily sunset time across our sample, meaning that we identify 1.9 hours per week of intra-annual sleep variation. Practically, this represents a substantial change in time use—roughly equal in magnitude to the 2.1 weekly work hours lost by the average individual during the most recent recession (Aguiar et al., 2013).[19] The second column of Table D2 shows that daily sunset time also affects earnings in a location. A

[19] Our results suggest that the sunset time influences sleep across the sample, while recession-induced work time changes are concentrated among those who lose their jobs, so individual responses to these two shocks will likely differ even if the averages are the same.

sunset time one hour later reduces earnings by 0.5%, on average. This effect is also statistically significant at the 1% level.

Table D2: Short-run effects of sunset on sleep and wages from ATUS

	(1) First stage Sleep	(2) Reduced form ln(earnings)
Daily sunset time	-0.38***	-0.0051***
	(0.042)	(0.0017)
Individual controls	Yes	Yes
Time controls	Yes	Yes
Location FEs	Yes	Yes
Observations	61368	61368
Adjusted R^2	0.12	0.31

Note: The table shows results from estimating Equation (3) (first column) and Equation (4) (second column) on ATUS data. The dependent variable is indicated at the top of each column. Earnings refers to "usual weekly earnings". Sleep is measured in hours per week and sunset time in hours. Controls are discussed in Section 3.1 and are location fixed effects; race indicators; age indicators; a gender indicator; indicators for holiday, day of week, and year; and occupation indicators. Standard errors, clustered at the FIPS code (location) level, are reported in parentheses. Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1.

Both estimates include individual controls for race (indicators for white, black, Asian, and other), age, age squared, a gender indicator, and detailed occupation code indicators (23 categories). Time controls are an indicator for holidays, separate indicators for each day of the week, and year fixed effects. The location fixed effects are at the most disaggregated FIPS code level available for each observation (county, CBSA, or state). The fixed effects absorb any spatial differences in sunset time, leaving only the seasonal component with which to identify the coefficient of interest.

We cluster standard errors at the FIPS code level. This clustering reflects that the exogenous variation is at the group rather than the individual level and varies by location. As shown in the robustness checks below, clustering at higher levels does not change the inference. Other robustness checks related to concerns about seasonal confounders are located in Section 3.3.1.

Table D3 presents estimates of the long-run effects of sunset on sleep (Equation 5) and earnings (Equation 6) using a cross-section in locations from ATUS. Column 1 shows that average weekly sleep falls by approximately one hour in a location where the sun sets one hour later. In the US, time zones create just over an hour of variation in long-run sunset time across locations. Thus average sunset induces about one hour per week of sleep variation, roughly half the variation induced by the short-run instrument. The second column shows that for a location where average sunset is one hour later, earnings are more than 4% lower on average.

Table D3: Long-run effects of sunset on sleep and wages from ATUS

	(1) First stage Sleep	(2) Reduced form ln(earnings)
Avg. sunset time	-0.93***	-0.045***
	(0.28)	(0.017)
Geographic controls	Yes	Yes
Demographic controls	Yes	Yes
Observations	529	529
Adjusted R^2	0.125	0.811

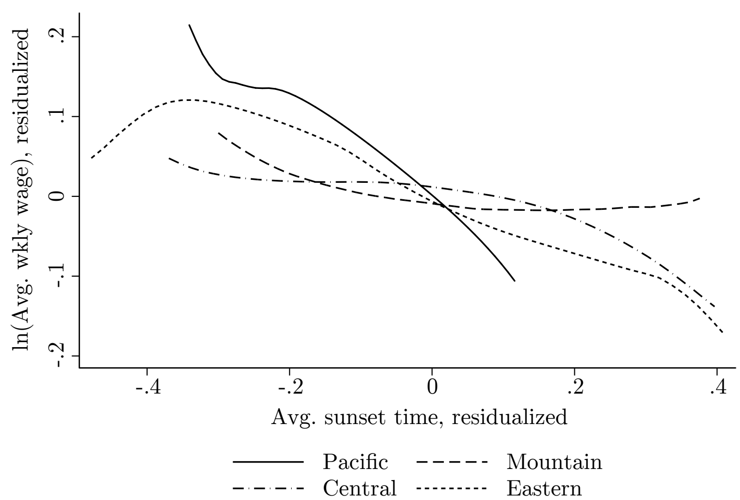
Note: The table shows results from estimating Equation (5) (first column) and Equation (6) (second column) on ATUS data. The dependent variable is indicated at the top of each column. Earnings refers to "usual weekly earnings". Sleep is measured in hours per week and sunset time in hours. Controls are discussed in Section 3.1 and are: an indicator for coastal county, coastal distance, and their interaction; a ten-piece linear spline in latitude; mean age and mean squared age; percent female; race and occupation shares; and a five-piece linear spline in population density. White heteroskedasticity-robust standard errors are reported in parentheses. Significance indicated by: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

We report heteroskedasticity-robust standard errors (White, 1980). We do not cluster because annual average sunset varies exogenously across locations, however we show in Section 3.3.2 that our results are robust to clustering standard errors at the state level. Section 3.3.2 also presents general robustness, along with a more focused discussion of robustness to potential spatial confounders.

