I Z A Institute
of Labor Economics
Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 14770

Sunset Long Shadows: Time, Crime, and Perception of Change

Pavel Jelnov

OCTOBER 2021

I Z A Institute
of Labor Economics

Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 14770

Sunset Long Shadows: Time, Crime, and Perception of Change

Pavel Jelnov
Leibniz Universität Hannover and IZA

OCTOBER 2021

ISSN: 2365-9793

IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Sunset Long Shadows: Time, Crime, and Perception of Change

How long survives perception of change after evaporation of the actual change? I investigate the effect of daylight on crime and fear of crime. Forty years of reforms shifted the boundaries between Russian eleven time zones. I find that a permanent switch to a later sunset leads to a two year long decrease in robbery and has no effect on homicide. The magnitude of the effect on robbery is similar to the previous estimates from other countries immediately after daylight saving time transitions. Even though the actual effect lasts two years, women report in a 10-year perspective increased feeling of safety even in darkness. However, men report increased feeling of safety only as long as the actual decrease in robbery persists.

JEL Classification: J18

Keywords: crime, daylight saving time, fear of crime, homicide, robbery, Russia, time zones

Corresponding author:
Pavel Jelnov
Wirtschaftswissenschaftliche Fakultät
Institut für Arbeitsökonomik
Leibniz Universität Hannover
Königsworther Platz 1
30167 Hannover
Germany
E-mail: jelnov@aoek.uni-hannover.de

1 Introduction

The ongoing climate and Covid-19 crises refreshed our understanding that despite industrialization, humans depend on nature. There is increasing interest in how environmental factors affect economics (Burke et al., 2015, Cole et al., 2017, Dell et al., 2014, Deschênes and Greenstone, 2011, Dillender, 2021, Hajdu and Hajdu, 2021, Jessoe et al., 2018, de Oliveira et al., 2021). One of the most important natural resources is daylight. Yet modern institutions that accommodate society to daylight, i.e., time zones and daylight saving time (DST), remain variable and their efficiency remains, more than a century after their introduction, uncertain. For instance, recently, the European Union relied on a poll in a decision to abolish DST transitions. This decision set a dilemma in front of each EU member to which time zone it shall belong.

In this paper, I investigate the effect of daylight on crime and fear of crime. I study the case of Russia, the biggest time laboratory in the world, which has eleven time zones and exercises frequent reforms that shift their boundaries. I employ official records to assess the effect of time reforms on crime and use longitudinal survey data to assess the effect of the reforms on fear of crime and on the related behavior.

The paper touches three issues: the optimal regional time zone, the divergence between actual crime and crime perception, and the long-run effects of ambient light on crime and behavior. Hence, the paper makes several contributions. First, to the best of my knowledge, it is the first use of a natural experiment to assess not only the immediate but also the long run effect of daylight on crime. Previous studies estimated the regression discontinuity at DST transitions, which is, by the seasonal design of DST, cancelled out in the long run. Contrary, I consider permanent changes and their long-run effect.[1]

The second contribution is to a discussion on divergence between crime and its perception. To this end, I utilize a longitudinal survey that inquires about respondent's feeling safe to walk in *darkness*. Even though no time reform can change the nature of darkness, I find a strong permanent effect of time reforms on the perceived safety indicator. Finally, the paper contributes to the literature on the interaction between environment and society. In

[1] Bünnings and Schiele (2021) analyze the permanent effect of daylight on traffic accidents in the United Kingdom.

particular, the fit between the natural and the social schedules has long been investigated in the medical literature and has recently received growing attention from economists.

My empirical analysis is divided into two parts. In the first part, I estimate the effect of time reforms on crime. In line with most previous literature, I focus on robbery, the most relevant crime in the context of outside meetings between offenders and victims. I also analyze homicide. For identification, I apply the lags-and-leads model (Hajdu and Hajdu, 2021) and check the robustness of the results using the Borusyak et al. (2021) method for event analysis.

I document a decrease of around 11% in robbery in the first year after a shift to a higher time zone, i.e., one-hour later sunset, and a decrease of around 13% two years after the reform. This result is almost equal to the recent estimates of the immediate DST transition effect in Uruguay, provided in Tealde (2021), who follows Domínguez and Asahi (2019), Munyo (2018), Umbach et al. (2017), Toro et al. (2016), Doleac and Sanders (2015), and Toro et al. (2015) in investigation of the effect of DST transitions on crime in the western hemisphere. Therefore, I document that the immediate effect, documented in the previous literature for DST transitions, can be observed for two years if the transition is permanent. However, I find no effect on homicide.

In the second part of the analysis, I confront the actual effect on crime with its perception by individuals. Even though the effect on robbery persists for two years and there is no effect on homicide, I find that the behavioral effect of time reforms is strong and permanent. Analysis of longitudinal survey data shows that in a 10-year perspective women are still more likely to report feeling safe to walk in darkness in their neighborhood of residence. The effect on men lasts only two years, similarly to the effect on actual robbery. The results are similar in European and Asian regions of Russia.

Not only the reported fear is affected but also the actual behavior. I find that following the reforms, individuals increase their walking for daily needs. This result is related to the causal effect of crime on walking, documented in Janke et al. (2016) and is in line with findings on the positive effect of DST on outdoor activity, found in Wolff and Makino (2012). I also investigate the effect on sleep and find a two-year decrease in sleep, correspondingly to the literature that studies sleep in the context of time use (Giuntella and Mazzonna, 2019, Umbach et al., 2017, Hamermesh et al., 2008). The decrease in sleep indicates a shift toward other activities, which may be related to the enhanced feeling of safety to spend time outside.

The effect of daylight on crime and fear of crime is nested in the literature on the interaction between physical environment and humans (Triguero-Mas et al., 2015). For instance, heat and large variations in climate are associated with a higher incidence of crime and conflict (Baysan et al., 2019, Ranson, 2014). The effect of weather on aggression is combined of the direct effect of heat, sunlight, humidity, etc., on mood and the indirect effect through economy, outside activity, and social interaction (Keller et al., 2005). Daylight directly affects human physiology and psychology through vision, skin, and nonvisual ocular actions on the circadian clock in the brain and on other neuronal pathways (Münch et al., 2017). It mitigates seasonal affective disorder (SAD), depression with typical onset during the fall or winter and remission in the spring (Azmitia, 2020, Kurlansik and Ibay, 2012, Keller et al., 2005). The emotional effects of light may include not only decreased aggression but also decreased fear (Kawamura et al., 2019, Yoshiike et al., 2018, Warthen et al., 2011). Therefore, both the actual crime and fear of it should be assessed as separate outcomes.

In addition, a body of research documents that light, sun, clear air, and good weather contribute to honest and pro-social behavior (Lu et al., 2018, Guéguen and Jacob, 2014, Guéguen and Stefan, 2013, Guéguen and Lamy, 2013, Zhong et al., 2010, Rind and Strohmetz, 2001, Rind, 1996). Experimental evidence shows that improved street lighting is associated with a lower crime rate (Tealde, 2021, Chalfin et al., 2019, Arvate et al., 2018, Welsh and Farrington, 2008), and improved lighting at sport facilities leads to decreased violence (Amorim et al., 2016). These effects may be related not only to the increased probability of arrest and punishment when the crime is committed in light (Tealde, 2021) but also to decreased aggression. Related socio-ecological mechanisms were found also in the effect of the landscape and land use of public spaces on crime and fear of crime (Rišová and Madajová, 2020, Mak and Jim, 2018, Twinam, 2017, Tandogan and Ilhan, 2016, Sreetheran and Van Den Bosch, 2014) and in the difference between police behavior in light and darkness (Horrace and Rohlin, 2016).

Finally, the economic context of this paper is the social cost of crime. Exploration of incentives to commit crime dates back to Becker (1968), while discussion and estimates of the social cost of crime can be found in Koppensteiner and Menezes (2021), Ponomarenko and Friedman (2017), Welsh et al. (2015), Wickramasekera et al. (2015), Anderson et al. (2012), Heaton (2010), McCollister et al. (2010), and Ayres and Levitt (1998). Recently, Velásquez (2020) and earlier Dell (2015) document the negative effect of Mexican drug war on the weak segment of the labor force, i.e., female and informal sector workers. From behavioral perspective, Becker and Rubinstein (2011) provide a conceptual framework and empirical evidence that fear of terrorism is endogenous to the individual economic activity. Fear of crime is addressed in Janke et al. (2016), Stearns (2012), Czabanski (2008), Dolan and Peasgood (2007), and Moore and Shepherd (2006).

The paper proceeds as follows. Section 2 addresses the history of Russian time zone reforms. In Section 3, I use province-level crime records to investigate the effect of time reforms on crime, and in Section 4, I utilize a longitudinal survey to assess the effect of the reforms on fear of crime, walking, and sleep. Section 5 concludes.

2 Background

Russia is an exceptional case to study the effects of environmental variables. It uniquely combines vast geography with a relatively homogeneous population, 80% of which are ethnic Russians. The Soviet government imposed geographic redistribution of population alongside cultural equalization. As a result, the Soviet heritage is a large but culturally homogeneous country, where ethnic Russians are the major group in most regions, and minorities are mostly concentrated in a few "national republics" and in the far north. The combination of diverse geography and homogeneous population enhances identification of the causal effects of environmental variables on socio-economic indicators.

The longitudinal distance between Russian westernmost province,[2] Kaliningrad, and the easternmost province, Chukotka, is 157°, corresponding to 11 natural (nautical) time zones. Russia indeed has eleven time zones, shown on the map in Figure A.1 in Appendix. A natural time zone is the one that places the sun in the zenith around 12pm. However, the Russian time zones are not natural in this sense in most of the regions. For instance, in the period between 1990 and 2015, the time was equal to the natural one in only 196 out of 2,162 province-year cases. Between 1995 and 2014, the number was only 20 out of 1,662. In almost all of the other cases, institutional time exceeds the natural time. Between 1990 and

[2] The official Russian term for a province is "federal subject".

2015, institutional time exceeded the natural time by one hour in 52% of the province-year cases, and by two hours in 38% of the cases. It means that sunrise and sunset in Russia are, roughly speaking, institutionally late.

Russia differs from other countries by its frequent time reforms. The time zones were introduced in 1919 and were expanded to the whole territory of the Soviet Union in 1924. The introduction of the time zones was followed by a long list of changes that continue to this day. In particular, the difficulty to manage eleven time zones led Russia to experience 19 time reforms since 1980. In addition to the reforms that shifted the time zone boundaries, Russia observed DST from 1981 to 2011. To illustrate these time changes, Figure 1 shows the sunrise and sunset times in 2002 in the Siberian city of Tomsk. The figure exhibits three discontinuities. The first discontinuity is the DST transition in March, common to entire Russia. The second discontinuity is the reform that took place in May, affected only Tomsk, and set its clocks back by one hour. The third discontinuity is the October shift back from DST.

The frequency of time reforms has been particularly high since the late 1980s, before and after the collapse of the Soviet Union. Time zones affect coordination between regions (Christen, 2017, Hattari and Rajan, 2012, Hamermesh et al., 2008, Stein and Daude, 2007, Kikuchi et al., 2006), but Russian time reforms are related to a more general struggle between the country's vast geography and political centralization.[3]

In particular, about 50 out of the total[4] of 85 provinces use Moscow Time. The 2010–2018 cycle of reforms illustrates its dominance but shows also the resistance to time coordination with Moscow. The president stated in 2009 that distant regions should be set "closer" to Moscow, which should improve the coordination between the local governments and the central one. In the following year, the number of time zones shrank from eleven to nine (by changing the time in five provinces), and the number of provinces in the Moscow Time zone increased from 50 to 52 and increased further to 54 in 2014 after annexation of Crimea.