Comparing the two reduced form estimates, one can see that the short-run estimate is about 10% the size of the long-run estimate. This is within the 0 to 25% range suggested by the measurement error analysis performed in Section D.1. We find further evidence that timing mismatch attenuates our short-run results when we estimate using the subsample of workers who receive hourly wages (reported in Appendix Table D12). Barattieri et al. (2010) show that workers who receive an hourly wage have a lower earnings change frequency than salaried workers. Evaluating the attenuation Equation (15) using the uncorrected series from Barattieri *et al*, we would expect the non-hourly worker estimate to be 1.4 times the size of the hourly worker estimate. Indeed, when we compare estimates from these two sub-samples, the non-hourly worker reduced form estimate is 1.5 times larger.[20]

Our ATUS results pool locations from all four continental U.S. time zones. If our hypothesized causal relationships between sunset, sleep, and wages hold, however, we would expect to find similar results within each time zone. The incomplete geographic coverage of ATUS limits our ability to explore within time zones, so we turn instead to the QCEW, which includes all U.S. counties. The added spatial richness of QCEW also allows us to calculate semi-parametric estimates of the reduced form relationship between average sunset time and wages, shown in Figure 3. The figure shows a locally-weighted kernel regression of residual log earnings on residual average sunset time within each time zone.

Figure 3: Long-run effects of sunset on wages in QCEW

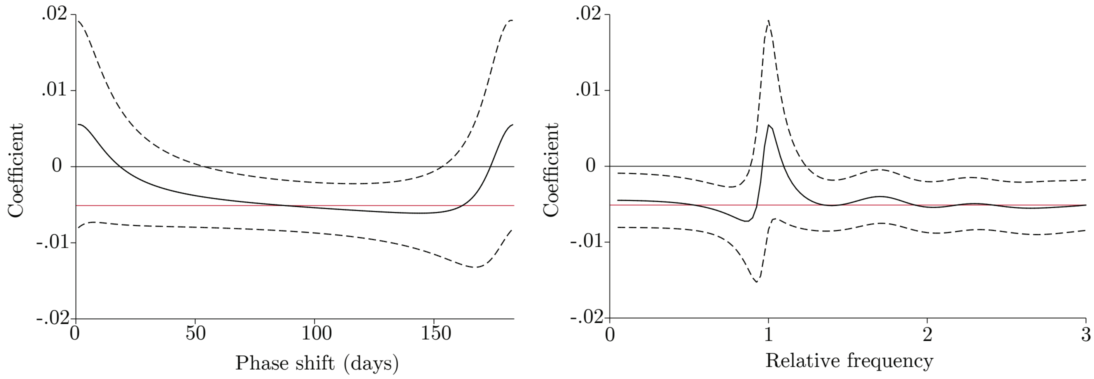


Note: Underlying wage data are a cross-section in locations from QCEW, 1990-2013. The figure shows the relationship between residualized log average wage and residualized sunset time for counties in the Eastern, Central, Mountain, and Pacific time zones. Time zone borders, defined as longitudes at which multiple time zones overlap, are excluded for the reasons discussed in Section D.0.2. Residualization is with respect to coastal distance, latitude, median age, percent female, percent black, and percent white. Demographic controls are from 2010 Census data.

[20]This result has further implications for the amount of measurement error in CPS wage variables that is beyond the scope of this paper.

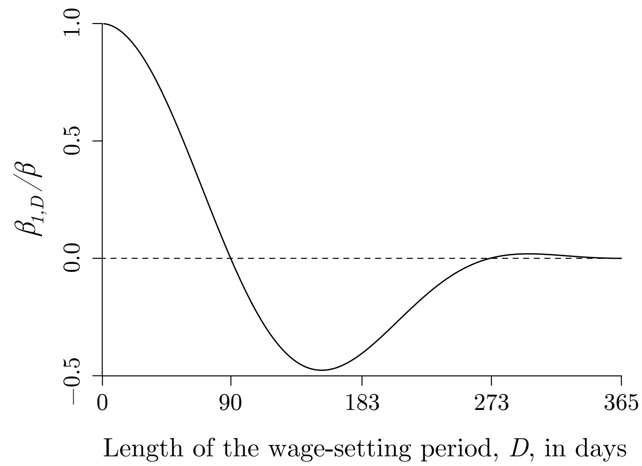
Figure 4: Robustness of short-run ATUS reduced form to seasonal controls

(a) Phase shifted control (b) Frequency control



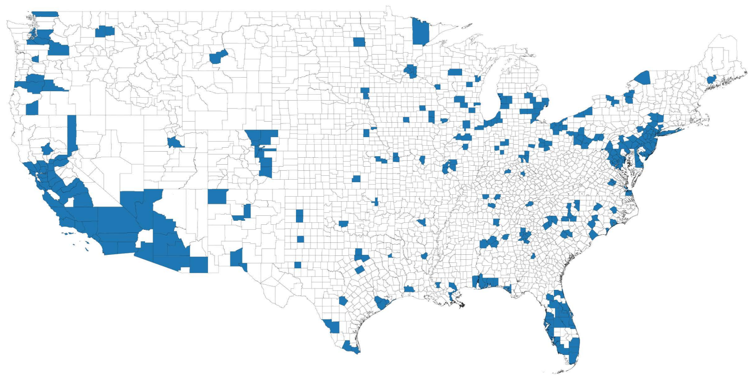
Note: Panel 4a shows estimates of the coefficient on sunset time in the reduced form Equation (4) when a phase-shifted seasonal control variable is included. The degree of phase shifting is indicated on the x-axis. Panel 4b shows estimates of the same coefficient when seasonal control variables with differing relative frequency are included. 95% confidence intervals are shown by dashed lines and the baseline reduced form estimate is given in red.

Figure 5: Short-Run estimate versus true estimate



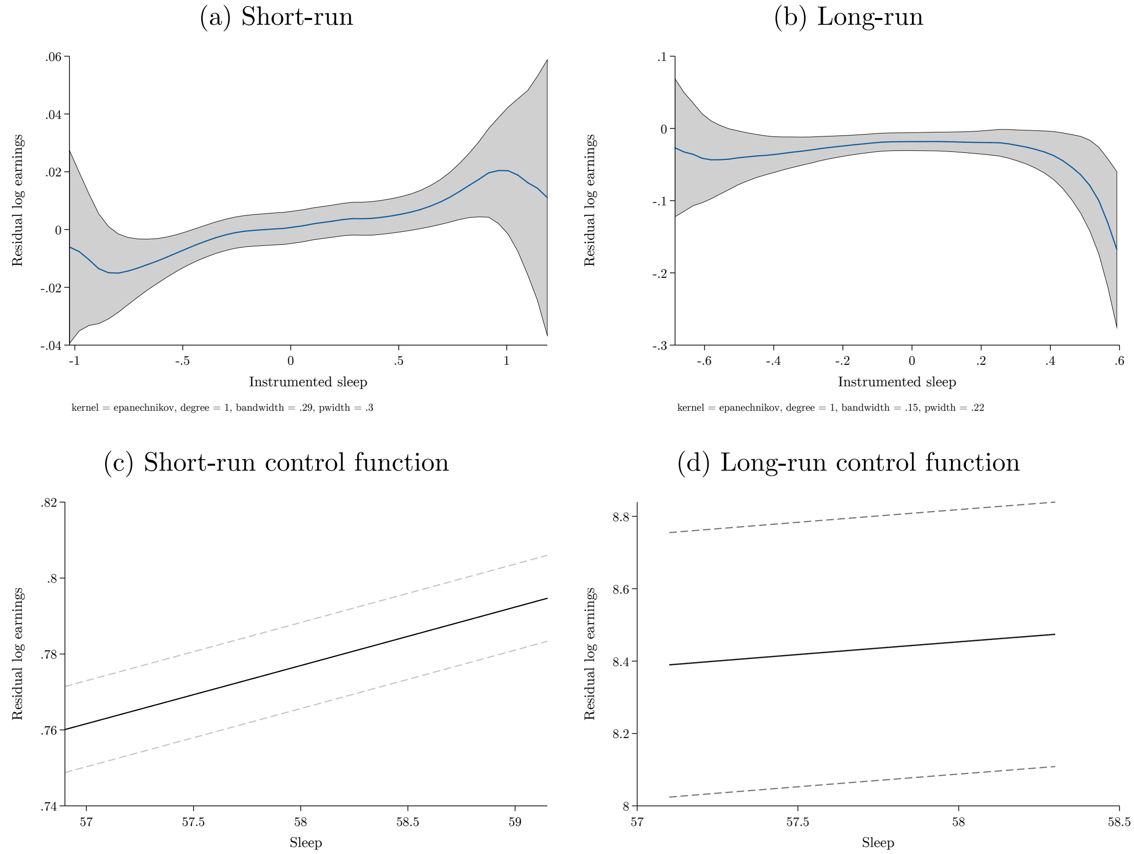
Note: The figure shows the ratio of the probability limit of the short-run estimate to the true estimate on the y-axis for a range of possible frequencies of earnings changes on the x-axis. A value of 1 on the y-axis indicates no bias, while a negative value indicates that the estimated coefficient has the wrong sign.