[3]In some countries, time is being manipulated not only for political but even for symbolic reasons, e.g., in revolutionary China, Francoist Spain, Chávez-led Venezuela, and North Korea. In China, the tension between forced centralization and nature created the anomaly of Xinjiang time, where two times with a two-hour difference are used in the same province, and the separation lies along ethnic rather than geographic lines.

[4]Including Crimea and Sevastopol, annexed in 2014. The annexation is not recognized internationally.

However, the implementation of the 2010 reform was unpopular. The reform was recognized as a failure already in 2011, leading to another reform. The third reform, in 2014, restored the two missing time zones. Furthermore, ten provinces changed their time in 2016, an additional province changed it in 2018 but returned to the former time in 2020. This cycle of reforms exhibits the unresolved trade-off between nature and centralization.

As a result, while some reforms, such as introduction of the DST in 1981 and its abolition in 2011, affect the entire country, many reforms involve only specific regions. Table 1 lists all the reforms that took place since 1980. The table indicates the number of the affected provinces and shows the direction of each change.[5]

Throughout the paper, I use the following definition:

"Treatment" is a change in time difference from Moscow.

This definition of treatment is equivalent to the sunrise or sunset times stripped from the location, year, and day of the year fixed effects. The location fixed effects control for the mean time at the location. The year fixed effects control for any time changes common to all of the country, e.g., DST transitions (which keep time difference from Moscow unchanged). Finally, the day of the year fixed effects control for the curve of sunrise and sunset times over the year. Because the day fixed effects absorb any seasonal fluctuations in the daylight duration at a given location, the sunrise and sunset times are fully correlated in presence of the location and day fixed effects. Therefore, time difference from Moscow fully absorbs any reform-driven fluctuations in the sunrise and sunset times.

There are 18 provinces that satisfy this definition. Figure 2 shows the map of Russia, where the treated provinces are shadowed. Figure 3 follows the reform-driven changes in time difference from Moscow in these provinces.

The largest (in terms of population) group of treated provinces covers the banks of Volga river, close to the boundary between European and Asian parts of Russia. This is where the vast Moscow Time zone ends. Because of the dominance of Moscow Time (UTC+3), the Samara Time (UTC+4), which runs along Volga, covers a relatively small region. Struggling to exist, the Samara Time zone experienced several changes to its boundaries between 1988

[5] Because I use crime records from 1990 on and consider the 10-year lag of time reforms, 1980 is the first year entering this paper's analysis.

and 1992, was completely extinct between 2010 and 2014, included only two provinces in 2014, was expanded to five provinces in 2016, to six provinces in 2018 and shrank back to five provinces in 2020.

The second region that is subject to relatively frequent reforms includes provinces in the orbit of the strong city of Novosibirsk in Western Siberia. Between 1991 and 2002 and in 2016, different provinces in this region experienced time changes. For instance, the 1995 reform in Altai and the 2002 reform in Tomsk followed the 1993 reform in Novosibirsk, partly because many workers travel frequently between Altai, Tomsk, and Novosibirsk, and time coordination with Novosibirsk synchronizes the train schedule.

In addition, the map in Figures 2 indicates that special reforms involved also the far east of Russia as well as its westernmost province, Kaliningrad. Not surprisingly, the trade-off between centralization and nature is salient in the western and eastern periphery of the vast country. Finally, following its internationally unrecognized annexation by Russia in 2014, also Crimea became a "treated" region when it set its clocks by two hours forward to align with Moscow Time. Yet I exclude Crimea from the analysis, because it was not incorporated in Russia until 2014.

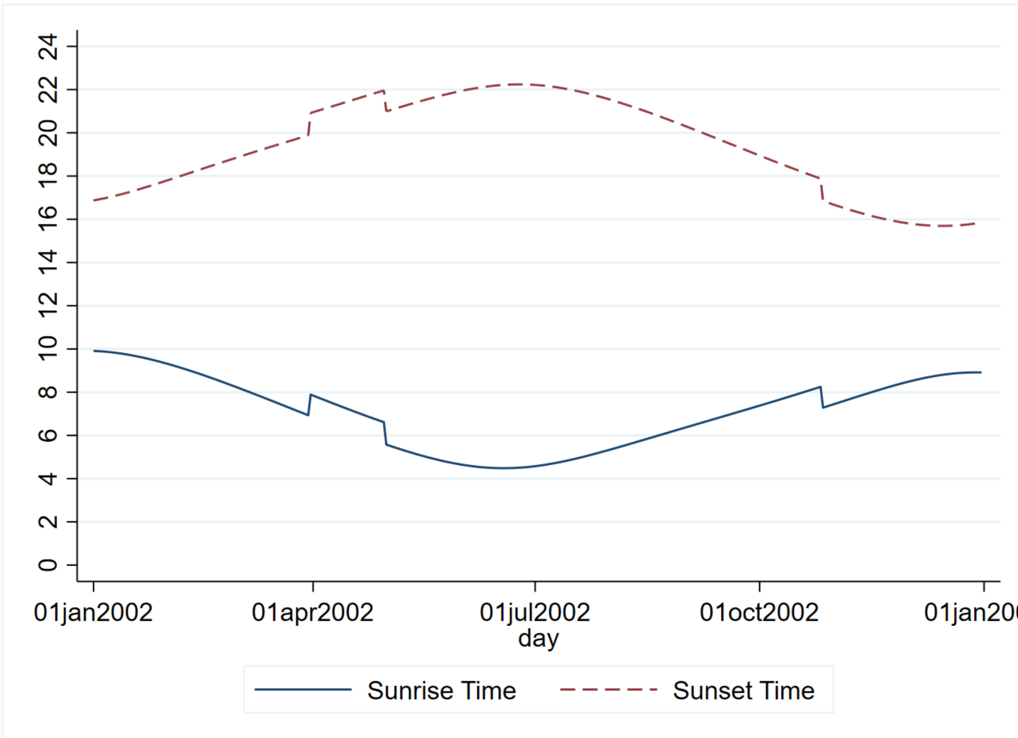
3 Time Reforms Effect on Crime

Data

I use the annual reports "Regions of Russia: Socioeconomic Outcomes" published by the Federal State Statistics Service (Rosstat). The annual reports cover a wide range of topics, aggregated on provincial level. The first report is from 2002 but it includes data also for 1990, 1995, and from 1998 on. I exclude Crimea and Sevastopol, annexed in 2014, and the remaining sample consists of 83 provinces. The incidence of robbery appears in this data for the 2001–2018 period, while the incidence of homicide appears for the 1990–2018 period.

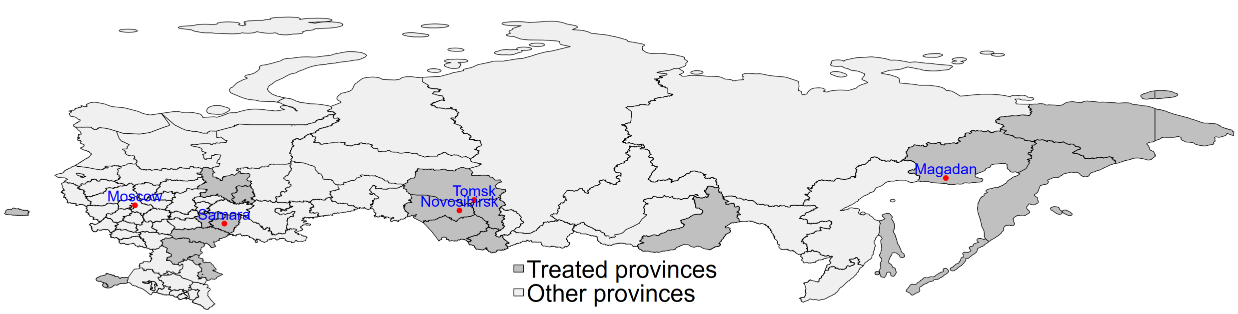
Table 2 presents the summary statistics, separately for untreated and treated provinces. The treated are 16 provinces that experienced a change in time difference from Moscow during the 2001–2018 period, for which I have robbery data (Kirov and Kaliningrad, shown in Figure 3, experienced such a change before 2001). The variables in the table are homicide and

Figure 1: Sunrise and sunset times in Tomsk in 2002



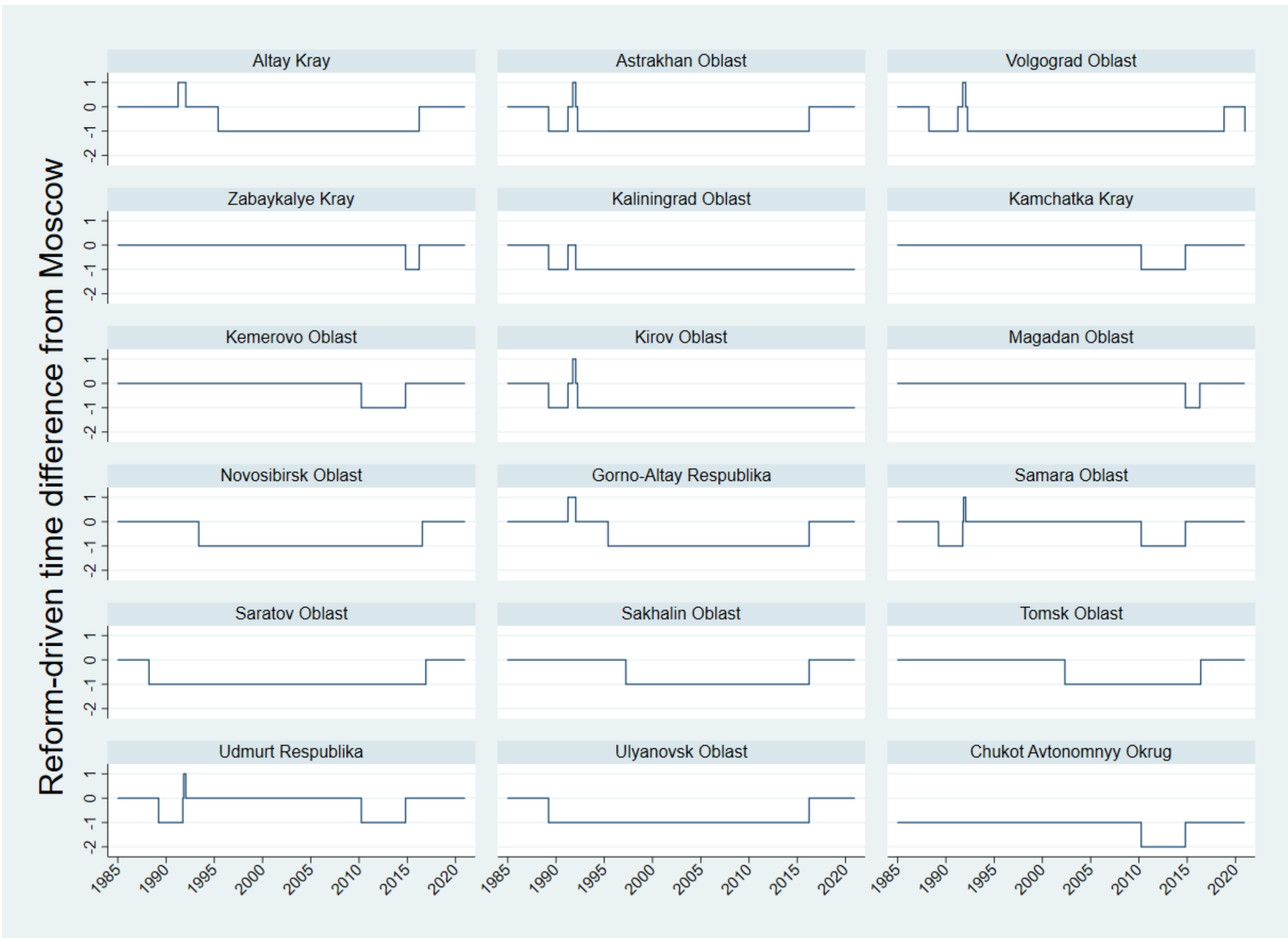
Note: The figure shows the sunrise and sunset times in Tomsk, a province capital in Western Siberia, in 2002. The discontinuities are, in chronological order, the transition to DST, common to all Russia, a time reform, special to Tomsk, and the transition back from DST.

Figure 2: Treated provinces



Note: The figure shows the provinces that experienced a change in time difference from Moscow since 1980. The evolution of time difference from Moscow in these provinces is shown in Figure 3. The map includes Crimea, annexed by Russia in March 2014. The annexation is not recognized internationally.

Figure 3: Change in time difference from Moscow in the treated provinces



Note: The figure shows change in time difference from Moscow in the treated provinces.

Table 1: Russian time reforms

Date	Affected provinces	Direction	Remarks
April 1, 1981	All	$\uparrow$	First DST transition
April 1, 1982	Chukotka	$\downarrow$	
March 27, 1988	Volgograd, Saratov	$\downarrow$	
March 26, 1989	6 provinces	$\downarrow$	
March 31, 1991	78 provinces	$\downarrow$	
October 30, 1991	Samara, Udmurtia	$\uparrow$	
January 19, 1992	75 provinces	$\uparrow$	
March 29, 1992	Astrakhan, Volgograd	$\downarrow$	
May 23, 1993	Novosibirsk	$\downarrow$	
May 28, 1995	Altai Krai, Altai Republic	$\downarrow$	
March 30, 1997	Sakhalin	$\downarrow$	
May 1, 2002	Tomsk	$\downarrow$	
March 28, 2010	5 provinces	$\downarrow$	The number of time zones decreases to 9
August 31, 2011	All	$\uparrow$	Elimination of DST
March 30, 2014	Crimea, Sevastopol	$\Uparrow$	
October 26, 2014	80 provinces	$\downarrow$ (78 provinces) $\Downarrow$ (2 provinces)	Restoration of 11 time zones
March 27 to December 4, 2016	10 provinces	$\uparrow$	
October 28, 2018	Volgograd	$\uparrow$	
December 27, 2020	Volgograd	$\downarrow$	

Note: The table lists time reforms in Russia since 1980. The signs $\uparrow$ and $\downarrow$ correspond to a shift of one hour, while $\Uparrow$ and $\Downarrow$ correspond to a shift of two hours. The number of affected provinces is given in accordance with the administrative division of Russia in 2020. Crimea was annexed by Russia in March 2014, but the annexation is not recognized internationally.

Table 2: Summary statistics of provincial data

Variable	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Obs.	Years
	Untreated provinces			*Treated provinces*			
ln (Homicide rate)	2.614	0.623	1,613	2.779	0.568	448	1990–2018
ln (Robbery rate)	4.225	1.020	1,170	4.549	0.694	324	2001–2018
ln (Alcohol sales)	1.945	0.695	1,304	2.069	0.344	378	1998–20018
ln (Personal income)	8.436	2.365	1,662	8.482	2.320	466	1990–2019
ln (GDP per capita)	11.356	1.419	1,502	11.477	1.361	432	1995–2018
ln (Population)	7.162	0.897	1,625	6.877	1.103	450	1990–2018
ln (Academic graduates rate)	1.561	0.691	1,580	1.644	0.593	428	1990–2018
ln (Restauran visits per capita)	0.356	2.497	1,361	0.436	2.431	378	1990–2018
Share of working-age population	0.594	0.036	1,690	0.609	0.041	468	1990–2019
Sex ratio	0.876	0.045	1,690	0.902	0.060	468	1990–2019

Note: The table presents summary statistics of province-level annual records of socio-economic indicators. Alcohol is per-capita annual sales of pure alcohol in liters, calculated from Rosstat data on alcohol sales. In order to calculate the pure alcohol, I weight the sales of different beverages by their alcohol content: 0.4 for vodka and cognac, 0.12 for wine and sparkling wine, and 0.05 for beer. Source of data: Rosstat "Regions of Russia" annual reports.

robbery rates per 100,000 of population, pure alcohol sales per capita, mean personal income and GDP per capita, rate of new academic graduates per 1,000 of population, restaurant visits per capita, share of population in working age,[6] and the sex ratio. The summary statistics show that the treated provinces are slightly more developed in terms of income and education but have higher homicide and robbery rates.

Baseline estimation

I start with a two-way fixed effects model, where the explanatory variables are leads and lags of time difference from Moscow. A similar model can be found in Hajdu and Hajdu (2021), who investigate the effect of temperature on fertility. For implementation of this approach with annual data, the treatment variable is averaged over the calendar year:

[6] Working age is 16–59 for men and 16–54 for women.

$$\bar{T}_{iy} = \sum_{t=1}^{365} T_{ity} \tag{1}$$

where T is time difference from Moscow in province (or location in individual data in Section 4) i on date t of year y.

The econometric model includes n leads and 10 lags of T:

$$ln(c_{iy}) = \sum_{k=-10}^{n} \beta_k \bar{T}_{i,y+k} + \gamma_i + \delta_y + X_{iy}\mu + \varepsilon_{iy} \tag{2}$$

where c is the crime indicator (robbery or homicide rate per 100,000 of population). The fixed effects are γ_i for the province and δ_y for the year. The residuals ε_{iy} are clustered on the provincial level. X_{iy} is a vector of control variables, which includes logged mean personal income and its square and logged GDP per capita and its square.

Note that this model is *not* a canonical "event study". The explanatory variables are the leads and lags of time difference from Moscow and not the dummies for the number of years until/since treatment. As Section 2 discusses and Table 1 and Figure 3 show, the multiple time reforms generate complex changes in time difference from Moscow. These changes are not simple "from 0 to 1" events. Some treated provinces shift the time up on some reforms and shift it down on other reforms. Therefore, the empirical setup here is not the canonical "staggered rollout" story, when the same treatment is given to different panel units in different years. Yet as a robustness check below, I limit the sample to provinces that experience a single monotonic change in time difference from Moscow, such that the setup fits the "staggered rollout" definition, and apply the Borusyak et al. (2021) robust estimator for event studies.

Back to the model in Equation (2), the number of leads is n. The role of the leads is to test for the correlation of the current outcome with future treatment. There is a trade-off between the number of leads and the sample size: leads erase the last years in data, because for these years the future time difference from Moscow is not yet known. In order to lose no data, I estimate the model with three leads at most.

Table 3 presents the estimation results for n=1, 2, and 3 for logs of robbery (columns 1–3) and homicide (columns 4–6) rates. The results show that leads are not related to the current robbery rate. In terms of identification, future reforms are not correlated with the current outcome. The most important results are the coefficients of lags. The first lag has a -0.114 log points effect (10.8%) on robbery when n=3 (-0.12 log points when n=1). The second lag has a -0.142 log points effect (13.2%) on robbery when n=3 (-0.129 log points when n=1). Both lags are statistically significant. From the third lag on, there is no statistically significant effect of past time reforms on robbery.

These results can be compared to the existing estimates from regression discontinuity studies that use DST transitions and explore their immediate effect on robbery. Tealde (2021) finds a decrease of 17% in robbery in Uruguay, Doleac and Sanders (2015) find a 7% effect in the U.S., Domínguez and Asahi (2019) document a reduction of 20% in major Chilean cities, and Munyo (2018) finds a 24% decrease in Montevideo. Therefore, my results fall in the range of existing estimates from regression discontinuity studies. However, my paper is the first one to show that the effect may persist for two years if the time shift is permanent.

I find no effect of time reforms on homicide (columns 4–6). The difference in the results for robbery and homicide is supportive evidence that the estimated coefficients are indeed the causal effect of time reforms. Plausibly, the effect on robbery *should* be stronger than on homicide. Robbery is more likely than homicide to be a result of random outside meetings between offenders and victims, and, therefore, is more sensitive to environmental conditions, such as ambient light. The fact that the results are different for robbery and homicide implies that the first and second lag effects on robbery are *not* a result of general crime trends in the treated provinces, correlated but not implied by time reforms.

Robustness checks and alternative models

Sensitivity analysis

I perform a set of sensitivity and robustness checks of the baseline results. First, I consider the sensitivity of the time reforms effect to exclusion from the sample of any treated province. Figure A.2 in the Appendix shows the first lag coefficient, when I estimate Equation (2) and exclude the 18 treated provinces one by one. Similarly, Figure A.3 shows the sensitivity of the second lag coefficient. The first bar in each figure depicts the coefficient in the full sample and

Table 3: Time reforms effect on robbery and homicide rates

	Robbery	Robbery	Robbery	Homicide	Homicide	Homicide
	(1)	(2)	(3)	(4)	(5)	(6)
Time (+3)			-0.0144			-0.0722
			(0.0834)			(0.0703)
Time (+2)		-0.0904	-0.0751		-0.0301	0.0465
		(0.0981)	(0.0752)		(0.0567)	(0.0588)
Time (+1)	-0.103	-0.00432	-0.00955	-0.0178	0.0135	-0.00983
	(0.0850)	(0.0836)	(0.0702)	(0.0546)	(0.0539)	(0.0534)
Time	-0.0140	-0.0419	-0.0396	0.0130	0.00521	0.0147
	(0.0523)	(0.0558)	(0.0514)	(0.0395)	(0.0366)	(0.0392)
Time (−1)	-0.120**	-0.113**	-0.114**	0.0126	0.0149	0.00947
	(0.0510)	(0.0525)	(0.0495)	(0.0387)	(0.0364)	(0.0375)
Time (−2)	-0.129*	-0.139*	-0.142**	-0.00967	-0.0129	-0.0230
	(0.0724)	(0.0712)	(0.0697)	(0.0471)	(0.0463)	(0.0479)
Time (−3)	0.0292	-0.0105	-0.00554	0.0318	0.0263	0.0407
	(0.101)	(0.114)	(0.116)	(0.0604)	(0.0635)	(0.0668)
Time (−4)	-0.0141	0.0677	0.0624	-0.0484	-0.0403	-0.0471
	(0.131)	(0.149)	(0.139)	(0.0855)	(0.0875)	(0.0897)
Time (−5)	-0.129	-0.187	-0.184	0.0110	0.00818	0.0102
	(0.171)	(0.135)	(0.146)	(0.0332)	(0.0319)	(0.0314)
Time (−6)	0.0702	0.0875	0.0852	-0.0310	-0.0307	-0.0344
	(0.134)	(0.126)	(0.132)	(0.0580)	(0.0579)	(0.0587)
Time (−7)	-0.0684	-0.0825	-0.0826	-0.0206	-0.0223	-0.0227
	(0.0984)	(0.106)	(0.106)	(0.0375)	(0.0375)	(0.0377)
Time (−8)	0.0299	0.0210	0.0234	-0.0267	-0.0278	-0.0268
	(0.126)	(0.122)	(0.130)	(0.0193)	(0.0189)	(0.0191)
Time (−9)	-0.0431	-0.0274	-0.0297	0.00232	0.00296	0.00168
	(0.199)	(0.192)	(0.200)	(0.0480)	(0.0474)	(0.0479)
Time (−10)	-0.0512	-0.0632	-0.0644	-0.0330	-0.0351	-0.0403
	(0.112)	(0.107)	(0.104)	(0.0492)	(0.0469)	(0.0453)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,455	1,455	1,455	1,929	1,929	1,929
Number of provinces	83	83	83	83	83	83

Notes: The table presents the results of estimation of Equation (2) with standard errors clustered on province level. *Time* is time difference from Moscow. Control variables include log of GDP per capita, its square, log of mean personal income, and its square. The dependent variables are log of annual robbery and homicide cases per 100,000 of population. *** p<0.01, ** p<0.05, * p<0.1.

corresponds to the baseline results in column 3 of Table 3. Other bars show the coefficient when one treated province is excluded from the sample. The conclusion from the graphs is that exclusion of no province erases the treatment effect one and two years after treatment. The magnitude of the first lag is always around -0.1 log points, and the magnitude of the second lag is always between -0.1 and -0.2 log points. This sensitivity analysis is important for the exclusion restriction: the sign and the magnitude of the coefficients is robust to exclusion of any treated province. Therefore, the treatment effect on robbery is not driven by any local trend correlated with treatment.