Figure 6: ATUS county-level geocoding



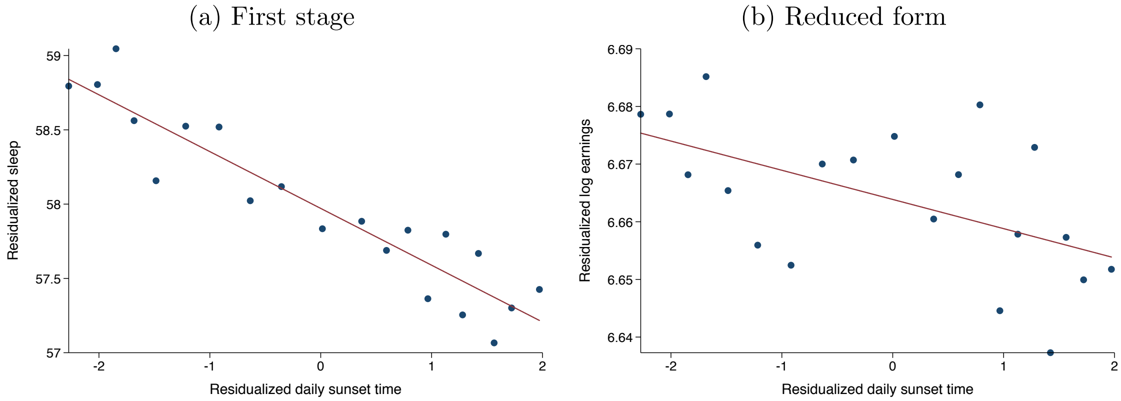
Note: The map shows, in blue, locations in the continental United States where we are able to geocode ATUS records at the county level.

Figure 7: Non-parametric causal relationship between wages and sleep



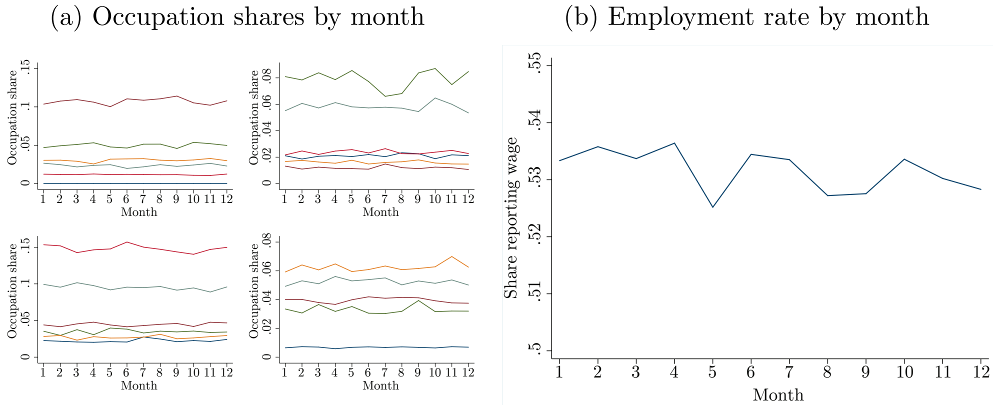
Note: The top two panels shows two kernel regressions of residual log wage on sleep instrumented by daily sunset time. Both regressions use an Epanechnikov kernel. The left panel uses the short-run ATUS sample and a bandwidth of 0.29, and the right panel uses the long-run ATUS sample and a bandwidth of 0.14. The bottom two panels show control function estimates based on Kim and Petrin (2013). The left panel again uses the short-run ATUS sample, and the right panel uses the long-run ATUS sample.

Figure 8: Short-run estimates of the conditional expectation function



Note: Created using binscatter. Sunset time is divided into 20 bins by quantile and means for the y-axis variables are computed within each. Sample and controls are the same as in Table D3, so the fitted line (dashed) which is estimated using OLS, corresponds to the estimates presented in that table.