Effect monotonicity

Second, I address the fact that some reforms shifted the time in some provinces by two hours. This feature of Russian time reforms may be used to test for monotonicity of their effect. The question here is whether the effect of a two-hour shift has the same sign and is stronger than that of a one-hour shift. I round the average annual time difference from Moscow and estimate a model where the treatment variables are a set of dummies for the rounded reform-driven time difference from Moscow.

The model is

$$ln(c_{iy}) = \alpha_1 T^1_{iy} + \alpha_2 T^2_{iy} + \hat{\gamma}_i + \hat{\delta}_y + X_{iy}\hat{\mu} + \hat{\varepsilon}_{iy} \tag{3}$$

where T_1 and T_2 are, respectively, dummy variables for one-hour and two-hour time differences from Moscow (after rounding).

Table 4 shows the results for robbery and homicide. A one-hour change in time difference from Moscow is associated with a 0.21–0.23 log points decrease in robbery rate (column 1 without controls, column 2 with controls), while a two-hour change is associated with a 0.32–0.34 log points decrease. Again, there is no effect on homicide (columns 3 and 4).

Event analysis

Finally, I challenge the baseline two-way fixed effects model. The recent boost in econometric theory revises the common practice of difference-in-differences estimation of treatment effects

Table 4: Time reforms effect on robbery and homicide rates: ordinal treatment variable

	Robbery	Robbery	Homicide	Homicide
	(1)	(2)	(3)	(4)
Treatment = 1	-0.230***	-0.210***	0.009	0.019
	(0.078)	(0.078)	(0.053)	(0.048)
Treatment = 2	-0.342***	-0.322***	0.014	0.047
	(0.108)	(0.110)	(0.091)	(0.101)
Controls	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes
Observations	1,494	1,455	2,061	1,929
Number of provinces	83	83	83	83

Notes: The table presents the results of estimation of Equation (3) with standard errors clustered on province level. *Treatment* are two dummy variables, indicating the values of a rounded time difference from Moscow. Control variables include log of GDP per capita, its square, log of mean personal income, and its square. The dependent variables are log of annual robbery and homicide cases per 100,000 of population. *** p<0.01, ** p<0.05, * p<0.1.

(Athey and Imbens, 2021; Borusyak et al., 2021; Callaway and Sant'Anna, 2020; De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2020). This literature points out that the problem with the difference-in-differences approach when treatment is given to different panel units at different time is that units treated early serve as control units for those treated later. The early-treated units are affected by treatment, and, therefore, are not proper control units. The difference-in-differences estimator is a weighted mean of outcome differences between all pairs of units (Goodman-Bacon, 2021). The differences between early-treated and later-treated units may have negative weights, leading to a bias of the estimated treatment effect. The above-mentioned recent studies propose different alternative estimators that should correct the bias.

The setup that I analyze falls in the category of cases that this literature addresses, because of the multiple time reforms. However, the problem is not acute: only 16 out of 83 provinces experience variation in time difference from Moscow during the 2001–2018 period, for which I hold the robbery data. Therefore, vast majority of comparisons are between treated and never-treated units, and only a small number of comparisons is between later-treated and earlier-treated units. Yet I test the robustness of the two-way fixed effect model by applying the Borusyak et al. (2021) estimator for event studies. This estimator is the most recent contribution that outperforms the solutions proposed in De Chaisemartin and d'Haultfoeuille (2020), Sun and Abraham (2020), and Callaway and Sant'Anna (2020), and is a unique efficient linear unbiased estimator under some assumptions. The estimator is currently implemented for settings with a binary monotonic treatment status, where in each period treatment can either switch from zero to one or remain zero. Therefore, I exclude from the sample eight provinces with non-monotonic time difference from Moscow during the 2001–2018 period. The remaining data consists of 75 provinces, eight if which are treated. The event is a shift to a one-hour later time. All these reforms took place in the 2016–2018 period, while my data ends in 2018, such that I can only test for the reform effect up to two years forward. I can now apply the Borusyak et al. (2021) estimator (hereafter BJS) for the event study model, where the explanatory variables are dummies for the number of years until/since the event:

$$ln(c_{iy}) = \sum_{h=-m}^{2} \tau_k \mathbb{1}[K_{iy} = h] + \tilde{\gamma}_i + \tilde{\delta}_y + \tilde{\varepsilon}_{iy} \tag{4}$$

where K is the number of years since the event. Table 5 presents the results for m= 3, 4, and 5. The BJS estimator confirms the baseline results: a one-hour-forward time shift leads to a 0.101 (10.6%) log points decrease in robbery in a one-year and to a 0.189 log points (20.8%) decrease in a two-year perspective. The latter effect is statistically significant. The coefficient for the second lag is slightly higher than in the baseline estimation, suggesting that the negative weights problem in the difference-in-differences model indeed generated some toward-zero bias. However, this bias is not large, consistently with being the number of "forbidden comparisons" (comparisons with a negative weight) in the full data set small.

4 Time Reforms Effect on Fear of Crime

Data

For assessment of fear of crime and related behavioral outcomes, I utilize the Russian Longitudinal Monitoring Survey (RLMS) by Higher School of Economics (1994–2019). It is the main if not the only data set to be used for investigation of individual and household outcomes across Russia. The participants are individuals from 161 settlements in 40 locations in 33 provinces. So far, RLMS collected 372,000 individual and 138,000 household observations. The overall number of individuals that have been investigated is 57,000, of whom 45,000 were at least 18 years old at the time of at least one interview. Data is always collected between September and March, and in most of the waves it is collected between October and December. I restrict the sample to respondents at least 18 years old. For the purpose of clustering the standard errors by settlement, I exclude the few individuals who moved between RLMS settlements during the survey.

The map in Figure 4 shows the 40 locations. Empty circles indicate the untreated locations, full squares indicate 14 settlements in four treated locations in Volga region in the European Russia, and full diamonds indicate 14 settlements in four treated locations in Western Siberia in the Asian Russia.

Table 5: Event analysis: Borusyak, Jaravel, and Spiess (2021) estimator

	Robbery	Robbery	Robbery
	(1)	(2)	(3)
τ = -5			0.002
			(0.056)
τ = -4		-0.019	-0.019
		(0.072)	(0.078)
τ = -3	0.043	0.041	0.041
	(0.069)	(0.075)	(0.079)
τ = -2	0.046	0.045	0.045
	(0.079)	(0.085)	(0.089)
τ = -1	-0.032	-0.034	-0.033
	(0.088)	(0.094)	(0.098)
τ = 0	-0.036	-0.036	-0.036
	(0.067)	(0.067)	(0.067)
τ = 1	-0.101	-0.101	-0.101
	(0.074)	(0.074)	(0.074)
τ = 2	-0.189**	-0.189**	-0.189**
	(0.092)	(0.092)	(0.092)
Observations	1,350	1,350	1,350
Number of provinces	83	83	83

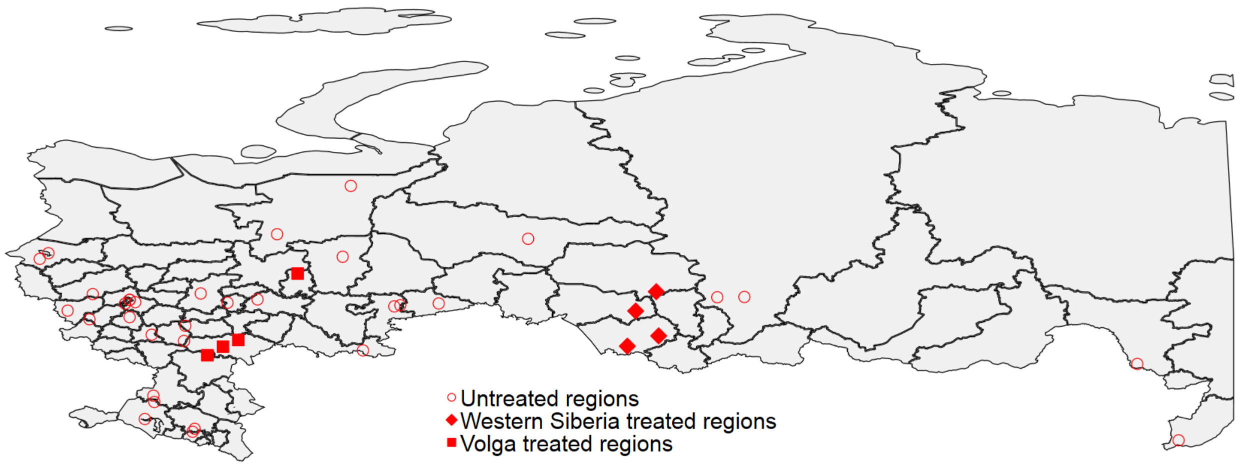
Notes: The table presents the results of BJS estimation of Equation (4) with standard errors clustered on province level. The variables τ represent the number of years since event. The event is a shift of time difference from Moscow one hour up. The model includes province and year fixed effects. The dependent variable is log of annual robbery cases per 100,000 of population. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Summary statistics of individual data

Variable	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Obs.	Years
					A: Men					
	Untreated locations			*Treated European locations*			*Treated Asian locations*			
Age	43.023	16.487	97,780	43.428	15.890	12,033	43.961	16.849	10,291	1994–2019
Safe walk	0.774	0.419	18,672	0.75	0.433	2,278	0.830	0.375	2,053	2009-2017
ln (Walking time)	4.818	0.89	30,475	4.991	0.867	3,961	4.941	0.965	3,147	1995-2012
ln (Sleeping)	8.036	0.203	11,135	8.044	0.196	1,333	8.038	0.192	963	1994-1998
ln (Alc. intake)	4.515	0.837	39,736	4.599	0.828	5,198	4.623	0.828	4,515	2006-2019
ln (Daily avg.)	2.175	1.169	38,627	2.185	1.181	5,104	2.226	1.174	4,413	2006-2019
					B: Women					
	Untreated locations			*Treated European locations*			*Treated Asian locations*			
Age	47.577	18.484	134,965	47.567	17.833	16,161	47.387	18.490	14,123	1994–2019
Safe walk	0.572	0.495	25,424	0.571	0.495	3,062	0.609	0.488	2,805	2009-2017
ln (Walking time)	4.757	0.842	41,901	4.845	0.847	5,331	4.87	0.939	4,335	1995-2012
ln (Sleeping)	8.024	0.213	14,765	8.021	0.198	1,743	7.992	0.227	1,297	1994-1998
ln (Alc. intake)	3.767	0.820	37,008	3.764	0.830	4,455	3.845	0.874	4,395	2006-2019
ln (Daily avg.)	0.988	1.104	34,533	0.920	1.119	4,251	1.049	1.178	4,149	2006-2019
					C: Sunrise and sunset times					
	Untreated locations			*Treated European locations*			*Treated Asian locations*			
Sunrise time	8.000	0.841	232,698	7.499	0.588	28,188	8.039	0.663	24,411	1994–2019
Sunset time	17.309	1.155	232,698	17.115	0.851	28,188	17.850	0.882	24,411	1994–2019

Note: The table presents the summary statistics of the relevant RLMS indicators. The "safe walk" variable is a binary representation of the answer to the question on feeling safe to walk in darkness. The value is one when the respondent reports feeling fully or relatively safe and zero when the respondent reports feeling not safe or absolutely not safe. The alcohol intake and daily average, are, respectively, the log of monthly alcohol consumption and the log of mean daily alcohol consumption on the days when the individuals drinks. The alcohol consumption is weighted by alcohol content: 0.4 for vodka and cognac, 0.12 for wine and sparkling wine, and 0.05 for beer. The sunrise and sunset time are average over the RLMS observations. The treated locations have a non-zero variance in time difference from Moscow: 14 settlements in four locations in the Volga region (European treated locations) and 14 settlements in four locations in Western Siberia (Asian treated locations). All other RLMS locations are labeled as untreated. The sample consists of all RLMS respondents at least 18 years old at the time of the interview.

Figure 4: RLMS locations



Note: The map shows the 40 locations (with 161 settlements), where RLMS data is collected. The empty circles are locations with zero variation in time difference from Moscow. The full squares are 4 sites with 14 locations in the Volga region with a non-zero variation in the treatment variable, and the full diamonds are are 4 sites with 14 locations in Western Siberia with a non-zero variation in the treatment variable. The map includes Crimea, annexed by Russia in March 2014. The annexation is not recognized internationally.

Estimation

I use the RLMS data to study the effect of time reforms on fear of crime and on time use that may be related or complementary to outside activity, i.e., walking and sleep. Table 6 shows the summary statistics of age, perceived safety to walk in darkness (represented binary for the purpose of summary statistics), walking and sleep times, and alcohol consumption (total monthly alcohol intake and average intake on the days of drinking). The summary statistics are presented separately for men and women and also distinguish between untreated, treated European (Volga region), and treated Asian (Western Siberia) locations. The untreated and the treated European and Asian locations show similar mean indicators, echoing the cultural homogeneity in most of Russia, mentioned in the beginning of Section 2. In the treated Asian locations the perceived safety of men is 5–8 pp stronger than in other locations. For women, the difference is only 3 pp. There is no difference between perceived safety in the untreated and treated European locations. In addition, Panel C of Table 6 presents the average sunrise and sunset times over the RLMS observations. The untreated and the treated Asian locations have similar sunrise times, while in the treated European locations the sunrise is half an hour earlier. Oppositely, untreated and treated European locations have similar sunset times, while in the treated Asian locations the sunset is half an hour later than in the untreated and forty minutes later than in the treated European locations.

The analysis of fear of crime consists of estimation of an ordered mixed-effect logit regressions, where the dependent variable is the answer to the following RLMS question:

Imagine that you are walking alone in darkness in your neighborhood of residence. How safe do you feel in such a situation?

The four alternative answers to this question are: (i) *Fully safe*, (ii) *Relatively safe*, (iii) *Not safe*, and (iv) *Absolutely not safe*.

The lags-and-leads model in Equation (2), applied in Section 3 for the aggregate annual data, is a poor fit for survey data. RLMS interviews its respondents on arbitrary dates, and there are no clear "lags" and "leads" with respect to their responses. Therefore, I adopt the approach implemented recently by Bento et al. (2020), who estimate the economic impact of climate change. They distinguish between weather, which is the temperature on a particular day, and climate, which is the average temperature over a long preceding period of time. Both variables are included in the same model, and Bento et al. (2020) interpret the long-run coefficient as a measure of adaptation to the climate change. I consider this model as an adequate fit for analysis of perception and behavior in the context of environmental changes.

I estimate the following mixed-effects model:

$$ln(\frac{p_{ijymd}(a)}{1-p_{ijymd}(a)}) = \theta_1 T_{jymd} + \theta_2 \bar{T}^{(k)}_{jymd} + \theta_3 \bar{T}^{(k\ to\ 10)}_{jymd} + s_j + \check{\gamma}_y + \check{\delta}_m + \nu_{ij} + \check{\varepsilon}_{ijymd} \quad (5)$$

where $p_{ijymd}(a)$ is the probability of individual i who lives in settlement j to give answer a to the question above, when asked on day d of month m in year y. T is the reform-driven time difference from Moscow on the day of interview.[7] The variables $\bar{T}^{(k)}$ and $\bar{T}^{(k\ to\ 10)}$ account, respectively, for the average time difference from Moscow over the past k and k to 10 years preceding the interview. For instance, if the interview took place on January 1, 2015, and k=5, then the variable $\bar{T}^{(5)}$ is the average time difference from Moscow from January 1, 2010, to December 31, 2014, while the variable $\bar{T}^{(5\ to\ 10)}$ is the average over the period from January

[7] By "reform-driven", I mean that the initial time difference from Moscow is normalized to zero.

1, 2005 to December 31, 2009. The coefficients of the variables T, $\bar{T}^{(k)}$, and $\bar{T}^{(k\ to\ 10)}$ may be, respectively, interpreted as the short-run, long-run, and permanent (or very long run) effects of the time reforms. The fixed effects are s_j for settlement, $\check{\gamma}_y$ for survey wave, and $\check{\delta}_m$ for calendar month. The individual random effect is ν_{ij}, and the residuals $\check{\varepsilon}_{ijymd}$ are clustered on the settlement level. For analysis of continuous indicators (walking and sleep), I estimate a linear version of Equation (5), where the dependent variable is $ln(indicator_{ijymd})$.

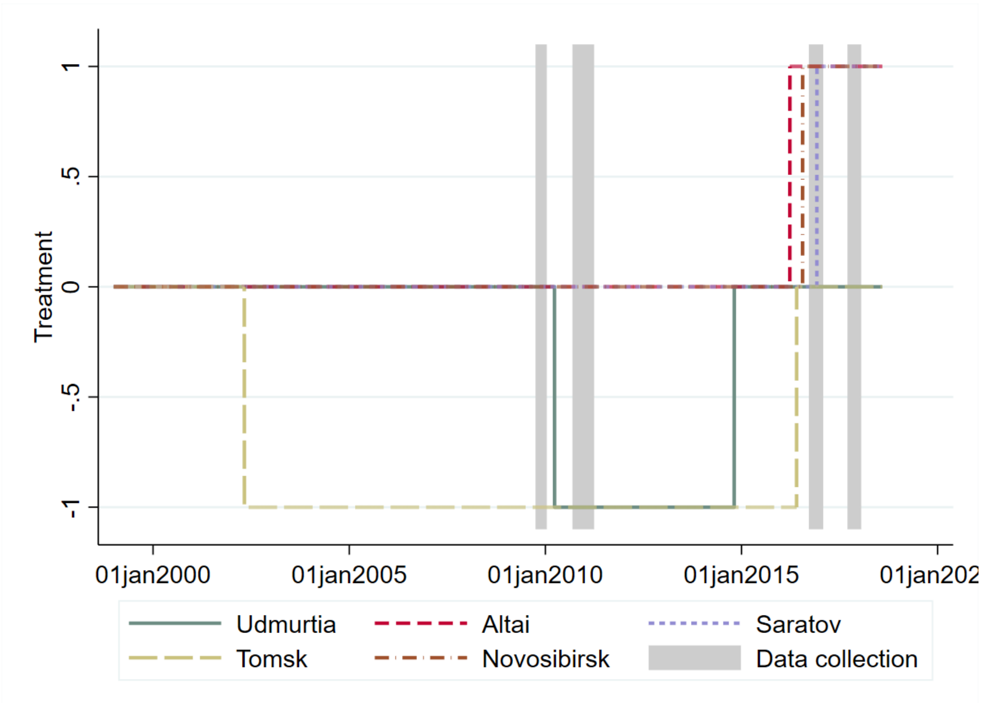
The "feeling safe to walk" indicator was inquired by RLMS in the 2009, 2010, 2016, and 2017 waves. Figure 5 shows the normalized time difference from Moscow at the treated RLMS locations during this period. The gray shadows indicate the periods of "feeling safe to walk" data collection. The gaps between the shadowed areas indicate either the waves when the question was not included in the questionnaire or the seasons between the survey waves. In Udmurtia (Volga region), T changes from zero to -1 in 2010 and back to zero in 2014. In Saratov (also in Volga region), it changes from zero to one during the 2016 wave. In Altai and Novosibirsk (in Western Siberia), T changes from zero to one before the 2016 wave, and in Tomsk (also in Western Siberia), it changes from zero to -1 in 2002 and back to zero before the 2016 wave.

Feeling safe to walk

Table 7 presents the average marginal effects of mixed-effect ordered logit regressions, separately for women and men. Equation (5) is estimated for k=2 (panel A) and k=6 (panel B). The results show a positive *permanent* effect of time reforms on women's safety perception. The propensity to feel fully and relatively safe to walk in darkness increase by 7–8 pp each as a function of reform-driven time difference from Moscow. The propensity to feel not safe decreases by 6–8 pp, and the propensity to feel absolutely not safe decreases by 7–9 pp. For men, only the first two post-reform years matter, corresponding to the actual decrease in robbery, shown in Section 3. The propensity to feel very safe increases by 16 pp at the expense of the propensity to feel relatively safe (a 2.5 pp decrease), the propensity to feel not safe (a 9 pp decrease), and the propensity to feel absolutely not safe (a 5 pp decrease).

For the reasons discussed in Section 2 and as shown on the map in Figure 4, RLMS locations include two treated regions: 14 settlements in four locations on the Volga river banks

Figure 5: Change in time difference from Moscow in the treated RLMS locations



Note: The figure shows the change in time difference from Moscow in RLMS locations that were treated during collection of the "feel safe to walk in darkness" indicator. The legend reports the names of the provinces, where the relevant RLMS locations are located. Udmurtia and Saratov are located in Volga region, while Altai, Tomsk, and Novosibirsk are located in Western Siberia. The shadowed regions show the dates when data on the indicator "feel safe to walk in darkness" was collected.

in Europe and 14 settlements in four locations in Western Siberia in Asia. The summary statistics in Table 6 show that the two regions are quite similar with respect to most indicators, but individuals feel safer to walk in darkness in Western Siberia rather than in Volga region. Moreover, the sunset time in Western Siberia is forty minutes later than in Volga region.

Do these differences between the two groups of treated locations matter for the time reforms effect? Table 8 reports the average marginal effects from mixed-effect ordered logit regressions, where the estimation is separate for the treated European plus all untreated and the treated Asian plus all untreated locations. The reforms effect in the first two post-reform years is statistically significant and of comparable magnitude in both regions. The propensity to feel fully or relatively safe increases by 16.4 pp in the European location and by 11.8 pp in the Asian ones. The effect in the long run (3–10 years after the reform) is even closer in the two regions, but its statistical significance in lower (significant at 10% in Europe, not significant in Asia). Overall, the results in Europe and Asia are similar and evident of external validity of this paper's analysis.

Behavioral effects

Walking

So far, the analysis documents the permanent effect of time reforms on the reported women's feeling safe to walk in darkness. The next step is to investigate whether the actual walking is affected.[8] RLMS includes the following question:

> *Taking into account all your commuting during a regular day - to work or studies and back, to the shops or for other needs, how much do you walk by average every day? Do not include walking* (for pleasure or exercise).

This RLMS question excludes recreational walking, which is a limitation on the collected indicator, but keeps walking for daily needs, which is still a valuable and relevant piece of data. The summary statistics of the log of average walking time in minutes appear in Table 6. The logged value is close to 5 and is similar in the untreated, treated European, and

[8]See also Wolff and Makino (2012) for the behavioral effects of DST transitions.