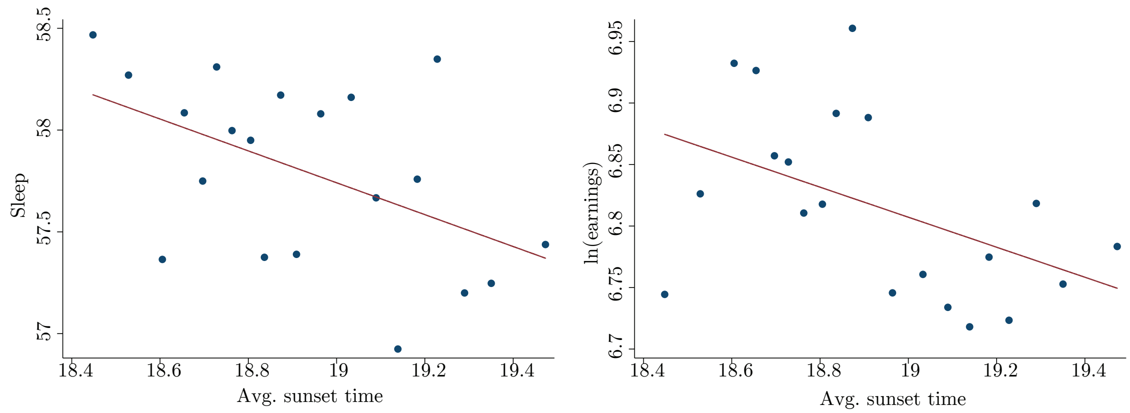
Figure 9: ATUS occupations do not exhibit seasonality



Note: Panel (a) Lines show average values of our 23 occupation dummies by month, pooled over the period 2003-2013 for our estimation sample. The occupation exhibiting a modest summer dip in the upper-right panel is "Arts, design, entertainment, sports, and media occupations." Excluding this occupation does not change our results. Panel (b) Line shows average value of dummy that equals 1 if the respondent reports a non-zero weekly or hourly wage, by month, pooled over the period 2003-2013 for all ATUS respondents.

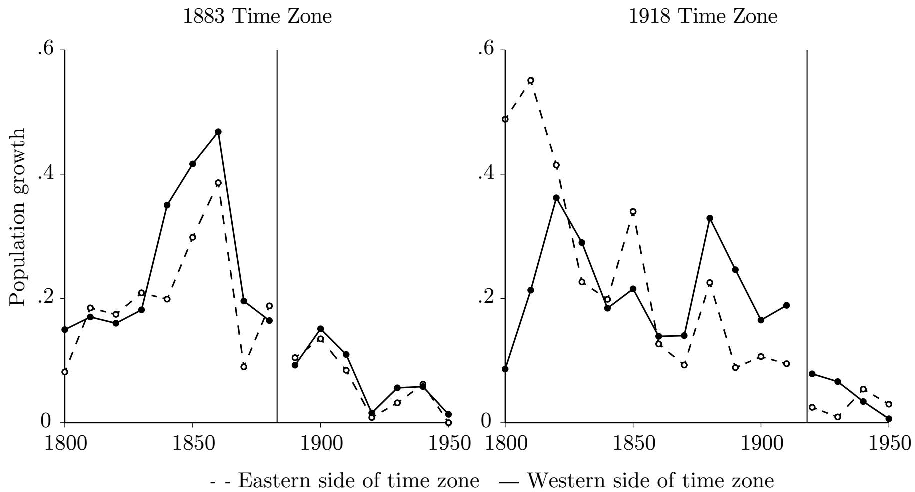
Figure 10: Long-run estimates of the conditional expectation function

(a) First stage (b) Reduced form



Note: Created using binscatter. Sunset time is divided into 20 bins by quantile and means for the y-axis variables are computed within each. Sample and controls are the same as in Table D2, so the fitted line (dashed) which is estimated using OLS, corresponds to the estimates presented in that table.