Table 7: Time reforms effect on feeling safe to walk

	Women				Men			
	(1)				(2)			
	Effect on feeling...				Effect on feeling...			
	Fully safe	Relatively safe	Not safe	Absolutely not safe	Fully safe	Relatively safe	Not safe	Absolutely not safe
				A: Model 1				
Time	-0.005	-0.005	0.004	0.005	-.034	.005	.019	.010
	(0.017)	(0.017)	(0.015)	(0.018)	(.034)	(.005)	(.019)	(.010)
Time (2)	0.066***	0.066***	-0.061***	-0.071***	.157***	-.025***	-.088***	.044***
	(0.027)	(0.027)	(0.025)	(0.029)	(.049)	(.008)	(.028)	(.015)
Time (2–10)	0.084***	0.084***	-0.077***	-0.090***	.019	-.003	-.011	-.005
	(0.025)	(0.025)	(0.023)	(0.027)	(.054)	(.009)	(.031)	(.015)
				B: Model 2				
Time	0.017	0.017	-0.016	-0.018	0.014	-0.002	-0.008	-0.004
	(0.011)	(0.011)	(0.010)	(0.012)	(0.028)	(0.004)	(0.016)	(0.008)
Time (6)	0.065***	0.065***	-0.060***	-0.070***	0.119	-0.019	-0.067	-0.033
	(0.022)	(0.022)	(0.020)	(0.024)	(0.080)	(0.013)	(0.045)	(0.023)
Time (6–10)	0.072***	0.072***	-0.066***	-0.077***	-0.056	0.009	0.031	0.016
	(0.016)	(0.017)	(0.016)	(0.018)	(0.055)	(0.009)	(0.031)	(0.016)
Observations	31,328				23,036			

Notes: The table presents marginal effects of mixed-effects multinomial logit regressions of Equation (5) using RLMS data. The results are of a single regression for women and a single regression for men, and the marginal effects are on the probabilities to give each of the possible answers to the question "Do you feel safe to walk in darkness in your neighborhood of residence?" The explanatory variables represent time difference from Moscow on the day of interview and its average over earlier periods. For a detailed explanation, see the text following Equation (5). The sample consists of individuals at least 18 years old. The fixed effects are of location, year, and month of the year, and the random effects are of individuals. The standard errors are clustered by settlement. ***p<0.01, **p<0.05, *p<0.1.

Table 8: Regional heterogeneity of the time reforms effect on feeling safe to walk

	Untreated + treated European provinces				Untreated + treated Asian provinces			
	(1)				(2)			
	Effect on feeling...				Effect on feeling...			
	Fully safe	Relatively safe	Not safe	Absolutely not safe	Fully safe	Relatively safe	Not safe	Absolutely not safe
Time	-0.027	0.009	0.019	0.016	-0.006	-0.002	0.004	0.004
	(0.039)	(0.013)	(0.028)	(0.024)	(0.032)	(0.009)	(0.022)	(0.019)
Time (2)	0.124**	0.040**	-0.088**	-0.076**	0.091**	0.027**	-0.063**	-0.055**
	(0.056)	(0.018)	(0.040)	(0.034)	(0.038)	(0.011)	(0.026)	(0.023)
Time (2–10)	0.066*	0.021*	-0.047*	-0.041*	0.073	0.021	-0.051	-0.044
	(0.038)	(0.012)	(0.027)	(0.024)	(0.084)	(0.025)	(0.059)	(0.051)
Observations	49,506				49,022			

Notes: The table presents marginal effects of mixed-effects multinomial logit regressions of Equation (5) using RLMS data. The results are of a single regression for women and a single regression for men, and the marginal effects are on the probabilities to give each of the possible answers to the question "Do you feel safe to walk in darkness in your neighborhood of residence?" The explanatory variables represent time difference from Moscow on the day of interview and its average over earlier periods. For a detailed explanation, see the text following Equation (5). The sample consists of individuals at least 18 years old. The "treated European" are 14 settlements in four treated locations in Volga region, and the "treated Asian" are 14 settlements in four treated locations in Western Siberia. The untreated are all other RLMS locations. The fixed effects are of location, year, and month of the year, and the random effects are of individuals. The standard errors are clustered by settlement. ***p<0.01, **p<0.05, *p<0.1.

Table 9: Time reforms effect on walking time

	All			Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Time	0.0825**	0.128**	0.215**	0.0872*	0.0841	0.174**	0.0804**	0.192**	0.276**
	(0.0366)	(0.0590)	(0.0873)	(0.0463)	(0.0539)	(0.0741)	(0.0327)	(0.0862)	(0.120)
Time (2)		-0.0522	-0.180		0.00361	-0.127		-0.128	-0.250*
		(0.0759)	(0.121)		(0.0649)	(0.109)		(0.0995)	(0.143)
Time (2--10)			0.143			0.147			0.135
			(0.0952)			(0.0919)			(0.104)
ln(Population)	0.0179*	0.0180*	0.0185*	0.0143**	0.0143**	0.0148**	0.0229	0.0233	0.0236
	(0.00971)	(0.00971)	(0.00995)	(0.00716)	(0.00714)	(0.00742)	(0.0154)	(0.0154)	(0.0156)
Observations	89,446	89,446	89,446	51,742	51,742	51,742	37,704	37,704	37,704
Number of ind.	28,489	28,489	28,489	15,972	15,972	15,972	12,517	12,517	12,517

Notes: The table presents marginal effects of mixed-effects linear regressions of Equation (5) using RLMS data. The dependent variable is log of daily walking in minutes. The explanatory variables represent time difference from Moscow on the day of interview and its average over earlier periods. For a detailed explanation, see the text following Equation (5). The sample consists of individuals at least 18 years old. The fixed effects are of location, year, and month of the year, and the random effects are of individuals. The standard errors are clustered by settlement. ***p<0.01, **p<0.05, *p<0.1.

treated Asian locations. Women walk 6% less than men in the untreated locations, 15% less in the treated European, and 7% less in the treated Asian locations.

I estimate a mixed-effect linear regression, where the dependent variable is log of walking time in minutes and the right hand side looks similarly to Equation (5). I add a control for the logged size of the settlement population, because larger settlements offer more recreational activities and longer walking distances. Table 9 presents the results of mixed-effect linear regressions for the full sample of adults (columns 1–3), women (columns 4–6), and men (columns 7–9). The results in all columns show that time difference from Moscow on the interview day is associated with a strong increase in walking of up to 0.17 log points for women and up to 0.28 log points for men. However, the regressions show no long-run effects.

Sleep

Finally, RLMS data allows to estimate the treatment effect on sleep. Although sleep is not directly related to fear of crime, it is related to the behavioral changes as a a result of a change in fear of crime. Sleep is the most time-consuming single activity. As complementary to other activities, it may be affected by any changes in the lifestyle, such as longer outside activity. Therefore, it may be indirectly related to changes in fear of crime. If individuals feel safe to walk outside at late hours, they may prefer outside activities to sleep. However, from the side of criminals, shorter sleep may increase crime, because sleep affects emotions and, in particular, the level of aggressiveness (Gibson and Shrader, 2018, Umbach et al., 2017). Sleep attracts attention of economists also because of its effect on productivity, health, and well-being (Giuntella and Mazzonna, 2019, Giuntella et al., 2017, Kuehnle and Wunder, 2016, Jin and Ziebarth, 2015a,b, Toro et al., 2015 Kountouris and Remoundou, 2014, Hamermesh et al., 2008, Kamstra et al., 2000).

RLMS monitored duration of sleep during the 1994–1998 waves, directly covering the time reforms of 1995 in Altai, located in Western Siberia, but also the long shadows of the many 1989–1991 reforms in both Volga region and Western Siberia. According to the summary statistics, shown in Table 6, women sleep 1% less than men. The difference between treated and untreated locations is below 1% for men and for women is the treated European locations, while women in the treated Asian locations sleep 3% less.

Table 10 presents the results of mixed-effects linear regressions for the log of sleep duration. The specification is identical to the one in walking analysis above. Columns 1–3 report the results for the full sample of adults, columns 4–6 report the results for women, and columns 7–9 report the results for men. In the two post-reform years (but not in a longer perspective), sleep decreases by 3% (around 12 minutes), indicating that other activities indeed receive more time. This result is similar to the finding of Giuntella and Mazzonna (2019) in the U.S. that one extra hour of natural light in the evening decreases sleep by 19 minutes. The effect on women is half as strong as on men (2% for women versus 4% for men).

Table 10: Time reforms effect on sleeping time

	All			Women			Men		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Time	-0.0128	0.00352	0.00532	-0.0206**	-0.0105	-0.00935	-0.00206	0.0222	0.0249
	(0.00786)	(0.0127)	(0.0133)	(0.00945)	(0.0112)	(0.0123)	(0.0128)	(0.0219)	(0.0221)
Time (2)		-0.0257**	-0.0300**		-0.0160*	-0.0188*		-0.0380**	-0.0446**
		(0.0103)	(0.0117)		(0.00839)	(0.0109)		(0.0183)	(0.0187)
Time (2--10)			0.0236			0.0153			0.0359
			(0.0238)			(0.0342)			(0.0317)
ln(Population)	-0.0492	-0.0485	-0.0491	-0.0417	-0.0412	-0.0415	-0.0602	-0.0590	-0.0604
	(0.0463)	(0.0463)	(0.0465)	(0.0510)	(0.0510)	(0.0512)	(0.0619)	(0.0620)	(0.0618)
Observations	31,192	31,192	31,192	17,776	17,776	17,776	13,416	13,416	13,416
Number of ind.	12,133	12,133	12,133	6,718	6,718	6,718	5,415	5,415	5,415

Notes: The table presents marginal effects of mixed-effects linear regressions of Equation (5) using RLMS data. The dependent variable is log of sleeping time in minutes. The explanatory variables represent time difference from Moscow on the day of interview and its average over earlier periods. For a detailed explanation, see the text following Equation (5). The sample consists of individuals at least 18 years old. The fixed effects are of location, year, and month of the year, and the random effects are of individuals. The standard errors are clustered by settlement. ***p<0.01, **p<0.05, *p<0.1.

5 Conclusions

This study investigates not only temporary but also permanent effect of time institutions on crime, fear of crime, and related behavior. I exploit a long series of time reforms in many Russian regions, located far away from each other. Arguably, the results derived in this paper have stronger external validity than results from the previous literature, due to the multiple time reforms that span decades and thousands of kilometers.

I corroborate recent findings from investigation of DST transitions and find a 11%–13% decrease in robbery one and two years after the clocks are set one hour forward. I add to this literature by showing that in the longer perspective, there is no effect on robbery. Furthermore, analysis of longitudinal survey data shows that even though the effect on robbery lasts two years, women increase in a 10-year perspective the propensity to feel safe while walking in darkness. For men, the time reforms effect on feeling safe to walk in darkness lasts as long as the actual effect on robbery. Moreover, I find that both genders indeed walk more but only in the short run after a reform. I also document that in the long run (but not permanently), men and women slightly decrease their sleep, indicating more time spent on other activities when the sunset is one hour later.

The main novelty in this paper is the finding that robbery decreases in a two year perspective, but women's reported feeling safe to walk improves permanently. What can explain it? A possible explanation is that beliefs about safety change slowly and persist after the actual robbery rate returns to the pre-reform level. In addition, a possible biological explanation of the decreased fear of crime is the emotional effect of additional daylight in the evening. But the most intriguing is the fact that women feel safer to walk in darkness. A shift of the time zone boundary may postpone the sunset but it cannot change the nature of darkness. Therefore, when the sunset is late, women feel safer *in general*. The conclusion and the policy implication of this finding is that environmental factors, in particular ambient light, have permanent behavioral effects, which may survive evaporation of some underlying mechanisms, such as decrease in robbery in this case.