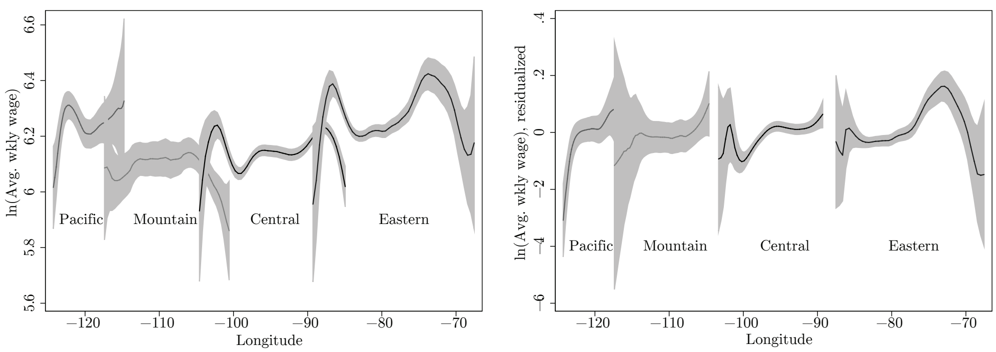
Figure 11: Historical time zone sorting



Note: The figure shows median growth rates between censuses in counties on the eastern and western edges of the 1883 (left panel) and 1918 (right panel) time zones. Eastern counties are represented by the dashed line and western counties are the solid line. All data are from Haines and Inter-university Consortium for Political and Social Research (2010).

Figure 12: Long-run effects of longitude on wages in QCEW

(a) Raw data (b) Residual wage



Note: The figures show kernel regressions of the log earnings on longitude within time zone. The shaded area represents a 95% confidence interval around the local linear fit. While the shaded regions for the Pacific and Mountain time zones intersect, the fitted lines do not have any common support. The gap between Pacific and Mountain fitted lines is smaller than for the Mountain-Central and Central-Eastern borders because the Pacific and Mountain zones share fewer common longitudes.

Table D4: Causal medical studies of sleep and performance

Study	Sleep change (hr/day)	Study duration (days)	Outcome	Elasticities (abs. value)
Belenky et al. (2003)	-4, -2, -1, +1	7	PVT speed	.7, .5, .7, 0
Cohen et al. (2010)	-2.5	21	PVT reaction time	18
Dinges et al. (1997)	-2.4	7	PVT lapses	6
Landrigan et al. (2004)	+.82	21	Serious medical errors	4.5
Lockley et al. (2004)	+.82	21	Attention failures	4
Van Dongen et al. (2003)	-4, -2	14	Memory task	3.3, 2.2
Vgontzas et al. (2004)	-2	7	PVT lapses	2.9
Mean magnitude				3.9

Note: Table includes all studies that experimentally manipulated sleep for at least 7 days, drawing on reviews by Van Dongen and Dinges (2005) and Banks and Dinges (2007). Studies of complete sleep deprivation were excluded. PVT stands for psycho-motor vigilance test, described in Section 2.1.

Table D5: Robustness of ATUS short-run reduced form estimate

	ln(earnings)			ln(earnings)	
No controls			*Quarter FEs*		
Daily sunset	-0.0080***	(0.0022)	Daily sunset	-0.0090***	(0.0033)
Work hours quadratic			*No holiday season*		
Daily sunset	-0.0049***	(0.0016)	Daily sunset	-0.0060***	(0.0020)
State clustering			Observations	53,053	
Daily sunset	-0.0051***	(0.0019)	*Temperature control*		
			Daily sunset	-0.0063***	(0.0024)

Note: The table shows results from estimating Equation (4). Dependent variable is indicated at the top of each column. Unless otherwise noted, controls, number of observations, and standard errors are the same as in Table D2. Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1.

Table D6: Robustness of long-run estimates

	ln(earnings)	
Only geographic controls		
Avg. sunset time	-0.075	(0.049)
Longitude control		
Avg. sunset time	-0.042**	(0.018)
State clustering		
Avg. sunset time	-0.045***	(0.015)
No time zone border counties		
Avg. sunset time	-0.037	(0.024)
Observations	450	

	ln(earnings)	
Time zone indicators		
Avg. sunset time	-0.043*	(0.023)
No Eastern time zone		
Avg. sunset time	-0.049*	(0.029)
Observations	244	
No high-wage cities		
Avg. sunset time	-0.033*	(0.019)
Observations	474	
Albouy QOL control		
Avg. sunset time	-0.032*	(0.019)

Note: The table shows results from estimating Equation (6). Dependent variable is the log of average earnings. Unless otherwise noted, controls, number of observations, and standard errors are the same as in Table D3. Results reported under "No high-wage cities" exclude workers in San Francisco, Los Angeles, Chicago, Boston, and New York. Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1.

Table D7: Effects on log median home value

	Log value	Log value
Sunset time	-0.0640***	-0.0574***
	(0.0232)	(0.0191)
Geographic controls	Yes	Yes
Demographic controls	No	Yes
Observations	2824	2824
Adjusted R^2	0.399	0.617

Note: White's heteroskedasticity-robust standard errors are reported in parentheses. Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1. Data are 2010 5-year ACS estimates. Sunset time is the average for a given county. Geographic controls include coastal distance and a ten-piece linear spline in latitude. Demographic controls include percent female, percent in four race categories, occupation shares, and a five-piece linear spline in population density.

Table D8: Waking time use as a function of sunset time

	Work time	Non-work time
Panel A: Short run		
Daily sunset time	-0.14**	0.34***
	(0.064)	(0.066)
Panel B: Long run		
Avg. sunset time	0.71	0.27
	(0.62)	(0.57)

Note: Data are from ATUS. The table shows results from estimating the first stage Equation (3) (Panel A) and Equation (5) (Panel B), replacing sleep time with either work time or waking non-work time as the dependent variable. Dependent variable is indicated at the top of each column. Controls, number of observations, and standard errors for Panel A are the same as in Table D2 and for Panel B are the same as in Table D3. Significance indicated by: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table D9: Waking non-work hours as a function of sunset time, selected groups

	Non-work time for	
	High work hours	Low wage earners
Panel A: Short run		
Daily sunset	-0.054	0.13
	(0.18)	(0.24)
Observations	3,851	5,930
Panel B: Long run		
Avg. sunset	-0.38	-1.85
	(1.24)	(2.92)
Observations	495	433

Note: Data are from ATUS. The table shows results from estimating the first stage Equation (3), replacing sleep time with waking non-work time as the dependent variable. In columns 1 and 3 the sample is workers who work more than 8 hrs on the diary date (7th percentile) and sleep less than 6 hrs (10th percentile). In columns 2 and 4 the sample is workers with log wages below 5.66 (10th percentile). Dependent variable is indicated at the top of each column. Unless otherwise noted, controls, number of observations, and standard error clustering are the same as in Tables D2 and D3. Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1.

Table D10: IV estimates of the effect of sleep on wages

	(1) Short-run ln(earnings)	(2) Long-run ln(earnings)
Sleep	0.013***	0.049**
	(0.0049)	(0.024)
Short-run controls	Yes	No
Long-run controls	No	Yes
Observations	61,368	529
Adjusted R^2	0.21	0.70
F-stat on IV	94.96	10.66
Elasticity	0.82	2.58

Note: The table shows instrumental variables estimates using ATUS data and based on the first stage and reduced form Equations (3) and (4) (column 1) and Equations (5) and (6) (column 2). The dependent variable is the log of usual weekly earnings. Controls for column 1 are the same as in Table D2 and for column 2 are the same as in Table D3. Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1.

Table D11: Bedtime and wake time

	(1) Bedtime	(2) Wake time
Panel A: Short-run		
Daily sunset time	0.12***	-0.034***
	(0.0049)	(0.0042)
Panel B: Long-run		
Avg. sunset time	0.46***	0.31***
	(0.039)	(0.035)

Note: Dependent variable is given at the top of the column. Controls, number of observations, and standard errors are the same as in Table D2 (Panel A) and Table D3 (Panel B). Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1.

Table D12: Additional short-run robustness checks

	ln(earnings)			ln(earnings)	
Only time controls and location FEs			*No occupation codes*		
Daily sunset	-0.0066***	(0.0021)	Daily sunset	-0.0046**	(0.0019)
No weekends			*Hourly workers only*		
Daily sunset	-0.0054**	(0.0023)	Daily sunset time	-0.0028*	(0.0016)
Observations	30169		Observations	32192	
Education controls			*No 4th or 1st quarter*		
Daily sunset	-0.0034**	(0.0016)	Daily sunset	-0.021***	(0.0061)
			Observations	30065	

Note: The table shows results from estimating Equation (4). Dependent variable is indicated at the top of each column. Unless otherwise noted, controls, number of observations, and standard errors are the same as in Table D2. Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1.

Table D13: Modified Breusch-Pagan heteroskedasticity test

	Residuals^2
1/Observations	0.10***
	(0.020)
Constant	0.011***
	(0.0014)
Observations	529
Adjusted R^2	0.045

Note: The dependent variable is the squared residual from estimating the unweighted version of (6). The variable "1/Observations" is the reciprocal of the number of ATUS interviews underlying a given location-level observation. Because the modified Breusch-Pagan test relies on the assumption of homokurtosis, we compute unmodified OLS standard errors. Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1.