Compliance with Ethical Standards: The author declares that he has no conflict of interest.

References

Amorim, R., V. Molina-Moreno, and A. Peña-García (2016): "Proposal for sustainable dynamic lighting in sport facilities to decrease violence among spectators," *Sustainability*, 8, 1298.

Anderson, D. A. et al. (2012): *The cost of crime*, Citeseer.

Arvate, P., F. O. Falsete, F. G. Ribeiro, and A. P. Souza (2018): "Lighting and homicides: evaluating the effect of an electrification policy in rural brazil on violent crime reduction," *Journal of Quantitative Criminology*, 34, 1047–1078.

Athey, S. and G. W. Imbens (2021): "Design-based analysis in difference-in-differences settings with staggered adoption," *Journal of Econometrics*.

Ayres, I. and S. D. Levitt (1998): "Measuring positive externalities from unobservable victim precaution: an empirical analysis of Lojack," *The Quarterly Journal of Economics*, 113, 43–77.

Azmitia, E. C. (2020): "Evolution of serotonin: sunlight to suicide," in *Handbook of behavioral neuroscience*, Elsevier, vol. 31, 3–22.

Baysan, C., M. Burke, F. González, S. Hsiang, and E. Miguel (2019): "Non-economic factors in violence: Evidence from organized crime, suicides and climate in Mexico," *Journal of Economic Behavior & Organization*, 168, 434–452.

Becker, G. and Y. Rubinstein (2011): "Fear and the Response to Terrorism: An Economic Analysis," Tech. rep., Centre for Economic Performance, LSE.

Becker, G. S. (1968): "Crime and punishment: An economic approach," in *The Economic Dimensions of Crime*, Springer, 13–68.

Bento, A., N. S. Miller, M. Mookerjee, and E. R. Severnini (2020): "A Unifying Approach to Measuring Climate Change Impacts and Adaptation," Tech. rep., National Bureau of Economic Research.

Borusyak, K., X. Jaravel, and J. Spiess (2021): "Revisiting event study designs: Robust and efficient estimation," *Unpublished working paper, version May*, 19, 2021.

Bünnings, C. and V. Schiele (2021): "Spring Forward; Don't Fall Back: The Effect of Daylight Saving Time on Road Safety," *Review of Economics and Statistics*, 103, 1–12.

Burke, M., S. M. Hsiang, and E. Miguel (2015): "Climate and conflict," .

Callaway, B. and P. H. Sant'Anna (2020): "Difference-in-differences with multiple time periods," *Journal of Econometrics*.

Chalfin, A., B. Hansen, J. Lerner, and L. Parker (2019): "Reducing crime through environmental design: Evidence from a randomized experiment of street lighting in New York City," Tech. rep., National Bureau of Economic Research.

Christen, E. (2017): "Time zones matter: The impact of distance and time zones on services trade," *The World Economy*, 40, 612–631.

Cole, S., X. Giné, and J. Vickery (2017): "How does risk management influence production decisions? Evidence from a field experiment," *The Review of Financial Studies*, 30, 1935–1970.

Czabanski, J. (2008): *Estimates of cost of crime: history, methodologies, and implications*, Springer Science & Business Media.

De Chaisemartin, C. and X. d'Haultfoeuille (2020): "Two-way fixed effects estimators with heterogeneous treatment effects," *American Economic Review*, 110, 2964–96.

de Oliveira, V. H., I. Lee, C. Quintana-Domeque, et al. (2021): "Natural Disasters and Early Human Development: Hurricane Catarina and Infant Health in Brazil," Tech. rep.

Dell, M. (2015): "Trafficking networks and the Mexican drug war," *American Economic Review*, 105, 1738–79.

Dell, M., B. F. Jones, and B. A. Olken (2014): "What do we learn from the weather? The new climate-economy literature," *Journal of Economic Literature*, 52, 740–98.

Deschênes, O. and M. Greenstone (2011): "Climate change, mortality, and adaptation: Evidence from annual fluctuations in weather in the US," *American Economic Journal: Applied Economics*, 3, 152–85.

Dillender, M. (2021): "Climate Change and Occupational Health Are There Limits to Our Ability to Adapt?" *Journal of Human Resources*, 56, 184–224.

Dolan, P. and T. Peasgood (2007): "Estimating the economic and social costs of the fear of crime," *British Journal of Criminology*, 47, 121–132.

Doleac, J. L. and N. J. Sanders (2015): "Under the cover of darkness: How ambient light influences criminal activity," *Review of Economics and Statistics*, 97, 1093–1103.

Domínguez, P. and K. Asahi (2019): “Crime time: How ambient light affects crime,” Tech. rep., IDB Working Paper Series.

Gibson, M. and J. Shrader (2018): “Time use and labor productivity: The returns to sleep,” *Review of Economics and Statistics*, 100, 783–798.

Giuntella, O., W. Han, and F. Mazzonna (2017): “Circadian rhythms, sleep, and cognitive skills: evidence from an Unsleeping giant,” *Demography*, 54, 1715–1742.

Giuntella, O. and F. Mazzonna (2019): “Sunset Time and the Economic Effects of Social Jetlag: Evidence from US Time Zone Borders,” *Journal of Health Economics*, 65, 210–226.

Goodman-Bacon, A. (2021): “Difference-in-differences with variation in treatment timing,” *Journal of Econometrics*.

Guéguen, N. and C. Jacob (2014): ““Here comes the sun”: Evidence of the Effect of Sun on Compliance to a Survey Request,” *Survey Practice*, 7.

Guéguen, N. and L. Lamy (2013): “Weather and helping: Additional Evidence of the Effect of the Sunshine Samaritan,” *The Journal of Social Psychology*, 153, 123–126.

Guéguen, N. and J. Stefan (2013): “Hitchhiking and the ‘sunshine driver’: Further effects of weather conditions on helping behavior,” *Psychological Reports*, 113, 994–1000.

Hajdu, T. and G. Hajdu (2021): “Temperature, Climate Change, and Human Conception rates: Evidence from Hungary,” *Journal of Population Economics*, 1–26.

Hamermesh, D. S., C. K. Myers, and M. L. Pocock (2008): “Cues for timing and coordination: latitude, letterman, and longitude,” *Journal of Labor Economics*, 26, 223–246.

Hattari, R. and R. Rajan (2012): “Sources of FDI flows to developing Asia: The roles of distance and time zones,” *China Economic Policy Review (CEPR)*, 01, 1–16.

Heaton, P. (2010): *Hidden in plain sight: What cost-of-crime research can tell us about investing in police*, Rand Corporation.

Horrace, W. C. and S. M. Rohlin (2016): “How dark is dark? Bright lights, big city, racial profiling,” *Review of Economics and Statistics*, 98, 226–232.

Janke, K., C. Propper, and M. A. Shields (2016): “Assaults, murders and walkers: The impact of violent crime on physical activity,” *Journal of Health Economics*, 47, 34–49.

Jessoe, K., D. T. Manning, and J. E. Taylor (2018): "Climate change and labour allocation in rural Mexico: Evidence from annual fluctuations in weather," *The Economic Journal*, 128, 230–261.

Jin, L. and N. R. Ziebarth (2015a): "Does Daylight Saving Time Really Make Us Sick?" *IZA Discussion Paper No.9088*.

——— (2015b): "Sleep and human capital: Evidence from daylight saving time," Tech. rep., HEDG, c/o Department of Economics, University of York.

Kamstra, M. J., L. A. Kramer, and M. D. Levi (2000): "Losing sleep at the market: The daylight saving anomaly," *American Economic Review*, 90, 1005–1011.

Kawamura, A., T. Yoshiike, A. Yoshimura, H. Koizumi, K. Nagao, Y. Fujii, M. Takami, M. Takahashi, M. Matsuo, N. Yamada, et al. (2019): "Bright light exposure augments cognitive behavioral therapy for panic and posttraumatic stress disorders: a pilot randomized control trial," *Sleep and Biological Rhythms*, 1–7.

Keller, M. C., B. L. Fredrickson, O. Ybarra, S. Côté, K. Johnson, J. Mikels, A. Conway, and T. Wager (2005): "A warm heart and a clear head: The contingent effects of weather on mood and cognition," *Psychological Science*, 16, 724–731.

Kikuchi, T. et al. (2006): "Time zones, outsourcing and patterns of international trade," *Economics Bulletin*, 6, 1–10.

Koppensteiner, M. F. and L. Menezes (2021): "Violence and human capital investments," *Journal of Labor Economics*, 39, 000–000.

Kountouris, Y. and K. Remoundou (2014): "About time: Daylight saving time transition and individual well-being," *Economics Letters*, 122, 100–103.

Kuehnle, D. and C. Wunder (2016): "Using the life satisfaction approach to value daylight savings time transitions: Evidence from Britain and Germany," *Journal of Happiness Studies*, 17, 2293–2323.

Kurlansik, S. L. and A. D. Ibay (2012): "Seasonal affective disorder," *American Family Physician*, 86, 1037–1041.

Lu, J. G., J. J. Lee, F. Gino, and A. D. Galinsky (2018): "Polluted morality: Air pollution predicts criminal activity and unethical behavior," *Psychological Science*, 29, 340–355.

Mak, B. K. and C. Y. Jim (2018): "Examining fear-evoking factors in urban parks in Hong Kong," *Landscape and Urban Planning*, 171, 42–56.

McCollister, K. E., M. T. French, and H. Fang (2010): "The cost of crime to society: New crime-specific estimates for policy and program evaluation," *Drug and Alcohol Dependence*, 108, 98–109.

Moore, S. and J. P. Shepherd (2006): "The cost of fear: shadow pricing the intangible costs of crime," *Applied Economics*, 38, 293–300.

Münch, M., A. E. Brøndsted, S. A. Brown, A. Gjedde, T. Kantermann, K. Martiny, D. Mersch, D. J. Skene, and A. Wirz-Justice (2017): "The effect of light on humans," *Changing perspectives on daylight: Science, technology, and culture*, 16–23.

Munyo, I. (2018): "Daylight saving time and crime: Does tiredness also affect criminal behavior?" *Journal of Applied Biobehavioral Research*, 23, e12115.

Ponomarenko, M. and B. Friedman (2017): "Benefit-Cost Analysis of Public Safety: Facing the Methodological Challenges," *Journal of Benefit-Cost Analysis*, 8, 305–329.

Ranson, M. (2014): "Crime, weather, and climate change," *Journal of Environmental Economics and Management*, 67, 274–302.

Rind, B. (1996): "Effect of Beliefs about Weather Conditions on Tipping," *Journal of Applied Social Psychology*, 26, 137–147.

Rind, B. and D. Strohmetz (2001): "Effect of Beliefs about Future Weather Conditions on Restaurant Tipping," *Journal of Applied Social Psychology*, 31, 2160–2164.

Rišová, K. and M. S. Madajová (2020): "Gender differences in a walking environment safety perception: a case study in a small town of Banská Bystrica (Slovakia)," *Journal of Transport Geography*, 85, 102723.

Sreetheran, M. and C. C. K. Van Den Bosch (2014): "A socio-ecological exploration of fear of crime in urban green spaces–A systematic review," *Urban Forestry & Urban Greening*, 13, 1–18.

Stearns, P. N. (2012): *American fear: The causes and consequences of high anxiety*, Routledge.

Stein, E. and C. Daude (2007): "Longitude matters: Time zones and the location of foreign direct investment," *Journal of International Economics*, 71, 96–112.

Sun, L. and S. Abraham (2020): "Estimating dynamic treatment effects in event studies with heterogeneous treatment effects," *Journal of Econometrics*.

Tandogan, O. and B. S. Ilhan (2016): "Fear of crime in public spaces: From the view of women living in cities," *Procedia Engineering*, 161, 2011–2018.

Tealde, E. (2021): "The Unequal Impact of Natural Light on Crime," *Journal of Population Economics (forthcoming)*.

Toro, W., R. Tigre, and B. Sampaio (2015): "Daylight Saving Time and incidence of myocardial infarction: evidence from a regression discontinuity design," *Economics Letters*, 136, 1–4.

——— (2016): "Ambient Light and Homicides," *Disponıvel em*.

Triguero-Mas, M., P. Dadvand, M. Cirach, D. Martínez, A. Medina, A. Mompart, X. Basagaña, R. Gražulevičienė, and M. J. Nieuwenhuijsen (2015): "Natural outdoor environments and mental and physical health: relationships and mechanisms," *Environment International*, 77, 35–41.

Twinam, T. (2017): "Danger zone: Land use and the geography of neighborhood crime," *Journal of Urban Economics*, 100, 104–119.

Umbach, R., A. Raine, and G. Ridgeway (2017): "Aggression and sleep: a daylight saving time natural experiment on the effect of mild sleep loss and gain on assaults," *Journal of Experimental Criminology*, 13, 439–453.

Velásquez, A. (2020): "The Economic Burden of Crime Evidence from Mexico," *Journal of Human Resources*, 55, 1287–1318.

Warthen, D. M., B. J. Wiltgen, and I. Provencio (2011): "Light enhances learned fear," *Proceedings of the National Academy of Sciences*, 108, 13788–13793.

Welsh, B. C. and D. P. Farrington (2008): "Effects of improved street lighting on crime," *Campbell Systematic Reviews*, 4, 1–51.

Welsh, B. C., D. P. Farrington, and B. R. Gowar (2015): "Benefit-cost analysis of crime prevention programs," *Crime and justice*, 44, 447–516.

Wickramasekera, N., J. Wright, H. Elsey, J. Murray, and S. Tubeuf (2015): "Cost of crime: A systematic review," *Journal of Criminal Justice*, 43, 218–228.

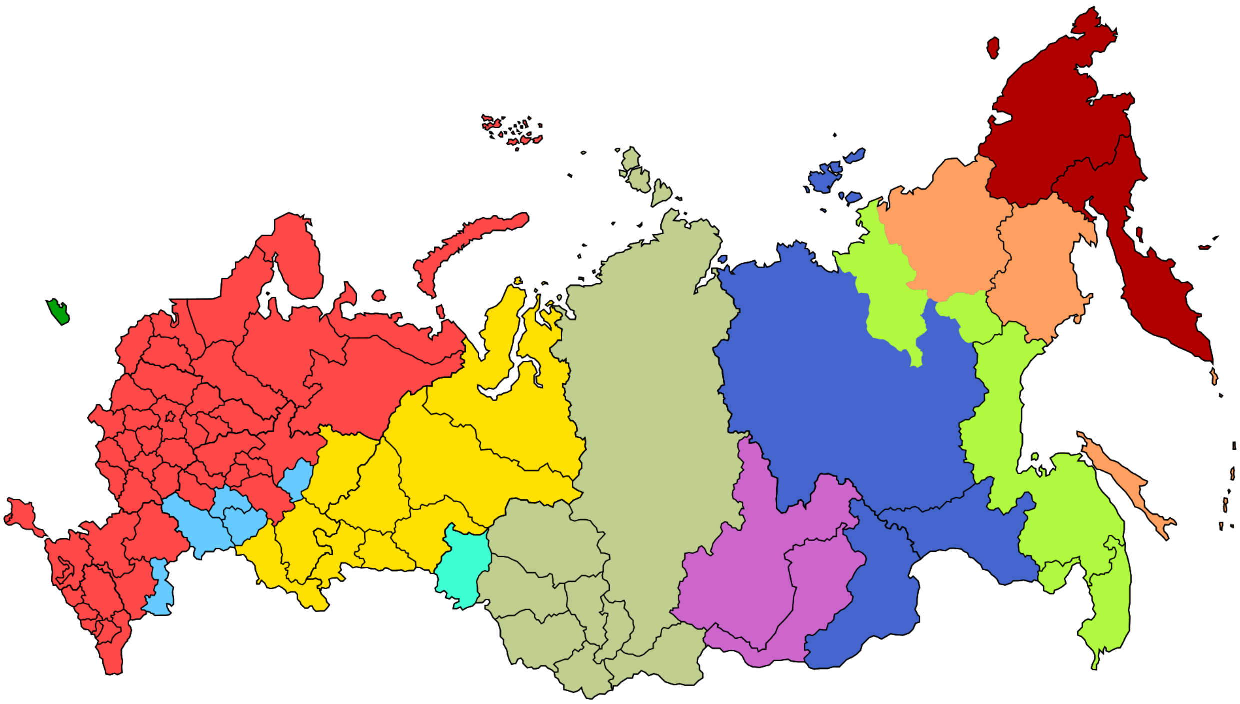
Wolff, H. and M. Makino (2012): "Extending Becker's time allocation theory to model continuous time blocks: Evidence from daylight saving time. IZA Discussion Papers 6787," *Institute for the Study of Labor (IZA)*.

Yoshiike, T., M. Honma, N. Yamada, Y. Kim, and K. Kuriyama (2018): "Effects of bright light exposure on human fear conditioning, extinction, and associated prefrontal activation," *Physiology & Behavior*, 194, 268–276.

Zhong, C.-B., V. K. Bohns, and F. Gino (2010): "Good lamps are the best police: Darkness increases dishonesty and self-interested behavior," *Psychological Science*, 21, 311–314.

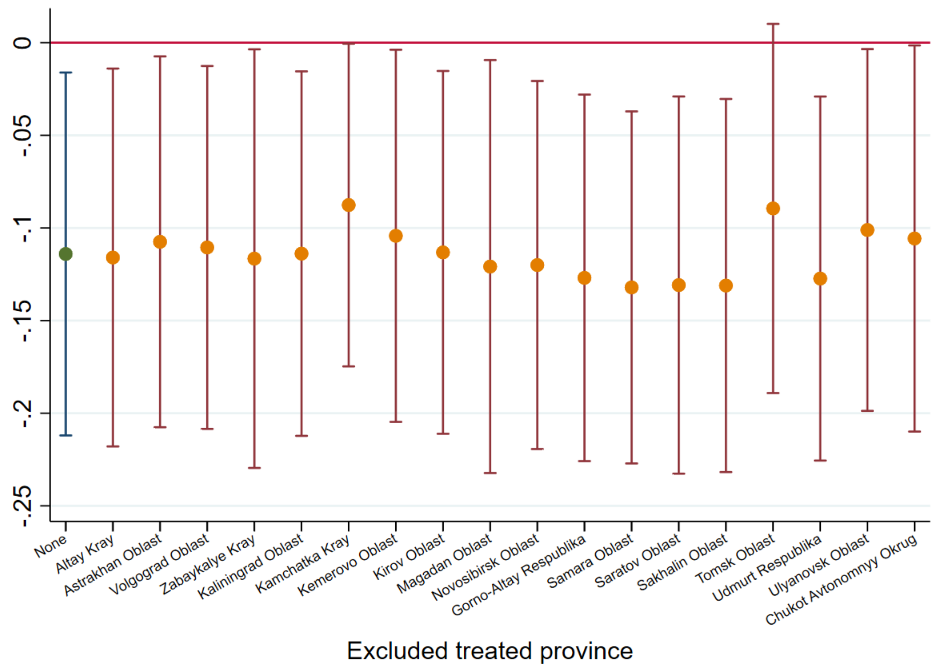
A Additional Figures

Figure A.1: Russian time zones in 2021



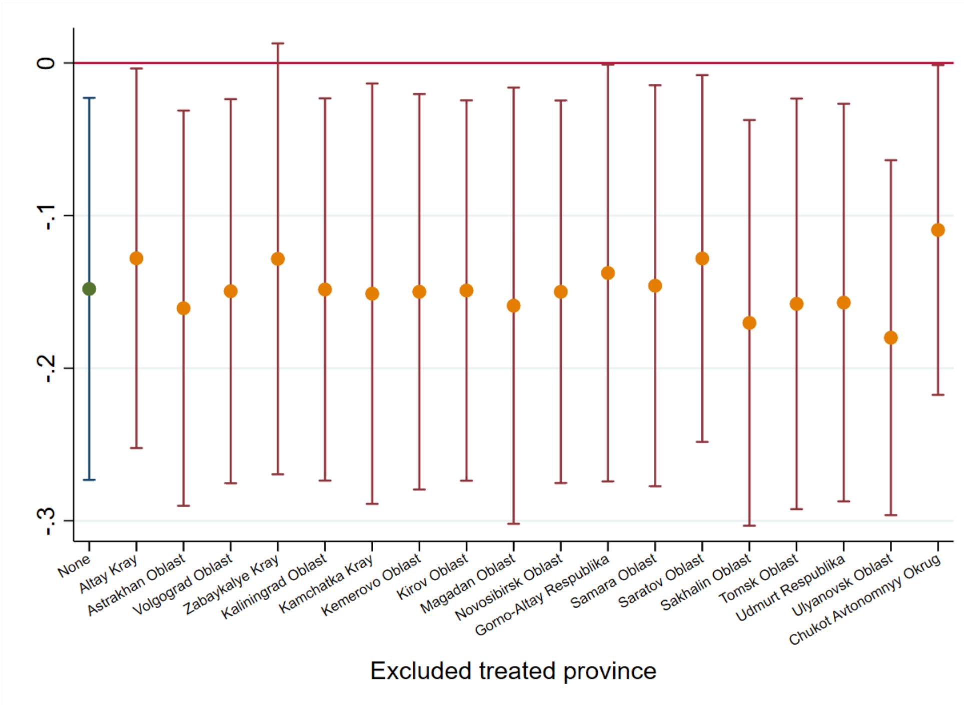
Time zones from west to east: KALT Kaliningrad Time UTC+2 (MSK–1), MSK Moscow Time UTC+3 (MSK), SAMT Samara Time UTC+4 (MSK+1), YEKT Yekaterinburg Time UTC+5 (MSK+2), OMST Omsk Time UTC+6 (MSK+3), KRAT Krasnoyarsk Time UTC+7 (MSK+4), IRKT Irkutsk Time UTC+8 (MSK+5), YAKT Yakutsk Time UTC+9 (MSK+6), VLAT Vladivostok Time UTC+10 (MSK+7), MAGT Magadan Time UTC+11 (MSK+8), PETT Kamchatka Time UTC+12 (MSK+9). The map includes Crimea, annexed by Russia in March 2014. The annexation is not recognized internationally. Attribution: Sycewicz, CC BY-SA 4.0, https://commons.wikimedia.org/w/index.php?curid=98116677.

Figure A.2: Sensitivity of the first lead treatment effect on robbery to exclusion of provinces



Note: The figure shows the coefficient of the first lag from Equation (2), where the dependent variable is log of robbery rate per 100,000 of population. The bars show the 95% confidence intervals. The first bar corresponds to the estimation using the full sample (column 3 in Table 3). Each of the other estimations employs the full dataset excluding one treated province.

Figure A.3: Sensitivity of the second lead treatment effect on robbery to exclusion of provinces



Note: The figure shows the coefficient of the second lag from Equation (2), where the dependent variable is log of robbery rate per 100,000 of population. The bars show the 95% confidence intervals. The first bar corresponds to the estimation using the full sample (column 3 in Table 3). Each of the other estimations employs the full dataset excluding one treated province.