Table D14: Long-run effects, weighted and unweighted

Panel A: Weighted			
	First stage	Reduced form	2SLS
	Sleep	ln(earnings)	ln(earnings)
Avg. sunset time	-0.93***	-0.045***	
	(0.28)	(0.017)	
Sleep			0.048**
			(0.023)
Observations	529	529	529
Adjusted R^2	0.125	0.811	0.699
F-stat on IV			11.22

Panel B: Unweighted			
	First stage	Reduced form	2SLS
	Sleep	ln(earnings)	ln(earnings)
Avg. sunset time	-0.86**	-0.0013	
	(0.44)	(0.022)	
Sleep			0.0015
			(0.025)
Observations	529	529	529
Adjusted R^2	0.119	0.648	0.650
F-stat on IV	3.86		

Note: The table shows results from estimating Equation (6). In Panel A location-level observations are weighted by the count of underlying ATUS respondents, while in Panel B they are unweighted. The dependent variable is indicated at the top of each column. Earnings refers to "usual weekly earnings". Controls are as reported below Table D3. White's robust standard errors reported in parentheses. Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1.

Table D15: Robustness: County characteristics

	Log pop. density	Pop. change frac.	Net migration frac.
Sunset time	-0.642***	-0.000931	-0.000980*
	(0.110)	(0.000814)	(0.000533)
Observations	3104	3104	3104
Adjusted R^2	0.012	0.000	0.001

	Log poverty rate	Labor force change	Unemployment rate
Sunset time	0.0157	0.00184	-1.412***
	(0.0221)	(0.00342)	(0.169)
Observations	3103	3103	3103
Adjusted R^2	-0.000	-0.000	0.023

Note: Dependent variable is indicated at the top of each column. All data are from the Census and observations are at the county level. Population, net migration, and unemployment rate are all 2012 values. Poverty is from 2011. Labor force change is from 2000 to 2010. White heteroskedasticity-robust standard errors are reported in parentheses. Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1.

Table D16: Additional robustness of long-run estimates

	Reduced form ln(earnings)	
No weekend diaries		
Avg. sunset time	-0.054**	(0.022)
Observations	527	
Education controls		
Avg. sunset time	-0.035**	(0.015)
Industry controls		
Avg. sunset time	-0.050***	(0.018)
Hourly workers only		
Avg. sunset time	-0.061***	(0.020)
Region indicators		
Avg. sunset time	-0.049**	(0.019)

Note: The table shows results from estimating Equation (6). Dependent variable is the log of average earnings. Unless otherwise noted, controls, number of observations, and standard errors are the same as in Table D3. Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1.

Table D17: QCEW summary statistics

Variable	Mean	Std. Dev.
Weekly wage	492.37	171.88
Weekly wage - goods	609.35	240.53
Weekly wage - services	431.84	161.19
Sunset time	18.38	.94
Observations	285,680	

Note: All data are from the Quarterly Census of Employment and Wages at the county level from 1990-2013.

Table D18: Hedonic robustness

	Log value	Log value	Log value	Log value
Sunset time	-0.0574***	-0.0599***	-0.0432***	-0.0536***
	(0.0191)	(0.0154)	(0.0162)	(0.0188)
Base controls	Yes	Yes	Yes	Yes
Industry shares	No	Yes	No	No
Educational attainment	No	No	Yes	No
Longitude	No	No	No	Yes
Observations	2824	2824	2824	2824
Adjusted R^2	0.617	0.769	0.740	0.621

Note: The table shows robustness checks for the hedonic results. White heteroskedasticity-robust standard errors are reported in parentheses. Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1. Data are 2010 5-year ACS estimates. Sunset time is the average for a given county. Column 1 reproduces the final column of our preferred results from Table D7 and "Base controls" denotes the controls from that model. Additional controls employed in this table are a 3-piece linear spline in coastal distance, 13 industry shares, and longitude.

Table D19: QCEW reduced form estimates

	ln(avg. wage)	ln(avg. wage)	ln(avg. wage)	ln(avg. wage)
Sunset time	-0.203***	-0.159***		
	(0.0213)	(0.0225)		
Pacific×sunset			-0.779***	-0.630***
			(0.164)	(0.150)
Mountain×sunset			-0.115	-0.189**
			(0.0829)	(0.0813)
Central×sunset			-0.140***	-0.0828***
			(0.0267)	(0.0289)
Eastern×sunset			-0.290***	-0.247***
			(0.0380)	(0.0442)
TZ dummies	Yes	Yes	Yes	Yes
Coastal distance	Yes	Yes	Yes	Yes
Demographic controls	No	Yes	No	Yes
Observations	2,409	2,409	2,409	2,409

Note: The table shows the linear regression analogue of Figure 3. Heteroskedasticity-robust standard errors are reported in parentheses. Significance indicated by: *** p<0.01, ** p<0.05, * p<0.1. Data are from the BLS Quarterly Census of Employment and Wages 1990-2013. Sunset time is the quarterly county average.